

EVIDENCE-BASED PRACTICES

Intern Seminar

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[The data] fail to prove that psychotherapy, Freudian or otherwise, facilitates the recovery of neurotic patients...[These findings] should give pause to those who wish to give an important part in the training of clinical psychologists to a skill unsupported by any scientific acceptable evidence. (Eysenck, 1952).

I read a paper at the General Annual Meeting of the British Psychological Society at Oxford, which was very well attended, on the Effect of Psychotherapy; this was later published in 1951 in the Journal of Clinical Psychology. At the end of the paper a Professor of Psychiatry got so enraged that he raced up the aisle shouting traitor, traitor and tried to engage me in fisticuffs, which was rather unwise as he was both old and fat, whereas I had been boxing for the university team! Fortunately he was restrained by some friends who pointed out to him that rational arguments are not negated by displays of physical aggression. (Eysenck, 1979)

Variable, often contradictory, conclusions have been reached by reviewers not only because of opposing philosophical and theoretical positions but also because of a lack of methodological sophistication. (Smith, 1980)

We urge APA site visitors for accreditation of doctoral programs make training in empirically validated treatments a high priority for review...We believe that, regardless of how well established a treatment procedure is, if APA is to sponsor a continuing education program, the organizers and presenters should be required to state in all promotional materials whether their techniques are empirically validated. (Task Force on Promotion and Dissemination of Psychological Procedures, 1995)

Internship programs were unlikely to require that students be competent in even one empirically validated treatment by the end of the internship year. (Crits-Christoph, Frank, Chambless, Brody, & Karp, 1995)

There is no reason to believe that partitioning interventions strategies by "brand names" carves nature at the joints. (Shapiro, 1996)

Funding agencies are enamored with manualized treatments, yet such an a priori or linear philosophy is inconsistent with the current clinical zeitgeist. (Stiles, et al., 1994)

Would we not be better off as a discipline inviting people to tell the real stories of their work -- to consider their own role as coparticipants in designing the questions, choosing participants, shaping the context, and structuring the results; choosing the language that seems to them suitable for sharing what they learn; and, in general, reflecting on the complexities of the process of knowing? Writing in APA style does indeed embody an epistemological stance and is, therefore, confining in these times of paradigm shifts and epistemological soul searching. (Josselson & Lieblich, 1996)

TABLE 5.7 Sequential listing of factors common across therapies that are associated with positive outcomes

<i>Support Factors</i>	<i>Learning Factors</i>	<i>Action Factors</i>
Catharsis	Advice	Behavioral regulation
Identification with therapist	Affective experiencing	Cognitive mastery
Mitigation of isolation	Assimilation of problematic experiences	Encouragement of facing fears
Positive relationship	Changing expectations for personal effectiveness	Taking risks
Reassurance	Cognitive learning	Mastery efforts
Release of tension	Corrective emotional experience	Modeling
Structure	Exploration of internal frame of reference	Practice
Therapeutic alliance	Feedback	Reality testing
Therapist/client active participation	Insight	Success experience
Therapist expertness	Rationale	Working through
Therapist warmth, respect, empathy, acceptance, genuineness		
Trust		

Lambert, M.J. & Bergin, A.E. (1994). The effectiveness of psychotherapy. In Bergin, A.E. & Garfield, S.L. (Eds.), Handbook of psychotherapy and behavior change, 4th edition. NY: John Wiley.

Table 1
Criteria for Empirically-Validated Treatments

Well-Established Treatments	
I.	At least two good between group design experiments demonstrating efficacy in one or more of the following ways:
A.	Superior to pill or psychological placebo or to another treatment.
B.	Equivalent to an already established treatment in experiments with adequate statistical power (about 30 per group; cf. Kazdin & Bass, 1989).
OR	
II.	A large series of single case design experiments ($n > 9$) demonstrating efficacy. These experiments must have:
A.	Used good experimental designs and
B.	Compared the intervention to another treatment as in I.A.
FURTHER CRITERIA FOR BOTH I AND II:	
III.	Experiments must be conducted with treatment manuals.
IV.	Characteristics of the client samples must be clearly specified.
V.	Effects must have been demonstrated by at least two different investigators or investigatory teams.
Probably Efficacious Treatments	
I.	Two experiments showing the treatment is more effective than a waiting-list control group.
OR	
II.	One or more experiments meeting the Well-Established Treatment Criteria I, III, and IV, but not V.
OR	
III.	A small series of single case design experiments ($n > 3$) otherwise meeting Well-Established Treatment Criteria II, III, and IV.

