Little Treatments, Promising Effects? Meta-Analysis of Single-Session Interventions for Youth Psychiatric Problems

Jessica L. Schleider, MA, AND John R. Weisz, PhD

Objective: Despite progress in the development of evidence-based interventions for youth psychiatric problems, up to 75% of youths with mental health needs never receive services, and early dropout is common among those who do. If effective, then single-session interventions (SSIs) for youth psychiatric problems could increase the accessibility, scalability, completion rates, and cost-effectiveness of youth mental health services. This study assessed the effects of SSIs for youth psychiatric problems.

Method: Using robust variance estimation to address effect size (ES) dependency, findings from 50 randomized-controlled trials (10,508 youths) were synthesized.

Results: Mean postintervention ES showed a Hedges *g* value equal to 0.32; the probability that a youth receiving SSI would fare better than a control-group youth was 58%. Effects varied by several moderators, including target problem: ESs were largest for anxiety (0.56) and conduct problems (0.54) and weakest for substance abuse (0.08; targeted in >33% of studies). Other problems yielded numerically promising but nonsignificant ESs (e.g., 0.21 for depression), potentially from low representation across trials. ESs differed across control conditions, with larger ESs for studies with no treatment (0.41) versus active controls (0.14); developmental periods, with greater ESs for children (0.42) than adolescents (0.19); intervention types, with largest ESs for youth-focused cognitive-behavioral approaches (0.74); and follow-up lengths, with smaller ESs for follow-ups exceeding 13 weeks. ESs did not differ for self- versus therapist-administered interventions or for youths with diagnosable versus subclinical problems.

Conclusion: Findings support the promise of SSIs for certain youth psychiatric problems and the need to clarify how, to what degree, and for whom SSIs effect lasting change.

Key words: single-session intervention, child mental health, intervention, meta-analysis

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Mental illness in children and adolescents (henceforth, "youths") is well-recognized as a major public health concern,[1,2] and the cost of youth mental illness (including health care, special education, juvenile justice, and decreased productivity) is estimated at $247 billion annually.[3,4] Accordingly, numerous interventions have been developed to treat and prevent youth psychiatric problems.[5,6] Although many have been identified as effective, they have not decreased rates of youth mental illness on a broad scale. In the United States, approximately 20% of youths develop a psychiatric disorder before 18 years of age—a rate that has persisted since at least the 1980s.[7-12]

There are several possible reasons for this discrepancy. Existing evidence-based interventions tend to be multisession, costly to administer, and inconsistently accessible to families in need. Up to 80% of youths experiencing psychiatric disorders go untreated every year.[13] Even among youths who do access services, dropout rates are high: the average treatment protocol for youth psychiatric disorders calls for 16.54 sessions,[5] but youths in outpatient clinics across the country attend an average of only 3.9 sessions.[14] Thus, it is critical to assess the promise of briefer, more accessible alternatives to traditional youth psychiatric services.

Recently, some researchers have begun to examine the effectiveness of single-session interventions (SSIs) designed to address youth psychiatric disorders and problems. Several narrative reviews have described SSIs for youths and adults.[15-17] Uniformly, these reviews—although not systematic—have concluded that SSIs might be capable of decreasing a wide variety of youth problems, including specific phobias,[18] disruptive behavior disorders,[19] and overall dysfunction in youth with multiple problems.[20] Consistent with this possibility, research has suggested that longer treatments do not always translate to superior clinical outcomes. In their meta-analysis of 447 randomized controlled trials (RCTs) of youth mental health treatments, Weisz *et al.*[5] found that the number of sessions specified in a treatment protocol was unrelated to the magnitude of that treatment's effect. Other meta-analyses have suggested that briefer interventions targeting parent–youth attachment difficulties[21] and youth conduct problems[22] might be more efficacious than lengthier ones. Such findings suggest the possibility that brief interventions might be capable of effecting significant clinical benefits, but a systematic meta-analysis is needed to clarify the strength of these benefits

Supplemental material cited in this article is available online.

and the conditions under which they are and are not found. The present study reports on the first such examination. We assessed whether, and to what degree, SSIs are effective, and whether their effectiveness varies as a function of youth problem type, demographic factors, prevention versus treatment programs, youth- versus parent-focused programs, and several other additional candidate moderators. Selection of candidate moderators was based on the meta-analysis of youth-focused treatment trials by Weisz *et al.*,[5] the most comprehensive existing meta-analysis, to our knowledge, of psychological treatments for internalizing, externalizing, and associated youth problems.

Given the lack of empirical reviews of the literature in this domain, we had no specific hypotheses for the magnitude of the effects of SSIs on youth mental health problems. Rather, the present meta-analysis was intended as an exploratory first step in gauging the promise of very brief approaches to decreasing and preventing youth psychopathology.

