
The Lab Versus the Clinic

Effects of Child and Adolescent Psychotherapy

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Four recent meta-analyses, involving more than 200 controlled outcome studies, have shown consistent evidence of beneficial therapy effects with children and adolescents. However, most of the studies involved experimental procedures, nonreferred subjects, specially trained therapists with small caseloads, and other features that may not represent conventional clinic therapy. Research focused on more representative treatment of referred clients in clinics has shown more modest effects; in fact, most clinic studies have not shown significant effects. Interpretation of these findings requires caution; such studies are few and most could profit from improved methodology. The clinic studies do raise questions as to whether the positive lab findings can be generalized to the clinics where most therapy occurs; however, the lab interventions that have worked so well may point the way to enhanced therapy effects in clinics.

At any one time, about 12% of the more than 63 million children and adolescents (referred to in this article as children) in the United States suffer from serious behavioral or emotional problems (Institute of Medicine [IOM], 1989; Saxe, Cross, & Silverman, 1988). Of these children, about 2.5 million receive treatment (Office of Technology Assessment, 1986), at an annual cost of more than \$1.5 billion (IOM, 1989). Beyond the financial cost, families of these children invest considerable energy and time, and mental health professionals devote entire careers, to the treatment process. How effective is the therapy provided at such great cost to the youngsters, their families, the therapists, and society? In this article we address that question.

Evidence From Meta-Analyses

From the perspective of controlled therapy outcome studies, evidence on the efficacy of child therapy appears to be quite positive. Probably the most efficient and objective means of summarizing the findings of such studies is the technique known as meta-analysis (see Mann, 1990; Smith, Glass, & Miller, 1980; but see also critiques, e.g., Wilson, 1985). Meta-analysis provides a means of quantitatively aggregating the findings of independent studies; it permits summary statements about a body of literature, including statements about the average effect of psychotherapy across studies, expressed in the form of a mean

effect size (ES). In this article we briefly review the findings of major child and adolescent meta-analyses; for a more detailed review, see Weisz and Weiss (in press).

Casey and Berman (1985)

In the first child meta-analysis, Casey and Berman (1985) reviewed 75 outcome studies, published between 1952 and 1983, and focused on children aged 12 and younger. The children were treated for such social adjustment difficulties as aggression or withdrawal (40% of the studies), hyperactive or impulsive behavior (13%), and phobias (12%), and such somatic problems as obesity and enuresis (4%). The interventions used included behavioral and cognitive-behavioral methods (56% of the studies) and nonbehavioral methods (48%), including client-centered and psychodynamic approaches. ES values were calculated by computing the mean posttherapy treatment-control group difference on outcome measures and dividing that difference by the pooled within-group standard deviation; ES was thus expressed in standard deviation units, with higher scores indicating more positive therapy effects (as in all the meta-analyses reviewed here). Across the 64 studies that included treatment-control comparisons, the mean ES was 0.71; the average treated child functioned better after treatment than 76% of control group children, averaging across outcome measures.

Weisz, Weiss, Alicke, and Klotz (1987)

In a second meta-analysis, Weisz, Weiss, Alicke, and Klotz (1987) included 105 outcome studies¹ published between 1952 and 1983, focused on children aged 4–18 years. Some 47% of the studies involved treatment of external-

Bernadette Gray-Little served as action editor for this article.

The work reported in this article was supported in part through grants from the National Institute of Mental Health (5 RO1 MH38240, 1 RO3 MH34652) to John R. Weisz. For a more detailed treatment of the issues raised here, and a more detailed description of the studies and meta-analyses reviewed, see the book, *Effects of Psychotherapy With Children and Adolescents* (Weisz & Weiss, in press).

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¹ In Weisz et al. (1987) we erroneously reported that 108 studies had been included; three studies that had been dropped for failure to meet our methodological criteria were mistakenly retained in our bibliography.

izing or *undercontrolled* problems (19% hyperactivity or impulsivity, 12% delinquency, 11% aggressive or undisciplined behavior, and 6% noncompliance); 42% of the studies involved treatment of internalizing or *overcontrolled* problems (24% phobias and anxiety and 17% social withdrawal or isolation); other treated problems included more difficult to classify "adjustment-emotional disturbance" (6%) and underachievement (6%). Across all the treatment groups of all the studies, 77% involved behavioral interventions and 17% nonbehavioral, with the remainder described too vaguely to be classified. ES values were calculated by dividing the posttreatment therapy-control group difference by the control group standard deviation. Across the 105 studies surveyed, the mean ES was 0.79: The average treated child functioned better after treatment than 79% of the control group. Weiss and Weisz (1990) subsequently analyzed the impact of various methodological factors on the ES values obtained in these studies.

