The Efficacy of Psychological, Educational, and Behavioral Treatment: Confirmation from Meta-Analysis

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The Efficacy of Psychological, Educational, and Behavioral Treatment

Confirmation From Meta-Analysis

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Conventional reviews of research on the efficacy of psychological, educational, and behavioral treatments often find considerable variation in outcome among studies and, as a consequence, fail to reach firm conclusions about the overall effectiveness of the interventions in question. In contrast meta-analytic reviews show a strong, dramatic pattern of positive overall effects that cannot readily be explained as artifacts of meta-analytic technique or generalized placebo effects. Moreover, the effects are not so small that they can be dismissed as lacking practical or clinical significance. Although meta-analysis has limitations, there are good reasons to believe that its results are more credible than those of conventional reviews and to conclude that well-developed psychological, educational, and behavioral treatment is generally efficacious.

Systematic knowledge about the efficacy of psychological, educational, and behavioral intervention for individual and social problems is almost entirely dependent on research conducted within the experimental or quasi-experimental framework. In any given treatment area, such research often yields an ambiguous mix of results—decidedly positive, suggestive, convincingly null, and hopelessly inconclusive. Research reviewers must then pick through these results with hopes of finding a preponderance of evidence supporting a conclusion about treatment efficacy. More specifically, they must attempt to sort and choose among studies on the basis of their methods, treatment variants, respondents, and the like to find those situations for which conclusions can be drawn.

It is a distressing observation that, over recent decades, the results of treatment research and reviews of that research have not yielded convincing support for the efficacy of many psychological, educational, and behavioral treatments. The controversial history of assessment of the effects of psychotherapy is representative. Some reviewers were adamant that the research showed no convincing effects (e.g., Eysenck, 1952, 1965), whereas others interpreted the evidence as generalized efficacy (e.g., Luborsky, Singer, & Luborsky, 1975). Similar controversy has characterized intervention in social work, counseling, education, criminal justice, organizational development (Fischer, 1978; Prather & Gibson, 1977), and a host of related areas. Rossi and Wright (1984) echoed many reviewers in these areas when they described evaluation research as a "parade of close-to-zero effects" (p. 342). Such controversy and pessimism has cast a shadow of doubt over all but a few claims for the efficacy of psychological, educational, and behavioral interventions.

The Advent of Meta-Analysis

A new approach to integrating and interpreting a body of treatment effectiveness research arose in the mid-1970s and has come to fruition in recent years. Dubbed "meta-analysis" by Glass (1976), this approach is quite different from the research integration practices that preceded it. In particular, it is characterized by its framing of research integration as, in large part, a research exercise in its own right. Eligible research studies are viewed as a population to be systematically sampled and surveyed. Individual study results and characteristics are then abstracted, quantified, coded, and assembled into a database that is statistically analyzed much like any other quantitative survey data.

Since Smith and Glass's (1977) pioneering meta-analysis of psychotherapy research, literally hundreds of meta-analyses have been conducted in different treatment research areas. Although much of this work has been rather crude and certainly is not above criticism, there can be no doubt that meta-analysis has become an accepted technique that has rapidly developed in conceptual, methodological, and statistical sophistication (Cook et al., 1992; Durlak & Lipsey, 1991; Glass, McGaw, & Smith, 1981; Hedges & Olkin, 1985; Hunter & Schmidt, 1990; Rosenthal, 1991a).

The purpose of this article is to examine the large body of meta-analyses of psychological, educational, and behavioral treatment research that has cumulated in the last decade and a half. It will perhaps not be surprising that this systematic approach to research integration has resulted in refinements of our understanding of the effects of treatment. What does not seem to be widely recognized,

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however, is that, in contrast to the previous era of conventional research reviews, meta-analysis has yielded stark, dramatic patterns of evidence for the general efficacy of such treatment.

Meta-Analysis of Treatment Research

The quantity and variety of meta-analysis of experimental and quasi-experimental treatment research has been so great that it is necessary to identify the boundaries of this review. Of interest here is meta-analysis of research on the effects of treatments that are based on manipulation of psychological variables and are intended to induce psychological change, whether emotional, attitudinal, cognitive, or behavioral (hereafter referred to as *psychological treatments*). The extensive meta-analysis of clinical trials research in medicine, therefore, falls outside the boundaries. Psychologically based intervention within medical settings (e.g., preoperative counseling), however, is included. Moreover, attention is restricted to those treatments that are directed at practical individual and social problems. Excluded, therefore, are meta-analyses of interventions and manipulations of primarily theoretical interest or those that do not represent currently practiced interventions in "real world" domains of applicability (e.g., teacher expectancy effects).

Also, within the area of psychological treatment it is necessary to consider the level or scope of intervention. At one end of a rough continuum we can distinguish treatment techniques—separable elements of intervention that do not, by themselves, constitute a freestanding treatment (e.g., self-disclosure by therapists or use of advance organizers in a teacher's lesson plan). At the other end of this rough continuum are broad policies or programs that combine many treatments and treatment elements, organizational arrangements, and so forth (e.g., school desegregation or mental health deinstitutionalization). We exclude both ends of this continuum to focus on midrange treatments, those relatively freestanding intervention packages with rather specific purposes that are deliverable at a defined site for a target population. In this category we include such interventions as psychotherapy, parent effectiveness training, medical patient education, smoking-cessation programs, job enrichment, computer-aided instruction, science curricula, and open classrooms (see Table 1 for a fuller list). Although there are gray areas at both ends of this midrange, we found it possible to categorize most interventions subjected to meta-analysis with reasonable confidence.

With the above boundaries in mind, a series of computer and manual searches was made of bibliographies of articles dealing with meta-analysis, various standard social science abstracts (Psychological Abstracts, Sociological Abstracts, etc.), and listings of unpublished materials (Dissertation Abstracts International, ERIC). All reports that appeared eligible on the basis of the title and abstract were retrieved, and 290 of them were found to meet the inclusion criteria. Because some reports presented more than one independent meta-analysis, the total number examined for the present study was 302. The search and retrieval effort was thorough and, although it doubtless missed some number of eligible reports, we believe that the resulting collection represents a high proportion of the available work of interest to this review.

Treatment Effects: Broad Patterns

Table 1 lists, by broad categories, the meta-analysis studies that were discovered in this search and the treatment areas they cover. As is evident, a number of these meta-analyses are replications, near replications, subsets, or have overlapping studies with others in the list. Thus some studies and some subjects are represented in more than one meta-analysis. We will come back to this matter later but, for now, will ignore the redundancies and make a general examination of the treatment effects found in this collection of meta-analyses.

The right-hand columns of Table 1 report the overall mean treatment effect size found in each meta-analysis and the number of studies on which it was based. The effect size metric used here is the standardized difference between the mean of the treatment group and the mean of the control group for a given outcome measure in a given study.[1] Typically, a mean effect size over all studies and all outcome measures is shown. When the original meta-analysis reported mean effect sizes for quite different categories of treatment or outcome, the highest level of aggregation is presented for the major category or categories under investigation. One exception to this procedure was for educational treatments in which the great preponderance of effects were on achievement measures. In such cases, only the mean achievement effect was recorded.

Given the inconsistent findings reported in conventional research reviews for many of these treatment areas and the high proportion of studies with statistically nonsignificant results identified in both conventional and meta-analytic reviews, one might expect quite a mix of mean treatment effect sizes in Table 1, with many hovering around zero. Moreover, given the wide range of different treatments represented, one might expect some proportion to have negative mean effect sizes (i.e., control groups outperforming treatment groups) and a quite modest proportion to have strongly positive mean effect sizes. After all, we would not expect every treatment to work well.

Figure 1 presents the distribution of mean effect sizes from Table 1. We do this solely for descriptive purposes, as an alternate depiction of the information in Table 1, and with no implication that these are independent data points or that they represent a statistical sample or population (later we will present a more refined distribution with better statistical properties).

The striking feature of Figure 1 is the strong skew toward positive effects. Of 302 meta-analyses, only 6 pro-

(text continues on page 1192)

[1] Effect size is typically computed as $(M_t - M_c)/s$, where M_t is the treatment group mean, M_c is the control group mean, and s is the pooled standard deviation or, sometimes, the control group standard deviation.

Table 1
Meta-Analysis Studies

Treatment area and reference	*M* effect size	*N*
1. Mental Health, Health		
1.1. Psychotherapy, General		
Psychotherapy; all outcomes (Smith, Glass & Miller, 1980)[a]	0.85	475
Psychotherapy with adults; all outcomes (Shapiro & Shapiro, 1982, 1983)	0.93	143
Psychotherapy vs. placebo controls; all outcomes (Prioleau, Murdock, & Brody, 1983)	0.42	32
Psychotherapy (random assignment studies with good controls); all outcomes (Landman & Dawes, 1982)	0.78	42
Psychotherapy; self-concept outcomes (Cook, 1988)[a]	0.37	34
Psychotherapy (individual); all outcomes (Tillitski, 1990)	1.16	9
Psychotherapy (group); all outcomes (Tillitski, 1990)	1.31	9
Psychotherapy with children; all outcomes (Casey & Berman, 1985)[a]	0.71	64
Psychotherapy with children and adolescents; all outcomes (Weisz, Weiss, Alicke, & Klotz, 1987)	0.79	108
Psychotherapy with adult neurotic patients; all outcomes (Nicholson & Berman, 1983)	0.68	67
Psychotherapy for neuroses, phobias & emotional-somatic complaints; all outcomes (G. Andrews & Harvey, 1981)	0.72	81
Psychotherapy for the treatment of depression; all outcomes (L. A. Robinson, Berman, & Neimeyer, 1990)	0.72	58
Psychotherapy for neurotic depression; all outcomes (Prince Henry Hospital, 1983)	0.65	10
Psychotherapy for unipolar depression in adults; all outcomes (Steinbrueck, Maxwell, & Howard, 1983)	1.22	16
Psychotherapy vs. drug therapy for the treatment of bulimia; all outcomes (Laessle, Zoettl, & Pirde, 1987)	0.95	23
Psychotherapy for bulimia; all outcomes (Bryan, 1989)[a]	0.92	31
Client-centered therapy, transactional analysis, and non-directive therapy; all outcomes (Champney & Schulz, 1983)	0.25	18
Mental health specialists vs. general medical practitioners; all outcomes (Balestrieri, Williams, & Wilkinson, 1988)	0.22	11
1.2. Psychotherapy, Cognitive Behavioral/Behavior Modification		
Cognitive behavioral therapies vs. nonspecific factors controls; all outcomes (Barker, Funk, & Houston, 1988)	0.67	17
Cognitive therapy for anxiety disorders; all outcomes (Berman, Miller, & Massman, 1985)	0.73	25
Cognitive therapy, modification of covert self-statements of adult patients; all outcomes (Dush, Hirt, & Schroeder, 1983)[a]	0.66	69
Cognitive therapy with nonpsychotic patients with clinic complaints; all outcomes (Miller & Berman, 1983)	0.77	48
Cognitive behavior therapy with adult populations; all outcomes (Polder, 1986)	0.69	53
Cognitive behavioral therapy; effect on trait anxiety and neuroticism (Jorm, 1989)	0.53	63
Cognitive behavioral therapy (paradoxical interventions); all outcomes (Shoham-Salomon & Rosenthal, 1987)	0.89	10
Cognitive behavioral therapy (paradoxical interventions); all outcomes (Hampton, 1988)[a]	0.15	29
Cognitive behavioral therapy (paradoxical interventions); all outcomes (Hill, 1987)[a]	0.99	15
Cognitive therapy for depression; Beck Depression Inventory outcomes (Dobson, 1989)	0.99	28
Cognitive and behavioral treatments of depression and phobic anxiety; all outcomes (Eifert & Craill, 1989)	0.83	36
Cognitive behavioral therapy with children; modification of self-statements (Dush, Hirt, & Schroeder, 1989)	0.37	48
Cognitive behavioral modification strategies with children; educationally relevant behavioral outcomes (Duzinski, 1987)[a]	0.47	45
Cognitive behavioral therapy with dysfunctional children; all outcomes (Durlak, Fuhrman, & Lampman, 1991)[a]	0.53	64
Cognitive therapy and systematic desensitization for public speaking anxiety; all outcomes (Allen, Hunter, & Donohue, 1989)	0.52	97
Systematic desensitization; all outcomes (Berman, Miller, & Massman, 1985)	0.62	25
Training children in use of verbal self-instructions to control their behavior in non-training situations; all outcomes (Rock, 1986)[a]	0.51	47
Behavior therapy vs. placebo controls; all outcomes (Bowers & Clum, 1988)	0.55	69
Behavioral self-management, social skills training, cognitive-behavioral therapy, and biofeedback/relaxation training with problem children; clinically relevant outcomes (Wyma, 1990)	0.61	43
Behavioral treatment (biofeedback) for Raynaud's disease; all outcomes (Montross, 1990)	1.06	18
Behavioral treatment (progressive relaxation therapy); all outcomes (Paterson, 1988)[a]	0.34	71
Behavioral treatment with spouse involvement in treatment of agoraphobia; effect on symptoms (Dewey & Hunsley, 1990)	0.10	6