METHOD

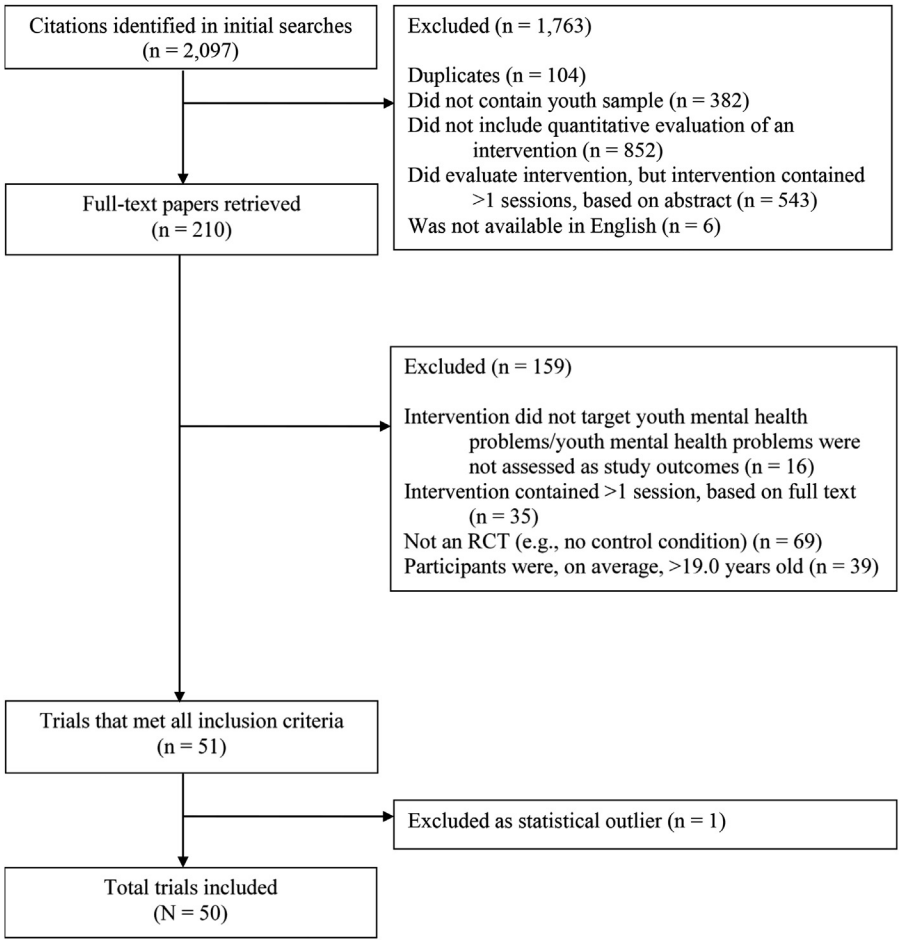
Search Strategy

We conducted searches in multiple bibliographic databases (PsychINFO, Eric, PubMed, ScienceDirect, Medline, and ProQuest) to identify peer-reviewed RCTs and unpublished dissertations describing SSIs for youth mental health problems (cutoff December 31, 2015). Search terms included *single-session, one-session, treatment, prevention, child, adolescent,* and *pediatric.* We also checked references of earlier narrative reviews and contacted researchers conducting work in this domain to maximize thoroughness in our search.

Inclusion and Exclusion Criteria

Criteria for study inclusion were English-language articles; youths randomly assigned to an SSI or a control condition, including no-treatment and waitlist and "active" controls (psychotherapy placebo or psychoeducation); mean youth age no older than 19.0 years; and outcome measurements administered to youths in the SSI and control conditions. We excluded trials in which SSI served as the control condition (e.g., SSI versus multisession cognitive-behavioral therapy [CBT]), because SSIs in these studies were often designed explicitly not to have therapeutic effects. We defined interventions as "single session" if they involved just 1 visit or encounter with a clinic, school, or program (1-day interventions involving multiple "modules" qualified under this definition). Treatment and prevention trials targeting any kind of psychiatric dysfunction were eligible for inclusion. Trials of SSIs designed to increase motivation for treatment or knowledge of psychiatric disorders were excluded unless intervention effects on psychiatric problems were empirically assessed. Figure 1 shows the study search and identification

FIGURE 1 Flowchart for study identification, screening, and inclusion. Note: RCT = randomized controlled trial.



flowchart. Initial study selection was conducted by the first author and 5 trained undergraduate research assistants; disagreements were resolved by discussion.