Kazdin, Bass, Ayers, and Rodgers (1990)

A third meta-analysis, by Kazdin, Bass, Ayers, and Rodgers (1990), actually began with a broad survey of 223 treatment studies published between 1970 and 1988, involving youngsters aged 4–18 years. Externalizing problems were the focus of treatment in 47% of the studies, internalizing problems in 16%, both externalizing and internalizing in 3%, and learning-academic problems in 15%. The interventions involved some form of behavior modification in 50% of the studies, cognitive-behavioral methods in 22%, group therapy in 9%, client-centered therapy in 5%, play therapy in 5%, and family therapy in 4%. ES values were calculated with procedures similar to those of Casey and Berman (1985) for the subset of 105 studies involving treatment-control comparisons. That subset was further subdivided. For the 64 studies involving comparisons between a treatment group and a *no-treatment* control group, the mean ES was 0.88; after treatment the average treated child was better off than 81% of the no-treatment youngsters. For the 41 studies comparing treatment and *active* control groups, the mean ES was 0.77; after treatment the average treated child was functioning better than 78% of the control group youngsters.

Weisz, Weiss, Morton, Granger, and Han (1992)

We have preliminary data to report from a meta-analysis by Weisz, Weiss, Morton, Granger, and Han (1992), based on 110 studies published between 1967 and 1991. The children, aged 2–18 years, were treated for externalizing problems in 40% of the studies (e.g., 29% self-control problems, 19% aggression, 19% delinquency, 6% noncompliance), for internalizing problems in 30% (e.g., 44% headaches and other somatic problems, 33% phobias and anxiety, 14% depression), and for other problems in 29% (e.g., 31% poor social relationships, 9% personality and self-concept problems). Behavioral treatments were used in 79% of the studies (e.g., 22% cognitive-behavioral, 18% respondent, 10% operant, 7% modeling), nonbehavioral

in 15% (e.g., 39% insight-oriented, 39% discussion groups, 17% client-centered), and mixed approaches in 7%. ES, calculated by the same method as in Weisz et al. (1987), averaged 0.71 across the 110 studies; after treatment, the average treated child was functioning better than 76% of control group children.

Specially Focused Meta-Analyses

In addition to the four broad-based child meta-analyses just described, we know of three more narrowly focused contributions. Hazelrigg, Cooper, and Borduin (1987) reviewed outcome studies of *family therapy* involving at least one parent and one child. Focusing on the 10 studies that compared family therapy to no-treatment control groups, Hazelrigg et al. found an ES of 0.45 (average treated case scoring better than 67% of the no-treatment group) on family interaction measures, and 0.50 (average treated case scoring better than 69% of no-treatment group) on behavior rating measures of outcome. Durlak, Fuhrman, and Lampman (1991) reviewed 64 studies of *cognitive-behavioral therapy* with 4–13-year-olds. The mean ES was 0.56; after therapy, the average treated child scored better on outcome measures than 71% of control group peers. Children aged 11–13 years (labeled "formal operational" by the authors) were more positively affected by these cognitive interventions (ES = 0.92) than were children aged 5–7 years (0.57) or 7–11 years (0.55). Finally, Russell, Greenwald, and Shirk (1991) addressed a dimension that is often interwoven with child psychopathology and treatment: *language proficiency*. They reviewed 18 child treatment studies (26 treatment-control group comparisons) that included at least one language measure. Across these studies, the mean ES for language change was 0.32; after therapy the average treated child outscored 63% of the untreated controls on language measures.