Table 2
Examples of Empirically Validated Treatments

Well-Established Treatments	Citation for Efficacy Evidence
ANXIETY AND STRESS:	
Cognitive behavior therapy for panic disorder with and without agoraphobia	Barlow et al. (1989) Clark et al. (1994)
Cognitive behavior therapy for generalized anxiety disorder	Butler et al. (1991) Borkovec et al. (1987)
Group cognitive behavioral therapy for social phobia	Heimberg et al. (1990) Mattick & Peters (1988)
*Exposure treatment for agoraphobia	Trull et al. (1988)
*Exposure treatment for social phobia	Feske & Chambless (1995)
Exposure and response prevention for obsessive-compulsive disorder	Balkom et al. (1994)
*Stress Inoculation Training for Coping with Stressors	Saunders et al. (in press)
Systematic desensitization for simple phobia	Kazdin & Wilcoxon (1976)
DEPRESSION:	
Cognitive therapy for depression	Dobson (1989)
Interpersonal therapy for depression	DiMascio et al. (1979) Elkin et al. (1989)
HEALTH PROBLEMS:	
*Behavior therapy for headache	Blanchard et al. (1987) Holroyd & Penzien (1990)
*Cognitive behavior therapy for irritable bowel syndrome	Blanchard et al. (1980) Lynch & Zamble (1989).
*Cognitive behavior therapy for chronic pain	Keefe et al. (1992) Turner & Clancy (1988)
*Cognitive-behavior therapy for bulimia	Agras et al. (1989) Thackwray et al. (1993)
Interpersonal therapy for bulimia	Fairburn et al. (1993) Wilfley et al. (1993)
PROBLEMS OF CHILDHOOD:	
*Behavior modification for enuresis	Houts et al. (1994)
Parent training programs for children with oppositional behavior	Walter & Gilmore (1973) Wells & Egan (1988)
MARITAL DISCORD:	
Behavioral marital therapy	Azrin, Bersalel et al. (1980) Jacobson & Follette (1985)
SEXUAL DYSFUNCTION:	
Behavior therapy for female orgasmic dysfunction and male erectile dysfunction	LoPiccolo & Stock (1986) Auerbach & Kilmann (1977)
OTHER:	
Family education programs for schizophrenia	Hogarty et al. (1986) Falloon et al. (1985))
Behavior modification for developmentally disabled individuals	Scotti et al. (1991) Kazdin (1977)
Token economy programs	Liberman (1972)

Probably Efficacious Treatments

Citation for Efficacy Evidence

ANXIETY:

Applied relaxation for panic disorder

*Applied relaxation for generalized anxiety disorder

*Exposure treatment for PTSD

*Exposure treatment for simple phobia

*Stress Inoculation Training for PTSD

*Group exposure and response prevention for obsessive-compulsive disorder

*Relapse prevention program for obsessive-compulsive disorder

Öst (1988)

Barlow et al., 1992

Borkovec & Costello, 1993

Foa et al. (1991)

Keane et al. (1989)

Leitenberg & Callahan (1973)

Öst et al. (1991)

Foa et al. (1991)

Fals-Stewart et al. (1993)

Hiss et al. (1994)

CHEMICAL ABUSE AND DEPENDENCE:

*Behavior therapy for cocaine abuse

*Brief dynamic therapy for opiate dependence

*Cognitive therapy for opiate dependence

*Cognitive-behavior therapy for benzodiazepine withdrawal in panic disorder patients

Higgins et al. (1993)

Woody et al. (1990)