Data Extraction, Coding, and Processing

Studies meeting inclusion criteria were coded for study and sample characteristics, intervention procedures, quality indicators, and multiple candidate outcome moderators (Table 1). All studies were doubly coded by the first author and 1 of 5 trained undergraduate research assistants. To assess inter-rater agreement, research assistants independently coded 8 to 10 randomly selected studies each; the first author served as the master coder against which responses of other coders were compared. Disagreements were resolved by discussion, collaborative article review, and independent recalculation of effect sizes (ESs), when applicable.

We coded each study's year and location (within versus outside North America; $\kappa = 1.00$); study type, including 4 kinds of trials: treatment (trials specifically targeting youths with a diagnosed psychiatric disorder or clinically increased symptoms based on a standardized measurement), indicated prevention (targeting youths with increased symptoms but who are not necessarily experiencing disorders), selective prevention (targeting subgroups of youths at risk for developing disorder[s], e.g., healthy children of parents with depression), or universal prevention (targeting all youths regardless of symptoms or risk; $\kappa = 0.79$); sample recruitment strategy (community-referred versus clinic-referred sample; $\kappa = 0.92$); and percentage of Caucasian participants (intraclass correlation coefficient [ICC] = 0.82), percentage of each gender (ICC = 0.92), and mean participant age (ICC = 0.98), each of which was dichotomized for moderator analyses (Table 1). We coded target problem type at the study level (conduct and aggression, anxiety, depression—including suicidal behaviors and mood problems), eating problems, substance use, or other problems (including low self-efficacy or self-esteem and low perceived social support; $\kappa = 0.84$; no studies targeted attention-deficit/hyperactivity disorder) and at the ES level, because several studies measured secondary intervention effects on nonprimary problems (including all the aforementioned problems and family problems; $\kappa = 0.81$). We coded whether youths in each study were required to have increased symptoms to participate based on formal diagnoses or clinical cutoffs ($\kappa = 0.82$); informant type at the ES level (youths versus parent, therapist, or teacher; $\kappa = 0.93$); length of intervention, when specified (<60 versus >60 minutes; $\kappa = 0.95$); whether SSIs were self-administered or administered by the therapist ($\kappa = 0.92$); and number of weeks from baseline to each assessment point (ICC = 0.96), including all assessment points for which data from the intervention and control groups were available (i.e., those for which ESs were calculable).

We also coded intervention and control conditions ($\kappa = 0.79$). SSI type codes were collapsed into 4 categories: youth-focused behavioral interventions (e.g., CBT, graded exposure, behavioral activation, psychoeducation, or a combination), youth-focused non-behavioral interventions (e.g., attention bias modification; "growth mindset" intervention), caregiver- and family-focused behavioral intervention (e.g., family- or parent-directed behavioral parent training; no studies evaluated parent- or family-based non-behavioral interventions), and motivational interviewing (identified as its own category because of prevalence among included studies). No studies examined medication-based SSIs. Control conditions were no treatment or waitlist, psychotherapy placebo, and psychoeducation (including informational handouts); the latter 2 control types were combined into 1 active control group because of small numbers.

We coded methodologic quality variables used in previous systematic reviews of psychiatric RCTs[5,23] that were reported with sufficient frequency and clarity to be applied across most studies. These variables were subject blindness to intervention condition; participant attrition (percentage of participants at randomization available for ES computation); presence of pretherapy training (for therapist-administered interventions); and presence of treatment manual or structured guide. The moderating effects of these variables were assessed using procedures described below. Inter-rater reliability for study quality variables ranged from κ values equal to 0.81 to 1.00.

Analyses

For each comparison between SSI and a control or comparison group, the ES indicating the difference between the 2 groups at posttest and any follow-up points was calculated (Hedges *g*) to account for small sample bias.[24] In calculating ESs, we used only those measurements that explicitly assessed symptoms of psychiatric disorders or problems, including those expressed in family or peer relationships.

Virtually all included RCTs provided multiple ESs from the same participants, violating assumptions of independence in traditional meta-analyses.[23] To use all available data, we used robust variance estimation (RVE),[25] which supports the inclusion of dependent ESs (i.e., ESs nested within samples within and across time points) by correcting the study standard errors to account for associations between ESs from the same sample. Compared with alternative modeling approaches (e.g., multivariate meta-analysis or multilevel regression), RVE focuses only on adjusting the standard errors, thereby requiring fewer distributional assumptions and less computational power.[26] Therefore, primary and moderator analyses were conducted using an RVE meta-analysis macro for SPSS[25] using weighted, random-effects models to account for anticipated between-study variance. Publication bias was assessed with the Egger test for asymmetry of the funnel plot.[27]

Outlier Detection

Consistent with established standards,[28] we defined outliers as ESs more than 3 standard deviations from the population coefficient. One included study was identified as an outlier: its 3 ESs were 16.32 to 27.55 times larger than the population coefficient.[29] This study was excluded from analyses. No additional outliers were identified.