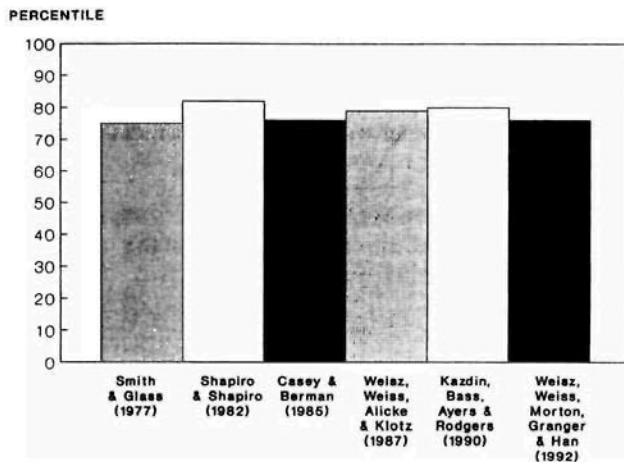
Adult and Child Meta-Analytic Findings

Figure 1 summarizes results (in percentile form) of the four broad-based child therapy meta-analyses and of two widely cited meta-analyses of predominantly adult psychotherapy studies (Shapiro & Shapiro, 1982; Smith et al., 1980). The figure shows that the mean effects reported in child meta-analyses are quite comparable to those of adult meta-analyses, and that both sets of findings point to quite positive effects of therapy. ES values ranged from 0.71 to 0.84, hovering near J. Cohen's (1988) threshold of 0.80 for a "large" effect.

Limitations of the Meta-Analytic Data: Research Therapy Versus Clinic Therapy

Because the meta-analyses reviewed above indicate consistently positive effects, and because they reflect such a large number of distinct outcome studies ($N > 200$) and subjects ($N > 12,000$), it may be tempting to conclude that the evidence on child therapy effects is now in, and that the news is very good. Such a conclusion may be premature. To explain why, we need to examine the evidence that forms the basis of the meta-analyses.

Figure 1
Findings of Five Meta-Analyses of Psychotherapy Outcome Research



Note. Numbers on the vertical axis reflect the posttherapy percentile standing of treated groups relative to control groups (i.e., the percentage of control group subjects scoring lower than treated subjects, pooling across outcome measures). Smith and Glass (1977) included studies of children, adolescents and adults; Shapiro and Shapiro (1982) included only studies of adults; the four remaining reports focused exclusively on child and adolescent studies. The Kazdin et al. (1990) bar represents our estimate from Kazdin et al.'s (1990) report. Kazdin et al. actually reported separate effect sizes of 0.88 for 64 treatment vs. no-treatment comparisons, and 0.77 for 41 treatment vs. active control group comparisons; we estimate a pooled effect size of 0.84, with a pooled percentile of 0.80. The bar for Weisz et al. (1992) shows results of a preliminary analysis.

Most of the studies included in the meta-analyses (particularly the behavioral studies and studies of the past decade) involved children, interventions, and treatment conditions that were relatively unrepresentative of conventional clinical practice. As noted by Weisz and Weiss (1989), in the majority of these controlled experimental studies, (a) youngsters were recruited for treatment, not actually clinic referred, which suggests that some were not seriously disturbed; (b) samples were selected for homogeneity, with all youngsters displaying a similar focal problem or problems (e.g., a specific phobia, or out-of-seat behavior in school); (c) therapy addressed the focal problems primarily or exclusively; (d) therapists were trained immediately before therapy in the specific techniques they would use; and (e) the therapy involved primary or exclusive reliance on those techniques. Moreover, (f) therapy was often guided by a structured manual and was monitored regularly for its integrity and adherence to a treatment plan. Such features tend to coalesce around a genre that might be termed *research therapy*.

In most conventional *clinic therapy*, by contrast, (a) the clients are seriously disturbed enough to have been referred to clinics; (b) treated youngsters are heterogeneous, with many problems and disorders represented in a therapist's caseload, and most youngsters referred for multiple problems; (c) therapy is usually directed not at

one focal problem but at a range of problems; (d) therapists are not likely to have the luxury of intensive recent training in all or most of the techniques they use; (e) therapy cannot be properly limited to a few techniques on which the therapists can concentrate all their energies; and (f) neither treatment manuals nor systematic external monitoring of treatment integrity are common. More generally, therapists typically carry large and diverse caseloads, have heavy paperwork requirements, and face other distractions from their therapeutic work. Thus, conditions in most child clinics may be rather different from (and less optimum than) the conditions typically arranged for controlled outcome studies (see also Persons, 1991).

Other differences between child clinical practice and child therapy outcome research have been noted by Kazdin et al. (1990). In a survey of 223 child outcome studies, Kazdin et al. found that child therapy research, unlike clinical practice, tends to (a) focus on children recruited from and treated in schools; (b) use group approaches rather than individual treatment; (c) use behavioral and cognitive-behavioral methods rather than psychodynamic, eclectic, and family-oriented approaches; (d) emphasize brief interventions averaging 8–10 weeks (vs. 27–55 weeks in clinical practice); (e) deemphasize involvement of parents and other family members; and (f) deemphasize consultation with teachers.