(table continues)

Table 1 (continued)

Treatment area and reference	M effect size	N
Behavioral therapy and tricyclic medication in the treatment of obsessive–compulsive disorder; all outcomes (Christensen, Hadzi-Pavlovic, Andrews, & Mattick, 1987)	1.02	27
1.3. Couseling, Psycho-Educational Treatment, Special Therapy		
1.3.1. Family/marital interventions		
Family therapy; all outcomes (Hazelrigg, Cooper, & Borduin, 1987)	0.36	20
Family therapy (conjoint); all outcomes (Markus, Lange, & Pettigrew, 1990)	0.57	19
Family therapy for child identified problems; all outcomes (Montgomery, 1991)	0.61	43
Family and marital therapies; behavioral outcomes (Shadish, 1992)[a]	0.70	58
Behavioral marital therapy; all outcomes (Hahlweg & Markman, 1988)	0.95	17
Behavioral premarital intervention studies; all outcomes (Hahlweg & Markman, 1988)	0.79	7
Parent effectiveness training; all outcomes (B. Cedar & Levant, 1990; R. B. Cedar, 1986)[a]	0.33	26
Marriage/family enrichment programs for nonclinical couples and families; all outcomes (Giblin, Sprenkle, & Sheehan, 1985)[a]	0.44	85
Minnesota Couple Communication Program (communication skills); immediate outcomes (Wampler, 1983)[a]	0.52	20
1.3.2. Treatment programs for offenders		
Treatment programs for juvenile delinquents; delinquency outcomes (Lipsey, 1992)[a]	0.17	397
Treatment programs for juvenile delinquents; all outcomes (Gottschalk, Davidson, Gensheimer, & Mayer, 1987a)	0.48	91
Treatment programs for adjudicated delinquents in residential/institutional settings; all outcomes (Garrett, 1985a, 1985b)	0.37	111
Treatment programs for juvenile delinquents (random assignment studies); delinquency outcomes (Kaufman, 1985)	0.25	20
Social learning treatment programs for juvenile delinquents; all outcomes (Mayer, Gensheimer, Davidson, & Gottschalk, 1986)	0.77	39
Diversion programs for juvenile delinquents; all outcomes (Gensheimer, Mayer, Gottschalk, & Davidson, 1986)	0.40	44
Behavioral treatment approaches for juvenile delinquents; long-term outcomes (Gottschalk, Davidson, & Mayer, 1987b)	0.40	25
Treatment programs for juvenile offenders; all outcomes (Whitehead & Lab, 1989)	0.27	50
Treatment programs for adult and juvenile offenders; all outcomes (D. A. Andrews et al., 1990)	0.20	80
Correctional treatment with adults; all outcomes (Losel & Koferl, 1989)	0.25	16
1.3.3. Meditation, psychological outcomes		
Meditation and relaxation techniques; effects on trait anxiety (Eppley, Abrams, & Shear, 1989)	0.42	145
Passive individual meditation techniques; psychological affective outcomes (Ferguson, 1981)	0.56	51
Transcendental meditation; effects on self-actualization (Alexander, Rainforth, & Gelderloos, 1991)	0.88	18
Effects of meditation; anxiety outcomes (Edwards, 1991)[a]	0.59	21
Effects of hypnosis; anxiety outcomes (Edwards, 1991)[a]	0.71	54
1.3.4. Other couseling, psycho-educational treatment or special therapy		
Innovative outpatient programs vs. traditional aftercare for mental health patients released from hospitals; all outcomes (Straw, 1982)[a]	0.36	130
Community-based alternatives vs. institutionalization for mental health patients; all outcomes (Straw, 1982)[a]	0.14	30
Deinstitutionalization programs for the chronically mentally ill; all outcomes (L. C. Harris, 1987)	0.36	111
The Primary Mental Health Project (identification and treatment of maladjusted school children); all outcomes (Stein & Polyson, 1984)[a]	0.25	7
Primary prevention program in mental health; all outcomes (Susskind & Bond, 1981)	0.08	13
Treatment by paraprofessionals in mental health, education, law, and social work vs. untreated controls; all outcomes (Truax, 1984)[a]	0.60	57
Companionship treatment (paraprofessionals) with children; all outcomes (Stein, 1987)	0.22	19
Training in interpersonal cognitive problem solving skills for children; effects on interpersonal skills and behavior adjustment (Almeida & Denham, 1984; Denham & Almedia, 1987)[a]	0.66	27
Group assertion training for students and adults; all outcomes (Branwen, 1982)[a]	1.51	40
Assertiveness training; effects on assertiveness and social skills (Shatz, 1984)	0.79	21
Alcohol and drug use prevention programs; behavior, attitudes and knowledge outcomes (Rundall & Bruvold, 1988)	0.27	76
(Bangert-Drowns, 1988)	0.41	33
(Tobler, 1986)[a]	0.30	98
Guidance and counseling programs in the regular school curriculum for high school; effects on psychological maturity (Sprinthall, 1981; see also 3.5.2.)[a]	1.20	6
Career counseling interventions; all outcomes (Oliver & Spokane, 1988; see also 3.5.2.)	0.48	58
Counseling and guidance programs in high school; all outcomes (Nearpass, 1990; see also 3.5.2.)[a]	0.38	77
Career education programs for K–12 students; all outcomes (Baker & Popowicz, 1983, see also 3.5.2.)	0.50	18

Table 1 (continued)

Treatment area and reference	M effect size	N
Primary prevention education programs in schools (e.g., career maturity, coping/communication skills, moral & psychological education, substance abuse, values); all outcomes (Baker, Swisher, Nadenichek, & Popowicz, 1984; see also 3.5.2.)[a]	0.55	41
Vocational programs for persons with mental illness; all outcomes (Bond, 1988)[a]	0.54	18
Mental practice of motor skills; effects on learning (Fletz & Landers, 1983)[a]	0.48	60
Social work interventions for mental illness; all outcomes (Videka-Sherman, 1988)[a]	0.51	30
Social skills training with schizophrenics (Benton & Schroeder, 1990)	0.65	27
Social skills training with children K–12; all outcomes (Hanson, 1989)[a]	0.65	63
Treatment of public speaking anxiety; effect on anxiety (Allen, 1989)	0.43	116
Self-administered psychological treatments for habits, phobias, affective disturbances and skills training; all outcomes (Scogin, Bynum, Stephens, & Calhoun, 1990)	0.34	40
1.4. Health Related Psychological or Educational Treatment		
1.4.1. Education/counseling for medical patients		
Educational or psychological interventions with adult hospitalized elective surgery patients; effects on patient well-being (Devine, 1984; Devine & Cook, 1983)[a]	0.46	105
Preoperative instruction of adults scheduled for surgery; effects on postoperative outcome (Hathaway, 1985)	0.44	68
Special preoperative preparation of children for surgery vs. routine nursing care; effects on anxiety (Howell, 1985)[a]	0.40	23
Psychological preparation of children for medical procedures; all outcomes (Saile, Burgmeier, & Schmidt, 1988)	0.44	75
Patient education for people with a chronic disease or medical problem; effects on compliance and health (Mazzuca, 1982)[a]	0.52	27
Psychological support for patients facing surgery or recovering from heart attacks; effects on anxiety, cooperation, and recovery (Mumford, Schlesinger, & Glass, 1982)[a]	0.49	34
Programs to increase compliance with medical treatment regimens; all outcomes (Posavac, Sinacore, Brotherton, Helford, & Turpin, 1985)	0.47	58
Patient education about treatment regimens, preventive behavior, self-care, etc.; all outcomes (Posavac, 1980)	0.74	23
1.4.2. Biofeedback/relaxation/medication training for clinical symptoms		
Biofeedback and relaxation training for migraine and tension headaches; improvement scores (Blanchard, Andrasik, Ahles, Teders, & O'Keefe, 1980)	0.63	35
Meditation and relaxation techniques; effect on blood pressure (Kuchera, 1987)	0.93	26
Relaxation training for clinical (medical) symptoms; all outcomes (Hyman, Feldman, Harris, Levin, & Malloy, 1989)[a]	0.52	48
1.4.3. Tobacco smoking cessation/reduction programs		
Smoking cessation/reduction programs; effects on abstinence (Feehan, 1984)	0.64	97
Smoking cessation/reduction programs (physician delivered); effect on quit rates (Dotson, 1990)[a]	0.34	8
Smoking cessation/reduction programs (worksite); effect on quit rates (Fisher, 1990)[a]	0.21	20
1.4.4. Psychological treatments for pain		
Music therapy in medicine to reduce pain; effect on pain reduction (Standley, 1986)[a]	0.98	29
Pain management interventions with children; behavioral, self-report and physiologic outcomes (Broome, Lillis, & Smith, 1989)[a]	0.39	30
Non-medical psychologically based treatment of chronic pain; all outcomes (Malone, Strube, & Scogin, 1989)	1.10	48
Cognitive coping strategies for the treatment of pain; effects on pain perception (Fernandez & Turk, 1989)	0.51	47
Multidisciplinary treatments for chronic back pain; all outcomes (Flor, Fydrich, & Turk, 1992)	1.25	65
1.4.5. Other health related psychological or educational treatment		
Psychosocial preventive care for the elderly; all outcomes (Wilson, Simson, & McCaughey, 1983)	0.45	8
Adolescent pregnancy education programs; all outcomes (Iverson & Levy, 1982)	0.35	14
Prenatal childbirth classes for adults; all outcomes (Jones, 1983)[a]	0.34	58
Training of new mothers about sensory/perceptual capabilities of newborns; effects on maternal-infant interaction (Turley, 1984)[a]	0.44	20
Behavioral treatment for obesity; effects on weight loss (O'Flynn, 1983)	1.06	80
Behavioral management of obesity for couples; effects on weight loss (Black, Gleser, & Kooyers, 1990)	0.33	12
The Feingold diet (free of food additives) for children; effects on hyperactivity (Kavale & Forness, 1983)	0.02	23
Treatment for stuttering; all outcomes (G. Andrews, Guitar, & Howie, 1980)	1.30	42
Stress management programs; all outcomes (Nicholson, Duncan, Hawkins, Belcastro, & Gold, 1988)	0.75	18
Stress coping interventions; all outcomes (Cannella, 1988)[a]	0.46	94

(table continues)

Table 1 (*continued*)