Woody et al. (1990)

Otto et al. (1994)

Spiegel et al. (1993)

DEPRESSION:

*Brief dynamic therapy

*Cognitive therapy for geriatric patients

Psychoeducational treatment

*Reminiscence therapy for geriatric patients Areean et al. (1993)

*Self-control therapy

Gallagher-Thompson & Steffen (1994)

Scogin & McElreath (1994)

Lewinsohn et al. (1989)

Scogin & McElreath (1994)

Fuchs & Rehm (1977)

Rehm et al. (1979)

HEALTH PROBLEMS:

*Behavior therapy for childhood obesity

*Group cognitive-behavior therapy for bulimia

Epstein et al. (1994)

Wheeler & Hess (1976)

Mitchell et al. (1990)

MARITAL DISCORD:

Emotionally focused couples therapy

*Insight-oriented marital therapy

Johnson & Greenberg (1985)

Snyder et al. (1989, 1991)

PROBLEMS OF CHILDHOOD:

*Behavior modification of encopresis

*Family anxiety management training for anxiety disorders

O'Brien et al. (1986)

Barrett et al. (in press)

OTHER:

Behavior modification for sex offenders

Dialectical behavior therapy for

borderline personality disorder

Habit reversal and control techniques

Marshall et al. (1991)

Linehan et al. (1991)

Azrin, Nunn & Frantz (1980)

Azrin, Nunn & Frantz-Renshaw (1980)

- Treatment by a mental health professional usually worked. Most respondents got a lot better. Averaged over all mental health professionals, of the 426 people who were feeling *very poor* when they began therapy, 87% were feeling *very good*, *good*, or at least *so-so* by the time of the survey. Of the 786 people who were feeling *fairly poor* at the outset, 92% were feeling *very good*, *good*, or at least *so-so* by the time of the survey. These findings converge with meta-analyses of efficacy (Lipsey & Wilson, 1993; Shapiro & Shapiro, 1982; Smith, Miller, & Glass, 1980).
- Long-term therapy produced more improvement than short-term therapy. This result was very robust, and held up over all statistical models. Figure 1 plots the overall rating (on the 0–300 scale defined above) of improvement as a function of length of treatment. This “dose–response curve” held for patients in both psychotherapy alone and in psychotherapy plus medication (see Howard, Kopta, Krause, & Orlinsky, 1986, for parallel dose–response findings for psychotherapy).
- There was no difference between psychotherapy alone and psychotherapy plus medication for any disorder (very few respondents reported that they had medication with no psychotherapy at all).
- While all mental health professionals appeared to help their patients, psychologists, psychiatrists, and social workers did equally well and better than marriage counselors. Their patients’ overall improvement scores (0–300 scale) were 220, 226, 225 (not significantly different from each other), and 208 (significantly worse than the first three), respectively.
- Family doctors did just as well as mental health professionals in the short term, but worse in the long term. Some patients saw both family doctors and

mental health professionals, and those who saw both had more severe problems. For patients who relied solely on family doctors, their overall improvement scores when treated for up to six months was 213, and it remained at that level (212) for those treated longer than six months. In contrast, the overall improvement scores for patients of mental health professionals was 211 up to six months, but climbed to 232 when treatment went on for more than six months. The advantages of long-term treatment by a mental health professional held not only for the specific problems that led to treatment, but for a variety of general functioning scores as well: ability to relate to others, coping with everyday stress, enjoying life more, personal growth and understanding, self-esteem and confidence.