RESULTS

Study Selection and Inclusion

Of the 2,097 examined abstracts (1,993 after removal of duplicate records), 210 full-text articles were retrieved for further consideration. Of these, 159 were excluded (Figure 1). Fifty-one studies met the inclusion criteria, and 50 (47 published articles and 3 dissertations) were included in the analyses after the exclusion of 1 outlier, as described earlier.

Characteristics of Included Studies

Table S1 (available online) presents additional details.

The 50 trials included a total of 10,508 participants, and each had an average ES of 5.98. Detailed study characteristics are listed in Table 1. Most trials (74.0%) evaluated prevention programs, with most of these being indicated or

TABLE 1 Results of Moderator Analyses Based on Robust Variance Estimation Models of 299 Dependent Effect Sizes (ESs) From 50 Studies

			Subgroup Analysis			Moderator Test	
Moderator	**Studies, n**	**ESs, n**	**ES (*g*)**	**95% CI**		**Test Statistic**	***p* Value**
Study year	50	299				$t_{294} = -0.49$	.62
Study location	50	299				$t_{298} = 2.14$	.04
North America	29	156	0.17***	0.08	0.26		
Outside North America	21	143	0.48***	0.20	0.76		
Sample type	50	299				$t_{298} = 1.82$	.07
Community or non-referred sample	31	169	0.41***	0.17	0.65		
Clinical or referred sample	19	130	0.17**	0.06	0.29		
Youth ethnicity[a]	33	216				$t_{215} = 1.23$	.22
Caucasian sample (≥50% Caucasian)	14	89	0.29**	0.12	0.46		
Non-Caucasian sample (<50% Caucasian)	19	127	0.17***	0.10	0.24		
Youth gender	50	299				$t_{294} = 1.73$	.09
Majority male (≥50% boys)	15	74	0.25*	0.07	0.41		
Majority female (>50% girls)	35	225	0.49***	0.24	0.78		
Developmental period	50	299				$F_{2,296} = 1.56$	.21
Childhood (mean age ≤11 y)[b]	13	73	0.42***	0.22	0.62		
Early adolescence (mean age 11.01–15.50 y)	14	98	0.44*	0.03	0.85		
Mid to late adolescence (mean age 15.51–19 y)[b]	23	128	0.19*	0.05	0.31		
Diagnosis and symptom elevation requirement	50	299				$t_{298} = 0.09$	.92
Required of all participants	21	136	0.30***	0.14	0.46		
Not required	29	163	0.31***	0.09	0.53		
Targeted problem (ES level, to address the fact that many studies assessed intervention effects on multiple problem types)	50	299				$F_{6,292} = 4.41$	<.001
Externalizing or conduct[c]	11	29	0.52***	0.21	0.83		
Anxiety[d]	17	67	0.59***	0.30	0.88		
Depression[d]	16	41	0.21	−0.06	0.48		
Substance abuse[c,d]	18	84	0.08*	0.003	0.17		
Eating disorders	3	12	1.29	−0.33	2.93		
Family relationship problems[c,d,e]	8	34	0.21	−0.01	0.43		
Other problems[c,d]	10	32	0.13*	0.004	0.25		
Targeted problem (study level; based on primary problem targeted by intervention)	50	299				$F_{5,293} = 2.42$	.03
Externalizing or conduct[c]	5	38	0.54***	0.32	0.76		
Anxiety[d]	15	75	0.56***	0.24	0.88		
Depression[c,d]	6	48	0.18	−0.06	0.42		
Eating disorders	3	20	1.05	−0.46	2.55		
Substance abuse[c,d]	18	99	0.08*	0.002	0.15		
Other problems[c,d]	3	19	0.18	−0.03	0.38		
Informant[f]	49	299				$t_{292} = 0.26$	.82
Youth	46	225	0.28**	0.08	0.48		
Parent	14	68	0.30***	0.11	0.51		
Intervention type[g]	50	299				$F_{3,295} = 0.82$	.48
Youth-focused behavioral[c]	13	74	0.74*	0.21	1.27		
Youth-focused non-behavioral	12	64	0.26**	0.07	0.45		
Caregiver- and family-focused behavioral	9	51	0.31*	0.03	0.59		
Motivational interviewing[c]	16	110	0.11***	0.05	0.17		
Control condition	50	299				$t_{298} = -2.28$	.02
No treatment or waitlist	33	194	0.41***	0.20	0.62		
Psychotherapy placebo or psychoeducation	17	105	0.14*	0.026	0.252		
Intervention length	45	280				$t_{298} = 0.22$	.82
<60 min	26	143	0.29*	0.06	0.53		
>60 min	19	137	0.33***	0.16	0.48		