Treatment area and reference	*M* effect size	*N*
Psychological treatment of Type A Behavior; effects on risk for coronary heart disease (Nunes, Frank, & Kornfeld, 1987)	0.61	10
Subjective well-being interventions among elderly; subjective well being outcomes (Okun, Olding, & Cohn, 1990)[a]	0.42	31
Exercise interventions for depression; effects on depression (North, 1989)[a]	0.54	77
Educational interventions for diabetic adults; knowledge, metabolic control, self-care and psychological outcomes (Brown, 1990)[a]	0.43	82
Death education; attitude and affective outcomes (Durlak & Riesenberg, 1991)	0.28	47
2. Work Setting or Organizational Interventions		
Psychologically based organizational intervention programs; effects on worker productivity (Guzzo, Jette, & Katzell, 1985)	0.44	98
Sociotechnical systems interventions in organizations; all outcomes (Beekun, 1989)	0.41	17
Job enrichment or work redesign; effects on turnover (McEvoy & Cascio, 1985)[a]	0.35	5
Realistic job previews before entering an organization; effect on turnover		
(McEvoy & Cascio, 1985)[a]	0.18	13
(Reilly, Brown, Blood, & Malatesta, 1981)	0.14	11
Training programs for managerial or supervisory personnel; effects on learning, behavior, and results (Burke & Day, 1986)	0.42	70
Personal training techniques; sensitivity training (Falcone, 1986)[a]	0.63	106
Managerial human relations training; effects on managerial performance (Brannick, 1987)[a]	0.47	46
Employee training programs; effects on productivity (Leddick, 1987)	0.67	48
Organizational development programs; effects on attitudes (Neuman, Edwards, & Raju, 1989)	0.32	126
Quality circles programs; effects on job satisfaction and job involvement (Eskew, 1989)	0.12	13
Management education in institutional settings; all outcomes (Niemiec, Sikorski, Clark, & Walberg, 1992)	0.85	22
3. Education		
3.1. General Education, K–12 and College		
3.1.1. Computer aided/based instruction		
Computer based instruction; effects on achievement (Gillingham & Guthrie, 1987)	1.05	13
Computer based instruction, K–12; effects on achievement (J. A. Kulik & Kulik, 1987)	0.31	199
Computer based instruction with elementary school students; all outcomes (Niemiec, 1985; Niemiec, Samson, Weinstein, & Walberg, 1987)[a]	0.45	48
Computer assisted instruction with elementary school students; effects on achievement (Ryan, 1991)[a]	0.31	40
Computer assisted vs. conventional instruction for elementary students; effects on achievement (C. C. Kulik, Kulik, & Bangert-Drowns, 1984)	0.48	25
Computer aided instruction vs. conventional methods in secondary school classrooms; effects on achievement (J. A. Kulik, Bangert, & Williams, 1983)[a]	0.32	51
Computer-based education for junior and senior high school students; effect on achievement (Bangert-Drowns, Kulik, & Kulik, 1985)	0.26	42
Computer aided instruction vs. conventional methods for college instruction; effects on achievement (C. C. Kulik, Kulik, & Cohen, 1980)[a]	0.25	59
Computer assisted instruction for exceptional (special education) students, elementary through high school; effects on achievement (Schmidt, Weinstein, Niemiec, & Walberg, 1986; see also 3.5.3.2.)[a]	0.66	18
Computer aided instruction with learning disabled and educable mentally retarded students; effects on achievement (McDermid, 1990; see also 3.5.3.2.)	0.57	15
Computer assisted mathematics instruction vs. traditional instruction, elementary and secondary students; effects on math achievement (Burns, 1982; see also 3.5.1.)	0.35	40
Computer assisted mathematics instruction and computer programming, elementary and secondary students; effects on math achievement (Lee, 1990; see also 3.5.1.)[a]	0.38	72
3.1.2. Programmed or individualized instruction		
Individualized instruction; effects on achievement (Hood, 1991)[a]	0.17	70
Individualized systems of instruction for 6–12 grade students; effects on achievement (Bangert, Kulik, & Kulik, 1983)	0.10	51
Individualized instruction in science courses vs. traditional lecture methods, secondary school students; effects on achievement (Aiello & Wolfle, 1980; Aiello, 1981; see also 3.5.1.)[a]	0.35	115
Individualized mathematics instruction for elementary and secondary students; effects on math achievement (Hartley, 1977; see also 3.5.1.)[a]	0.29	153
Self-paced modularized individualized mathematics instruction vs. traditional instructions for elementary and secondary students; effect on achievement (Horak, 1981; see also 3.5.1.)	−0.07	41
Programmed instruction vs. conventional instruction with secondary school students; effects on achievement (C. C. Kulik, Schwalb, & Kulik, 1982)[a]	0.08	48
Programmed instruction vs. conventional instruction for college teaching; effect on achievement (J. A. Kulik, Cohen, & Ebeling, 1980)[a]	0.28	56

Table 1 (*continued*)

Treatment area and reference	*M* effect size	*N*
Keller's personalized system of instruction (PSI) vs. traditional lecture methods for college teaching; effects on achievement (J. A. Kulik, Kulik, & Cohen, 1979a)[a]	0.49	72
Mastery learning with Kellers's Personalized System of Instruction & Bloom's Learning for Mastery with college students; all outcomes (C. C. Kulik, Kulik, & Bangert-Drowns, 1990)[a]	0.52	103
Feedback about correct answers in computerized and programmed instruction with adult learners; effects on learning (Schimmel, 1983)[a]	0.47	15
3.1.3. Audio and visual based instruction		
Visual-based instruction (film, TV, etc.) vs. conventional teaching for college students; effects on achievement (Cohen, Ebeling, & Kulik, 1981)[a]	0.15	65
Postlethwait's audio-tutorial method of instruction vs. traditional lecture methods in college teaching; effects on achievement (J. A. Kulik, Kulik, & Cohen, 1979b)[a]	0.20	47
Visual media instruction for students in nursing education; effects on attitude change (Schermer, 1984)[a]	0.68	12
Interactive video instruction; effects on achievement (McNeil & Nelson, 1990)[a]	0.50	63
Interactive video instruction in defense training, industrial training and higher education; effects on knowledge, performance, retention and instruction completion time (Fletcher, 1990)	0.50	28
3.1.4. Cooperative task structures		
Cooperative vs. uncooperative task structures; effects on achievement and productivity (Johnson, Maruyama, Johnson, Nelson, & Skon, 1981)[a]	0.72	122
Cooperative learning with K–12 students; all outcomes (Hall, 1989)	0.30	37
Cooperative vs. competitive and individualistic instructional approaches in adult education; effects on achievement (Johnson & Johnson, 1987)[a]	0.62	133
Cooperative learning with students with mild disabilities; effects on achievement (Stevens & Slavin, 1991; see also 3.5.3.2.)	0.31	11
Cooperative learning methods with handicapped K–12 students in mainstreamed classrooms; effects on achievement (Carlson, 1987; see also 3.5.3.2.)	0.16	13
Cooperative vs. noncooperative task arrangements for handicapped–nonhandicapped and ethnically different groups; all outcomes (Johnson, Johnson, & Maruyama, 1983; see also 3.5.3.2.)[a]	0.75	98
3.1.5. Student tutoring		
Student tutoring of elementary and secondary students (tutor's experience); effects on achievement (Cohen, Kulik, & Kulik, 1982)[a]	0.33	38
Student tutoring of elementary and secondary students; effects on achievement (Cohen, Kulik, & Kulik, 1982)[a]	0.40	52
Tutoring of special education students by other special education students (tutor's experience); effects on achievement (S. B. Cook, Scruggs, Mastropieri, & Castro, 1986)[a]	0.65	19
Tutoring of special education students by other special education students; effects on achievement (S. B. Cook et al., 1986)[a]	0.59	19
Tutorial methods of training the conservation concept in preoperational children; effects on mastery (Phillips, 1983)[a]	0.98	302
3.1.6. Behavioral objectives, reinforcement, cues, feedback, etc.		
Behavioral objectives for instruction with elementary through adult students; effects on achievement (Asencio, 1984)[a]	0.12	111
Positive reinforcement in the classroom; effects on learning (Lysakowski & Walberg, 1980, 1981)[a]	1.17	39
Instructional cues, student participation, and corrective feedback in the classroom; effects on learning (Lysakowski & Walberg, 1982)	0.97	54
3.1.7. Other general education		
Mastery learning, group based, grades 1–12 and college; all outcomes (Guskey & Pigott, 1988)[a]	0.61	43
Mastery learning, group based, primary and secondary students; effects on achievement (Slavin, 1987b)	0.25	17
Home instruction supported by school-based programs for elementary school children; effects on achievement (Grane, Weinstein, & Walberg, 1983)[a]	0.68	29
Assignment of homework to elementary and secondary students; effects on achievement (Paschal, Weinstein, & Walberg, 1984)[a]	0.30	15
Modality based instruction; effects on achievement (Kavale & Forness, 1987)[a]	0.14	39
Technology based instructional approaches with American and Japanese students; effects on achievement (Shwalb, 1987)[a]	0.41	116
Technology based, non-technology based and combination teaching strategies with the mathematically disadvantaged; all outcomes (Williams, 1990)[a]	0.14	127
Use of simulation games in instruction; effect on achievement (Dekkers & Donatti, 1981)[a]	0.28	93
Instructional simulation games vs. conventional instruction; effects on cognitive learning (Szczurek, 1982)	0.33	33
Enrichment programs for gifted students; cognitive, creativity and affective outcomes (Wallace, 1990)[a]	0.55	20

(table continues)

Table 1 (*continued*)

Treatment area and reference	M effect size	N
Psychological and affective interventions for underprepared learners; grade-point average and persistance outcomes (W. L. Collins, 1987)[a]	0.36	14
3.2. Classroom organization/environment		
3.2.1. Open classroom vs. traditional		
Open classroom vs. traditional plan; effects on achievement		
(Giacomia & Hedges, 1982)[a]	−0.07	153
(Hetzel, Rasher, Butcher, & Walberg, 1980)	−0.03	25
(Madamba, 1981)	0.01	72
(Peterson, 1980)	−0.13	45
3.2.2. Class size		
Small class size vs. large class size, all grade levels; effects on achievement (Hedges & Stock, 1983)[a]	0.20	77
Small class size (under 30) vs. large class size (over 30), all grade levels; effects on achievement (Glass & Smith, 1979)	0.21	77
Small class size (under 30) vs. large class size (over 30); effects on student and teacher attitudes and climate of instruction (Smith & Glass, 1980)	0.53	59
3.2.3. Between and within class ability grouping		
Between and within class ability grouping of secondary school students; effects on achievement (C. C. Kulik & Kulik, 1982a, 1982b)[a]	0.10	52
Between and within class ability grouping of elementary students; effects on achievement (Slavin, 1987a)[a]	0.22	39
Between and within class ability grouping of secondary students; effects on achievement (Slavin, 1990)	−0.03	29
Between class ability grouping of elementary students; effects on achievement (C. C. Kulik & Kulik, 1984)	0.19	31
Between class ability grouping in grades K–12; effects on achievement (Noland, 1985)[a]	0.01	50
Between class ability grouping for gifted students; effects on achievement (Goldring, 1990)[a]	0.32	23
3.2.4. Other classroom organization/environment		
Pull-out programs for gifted students, grades K–9; effects on achievement (Vaughn, Feldhusen, & Asher, 1991)[a]	0.47	9
Full vs. half-day kindergarten; all outcomes (Karweit, 1987)[a]	0.48	11
3.3. Feedback to teachers		
Feedback to teachers about individual academic performance of students, grades K–12; effects on achievement (Fuchs & Fuchs, 1986)[a]	0.70	21
Feedback of student ratings to college instructors during a course; effects on student assessment and outcome (Cohen, 1980)	0.38	17
(L'Hommedieu, Menges, & Brinko, 1990)[a]	0.30	28
Teacher consultation for modifying teacher behavior and attitudes; effects on teacher and student behavior and attitudes (Batts, 1988)[a]	0.66	40
Staff development training procedures for changing teacher's attitudes, knowledge and skill acquisition; effects on attitudes, knowledge and skill acquisition (Bennett, 1988)[a]	1.01	112
3.4. Test Taking		
3.4.1. Coaching programs for test performance		
Coaching programs for achievement test performance, elementary through college; effects on test scores (Bangert-Drowns, Kulik, & Kulik, 1983)[a]	0.25	30
Coaching programs on SAT aptitude tests for college students; effects on test scores (DerSimonian & Laird, 1983)	0.19	22
Coaching programs for SAT and other aptitude tests, elementary through college; effects on tests scores (J. A. Kulik, Bangert-Drowns, & Kulik, 1984)[a]	0.33	35
Coaching for the SAT aptitude tests; effects on test scores (Messick & Jungeblut, 1981)	0.15	12
(Becker, 1990)	0.30	23
Training in test-taking skills for elementary and secondary students; effects on achievement test scores (Samson, 1985)[a]	0.33	24
Training in test-taking skills on standardized achievement tests for elementary students; effects on test scores (Scruggs, Bennion, & White, 1984)	0.21	24
Practice test taking on aptitude and achievement tests, elementary through college; effects on test scores (J. A. Kulik, Kulik, & Bangert, 1984)	0.32	40
3.4.2. Test anxiety		
Therapy for test anxiety; effects on performance (O'Bryan, 1985)	0.36	119
Therapy for test anxiety; effects on anxiety (O'Bryan, 1985)	1.07	119
Therapy for test anxiety; all outcomes (Hembree, 1988)	0.63	125
(Thompson, 1987)[a]	0.57	195
Therapy for test anxiety (college students); all outcomes (Dole, Rockey, & DiTomasso, 1983)	0.80	46
Therapy for test anxiety (college students); effects on anxiety and performance (M. M. Harris, 1988)	0.58	70
3.4.3. Examiner		
Familiar vs. unfamiliar examiner testing children; effects on test performance (D. Fuchs & Fuchs, 1985)[a]	0.35	22