In several respects, then, the procedures and conditions associated with conventional clinic-based therapy for children are different from those typically arranged for outcome research. It is thus unclear whether the findings of meta-analyses, based on that research, can be generalized to the clinic-based therapy that is most often provided to disturbed children in the United States. This suggests that an important transcontextual validity question (Weisz, 1978) remains to be answered: Are the benefits of therapy as demonstrated in controlled outcome studies matched by the benefits of actual clinical practice with children?

Evidence From Clinic-Based Studies

Although answering this question may appear to be a rather straightforward task, a significant obstacle stands in the way of evaluating the effects of clinic-based therapy with referred children: Legal and ethical constraints almost always prevent clinics from randomly assigning treatment applicants to no-treatment control conditions. Thus, the rigorous comparisons of randomly selected treatment and no-treatment control groups that are the forte of controlled, experimental outcome studies are rarely possible in clinic settings.²

² It is sometimes possible to randomly assign children to receive or not receive *particular* treatments *in addition to* the regular treatment program that *all* clinic children receive; what we refer to here are studies involving random assignment to standard clinic treatment versus no-treatment or active placebo conditions. It is also sometimes possible to quasi-randomly assign clinic applicants to waiting-list control groups. However, this approach has limitations. In most clinics, wait-list assign-

To address this problem, researchers have tried to develop outcome assessment methods that do not require random assignment of clinic applicants to control groups. An early step in this direction was taken by Eysenck (1952) in a study of adult therapy effects. He compared reported rates of improvement across 19 studies of psychotherapy with "neurotic" adults to estimated base rates of improvement in studies of neurotic adults treated more custodially in state hospitals. Treated adults' rate of improvement fell below the hospital base rate of 72% improvement, leading Eysenck to conclude that "The figures fail to support the hypothesis that psychotherapy facilitates recovery from neurotic disorder" (p. 323; But see critiques of Eysenck's study, e.g., Rosenzweig, 1954).

Levitt's (1957b) cross-study comparison. Levitt (1957b) followed Eysenck's (1952) general procedure to estimate the effects of therapy for "neurotic" children. The base rate of improvement without treatment was estimated from two follow-up studies of children who had dropped out after being accepted for clinic treatment. To this base rate, Levitt compared the improvement rates reported for treated children in 18 published reports of outcome at treatment termination and 17 published reports of outcome at follow up. The improvement rate was 72.5% for untreated children, 74% for treated children. These results, Levitt concluded, "fail to support the view that psychotherapy with 'neurotic' children is effective" (p. 195). Levitt's report sparked several critiques (e.g., Barrett, Hampe, & Miller, 1978; Eisenberg & Gruenberg, 1961; Halpern, 1968; Heinicke & Goldman, 1960; Hood-Williams, 1960). Several criticisms were related to the fact that Levitt had estimated base rates of improvement for treated and untreated groups from different studies, and thus different clinic settings and different population bases. Because comparisons were made across (rather than within) studies, it was not possible to assess directly the demographic, developmental, or clinical similarity of the treated and untreated groups being compared. The comparison was thus rather indirect, and the possibility of initial uncontrolled group differences necessarily remained unexamined. Other clinic-based studies, though, have provided more direct and controlled comparisons of treated and untreated groups. We turn now to those studies, reflecting three different research strategies.

Treated children versus matched children in the general population: Shepherd, Oppenheim, and Mitchell (1966). One approach, taken in a British study by Shepherd, Oppenheim, & Mitchell (1966), was to form matched pairs of children treated in clinics and unreferred, untreated children identified through a general population survey. Some 50 pairs, aged 5-15 years, were matched for demographic and clinical characteristics (e.g., problem profiles). Clinician ratings of problem severity

showed no reliable differences between the two groups, but a few differences were found among parent variables (e.g., clinic mothers, more than nonclinic mothers, thought their children's problems required professional help). Shepherd et al. compared adjustment in the two groups about two years after the initial assessments. In-home clinical interviews were conducted, and improvement ratings were made by trained judges who were unaware of the clinic-nonclinic status of the children. The ratings indicated that 63% of clinic cases had improved over the two years, and 61% of the nonclinic children had improved. Moreover, among clinic children, number of treatment sessions bore no relation to whether children improved or deteriorated. Neither finding supported the efficacy of clinic treatment.