TABLE 1 Continued

Moderator	Studies, n	ESs, n	Subgroup Analysis			Moderator Test	
			ES (*g*)	95% CI		Test Statistic	*p* Value
Intervention administration[h]	50	299				$t_{298} = -1.27$	.21
Self-administered	11	73	0.21***	0.09	0.32		
Therapist-administered	43	226	0.33***	0.15	0.51		
Treatment vs. prevention	50	299				$F_{3,291} = 0.05$	.98
Treatment	13	70	0.41*	0.13	0.69		
Indicated prevention	17	104	0.25***	0.11	0.40		
Selective prevention	8	39	0.47	−0.21	1.16		
Universal prevention	12	86	0.33**	0.10	0.57		
Follow-up length	50	299				$F_{2,296} = 2.13$	.12
≤2 wk[c]	20	103	0.46***	0.21	0.72		
2.01–12.99 wk	25	131	0.31*	0.05	0.56		
≥13.00 wk[c]	13	65	0.07**	0.03	0.11		
Publication status	50	299				$t_{298} = 2.10$	.04
Peer-reviewed journal article	47	288	0.34***	0.18	0.48		
Unpublished dissertation	3	11	−0.08	−0.46	0.29		
Participant blindness to assessment	50	299				$t_{298} = 0.68$	.38
Blinded	10	58	0.23*	0.003	0.46		
Unblinded (if unreported, unblinded was assumed)	40	241	0.33***	0.15	0.51		
Pre-intervention therapist training	50	299				$F_{2,296} = 1.49$	.23
Training reported	35	224	0.26***	0.12	0.40		
No training reported	7	44	0.61	−0.09	1.31		
Training not applicable (e.g., computer-based intervention)	8	31	0.23***	0.10	0.36		
Participant attrition	50	299				$t_{294} = -0.64$	.38
<20%	35	214	0.32**	0.12	0.52		
≥20%	15	85	0.26***	0.15	0.39		

Note: Some moderators were missing for certain studies. Each study can contribute multiple ESs; thus, study sample size across subgroups can exceed the total study sample size for the ES-level moderators. g = Hedges g.

[a] *Seventeen studies provided no information on race or ethnicity of participants.*

[b] *Marginally significant (p < .10) pairwise differences between these subgroups.*

[c] *Within each moderator having more than 2 subgroups, identical superscript c indicates significant (p < .05) pairwise comparisons between subgroups.*

[d] *Within each moderator having more than 2 subgroups, identical superscript d indicates significant (p < .05) pairwise comparisons between subgroups.*

[e] *No study in the present sample targeted family relationship problems as a primary outcome; however, multiple studies measured outcomes in this domain.*

[f] *Only 1 study included teacher-reported measurements (representing 2 ESs) and only 2 studies included therapist-reported measurements (representing 4 ESs). Thus, this moderation test was restricted to ESs based on youth versus parent informants.*

[g] *The behavioral categories included behavioral and cognitive-behavioral intervention approaches.*

[h] *Some studies reported interventions with more than 2 conditions, including self-administered and therapist-administered interventions.*

*p < .05; **p < .01; ***p < .001.

selective preventive SSIs. Most trials tested therapist-administered SSIs, and 11 tested self-administered interventions (e.g., attention bias modification; "growth mindset" programs; self-affirmation interventions). Eighteen trials primarily targeted substance use, 15 targeted anxiety, 6 targeted depression, and no more than 5 targeted conduct problems, eating problems, and multiple or other mental health-related problems (e.g., child–parent relationship distress; any presenting mental health problem; peer violence and alcohol abuse, equally).

Risk of bias in studies was variable. In 40 included studies, participants were aware of their intervention condition or allocation concealment was not mentioned in the study; in 15 studies, attrition exceeded 20%; and some form of pre-intervention therapist training was reported in 35 studies. All studies reported using a treatment manual or structured guide for the SSIs they evaluated.

Overall SSI Effect

A weighted, random-effects meta-regression model using RVE tested the overall effect of SSIs compared with control conditions across 299 ESs. Mean ES was 0.32 (95% CI 0.17, 0.46, $p < .001$); the probability that a youth receiving SSI would fare better than a youth in a control group was 58%.[30] Heterogeneity statistics suggested significant between-study variance ($Q_{49} = 463.27$, $p < .001$, $\tau^2 = 0.29$). I^2 indicated that 89.42% of variance observed was true between-study variance. Thus, moderator analyses were conducted as planned.