Table 1 (continued)

Treatment area and reference	*M* effect size	*N*
3.5. Specific Instructional or Content Areas		
3.5.1. Science and math instruction		
Modern ("new") mathematics curricula vs. traditional instruction; effects on achievement (Athappily, Smidchens, & Kofel, 1983)[a]	0.24	134
Three major activity-based elementary science programs vs. traditional curriculum; effects on achievement (Bredderman, 1983)[a]	0.34	57
New science curriculum vs. traditional curricula with primary and secondary students; effects on achievement (Kyle, 1982; Shymansky, 1984; Shymansky, Kyle, & Alport, 1982, 1983)[a]	0.37	105
(Shymansky, Hedges, & Woodworth, 1990)	0.30	81
Innovative science curricula vs. traditional instruction, grades 6–12; effects on achievement (Weinstein, Boulanger, & Walberg, 1982)	0.47	33
Instructional systems in science education vs. traditional instruction, grades K–12; effects on achievement (Willett, Yamashita, & Anderson, 1983)[a]	0.07	130
Teaching students to control variables in science education, all grades and college; effects on learning (Ross, 1988)[a]	0.73	62
Innovative science teaching techniques vs. traditional techniques, grades 6–12; effect on achievement (Boulanger, 1981)[a]	0.55	51
Innovative approaches to teaching college economics vs. traditional lecture methods; effects on achievement (C. L. Cohn, 1985)[a]	0.20	48
Instruction in problem-solving in science and mathematics vs. conventional instruction for K–12 students; effects on achievement (Curbelo, 1985)[a]	0.54	68
Teaching biology as inquiry vs. traditional methods for high school and college students; effects on achievement (El-Nemr, 1980)[a]	0.16	59
Inductive vs. deductive approaches to science teaching, grades 4–12; effects on achievement (Lott, 1983)	0.06	24
Systematic methods of teaching mathematics problem-solving to elementary and secondary students; effects on problem solving achievement (Marcucci, 1980)[a]	0.13	33
Innovative science teaching techniques vs. traditional techniques, grades 6–college; effects on achievement (Wise & Okey, 1983)[a]	0.35	160
Diagnostic testing and feedback vs. none during science instruction, middle school through college; effects on achievement (Yeany & Miller, 1983)	0.53	21
Treatment of mathematics anxiety; effects on anxiety (Hembree, 1990)[a]	0.37	115
Mathematics instructional method, K–12; effects on attitudes (Bradford, 1991)[a]	0.15	102
Computer assisted mathematics instruction vs. traditional instruction, elementary and secondary students; effects on math achievement (Burns, 1982; see also 3.1.1.)	0.35	40
Computer assisted mathematics instruction and computer programming, elementary and secondary students; effects on math achievement (Lee, 1990; see also 3.1.1.)[a]	0.38	72
Individualized instruction in science courses vs. traditional lecture methods, secondary school students; effects on achievement (Aiello, 1981; Aiello & Wolfle, 1980; see also 3.1.2.)[a]	0.35	115
Individualized mathematics instruction for elementary and secondary students; effects on math achievement (Hartley, 1977; see also 3.1.2.)[a]	0.29	153
Self-paced modularized individualized mathematics instruction vs. traditional instructions for elementary and secondary students; effect on achievement (Horak, 1981; see also 3.1.2.)	−0.07	41
Computer programming instruction; cognitive outcomes (Liao & Bright, 1991)[a]	0.41	65
3.5.2. Special content other than science and math		
Reading instruction strategies for elementary students; effects on achievement (Pflaum, Walberg, Karegianes, & Rasher, 1980)	0.60	31
Reading improvement and/or study skills programs for college students; effects on reading ability, GPA, and study habits (Sanders, 1979)[a]	0.94	28
Whole language and language experience approaches to teaching reading; effects on language achievement (Stahl & Miller, 1989)[a]	0.09	54
Instructional programs for teaching writing composition, elementary through college; effects on writing quality (Hillocks, 1984)[a]	0.28	60
Accelerated instruction for gifted students; effects on achievement (J. A. Kulik & Kulik, 1984)[a]	0.88	13
Creativity training techniques; effects on creative performance and other outcomes (C. M. G. Cohn, 1985)[a]	0.57	106
Creative thinking training programs; effects on Torrance Test of Creative Thinking (Rose & Lin, 1984)	0.47	46
Creative drama with elementary students; effect on achievement (Kardash & Wright, 1987)[a]	0.67	16
Primary prevention education programs in schools (e.g., career maturity, coping/communication skills, moral & psychological education, substance abuse, values); all outcomes (Baker, Swisher, Nadenichek, & Popowicz, 1984; see also 1.3.4.)[a]	0.55	41

(table continues)

Table 1 (*continued*)

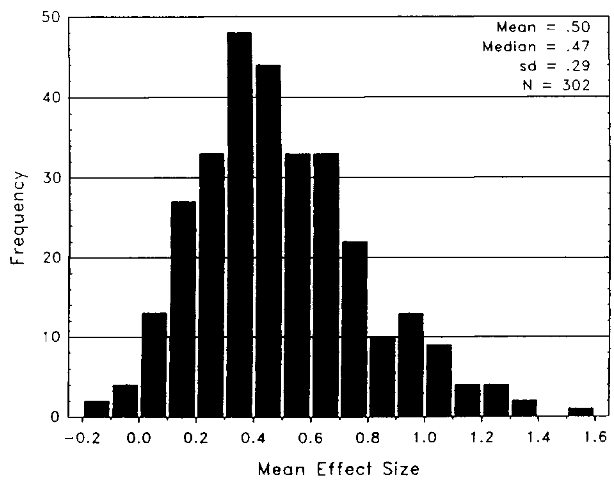
Treatment area and reference	*M* effect size	*N*
Programs for training moral judgment, junior high through adults; effects on Defining Issues Test (Schlaefli, Rest, & Thoma, 1985)[a]	0.25	55
Career education programs for K–12 students; all outcomes (Baker & Popowicz, 1983; see also 1.3.4.)	0.50	18
Guidance and counseling programs in the regular school curriculum for high school; effects on psychological maturity (Sprinthall, 1981; see also 1.3.4.)[a]	1.20	6
Career counseling interventions; all outcomes (Oliver & Spokane, 1988; see also 1.3.4.)	0.48	58
Counseling and guidance programs in high schools; all outcomes (Nearpass, 1990; see also 3.5.2.)[a]	0.38	77
Nutrition education programs for school age children; effects on knowledge behavior, and attitudes (Levy, Iverson, & Walberg, 1980)[a]	1.25	6
Vocabulary instruction, elementary through college; effects on learning and comprehension (Stahl & Fairbanks, 1986)[a]	0.90	52
(Klesius & Searls, 1990)[a]	0.32	15
Vocabulary instruction with poor readers, 3rd–12th grades; effects on word knowledge and comprehension (Marmolejo, 1990)	0.47	15
3.5.3. Preschool and special education; developmental disabilities		
3.5.3.1. Early intervention for disadvantaged or handicapped		
Headstart early childhood education programs; cognitive outcomes (Administration for Children, Youth, and Families, 1983)[a]	0.34	71
(R. C. Collins, 1984)	0.33	49
Preschool intervention programs for culturally disadvantaged children; 5–14 year follow-up effects on achievement and cognitive outcomes (Goldring & Presbrey, 1986)[a]	0.24	8
Early intervention programs for environmentally at-risk (disadvantaged) infants; effects on IQ and other variables (Casto & White, 1984; Utah State University Exceptional Child Center, 1983)[a]	0.43	26
Early intervention programs with handicapped preschoolers; all outcomes (Casto & Mastropieni, 1986; Utah State University Exceptional Child Center, 1983)[a]	0.68	74
Intervention programs for kindergarten children; all outcomes (Lewis & Vosburgh, 1988)	0.41	65
3.5.3.2. Special education programs or classrooms		
Special education classroom placement vs. regular class placement for exceptional children; effects on achievement (Carlberg & Kavale, 1980)	−0.15	50
Early childhood special education; all outcomes (Snyder & Sheehan, 1983)	0.48	8
Mainstreaming vs. segregated special education for disabled K–9 students; effects on achievement (Wang & Baker, 1986)	0.44	11
Direct instruction in special education; effects on achievement, intellectual ability, readiness skills, on-task behavior and affect (White, 1987)[a]	0.84	25
Educational interventions for at-risk populations (students in danger of failing to complete their education), K–12; effects on achievement (Slavin & Madden, 1989)[a]	0.63	28
Computer assisted instruction for exceptional (special education) students, K–12; effects on achievement (Schmidt et al., 1986; see also 3.1.1.)[a]	0.66	18
Computer aided instruction with learning disabled and educable mentally retarded students; effects on achievement (McDermid, 1990; see also 3.1.1.)	0.57	15
Cooperative learning with students with mild disabilities; effects on achievement (Stevens & Slavin, 1991; see also 3.1.4.)	0.31	11
Cooperative learning methods with handicapped K–12 students in mainstreamed classrooms; effects on achievement (Carlson, 1987; see also 3.1.4.)	0.16	13
Cooperative vs. noncooperative task arrangements for handicapped–nonhandicapped and ethnically different groups; all outcomes (Johnson, Johnson, & Maruyama, 1983; see also 3.1.4.)[a]	0.75	98
3.5.3.3. Perceptual-motor and sensory stimulation treatment for developmental disabilities		
Perceptual–motor training for learning disabled and disadvantaged children; effects on academic, cognitive, and perceptual-motor outcomes (Kavale & Mattson, 1983)	0.08	180
Frostig training for development of visual perception in children with learning problems; effects on perceptual skills and academic achievement (Kavale, 1984)[a]	0.09	59
Sensory integration therapy for patients with developmental disabilities or learning disabilities; effects on academic achievement, motor performance, and language function (Ottenbacher, 1982)	0.79	8
Clinically applied vestibular stimulation as a sensory enrichment therapy for infants at risk and children with developmental delay; effects on cognitive, language, motor, alertness, and physiological outcomes (Ottenbacher & Petersen, 1984)[a]	0.71	14
Tactile stimulation of developmentally delayed and at-risk infants; all outcomes (Ottenbacher et al., 1987)	0.58	19