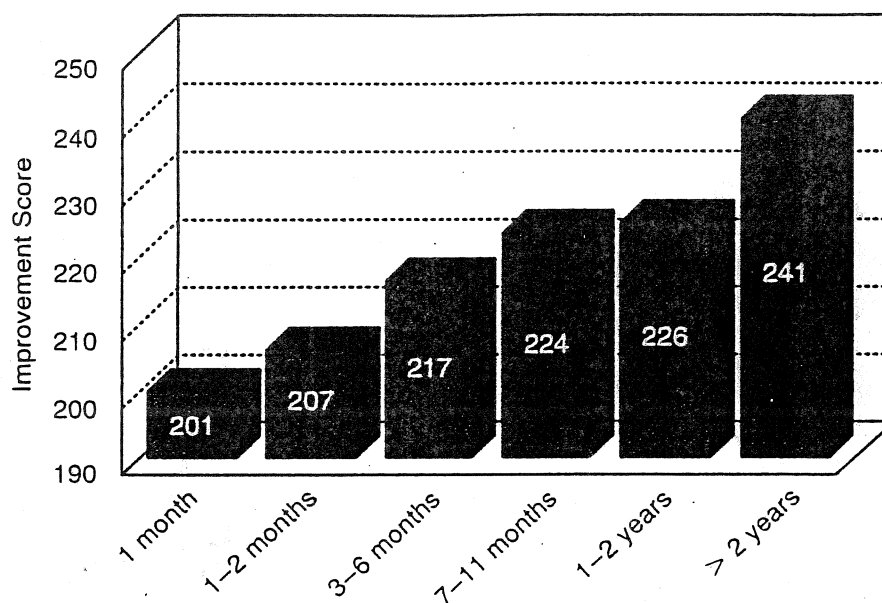
- Alcoholics Anonymous (AA) did especially well, with an average improvement score of 251, significantly bettering mental health professionals. People who went to non-AA groups had less severe problems and did not do as well as those who went to AA (average score = 215).
- Active shoppers and active clients did better in treatment than passive recipients (determined by responses to “Was it mostly your idea to seek therapy? When choosing this therapist, did you discuss qualifications, therapist’s experience, discuss frequency, duration, and cost, speak to someone who was treated by this therapist, check out other therapists? During therapy, did you try to be as open as possible, ask for explanation of diagnosis and unclear terms, do homework, not cancel sessions often, discuss negative feelings toward therapist?”).
- No specific modality of psychotherapy did any better than any other for any problem. These results confirm the “dodo bird” hypothesis, that all forms of psychotherapies do about equally well (Luborsky, Singer, & Luborsky, 1975). They come as a rude shock to efficacy researchers, since the main theme of efficacy studies has been the demonstration of the usefulness of specific techniques for specific disorders.
- Respondents whose choice of therapist or duration of care was limited by their insurance coverage did worse, as presented in Table 1 (determined by responses to “Did limitations on your insurance coverage affect any of the following choices you made? Type of therapist I chose; How often I met with my therapist; How long I stayed in therapy”).

The Effectiveness of Psychotherapy

The Consumer Reports Study

Seligman, E.P. (1995). The effectiveness of psychotherapy: The Consumer Reports study. *American Psychologist*, 50, 965–974.

Figure 1
Duration of Therapy



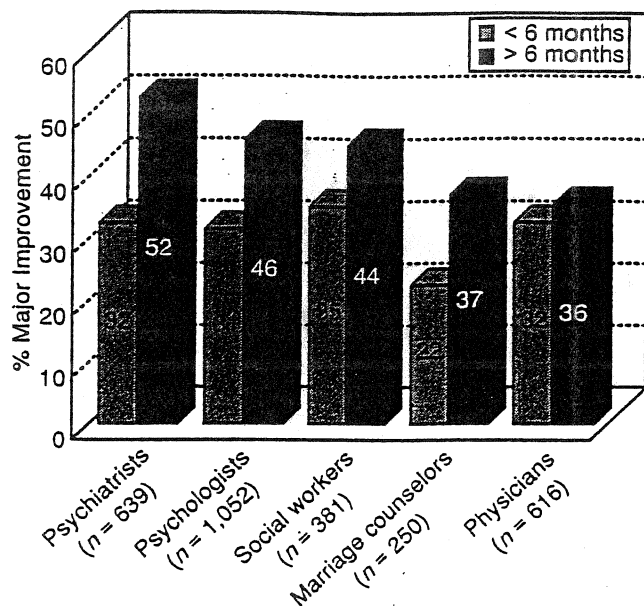
Note. $N = 2,846$. The 300-point scale is derived from the unweighted sum of responses to three 100-point subscales. The subscales measured specific improvement (i.e., how much treatment helped with problems that led to therapy), satisfaction with therapist, and global improvement (i.e., how respondents felt at time of survey, compared with when they began treatment).