Detailed moderation results are presented in Table 1; findings are summarized below.

Moderation by Youth Problem Type

SSI effects differed by youth problem type at the ES and study levels. At the ES level, effects were largest for anxiety ($g = 0.58$), followed by conduct problems ($g = 0.52$); differences between ESs for anxiety and conduct problems were nonsignificant. Smaller but still significant effects emerged for other problems (e.g., low self-esteem or self-efficacy; $g = 0.13$) and substance abuse ($g = 0.08$). Overall effects were nonsignificant for depression ($g = 0.21$, 95% CI −0.06 to 0.48) and family relationship problems ($g = 0.21$, 95% CI −0.01 to 0.43), owing in part to small study samples. In addition, an especially large but nonsignificant effect emerged for problems related to eating disorders ($g = 1.29$, 95% CI −0.53 to 2.93). Effects on anxiety and conduct problems were significantly larger than those for depression, substance abuse, family relationship, and other problems. These same patterns emerged when effects were assessed at the study level: SSIs that primarily targeted anxiety or conduct problems ($g = 0.56$) yielded significantly larger effects than those targeting depression ($g = 0.18$, 95% CI −0.06 to 0.42), substance abuse ($g = 0.08$, 95% CI 0.002–0.15), or other problems ($g = 0.18$, 95% CI −0.03 to 0.38).

Moderation by Youth Problem Severity

Across multiple between-studies indicators, severity of youths' pre-intervention problems did not moderate SSI effects. Specifically, effects did not differ for clinic-referred (i.e., diagnosed) versus community youth samples; in studies that required youths to present with a diagnosis or increased symptoms versus those that did not; or for treatments (i.e., trials for youths with psychiatric diagnoses) versus preventive interventions (which did not require diagnoses), regardless of preventive intervention type (indicated, selective, or universal).

Moderation by Demographic Factors

Between-studies analyses indicated significantly larger effects for studies conducted outside North America ($g = 0.48$) than for those conducted within North America ($g = 0.15$; this discrepancy could have emerged because most substance abuse trials, which had smaller overall ESs, were conducted within North America) and marginally greater effects for children no older than 11 years ($g = 0.42$) than for adolescents older than 15.5 years ($g = 0.19$). However, overall effects were significant for studies conducted within and outside North America and for all age groups assessed. No significant differences emerged by majority youth gender or race or ethnicity.

Moderation by Intervention Factors

All identified intervention types yielded significant effects, but the magnitude of these effects varied considerably. Youth-focused behavioral SSIs had the largest overall effect ($g = 0.74$). Youth-focused non-behavioral ($g = 0.26$) and caregiver- and family-focused behavioral SSIs ($g = 0.31$) yielded numerically smaller effects, but neither differed statistically from the effect of youth-focused behavioral SSIs. Motivational interviewing SSIs yielded the smallest overall effect ($g = 0.11$). This effect was significantly smaller than that of youth-focused behavioral SSIs but did not differ significantly from effects of other intervention types.

Regarding intervention administration strategy, therapist-administered SSIs yielded a numerically larger effect ($g = 0.33$) than did self-administered SSIs ($g = 0.21$), but these effects did not significantly differ from one another. Separately, no differences emerged between SSIs that were briefer versus longer than 60 minutes ($g = 0.29$ and 0.32, respectively).

Moderation by Control Condition Type

Type of control condition significantly moderated overall effects. Effects were largest for trials with no-treatment or waitlist control conditions ($g = 0.40$) and significantly smaller—but still significantly larger than 0—for trials with an active control condition (psychosocial placebo or brief psychoeducation; $g = 0.14$).

Moderation by Follow-Up Length

Follow-up length did not emerge as a significant overall moderator. However, specific contrasts indicated that some effects differed by follow-up length. Largest effects emerged for studies with follow-ups spanning 0 days (i.e., immediately after an intervention) to 2 weeks ($g = 0.46$). Effects were numerically but nonsignificantly smaller for follow-ups spanning 2.01 to 12.99 weeks ($g = 0.31$) and smallest for follow-ups exceeding 13 weeks ($g = 0.07$). Specific contrasts indicated that effects at least 13 weeks after intervention were significantly smaller than those observed 0 to 2 weeks after intervention but nonsignificantly smaller than those 2.01 to 12.99 weeks after intervention.