Table 1 (*continued*)

Treatment area and reference	*M* effect size	*N*
Early intervention and sensory stimulation programs for organically impaired developmentally delayed children; effects on development, motor, cognitive, language, social, and self-help outcomes (Ottenbacher & Petersen, 1985)	0.97	38
3.5.3.4. Remedial language programs and bilingual instruction		
Remedial and developmental language programs for linguistically deficient or disadvantaged preschool and elementary students; outcomes on Illinois Test of Psycholinguistic Ability (Kavale, 1980, 1981, 1982)[a]	0.39	34
Bilingual vs. English instruction in K–12 school programs; effects on achievement (Willig, 1985)[a]	0.12	16
Language therapy/training for language/learning disabled children; effects on language improvement (Nye, Foster, & Seaman, 1987)[a]	1.04	43
Language interventions for preschool children; language and non-language outcomes (Piorier, 1990)[a]	0.50	61
3.5.3.5. Other special education		
Educational treatment programs for emotionally disturbed students; effects on achievement and classroom behavior (Rosenbuam, 1983)	1.02	99
Special classroom or residential treatment for behaviorally disordered students; all outcomes (Skiba & Casey, 1985)	0.93	10
Training for mentally retarded persons on memory and learning tasks; all outcomes (Mattson, 1985)[a]	0.70	96
Special remedial programs for high risk and disadvantaged college students; effects on achievement (C. C. Kulik, Kulik, & Shwalb, 1983)[a]	0.27	60
3.5.4. Teacher training		
3.5.4.1. Inservice training for teachers		
Inservice training for elementary and secondary school teachers; all outcomes (Harrison, 1981)	0.80	47
(Wade, 1984, 1985)	0.52	91
Inservice training for elementary and secondary school teachers; effect on teachers and their students (Joslin, 1981)[a]	0.47	137
Science inservice training for teachers; effects on teachers and their students (Enz, Horak, & Blecha, 1982)[a]	0.84	16
Inservice and preservice training of teachers in the inquiry strategy for teaching science; effects on teachers (Sweitzer & Anderson, 1983)[a]	0.77	68
Human relations training programs for teachers; all outcomes (A. W. Robinson & Hyman, 1984)[a]	0.51	14
Strategy analysis training for science teachers; effects on teachers and their students (Yeany & Porter, 1982)[a]	1.31	12
Classroom management training programs for teachers; effects on student achievement and, teacher and student behavior, attitudes and affect (A. W. Robinson, 1989)[a]	0.47	79
3.5.4.2. Practice or field experience during teacher training		
Practice or beginning teaching; effects on self-concept and attitudes (Colosimo, 1982, 1984)	0.30	7
Classroom field experience for college students in teacher education programs; effects on teachers (M. R. Malone, 1984)[a]	0.12	40
Field experience in instructional settings during teacher training; effects on self-concept and teaching attitudes (Samson, Borger, Weinstein, & Walberg, 1984)	0.23	38
3.6. Miscellaneous Educational Interventions		
Hawthorne effect in educational research; all outcomes (Adair, Sharpe, & Huynh, 1989)[a]	0.20	38
Placebo control group effect in educational research; all outcomes (Adair, Sharpe, & Huynh, 1990)[a]	0.62	57
Continuing medical education for physicians; all outcomes (Beaudry, 1989)[a]	0.60	41
Interventions designed to enhance the communication skills of health-care providers; effects on communication skills (Anderson & Sharpe, 1991)	0.62	25
Continuing education for nurses, test of Cervero Model; effects on nursing practice (Waddell, 1991)	0.73	34
Training programs for graduate level counselors (Microcounseling Approach); all outcomes (Baker, Daniels, & Greeley, 1990)[a]	0.63	23
(Baker & Daniels, 1989)[a]	0.83	79
Training programs for graduate level counselors (Human Resource Training/Development Approach); all outcomes (Baker, Daniels, & Greeley, 1990)[a]	1.07	8
Training programs for graduate level counselors (Interpersonal Process Recall Approach); all outcomes (Baker, Daniels, & Greeley, 1990)[a]	0.20	10
Career development courses for college students; effect on maturity and decidedness (Hardesty, 1991)	0.40	12
Interventions to modify attitudes toward persons with disabilities; effects on attitudes (Shaver, Curtis, Jesunathadas, & Strong, 1989)	0.37	273
Mass media campaigns; effects on automobile occupant restraint behavior (Moore, 1990)[a]	0.14	35

[a] Studies included in refined distribution.

Figure 1
Distribution of Mean Effect Sizes From All Meta-Analyses



duced negative mean effect sizes (and 3 of these were in the same treatment area), and relatively few mean effect sizes were in the immediate vicinity of zero. More than 90% of the mean effect sizes were 0.10 or larger, and 85% were 0.20 or larger. There is little in conventional reviews and past discussion of these treatment areas, either individually or collectively, that prepares a reviewer for the rather stunning discovery that meta-analysis shows nearly every treatment examined to have positive effects.

Indeed, the effect size distribution in Figure 1 is so overwhelmingly positive that it hardly seems plausible that it presents a valid picture of the efficacy of treatment per se. What seems more likely is that these results reflect some artifact or misrepresentation that makes them look stronger than they actually are. Before drawing any conclusion about the efficacy of psychological treatment, therefore, we must attempt to identify and examine what potential distortions we can in the distribution of meta-analysis treatment effect estimates.

Methodological Quality

One possible explanation for the strong skew toward positive effect sizes in meta-analyses of treatment research is bias resulting from the type of research designs typically used to study treatment effectiveness. Any methodological artifact that caused treatment effects to be overestimated and was also widespread in primary studies would inflate the mean effect sizes found in meta-analyses based on those studies.

It is relatively easy to identify widespread methodological features of treatment effectiveness research that would potentially act to underestimate treatment effect sizes (e.g., unreliable, insensitive, or irrelevant outcome measures and inconsistent or incomplete treatment implementation; Boruch & Gomez, 1977; Hunter & Schmidt, 1990; Lipsey, 1990). Methodological artifacts that would serve to inflate effect size estimates, however, are not so readily identifiable. The most obvious candidate is selection bias favoring treatment groups in designs that do not use random assignment to treatment conditions. If treatment groups often consist of respondents whose initial, pretreatment status is better than that of the control groups with which they are compared, their posttreatment status is also likely to be better, whether or not they have received effective treatment. Because nonequivalent comparison group and other such quasi-experimental designs are quite common in treatment effectiveness research—indeed, more common than randomized designs in many areas (Lipsey, Crosse, Dunkle, Pollard, & Stobart, 1985)—there is potential for widespread bias.

Fortunately, meta-analysts often consider the possibility that nonrandomized designs will yield different effect size estimates than randomized designs. A number of the meta-analyses listed in Table 1 provided a breakdown of the mean effect size for different design categories, typically random versus nonrandom assignment and, sometimes, one-group pre-and-post designs as well. Others divided primary studies according to some coding of methodological quality in which method of subject assignment was heavily weighted. These various stratifications make it possible to compare the distribution of mean treatment effects found for different design configurations.

Table 2 presents the mean effect sizes for different design and methodological quality categories for the subset of meta-analyses listed in Table 1 that provide such breakdowns. For purposes of Table 2, meta-analyses were selected only if they reported a mean effect size separately for different design categories or quality levels for a body of research studies in the same treatment area. In cases where more than one meta-analysis reported such information for the same treatment domain, the meta-analysis with the most complete information or, if that was equivalent, the one using the largest number of primary studies was selected.

Table 2
Methodological Quality Comparisons for Meta-Analyses Providing Information

Comparison	Effect size *M*	Effect size *SD*	*N*
Control/comparison designs			
Random studies	0.46	0.28	74
Nonrandom studies	0.41	0.36	74
Design type			
Control/comparison	0.47	0.29	45
One-group pre–post	0.76	0.40	45
Methodological quality ratings			
High	0.40	0.27	27
Low	0.37	0.29	27

Note. For each comparison, only those meta-analyses that provided a breakout for that comparison were included (e.g., 74 meta-analyses provided a mean effect size for random and nonrandom studies).

The information displayed in Table 2 reveals that the mean effect size for nonrandomized control or comparison group designs is actually slightly smaller than that for randomized designs. If we assume that the same pattern holds for those meta-analyses that did not report this comparison, we must conclude that the mean effect sizes of Figure 1 are not inflated by inclusion of studies with such designs in the respective meta-analyses. Indeed, it would appear that, if anything, inclusion of nonrandomized comparison group designs, on average, slightly suppresses the overall effect size a meta-analysis yields.

By contrast, Table 2 shows a different result when we compare the effect sizes from one-group pre-and-post designs with those from control or comparison group designs (random and nonrandom combined) for those 45 meta-analyses that included and broke out both types. One-group pre-and-post designs yielded effect sizes that averaged 61% larger than those resulting from control or comparison group designs in the same treatment areas. It seems clear, therefore, that one-group pre-and-post designs do have the potential to substantially inflate mean effect sizes if they are included in a meta-analysis (more on this later).[2]

Also included in Table 2 are the results of comparing effect sizes for studies rated high in methodological quality with those rated low among meta-analyses that coded quality and reported a breakdown. Methodological quality is coded many different ways by meta-analysts. Most schemes represent internal validity as a predominant component, especially whether assignment to conditions was randomized. Some schemes, however, include various other factors related to construct, statistical conclusion, or external validity.

As Table 2 indicates, the 27 meta-analyses that compared mean effect sizes for studies rated high and low for methodological quality found little difference. As with the random versus nonrandom comparison studies, the small difference favored higher quality studies. Again, we see that inclusion of lower quality studies in these meta-analyses would, on average, slightly lower the overall mean effect size found, not inflate it.

Further evidence on this point is provided by 23 additional meta-analyses that reported the correlation between study-level effect sizes and the meta-analyst's ratings of the methodological quality of the studies. The mean correlation for those meta-analyses, weighted by the number of studies contributing to each meta-analysis, was −.01. Although the direction of this relationship is for lower quality studies to have higher effect sizes, its magnitude is so close to zero that it represents no inconsistency with the results reported earlier.

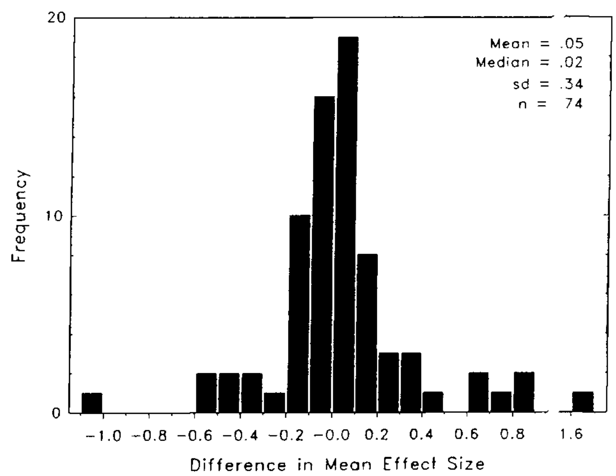
It may be useful to emphasize what is and is not implied by the foregoing analysis. These various comparisons do not indicate that it makes no difference to the validity of treatment effect estimates if a primary study uses random versus nonrandom assignment to conditions. Nor do they indicate that methodological quality is not important. What these comparisons do indicate is that there is no strong pattern or bias in the direction of the difference made by lower quality methods. In a given treatment area, poor design or low methodological quality may result in a treatment estimate quite discrepant from what a better quality design would yield, but it is almost as likely to be an underestimate as an overestimate.