A limitation of Shepherd et al.'s (1966) study is that the children in their untreated sample had not been referred to clinics. Mothers of these children, compared to the clinic mothers, showed less distress over their children's problems and less interest in professional help; and there may have been other differences between children and families who sought clinic treatment and those who did not. Thus, it is important to complement evidence such as Shepherd et al.'s with evidence on treated and untreated groups who *both* initially sought clinic treatment.

This brings us to another means of fashioning treatment-no-treatment comparisons: comparing treated and untreated children admitted to the same treatment facilities in the same time period, with untreated cases consisting of those who drop out before treatment. This approach addresses the problems associated with the use of nonreferred children (as in Shepherd et al., 1966) and with the use of therapy dropout and completer groups from different clinics and different time periods (as in Levitt, 1957b). Of course, it might be argued that dropouts and therapy completers could differ demographically or clinically (see, e.g., Hood-Williams, 1960). Yet, published studies examining a broad range of child demographic and clinical variables have revealed negligible differences between such groups (see Gould, Shaffer, & Kaplan, 1985; Levitt, 1957a; McAdoo & Roeske, 1973; Weisz, Weiss, & Langmeyer, 1987, 1989); moreover, it is possible to test for differences before comparing groups on outcome measures. Thus, under proper circumstances, children who drop out may be an acceptable (although, of course, not ideal) naturally occurring control group for outcome research in circumstances in which no randomly assigned control group can be constituted. In the following sections we describe four studies that used this approach.³

Therapy dropouts versus completers: I. Lehrman, Sir-luck, Black, and Glick (1949). In the earliest of these

ments cannot be truly random, because children with particularly serious problems receive priority; and wait-list controls usually can only be paired with brief therapy cases, and without follow up, because withholding treatment for long periods simply to match treated cases would violate the clinic mandate.

³ We know of two other studies that approximate this methodology. However, both (Jacob, Magnussen, & Kemler, 1972; Witmer & Keller, 1942) have design features that make them not quite appropriate for inclusion here. Witmer and Keller (1942) reported no significant effects of therapy; Jacob et al. (1972) reported mixed findings.

studies (which, incidentally, provided a base rate sample for Levitt, 1957b), Lehrman et al. (1949) focused on 3–20-year-olds seen at Jewish Board of Guardians (JBG) Child Guidance Clinics in New York, before April, 1942; 60% were seen for “primary behavior disorder” (e.g., hostile or impulsive behavior) and 25% for “psychoneurosis,” with other problems represented as well. Some 196 cases formed the treatment group; these were deemed “totally treated,” through methods that were labeled “transference psychotherapy” (p. 8). An additional 110 cases formed the control group. These had been observed by JBG staff and found eligible for services, but had not received a course of treatment at JBG or elsewhere (usually because parents had declined). Treatment and control groups were compared on a small number of demographic, family, and clinical characteristics early in their clinic contact. Few differences were found; however, the control group had a higher proportion of primary behavior disorder diagnoses than the treatment group (74% vs. 55%), and the reverse was true of the psychoneurosis category (20% vs. 28%).

One year after cases had been closed, outcomes were assessed. Each case was classified as success, partial success, or failure, on the basis of assessments of a JBG evaluation committee. Steps were taken to keep the committee unaware of group membership of the cases, but the interview and case material used by the committee may have been gathered and assembled by JBG staff who knew the group membership. At follow-up, the percentage classified as success was significantly higher for treated than for control cases (50.5% vs. 31.8%), and the percentage of failure ratings was slightly higher for control than for treated cases (30.0% vs. 26.0%). Given these differences, Lehrman et al. (1949) maintained, “The positive effect of the treatment was established beyond a doubt” (p. 80). This verdict may need to be tempered somewhat by concerns about diagnostic differences between the two groups, and about the degree of “experimental blindness” and impartiality of the staff on whose data outcome assessments were based. Nonetheless, Lehrman et al.’s study stands as an important early methodological and substantive contribution.