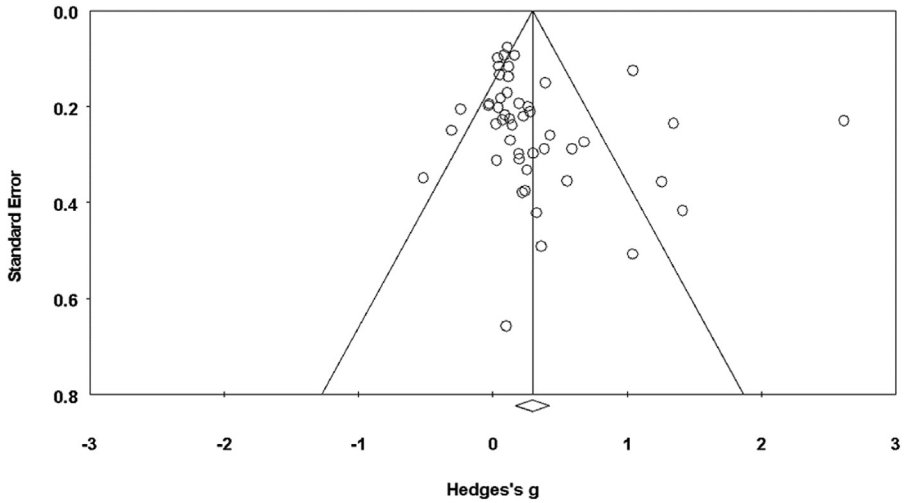
Moderation by Informant

Effects did not differ as a function of youth-reported versus parent-reported youth intervention outcomes.

Study Quality and Publication Bias

Overall effects did not differ significantly as a function of attrition rate (>20% versus ≤20%), the presence of therapist training (for therapist-administered SSIs), or subject blindness to intervention condition. Effects were larger for published peer-reviewed trials ($g = 0.33$) than for trials in unpublished dissertations ($g = -0.08$). However, this difference was nonsignificant ($p = .05$), and only 3 of the 50 studies in the present meta-analysis were unpublished dissertations. For publication bias, the slope of the Egger regression line was nonsignificant ($t_{49} = 1.57$, $p = .12$). Visual inspection of the funnel plot (Figure 2) suggests some asymmetry, with more precise trials tending to yield smaller ESs. However, these high-precision trials also tended to be motivational interviewing interventions for substance abuse, suggesting that any asymmetry might be due to the lower efficacy of these specific intervention types rather than to systematic bias.

FIGURE 2 Funnel plot with 95% CIs (diagonal lines). Note: The *y*-axis shows the standard error of each study mean. The *x*-axis shows the magnitude of each study's mean weighted effect size. Study values are distributed relatively symmetrically around the mean effect size based on a nonsignificant slope ($t_{48} = 1.57$, $p = .12$). Some asymmetry was observed based on visual inspection, which seemed to be driven by smaller effect sizes among more precise studies.



DISCUSSION

We conducted the first comprehensive, systematic meta-analysis, to our knowledge, evaluating SSIs for youth psychiatric problems. Across 50 RCTs representing 10,508 youths, SSIs demonstrated a significant beneficial effect in the small-to-medium range ($g = 0.32$). Effects were consistent regardless of youth problem severity and diagnostic status, suggesting the effectiveness of SSIs in youths with psychiatric disorders and subclinical problems. Practical implications of these results could be considerable: in some cases, SSIs could present a cost-effective alternative or adjunct to traditional youth psychiatric services, which are often inaccessible to youths in need. Several subgroup differences emerged that contextualize overall results and suggest directions for further study.

SSIs were most effective in decreasing anxiety and conduct problems ($g = 0.58$ and 0.52), the 2 most prevalent types of youth psychiatric disorders.[31] In contrast, SSIs targeting youth depression ($g = 0.21$) and eating disorders ($g = 1.29$) had numerically promising but nonsignificant overall effects, and the effect of substance abuse SSIs—although statistically significant—fell well below the "small" ES threshold ($g = 0.08$). Several mean effects are strikingly similar in magnitude to meta-analytic ESs of full-length youth psychological interventions: consistent with the present results, Weisz *et al.*[5] found small-to-medium ESs for treatments targeting youth anxiety ($d = 0.61$) and conduct problems ($d = 0.46$) than for depression ($d = 0.29$). Recent systematic reviews have found mixed evidence on the effectiveness of substance abuse interventions for adolescents and adults,[32-34] and meta-analyses have identified single-session programs for eating disorders (across age groups, including adults) as less effective than multisession approaches.[35] SSIs for youth anxiety and conduct disorders might yield larger effects for several possible reasons. First, concrete therapeutic strategies have been linked with treatment success in anxiety (e.g., graded exposure) and conduct problems (e.g., labeled praise, active ignoring, and timeout). Second, anxiety and conduct problems tend to occur earlier than depression, eating disorders, and substance abuse. Our findings suggest that younger children could respond somewhat better to SSIs than older adolescents—perhaps because they have had less time for maladaptive behaviors and beliefs to solidify. This could render SSIs more effective for younger youths. Third, depression, substance use, and eating disorders are characterized by motivational difficulties that can interfere with intervention engagement (anhedonia in depression; variable motivation for change in substance use and eating disorders). Thus, more intensive or extended efforts might be required to effectively address these problems.