This general point is made more evident if, instead of comparing effect size means for different design types, we difference those means within a given meta-analysis and examine the distribution of differences. Figures 2, 3, and 4 show the distributions of such differences for the three methodological comparisons in Table 2. The differences between effect size estimates based on randomized versus nonrandomized designs, for example, ranges from near −1.00 to over 1.00, even though the mean difference is modest (Figure 2). In some treatment areas, therefore, nonrandom designs (relative to random) tend to strongly underestimate effects, and in others, they tend to strongly overestimate effects. The distribution of differences on methodological quality ratings shows a similar pattern (Figure 4).

The type of control or comparison design and overall methodological quality do matter, therefore, but no consistent pattern emerges in the direction of bias introduced when less valid approaches are used. Quite a different pattern appears, however, with one-group pre-and-post designs, which, as Figure 3 shows, generally overestimate treatment effects.

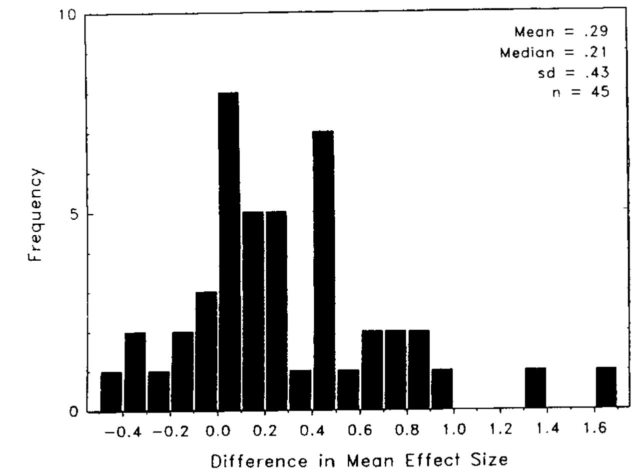
In all of these cases the results shown here provide no warrant for researchers to neglect the principles of

Figure 2
Distribution of Differences in Mean Effect Sizes for Random Minus Nonrandom Designs



[2] It is an open question why one-group pre-and-post studies yield inflated effect sizes. This may be an artifact of how meta-analysts handle the correlated scores from these studies when computing effect sizes, a confounding of maturational effects with treatment effects, or any of a number of other possibilities.

Figure 3
Distribution of Differences in Mean Effect Sizes for One-Group Pre–Post Minus Comparison Group Designs



good research design on the grounds that it makes no difference in the ultimate treatment effect estimate. What these results do show is that, for the range of treatment areas represented in available meta-analyses, no substantial skew in the distribution of treatment effect estimates is apparent because those meta-analyses include studies with nonrandomized comparison groups or weaker overall methodological quality. These factors, therefore, are not sufficient to account for the strong positive trend in mean treatment effect estimates shown in Figure 1.

One-group pre-and-post designs, on the other hand, clearly are capable of upwardly biasing the mean treatment effect estimates derived from meta-analysis. If many of the meta-analyses whose results are plotted in Figure 1 included a relatively high proportion of such studies, that fact might well account for the strong positive results shown there. As it happens, few of those meta-analyses did include pre-and-post studies and, for those, they represented a modest proportion of the total. After looking at some other potential biasing factors, we will refine the distribution of treatment effects to eliminate this source of bias.

Availability Bias

Another factor that might inflate the treatment effect estimates found in meta-analysis is a bias in the way meta-analysts select studies to include in their syntheses. If, from the whole population of eligible studies, those studies most readily available to meta-analysts, and thus most likely to be included, tended to show larger effects, whereas those not included showed smaller effects, the result would be a regular overestimation of treatment effects. The easiest studies to identify and locate in a meta-analysis, of course, are those that are formally published in journals and books and hence have the highest probability of being known to and cited by researchers in the field, listed by the major bibliographic services (e.g., Psychological Abstracts), and found in university libraries.

There is good reason to believe that published studies of treatment effectiveness research will tend to show higher effect sizes than unpublished studies (Greenwald, 1975). Authors may be more likely to attempt to publish a study that finds large, statistically significant effects (even though such results can occur solely by chance). Journal editors and reviewers, in turn, are likely to look more favorably on such results when they are submitted for publication. Moreover, there is direct evidence that larger effect sizes do indeed appear more frequently in the published than the unpublished research on the same treatment (Smith, 1980).

The question for our assessment of the strongly positive mean effects displayed in Figure 1, therefore, is whether they can be explained by differential effect sizes in published versus unpublished research combined with oversampling of published studies in the typical meta-analysis. Because many meta-analysts show some awareness of this issue, it is not uncommon for them to stratify the studies in their synthesis and report mean effect sizes separately by publication source. This provides a database we can use to examine the role that availability bias may have made in the overall distribution of treatment effects.

Separate estimates of the mean treatment effect for published versus unpublished studies were extracted whenever possible from each meta-analysis listed in Table 1. If two meta-analyses in the same treatment area yielded estimates, the one with the larger number of primary studies was selected. A total of 92 meta-analyses provided nonredundant comparisons by publication source; the results are presented in Table 3. As shown, there is clearly a differential between the mean treatment effect size es-

Figure 4
Distribution of Differences in Mean Effect Sizes for High Minus Low Methodological Quality

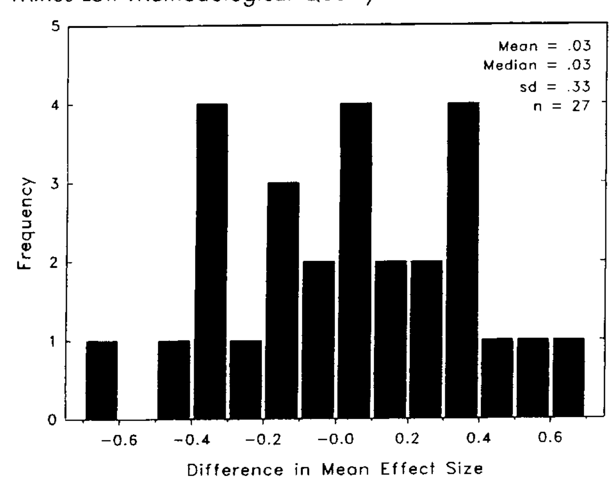


Table 3
Comparison of Effect Sizes Reported in Published Versus Unpublished Studies

	Effect size		
Document source	*M*	*SD*	*N*
Published studies	0.53	0.30	92
Unpublished studies	0.39	0.28	92

Note. Only those meta-analyses that provided a breakout for this construct were included.

timate derived from published studies and that derived from unpublished studies within the same set of meta-analyses. Published studies yielded mean effect sizes that averaged 0.14 *SD*s larger than unpublished studies. It is evident, therefore, that the treatment effects reported in published studies are indeed generally biased upward, relative to those in unpublished studies.

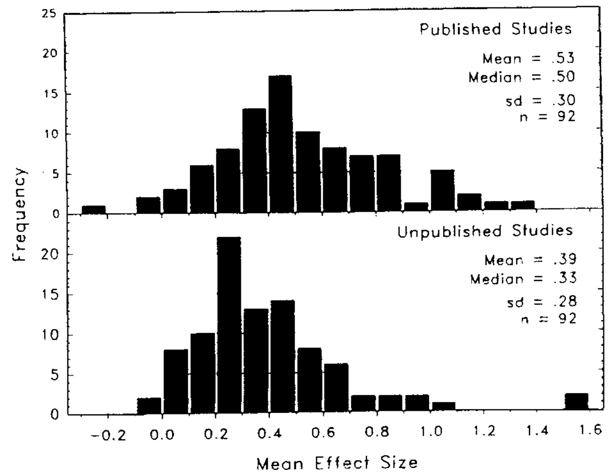
It is noteworthy, however, that the mean effect size estimates for both published and unpublished studies fall in the positive range; published studies are just more positive than unpublished studies. We would still find positive mean effect sizes in most treatment areas, even if we made the estimate entirely from the results of unpublished studies. This is shown in Figure 5, which plots the distributions for the effect size estimates summarized in Table 3. Even if we look at only the distribution of mean effect size estimates from unpublished studies, we find that nearly 89% are 0.10 or greater and 78% are 0.20 or greater. Moreover, because the true mean effect size for a given treatment across the full population of eligible studies should lie somewhere between the separate estimates from published and unpublished studies, we can be relatively confident that the distribution across treatment areas will be more positive than the estimates derived from unpublished studies alone.

Oversampling of published studies in a meta-analysis, therefore, does indeed upwardly bias treatment effect estimates. The amount of that bias, however, does not appear to be large enough to account for the generally positive findings of the meta-analyses conducted on psychological treatment research. Nonetheless, to get a better assessment of the distribution of the effects of psychological treatment, we should restrict our attention to those meta-analyses that base their estimates on both published and unpublished studies. We will return to this issue after examining additional factors that may be implicated in the positive findings of Figure 1.

Small Sample Bias

Hedges (1981) has demonstrated that the mean of effect sizes based on small subject samples is biased upward as a statistical estimator of the population effect size mean. If a sizeable proportion of the mean effect sizes represented in Figure 1 were based on studies using small samples, this bias might account for part of the skew toward positive effects.

Figure 5
Distributions of Mean Effect Sizes From Published and Unpublished Studies for Meta-Analyses Reporting Both Breakouts



The magnitude of the small sample bias can be easily calculated and proves to be negligible for effect sizes based on a total sample size of 50 or more (e.g., 25 each in the treatment and control conditions). Indeed, the total sample size must be as small as 10 or less before the bias is appreciable, that is, 10% or more inflation. Table 4 shows the actual mean effect sizes based on different sized samples for the 39 independent meta-analyses from Table 1 that broke out their results by sample size. Another 25 reported the correlation between sample size and effect size. The mean correlation for these meta-analyses, weighted by the number of studies contributing to each meta-analysis, was only −.03.

Table 4 shows that the difference between mean effect sizes based on samples of 50 or less was only 0.06 larger than that based on samples of 51–100. Even if a large

Table 4
Comparison of Effect Sizes Based on Studies With Different Sized Samples

	Effect size		
Sample size	*M*	*SD*	*N*
N less than 50	0.58	0.32	39
N 51 to 100	0.52	0.43	39
N more than 100	0.35	0.30	39

Note. Only those meta-analyses that provided a breakout for this construct were included.

proportion of the studies represented in the mean effect sizes of Figure 1 was based on small samples, therefore, the upward bias from that source would be modest. In fact, of the 134 meta-analyses that reported sample size information, the mean sample size per primary study was 122. Therefore, the typical effect size contributing to the means represented in Figure 1 does not appear to be based on small enough samples to yield appreciable bias from that source.

It is worth noting that for the 39 meta-analyses contributing to Table 4, effect sizes based on samples of more than 100 were considerably smaller than those for both categories of lesser sample sizes. This difference is not attributable to the statistical bias inherent in small sample estimation of effect sizes, because as noted above, that bias is known to be negligible for samples over about 50. Apparently this pattern represents an empirical finding that perhaps reflects distinctive differences in the nature of studies conducted with larger samples. Such studies may use different treatment variants, less well-implemented treatments, or different measures or methods—any one of which might influence effect size.

Table 5
Comparison of Effect Sizes Based on Studies With Different Control Conditions

Control condition	Effect size *M*	Effect size *SD*	*N*
No treatment control	0.67	0.44	30
Placebo treatment control	0.48	0.26	30

Note. Only those meta-analyses that provided a breakout for this construct were included.