Therapy dropouts versus completers: II. Levitt, Beiser, & Robertson (1959). In a second completer versus dropout study, Levitt et al. (1959) studied youngsters averaging about 10 years old when first seen at Chicago’s Institute for Juvenile Research. Two groups were compared: 237 treated cases, or *remainers*, and 93 untreated cases, or *defectors*. Defectors had been accepted for treatment but had dropped out before the first therapy session. Remainers had had an average of 18 therapy sessions, with therapy directed to the child alone (10% of the cases), to the child and parent (46%), or to the parent alone (44%). Any child treated elsewhere during the study was excluded. Remainer and defector groups were compared on 61 clinical and background variables assessed at the beginning of their clinic contact (e.g., gender, mental age, nature and severity of problems), with no reliable

differences found beyond chance expectancy (Levitt, 1957a).⁴

Outcomes were assessed at an average of about five years after clinic contact. The 26 outcome variables included scores on several psychological tests (e.g., Taylor Anxiety Scale, Minnesota Multiphasic Personality Inventory short form), parent and child ratings (e.g., on current severity of original presenting problems), clinician ratings (e.g., on manifest tension, affective tone of personality), and life adjustment indicators (e.g., completion of schooling, institutional residence). Levitt et al. (1959) found no remainer–defector group differences on outcome measures beyond chance expectancy. As for direction of the group differences, 16 showed more favorable scores for the defector group, whereas only 9 favored the remainer group (sign test $p = .21$). To enhance the possibility of identifying therapy effects, Levitt et al. redefined the remainers to select a *continuer* group, requiring a minimum of 10 therapy sessions, and reanalyzed for continuer–defector differences. The results were essentially the same as in the original analyses, with no reliable evidence of positive treatment effects.

Therapy dropouts versus completers: III. Ashcraft (1971). More than a decade after Levitt et al. (1959), Ashcraft (1971) published another follow-up comparison of clinic-treated youngsters with matched children who had dropped out after intake and had remained untreated. All children in both groups had been seen at one of two clinics in a metropolitan area, at some time during Grades 3–6. The treatment group consisted of 40 children and was 88% male, with a mean age of nine years and seven months, and a mean Wechsler Intelligence Scale for Children (WISC) IQ of 105.8. The dropout group numbered 43 and was 79% male, with a mean age of nine years and four months, and a mean WISC IQ of 104.6. Outcomes were assessed five years after clinic contact. All the children in both groups had been classified as underachievers who had “learning difficulties” that “stemmed from emotional difficulties requiring outpatient treatment” (p. 339). Thus, outcomes were assessed with measures of academic achievement: Stanford Achievement Test Total Achievement Score, Total Language Achievement Score, and Total Quantitative Achievement Score. The treated and untreated groups did not differ reliably on any outcome measure.

Therapy dropouts versus completers: IV. Weisz and Weiss (1989). The clinic-based studies reviewed thus far were all conducted more than two decades ago. One might question whether their findings are relevant to psychotherapy as it is currently practiced in clinics. To address this question, we now turn to a more recent study of clinic treatment, in which Weisz and Weiss (1989) com-

⁴ The comparison of remainer and defector groups on clinical and background variables (Levitt, 1957a) appears to have been conducted on groups that overlapped with but were not identical to the remainers and defectors in the treatment outcome study (Levitt et al., 1959). Levitt included 132 remainers and 208 defectors, whereas Levitt et al. included 237 remainers and 93 defectors.

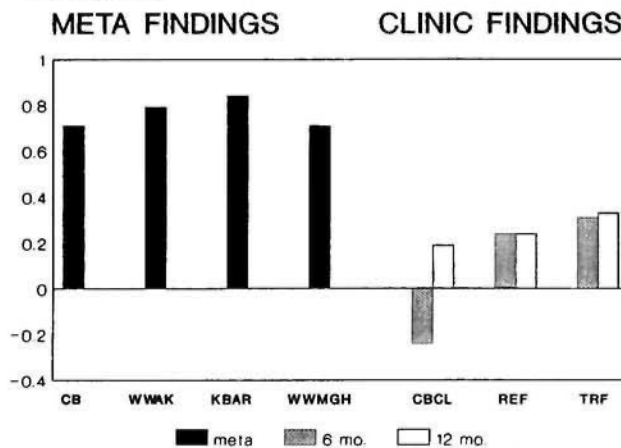
pared treatment completers and dropouts from nine outpatient mental health clinics. The completers were 93 children who had had at least 5 therapy sessions (average 12.4 sessions) and who had terminated with concurrence of their therapist. The dropouts were 60 children who had attended an intake session, had been judged by clinic staff to be in need of treatment, and had been assigned a therapist, but had not appeared for any sessions after intake. In addition, 14 who had received other mental health services during the period of the study were excluded from the dropout group. Completers averaged 11.0 years of age (at the 6-month follow-up) and were 64.5% male; dropouts averaged 10.9 years and were 63.3% male.