Table 1
Limitations on Insurance Coverage and Improvement

Limitations on your insurance coverage	Percent checking item ^a	Coverage limited		Coverage not limited	
		Overall score	Specific improvement	Overall score	Specific improvement
Type of therapist I chose	20	211	77	224	83
How often I met with my therapist	26	214	79	224	82
How long I stayed in therapy	24	212	78	224	83
Percent of any of the above	43	212	78	226	83

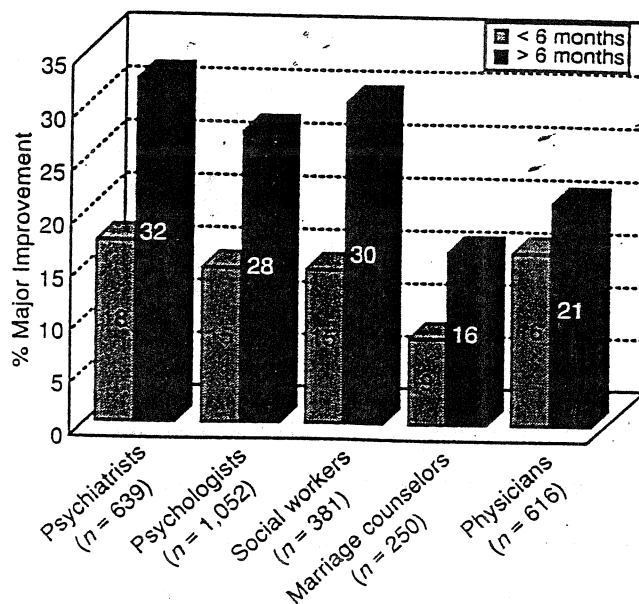
Note. $N = 2,900$. All differences for the overall scores were statistically significant at $p < .01$. The same held true for the specific score, except for "How often I met with my therapist," which was significant at $p < .05$. Statistical controls for both severity and duration were applied. Source: *Consumer Reports 1994 Annual Questionnaire*. ^amultiple responses permitted.

Figure 2
Improvement for Presenting Symptoms



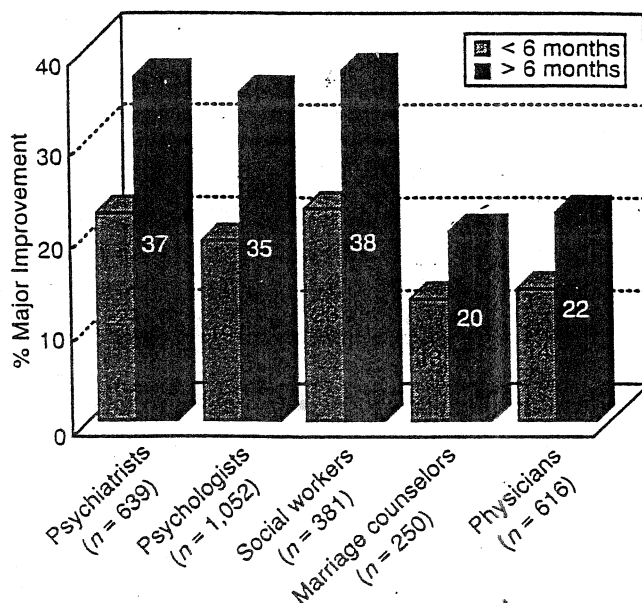
Note. N = 2,738. Percentage of respondents who reported that treatment "made things a lot better" with respect to the specific problem that led to treatment by psychiatrists, psychologists, social workers, marriage counselors, or family doctors, segregated by those treated for more than six months and those treated for less than six months.