Generalized Placebo Effect

Still another possible explanation for the strongly positive effects found in meta-analyses of studies of psychological treatment is that such positive effects are not actually due to the specific efficacy of the treatments provided. This might happen if the superiority of treatment group performance that is reflected in meta-analysis effect sizes resulted from some sort of placebo effect on the treatment group.[3] It may be that those generalized effects of treatment that are not usually present for control groups (e.g., receiving attention and having positive expectations) have fairly universal positive effects that show up in meta-analysis, even though the distinct elements of the treatments provided are ineffectual.

The hypothesis of a generalized placebo effect that yields widespread positive treatment effects is more difficult to appraise and interpret than the factors considered earlier. Two questions need to be addressed. First, is there any evidence that the generally positive effects of treatment meta-analyses could be accounted for by placebo effects alone? Second, even if they could, does that really undermine the claim that psychological treatment is generally efficacious? We will consider each of these in turn.

One line of evidence bearing on the placebo issue can be derived from the meta-analyses listed in Table 1. In some of the treatment areas represented there, placebo control groups are occasionally included in studies of treatment effects. And, in some cases, the meta-analyst coded and reported information on the effect size for the contrast between treatment and placebo controls separately from that between treatment and no treatment controls. Extraction of those separate estimates for 30 independent meta-analyses yielded the results shown in Table 5.

Table 5 shows that treatment effects estimated relative to placebo controls are indeed smaller, on average, than those estimated relative to no-treatment control conditions. Those effects do not reduce to zero, however. The distribution of effects relative to placebo still falls largely in the positive range (90% greater than 0.20) and thus shows evidence of "value added" by treatment beyond that attained with administration of placebos. The data for this comparison is limited, however. Rather few meta-analyses reported separate effect estimates for placebo controls, and the majority of those were in the area of mental health and thus do not necessarily extend to other treatment domains.

Another line of evidence on the generalized placebo hypothesis comes from those treatment research domains in which the customary comparison is not between a treatment condition and a "no-treatment" control condition but, rather, between the treatment of interest to the researcher (usually an innovative or experimental treatment) and "treatment as usual." This situation is often found, for instance, in research on educational interventions. A new curriculum is compared with the old curriculum, an open classroom is compared with a "normal" classroom, and so forth. The question in these studies is not whether the treatment of interest is better than nothing—because nothing is not a realistic option in the relevant settings—but whether it is better than established or traditional treatment.

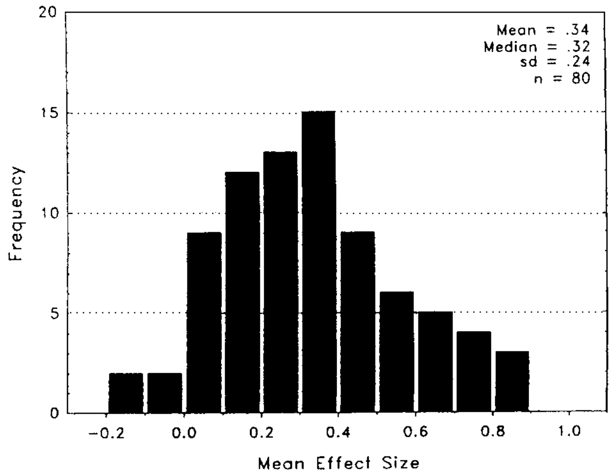
Positive treatment effect sizes in these domains are analogous to those derived from treatment versus placebo comparisons. To the extent that an experimental treatment shows better results than treatment as usual, it must be adding some useful element above and beyond generalized placebo effects (which presumably would also be represented in treatment as usual).

In Figure 6 the mean effect sizes are plotted for those independent meta-analyses of educational interventions in Table 1 that were based, as nearly as we could tell, on studies in which treatment versus treatment-as-usual comparisons predominated. As can be seen, this distribution also falls largely in the positive effect range and thus provides little indication of treatment effects attributable entirely to generalized placebo effects.

What we can glean from the limited analysis above is that there are quite likely some generalized placebo

[3] Thanks to J. D. P. Sinha for this suggestion.

Figure 6
Distribution of Mean Effect Sizes for Educational Studies in Which the Control Group Received an Alternate or Traditional Treatment



effects that contribute to the overall effects of psychological treatment, but their magnitude does not seem sufficient to fully account for those overall effects.

To the extent that the treatment effects shown in Figure 1 do reflect a boost from a generalized placebo effect, however, it is arguable whether this undermines their validity. In psychological treatment, unlike medical treatment, it is conceptually difficult to distinguish placebo effects from the treatment with which they are associated. In medical treatments a relatively clear separation is possible between the nature of, say, surgical or pharmaceutical intervention and the accompanying patient morale, expectations, social interaction, and the like. Psychological treatment, on the other hand, is often presumed to work through just those mechanisms of social interaction, expectations, and attitude change that likely constitute the key elements of the placebo effect. As Wilkins (1986) has argued, placebo effects may be constituent parts of psychological treatment, not artifacts to be separated out in any assessment of that treatment.

Summary of Identifiable Influences on Observed Effect Sizes

The considerations examined earlier indicate that there are indeed some factors that may upwardly bias the mean effect sizes shown in Table 1 and Figure 1. Two such factors are especially notable. First, one-group pre-and-post designs for assessing treatment effects seem almost universally to overestimate the size of those effects relative to randomized studies of the same treatment. Meta-analyses based in substantial part on such studies, therefore, cannot be accepted as sources of good estimates of the efficacy of treatment.

Second, it seems clear that there is a differential between the effect sizes derived from published studies and those found in unpublished studies of a given treatment. Published studies are more likely to report stronger—that is, larger and more positive—effects than unpublished studies. It follows, therefore, that meta-analyses based only on published studies cannot be expected to yield good estimates of overall treatment effects.

In addition, it seems likely that some portion of the positive results of psychological treatment stems from generalized placebo effects rather than the specific effects of the treatment delivered. The indication from the meta-analyses reviewed here, however, is that positive treatment effect sizes cannot be accounted for entirely by generalized placebo effects; indeed, such effects are rather modest. Moreover, given the inherently psychological nature of psychological treatment, it is arguable whether generalized placebo effects should be excluded from consideration when assessing such treatments.

Because the mean effect size array in Figure 1 includes the results of meta-analyses based, in part, on pre-and-post studies and those restricted to published studies, we must, therefore, ask whether those factors account for the surprisingly positive effects displayed there. It is worth remembering, incidentally, that there are many factors that may reduce observed effect sizes that cannot be examined in the available meta-analysis results. Our concern, however, is to guard against an overly optimistic assessment of treatment efficacy, and we thus emphasize those factors that may produce upward bias in effect sizes.

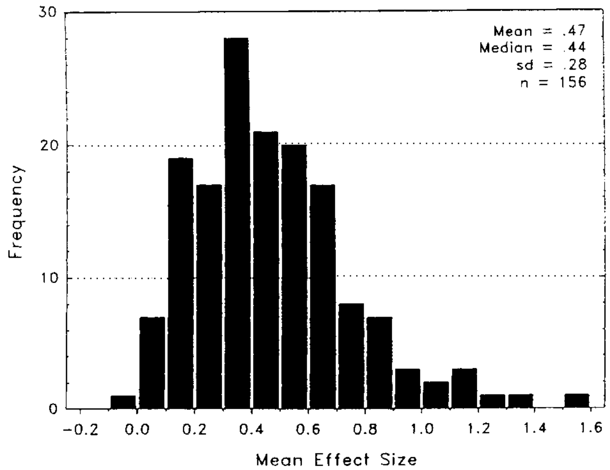
Refined Examination of the Distribution of Treatment Effects

We are now in a position to make a more refined and probing assessment of the distribution of mean treatment effects reported in the meta-analyses of Table 1 and Figure 1. For this purpose, we make the following selections: (a) We use only treatment effect estimates based on control or comparison group designs and eliminate those based on or mixed with estimates derived from one-group pre-and-post designs; (b) we use only treatment effect estimates based on both published and unpublished studies; and (c) in cases in which two or more meta-analyses cover the same or highly overlapping research literatures, we retain only the treatment estimates from the meta-analysis with the broadest coverage, that is, the largest number of studies.

The result of these refinements is a distribution of mean treatment effect estimates that are relatively independent, that is, do not substantially share studies or respondents, and that eliminate or at least appreciably reduce the biases identified earlier. In particular, this distribution should not seriously overestimate treatment effects because of inclusion of estimates based on one-group pre-and-post studies or because of estimates based entirely on published studies. The studies contributing mean effect sizes to this refined distribution are marked with superscript[a] in Table 1, presented earlier.

This refined distribution, shown in Figure 7, provides a reasonable basis for assessing the general efficacy of psychological, educational, and behavioral treatment.

Figure 7
Refined Distribution of Mean Effect Sizes From Selected Meta-Analyses



Collectively, the 156 meta-analyses represented there encompass approximately 9,400 individual treatment effectiveness studies (M = 60 per meta-analysis) and more than one million individual subjects (M = 8,055 per meta-analysis), estimated on the basis of the 59 meta-analyses that reported sample size information.

The grand mean treatment effect in this refined distribution is 0.47 *SD*s. That is, the average treatment group scored 0.47 *SD*s higher on the average outcome measure than did the average control group.[4] Moreover, Figure 7 shows a relatively modest proportion of meta-analyses that yielded mean treatment effect sizes in the zero and negative range. In particular, 83% of the mean effect sizes in the refined distribution were 0.20 or greater. Only one was actually negative. We are left, therefore, with the same observation with which we began—the evidence from meta-analysis indicates that the psychological, educational, and behavioral treatments studied by meta-analysts generally have positive effects. We turn now to the question of whether those positive effects are of meaningful practical magnitude.

Statistical Versus Practical Effects

Treatment effect estimates in standard deviation units have little intuitive meaning. How much of a treatment effect is 0.20 or 0.50 of a standard deviation? Is it possible that, although most of the mean effect sizes in Figure 7 are numerically positive, they represent small effects that are not practically or clinically meaningful in the contexts in which the respective treatments are applied?

The issue of relating statistical differences in measured treatment outcomes to practical significance is a complex and difficult one. The thrust of discussion in the technical literature, however, is recognition that numerically small statistical effects do not necessarily imply small practical effects (Abelson, 1985; Carver, 1975; Lipsey, 1990; Rosenthal & Rubin, 1982; Sechrest & Yeaton, 1982). One useful demonstration of this point is the translation of such bivariate statistical information as effect sizes into a more intuitively comprehensible form. Rosenthal and Rubin have suggested use of the binomial effect size display (BESD), a depiction of effects in terms of the proportion of treatment versus control subjects above a common success threshold (defined arbitrarily as the overall median).

In BESD terms, the grand mean effect size of 0.47 from the meta-analyses in Figure 7 can be represented as a contrast between a treatment group with a success rate of 62% versus a control group with a success rate of 38%. A 24-percentage-point spread between treatment and control success rates hardly sounds like a negligible difference. Correspondingly, an effect size of 0.20 translates to a 10-percentage-point spread between the treatment and control success rate, 55% versus 45%. Note that a 10% improvement on a 45% (control) baseline represents an increase of more than 20% (10/45)—a value that is hard to declare categorically trivial.

The practical significance of an effect, of course, is very much dependent on the nature of the outcome at issue and its importance to patients or clients. In a life-and-death situation, a mortality decrement of 5% or less may well be clinically significant. Rosenthal (1991b) has observed, for example, that the physicians' study on the effects of aspirin on heart attacks was judged conclusive and prematurely ended when the effect size reached 0.07 (in *SD*s), equivalent on the BESD to less than a 3.5-percentage-point spread between treatment and control groups.