However, closer analysis might be needed to fully understand the implications of our findings regarding depression. The mean effect of 0.21 might not reflect the full potential of depression-focused SSIs. Only 6 of the 50 trials targeted depression (ensuring a poorly powered test of significance); 2 were unpublished dissertations, 4 targeted older adolescents, and 3 used non-behavioral approaches—all factors linked to weaker outcomes. Future trials exploring the effect of behaviorally oriented SSIs for youth depression could clarify the promise of such interventions.

Separately, the significant but very small effects of SSIs targeting youth substance use might suggest the need for alternative intervention approaches in this domain. One possibility might be to scale-up dissemination of SSIs for youth anxiety and conduct problems, which are prominent risk factors for adolescent substance use.[36-38] Improving access to SSIs for these problems might have the secondary

benefit of decreasing substance use over time. Separately, it might be helpful to explore intervention approaches beyond motivational interviewing, a popular, widely used substance abuse intervention strategy that nonetheless emerged as an especially weak approach. Moreover, SSIs, regardless of approach, might be an inappropriate strategy for decreasing youth substance abuse, which can be extremely complex to treat. Future research on accessible follow-up and extended support strategies might produce significant clinical payoff in this domain.

Overall SSI effects waned over time, particularly for follow-ups exceeding 13 weeks. Notably, 9 of the 13 studies with follow-ups exceeding 13 weeks targeted substance abuse or depression, which might have accounted in part for smaller follow-up effects. Future investigations should assess whether accessible booster strategies might improve the durability of the effects of SSIs.

An important function of meta-analyses is identifying factors that are not associated with outcome differences. For example, in this meta-analysis, effects of therapist- and self-administered SSIs did not significantly differ. This result did not appear to be an artifact of trial design or problem type: of 11 self-administered SSIs in this meta-analysis, 8 were compared with active controls and 6 targeted depression or substance use, which were less responsive to SSIs overall. Relational factors, such as therapeutic alliance, are often posited to be active ingredients in the effects of traditional psychotherapy. However, there is little opportunity for the therapist–client relationship to develop in SSIs; accordingly, their specific content could be central to their efficacy. Given the potential of self-administered SSIs to decrease monetary costs and improve accessibility of youth psychological services, future research should prioritize evaluating their potential and readiness for broad dissemination.

Limitations of this meta-analysis suggest directions for future research. First, most SSIs in this collection were compared with rather weak control conditions (e.g., psychotherapy placebo). Future trials of SSIs should incorporate potent, active controls (e.g., usual clinical care; direct comparisons with multisession CBT) to more rigorously test the effects of SSIs. Second, most included trials relied exclusively on youth-reported outcomes. Incorporating multi-informant, multimethod assessments, including behavioral tests, data from trained observers, and biobehavioral metrics of intervention effects, could enrich future SSI outcome research. Third, although SSIs were generally more effective for younger children than for older adolescents, this effect might differ by target problem type. Similarly, although overall effects of prevention and treatment SSIs did not differ significantly, such differences could emerge within certain problem domains. We were unable to test these possibilities in the present meta-analysis: within each problem type, there were often too few studies in each age category, or of specific program types (universal prevention; selective prevention; indicated prevention; treatment), for ESs to be calculated reliably (e.g., n = 0–2). As the SSI literature grows, it will be increasingly possible, and potentially valuable, to test these and other problem-specific interaction effects. Fourth, certain psychiatric problems (e.g., depression, eating disorders) were poorly represented among included studies. Additional studies are needed to clarify the promise of SSIs targeting these domains.

In sum, SSIs could be effective in decreasing youth psychiatric dysfunction, particularly anxiety and conduct problems. Overall effects observed for SSIs were slightly smaller than those observed for multisession youth psychotherapy,[5] but their potential for scalability could magnify their benefits on a large scale. Future research should continue to evaluate the limits of SSIs and test candidate solutions. &

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Ms. Schleider and Dr. Weisz are with Harvard University, Cambridge, MA.

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Correspondence to Jessica L. Schleider, MA, Psychology Department, Harvard University, 33 Kirkland Street, Cambridge, MA 02138; e-mail: jschleider@fas.harvard.edu

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