Similarity of the groups was assessed on 11 variables that appeared relevant to later outcomes, such as child demographic factors (age, gender, birth order); child clinical measures (Child Behavior Checklist [CBCL; Achenbach & Edelbrock, 1983], internalizing, externalizing, and competence scores, Child Depression Inventory scores, number of therapy sessions prior to this intake); and family factors (socioeconomic status, number of children in the home, miles from home to clinic, and changes in family structure during the six months following intake). No comparison showed a significant group difference.

Weisz and Weiss (1989) collected three measures of adjustment at intake and at six months and one year later. CBCL parent reports were collected to provide information on a broad spectrum of clinically significant behavioral and emotional problems. Parents also gave severity ratings on up to three "major problems for which your child needs help" (identified at intake); this was intended to address specific problems that were a focus of treatment. Finally, Teacher Report Form (TRF; Achenbach & Edelbrock, 1986) reports were collected to provide information on clinically significant behavioral and emotional problems from a source outside the family and not involved in the treatment process. Teachers were not told that the child had been to a clinic; they were asked to fill in the TRF as part of a "youth survey." At six months (for all dropouts and for the 98% of completers who had finished treatment) and at one year, the two groups were compared on the outcome measures described above, with intake scores covaried. On none of the measures was there a significant group difference, at six months or at one year. In other words, the findings revealed no reliable effect of therapy on any of the measures.

For a rough comparison between these results and findings of the four broad-based meta-analyses of experimental studies, effect size estimates were calculated for Weisz and Weiss's (1989) findings. Figure 2 presents all the ES values. As the figure indicates, the six-month CBCL assessment actually showed a trend toward worse outcomes for treated children than for dropouts; however, for all three measures, at both points of assessment, effect sizes fell well below those found in the meta-analyses. Moreover, none of the clinic effect sizes was significantly different from 0.

Figure 2
Effect Sizes Found in Four Meta-Analyses of Child and Adolescent Psychotherapy Outcome Studies and in the Clinic-Based Outcome Study by Weisz and Weiss (1989)



The four bars on the left show results of the meta-analyses: CB = Casey & Berman (1985); WWAK = Weisz, Weiss, Alicka, & Klotz (1987); KBAR = Kazdin, Bass, Ayers, & Rodgers (1990), bar represents our estimate of the pooled effect size, based on Kazdin et al.'s report; WWMGH = Weisz, Weiss, Morton, Granger, & Han (1992), bar represents preliminary findings. The three bars on the right show findings of the Weisz and Weiss (1989) study: CBCL = Child Behavior Checklist findings; REF = findings on severity of primary referral problems, TRF = Teacher Report Form findings.

Five possible alternative explanations of these null results were analyzed. These included the possibilities of (a) excessive variability in the data causing null findings, (b) biased or defensive reporting by parents of dropout or continuer children, (c) hidden advantages favoring adjustment in the dropout group, (d) immediate postintake improvement in the dropout group leading to their dropping out, and (e) bias caused by the voluntary nature of subject participation in the research. Close scrutiny and data analyses bearing on these explanations raised doubts about their plausibility (see Weisz & Weiss, 1989, pp. 745-746). This further supported a straightforward interpretation of the findings (i.e., that psychotherapy simply had not had a significant effect on the measures used).

Random assignment: De Fries, Jenkins, and Williams (1964). Finally, we turn to the one clinic-based study we have found that involved true random assignment to treatment and control groups. De Fries et al. (1964) studied 6-15-year-olds, all described as seriously disturbed, all having a psychiatric diagnosis, and all in foster care in Westchester County, New York. They formed 27 child pairs, with pair members matched for age, gender, ethnic group, IQ, and psychiatric diagnosis. One member of each pair was randomly assigned to receive usual welfare department services; the other member received psychotherapy, plus enhanced services for the foster family. Before intervention, three clinically experienced judges (unaware of group assignment) rated all

the children on (a) relationships in the foster home, (b) adjustment in school and community, (c) symptomatology, and (d) well-being and productivity. Means on each rating were almost identical for the two groups, indicating successful matching.