Although psychological treatments rarely deal with life-and-death issues, it is illuminating to compare the range of statistical effects shown in Figure 7 with the effects of medical treatment, a domain of acknowledged (though not universal) efficacy. To accomplish this, we searched for meta-analyses of medical interventions whose results were stated in, or could be converted to, the standard deviation metric so that they could be compared with the results of psychological interventions shown in Figure 7.[5] We did not attempt to be exhaustive and doubtless missed many pertinent reports. For those reports we found, however, we exercised no selectivity other than requiring statistically comparable effect metrics and a summary judgment by the author of the report that the treatment was judged effective. This latter requirement

[4] This value is not greatly different from the grand mean effect size of the unrefined distribution (Figure 1), which was 0. 50 SDs, nor do the standard deviations of these distributions differ greatly. Inclusion of meta-analyses using one-group pre-and-post studies, selecting only published studies, or overlapping the research base of other meta-analyses thus did not strongly bias the distribution of Figure 1, although the potential was certainly there.

[5] Although a considerable number of meta-analyses have been conducted in the medical field, most report only odds ratios or other effect indicators that cannot generally be converted to the standardized mean difference metric without additional information (e. g., marginals or base rates) that is often unreported.

ensured that we were comparing psychological treatment to successful medical treatment and not to treatment failures.

The results of the meta-analyses of medical treatment that we found by this procedure are presented in Table 6 under three headings. The first listing is for the mean effects of successful medical intervention on mortality. Not surprisingly, given the life-and-death issue involved, treatments yielding numerically small effect sizes were nonetheless judged beneficial. The range of effect sizes for these treatments (0.08 to 0.47) falls below the grand mean effect size for psychological treatment (see Figure 7).

The second section of Table 6 shows the mean effect sizes on medical outcomes other than mortality for various treatments judged beneficial in the meta-analysis reports we located. These effect sizes ranged from 0.24 to 0.80, quite comparable with the range of effect sizes shown in Figure 7 for psychological treatments.

Finally, in the last section of Table 6, we present the results of those meta-analyses we located that estimated the effects of medical interventions on psychological or behavioral outcomes, not unlike many of those represented in Figure 7. These mean effect sizes varied from 0.11 to 0.96, a range that, once again, fell well within that shown in Figure 7 for psychological treatment effects.

The point of these comparisons is not to argue that psychological treatment is as effective as medical treatment. There are too many differences in treatment, respondents, research contexts, and the nature of the outcome variables to make such a simple claim. Furthermore, it may well be that psychological treatment aimed at, say, improvement in employees' job satisfaction needs to achieve much larger statistical effects to have noticeable consequences than a medical treatment for angina. What does seem clear, however, is that in assessing meta-analytic estimates of the effects of psychological, educational, and behavioral treatment, we cannot arbitrarily dismiss statistically modest values (even 0.10 or 0.20 *SDs*) as obviously trivial. Translated into BESD success rates, they do not seem indisputably negligible and comparable numerical values are judged to represent benefits in the medical domain, even when similar outcome variables are at issue. On balance, therefore, the magnitude of effect size estimates that meta-analysis reveals for psychological treatment seems sufficiently large to support the claim that such treatment is generally efficacious in practical as well as statistical terms.

Table 6
Selected Meta-Analyses of Medical Treatment Judged Effective

Outcome variable	Mean effect size or effect size range
Medical	
Mortality	
Aortocoronary bypass sugery (Lynn & Donovan, 1980)	0.15
AZT for AIDS (Rosenthal, 1991b)	0.47
Cyclosporine in organ transplants (Rosenthal, 1991b)	0.30
Chemotherapy for breast cancer (EBCTCG, 1988)	0.08 to 0.11
Intravenous streptokinase for myocardial infarction (Stampfer, Goldhaber, Yusuf, Peto, & Hennekens, 1983)	0.08
Other outcomes	
By-pass surgery; effects on angina (Lynn & Donovan, 1980)	0.80
Dipyridamole; effects on angina (Sacks, Ancona-Berk, Berrier, Nagalingam, & Chalmers, 1988)	0.24
Drug treatment for arthritis; various outcomes (Felson, Anderson, & Meenan, 1990)	0.45 to 0.77
Cyclosporine; effects on organ rejection (Rosenthal, 1991b)	0.39
Anticoagulants; effects on thromboembolism rates (Chalmers, Matta, Smith, & Kunzler, 1977)	0.30
Psychological	
Drug treatment for behavioral disorders; behavioral and cognitive outcomes (Kavale & Nye, 1984)	0.28 to 0.74
Electroconvulsive therapy; effects on depression (Janick et al., 1985)	0.80
Drug treatment for hyperactivity; cognitive, behavioral and social outcomes (Kavale, 1982; Ottenbacher & Cooper, 1983; Thurber & Walker, 1983)	0.47 to 0.96
Neuroleptic drugs for dementia; effects on agitation (Schneider, Pollock, & Lyness, 1990)	0.37
Hypertensive drug therapy; effects on quality of life (Beto & Bansal, 1992)	0.11 to 0.28

Discussion

What we conclude from this broad review of meta-analytic evidence is that well-developed psychological, educational, and behavioral treatments generally have meaningful positive effects on the intended outcome variables. The number and scope of effective treatments covered by this conclusion are impressive, and the magnitude of the effects for a substantial portion of those treatments is in a range of practical significance by almost any reasonable criterion.

Furthermore, we have found that this broad positive finding cannot be explained away by any simple hypotheses of bias stemming from inclusion of studies using weak research designs, oversampling of published studies, or heavy representation of very small sample studies. Also, whether one views placebo effects as artifacts that inflate treatment results or an inherent constituent of psychological treatment, their magnitude appears to be too modest to account for more than a portion of the generally positive effects of such treatment.

We acknowledge that the information available in the current treatment research meta-analysis literature is too crude to permit a truly probing analysis of the potential biases in estimates of treatment effect sizes. Thus the factors we examined may still create bias in ways too subtle for us to detect, or other artifacts we did not or could not examine may yet account for the broad positive findings. On the other hand, it is worth mentioning once again that most of the factors with potential to bias treatment effect estimates that come readily to mind operate to produce underestimates, not overestimates. If the treatment effect estimates in Figure 7 were corrected for unreliability of measurement, range restriction, incomplete treatment implementation, and variability due to stable individual differences in respondents, they would certainly increase appreciably (see Boruch & Gomez, 1977; Hunter & Schmidt, 1990; Lipsey, 1990). Therefore, although the description presented here may still overestimate treatment effects due to unexamined sources of upward bias, it also almost certainly reflects the influence of substantial downward bias.

We thus believe that a strongly favorable conclusion about the efficacy of well-developed psychological treatment is justified by the results of meta-analytic investigation. We must emphasize, however, the limitation of this conclusion to well-developed treatment approaches and elaborate on what that means. The sweep of the positive findings reported here is so broad as to perhaps suggest that virtually everything works in psychological treatment. That would be a false conclusion. The meta-analytic evidence reviewed here, despite its breadth, falls far short of encompassing the full range of psychological, educational, and behavioral practice. Meta-analysis is only possible for treatment approaches that have generated a corpus of research sufficient in quantity and comparability for systematic analysis within a statistical framework. Such a body of studies, in turn, is only likely to be produced for widely used and well-developed approaches growing out of established theory or practice, or for promising innovations. Thus the treatment approaches represented in meta-analysis and reviewed in this article represent rather mature instances that are sufficiently well developed and credible to attract practitioners and sufficiently promising (or controversial) to attract a critical mass of research. For treatment approaches meeting these criteria, it is perhaps not surprising that a high proportion do prove at least moderately efficacious.

What may be more surprising to those not familiar with the advantages of meta-analysis as a research synthesis technique is the failure of conventional research-reviewing techniques over the decades to identify more decisively the generally positive effects of psychological treatment. Indeed, most of the meta-analyses reviewed here are introduced with commentary on the inconclusiveness or controversy of prior conventional research reviews. If well-developed and well-researched treatments are broadly and robustly skewed toward positive results, as shown in Figures 1 and 7, why has this not been more readily apparent from conventional reviews of the respective research?

The fault here almost surely lies with the flaws in conventional reviewing practice. This has been discussed extensively elsewhere (e.g., Cook & Leviton, 1980; Hunter & Schmidt, 1990; Schmidt, 1992), but the essence of the problem with conventional research reviews is a naive use of vote-counting assessments of the statistical significance of study outcomes (Hedges & Olkin, 1980). When alpha is set at the usual levels (e.g., .05) to limit Type I error, Type II error will be unrestrained and can range very high (e.g., 50%–90%) unless sample sizes are quite large (Schmidt, 1992). Type II error, recall, is the probability of failing to reject the null hypothesis when, in fact, it is false. Because, as Figures 1 and 7 show, the null hypothesis is generally false in the treatment research reviewed here and, also, sample sizes are modest, high Type II error rates will result in a large proportion of spurious null (statistically nonsignificant) results in treatment research.[6] Conventional reviewers inspecting a body of treatment research in which a sizeable proportion of studies did not yield statistically significant results have generally not recognized the high Type II error rates and have felt that there was little basis for judging the treatment to be broadly efficacious.

Meta-analysis, by comparison, is based on an aggregation of statistical estimates of the magnitude of treatment effects irrespective of whether, individually, they are statistically significant. Statistical tests are then applied to the aggregate results (e.g., the mean and variance of the distribution of study level effect sizes; Hedges & Olkin, 1985; Hunter & Schmidt, 1990). The aggregation of samples inherent in meta-analysis greatly increases statistical power and decreases Type II error. In cases in which the null hypothesis is false (i.e., treatment is effective) and individual studies use modest sample sizes (e.g., under 500), therefore, the conclusions of meta-analysis can diverge markedly from those of conventional reviews. The evidence reviewed here indicates that psychological treatment generally presents such a case.

Although meta-analysis offers significant advantages as a research synthesis technique, especially with regard to statistical issues, it is not without limitations of its own. Most striking in the present situation are the deficiencies in practice, rather than those inherent in the technique itself. In the applications reviewed above, simple checks of the dependence of the effect size estimates on the methodological characteristics of the primary

[6] The mean sample size for the studies in those 156 meta-analyses in Figure 7 that reported usable sample size information was 134, or about 67 each in the treatment and comparison groups. The statistical power with that sample size, alpha equal to .05, and a treatment effect of 0.47 (the mean in Figure 7), is 0.76. Thus despite the positive treatment effect in this average case, 24% of the individual studies would be expected to yield statistically nonsignificant results; that is, the Type II error rate equals .24. As the effect size (ES) ranges below the mean of 0.47, or sample size falls below 67 per group, power drops off quite sharply. With ES = 0.20 and n = 50, for example, the Type II error rate jumps to .83.

studies or the extent of the sampling of unpublished studies are far from universal. Moreover, most of these meta-analyses were confined to estimating the mean effect size over the studies of interest with little attention, beyond crude stratifications, to probing the variations in treatments, respondents, and outcomes that would better reveal the circumstances of more and less effective implementations. As a consequence, what is learned about psychological treatment from these hundreds of meta-analyses is well short of the potential inherent in meta-analytic technique.

Moreover, even in its most advanced and differentiated form, meta-analysis is limited by the nature of the primary studies to which it is applied. Those studies too often report only crude comparisons between undifferentiated "black box" treatment packages and control conditions with little attention to potential interactions with client characteristics, the range of outcome variables, or temporal factors (Lipsey, 1988; Lipsey et al., 1985).

The proper agenda for the next generation of treatment effectiveness research, for both primary and meta-analytic studies, is investigation of which treatment variants are most effective, the mediating causal processes through which they work, and the characteristics of recipients, providers, and settings that most influence their results. Such a research agenda is justified by a basic assumption that psychological treatment can be, and generally is, effective, so that the questions of interest are not whether it works but how it works and how it can be made to work better. The present generation of meta-analytic research supports that assumption.

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