Therapy appears to have lasted three years and to have been conducted by professional therapists, each supervised by a psychiatrist. After the intervention, outside judges, unaware of group assignment, were asked to use data from opening and closing psychiatric interviews to place each child in an *improved*, *no change*, or *worsened* category. The judges, working independently, agreed in 81% of their judgments, and were in adjacent categories for the remaining 19%. There were no significant treatment-control differences on this outcome measure. A second goal of intervention had been to prevent institutionalization. This goal was also not achieved; institutionalization occurred somewhat more often among the treated children than among the controls. In summary, this clinic-based study, using random assignment methodology, found no reliable treatment effects.

Implications of the Clinic-Based Findings

The most immediate conclusion suggested by the studies reviewed in this article is that, for clinic-referred children treated by practicing clinicians in clinic settings, the effects of psychotherapy may not be nearly so positive as the findings presented in the meta-analyses indicate. In fact, with the exception of Lehrman et al. (1949), the clinic-based studies have not shown statistically significant effects of psychotherapy. Certainly, the methodologies used in most of these studies fell short of the ideal, with random assignment achieved in only one instance. Moreover, most of the clinic-based studies were conducted decades ago, at a relatively immature stage in the development of therapeutic approaches and standardized assessment of child psychopathology and therapy outcomes. Thus, the need for careful, current replication of the clinic studies is clear. Although it would be a mistake to accept the clinic findings uncritically, it would also be a mistake to ignore or dismiss these findings and the sobering state of affairs with which they confront us: Currently, we lack convincing evidence that the large positive effects of psychotherapy demonstrated in controlled psychotherapy research, and summarized in the meta-analyses, are being replicated in the clinic and community settings where most real-life interventions actually occur.

If the clinic-based findings discussed here are valid and should eventually prove to be well-replicated, they should not be viewed as entirely discouraging. Indeed, they may represent a glass that can be seen as both half empty and half full. From the half-empty perspective, clinic-based therapy for children may not be as effective as one would hope. But from the half-full perspective, the results of the meta-analyses suggest that under proper conditions child therapy may be very effective. When the findings are viewed in this light, a key task for researchers becomes that of identifying those proper conditions under which effects of child therapy may be optimized.

Identifying such conditions may require adapting research therapies to actual clinical conditions with referred clients, then testing the efficacy of research-based therapies in clinic settings. It is possible that research therapy has positive effects because it tends to involve clear delineation of focal problems that will be targets of treatment (i.e., ruling out efforts to treat "the whole child"), precise matching of these problems to treatment methods, and selection of treatment methods that have empirical support. On the other hand, it is also possible that research therapy appears more effective in part because the youngsters being treated are less seriously disturbed or more responsive to intervention than those youngsters typically treated in clinics, or because research therapists are free of constraining conditions (e.g., heavy caseloads) under which clinic therapists often work. Until research therapy is brought into the clinics, it will be difficult to evaluate such possibilities in a definitive way.

Renewing Ties Between Research and Practice

Constructing research of the sort described above will almost certainly require enriched collaboration between researchers and practitioners, a process that may prove healthy for both groups. One of the most widely voiced complaints of practicing clinicians over the years has been that psychotherapy research is of little use to them (see Elliott, 1983; Kupfersmid, 1988; Luborsky, 1972; Orlinsky & Howard, 1978; Parloff, 1980). As Strupp (1989) put it, psychotherapists "have recurrently complained that they can learn but little from psychotherapy research" (p. 717). When clinical psychologists are asked to rank order the usefulness to their practice of various sources of information, research articles and books are generally rated at the bottom of the scale (L. Cohen, 1979; L. Cohen, Sargent, & Sechrest, 1986; Morrow-Bradley & Elliot, 1986). Researchers themselves appear to agree with the concern; as Morrow-Bradley and Elliot noted,

With virtual unanimity, psychotherapy researchers have argued that (a) psychotherapy research should yield information useful to practicing therapists, (b) such research to date has not done so, and (c) this problem should be remedied. (p. 188)

We suggest that psychotherapy research with children may have considerable relevance to the work of clinicians, and conversely, that the work of clinicians may make important contributions to research. If it is true that child therapy is more effective under research conditions than under clinic conditions, outcome research may well prove useful to practicing clinicians and their young clients by specifying those conditions under which therapeutic gains can be maximized. On the other hand, to achieve this goal, researchers will need to rely heavily on the wisdom and perspective of clinicians whose knowledge about children and treatment has been shaped in the crucible of real life.

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