Figure 3
Improvement Over Work and Social Domains



Note. N = 2,738. Mean percentage who reported that treatment "made things a lot better" with respect to three domains: ability to relate to others, productivity at work, and coping with everyday stress. Those treated by psychiatrists, psychologists, social workers, marriage counselors, and physicians are segregated by treatment for more than six months versus treatment for less than six months.

Figure 4
Improvement Over Personal Domains

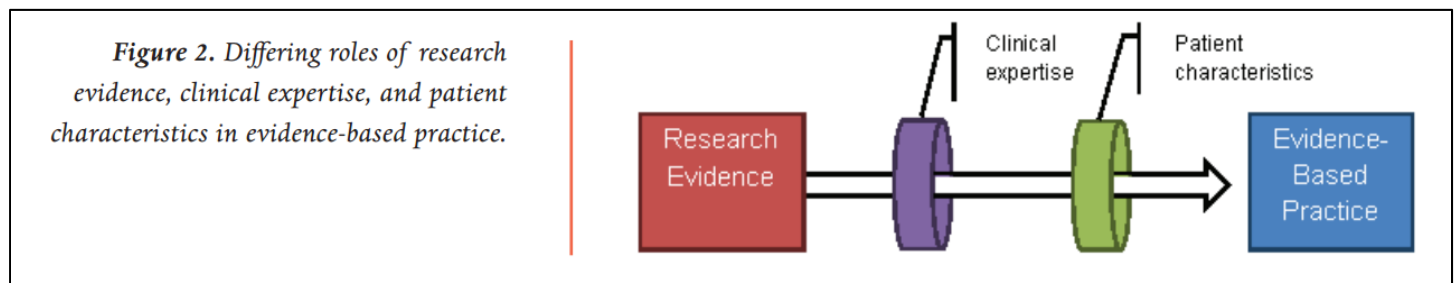
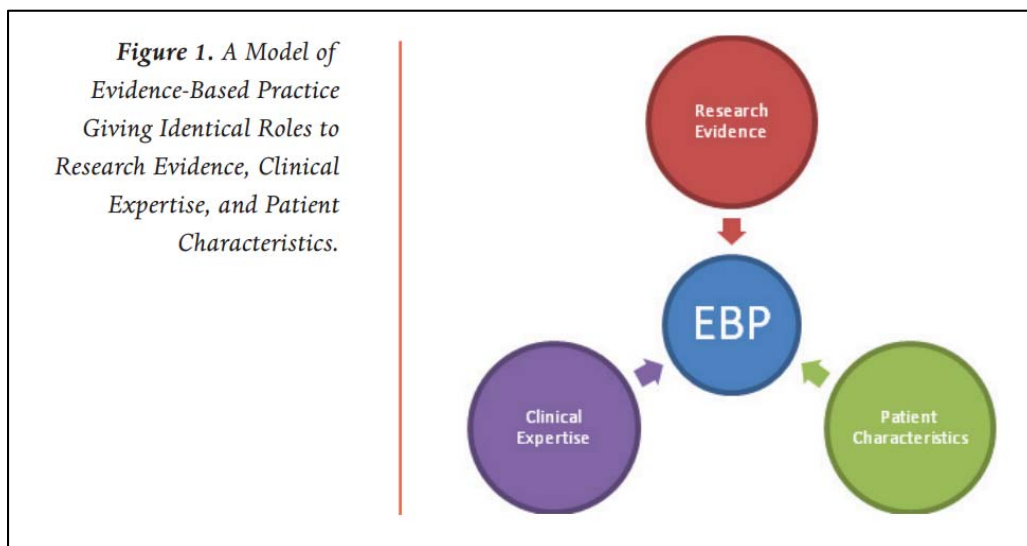


Note. N = 2,738. Mean percentage who reported that treatment "made things a lot better" with respect to four domains: enjoying life more, personal growth and insight, self-esteem and confidence, and alleviating low moods. Those treated by psychiatrists, psychologists, social workers, marriage counselors, and physicians are segregated by treatment for more than six months versus treatment for less than six months.

Table 2. Common critiques of the EST movement and suggested changes

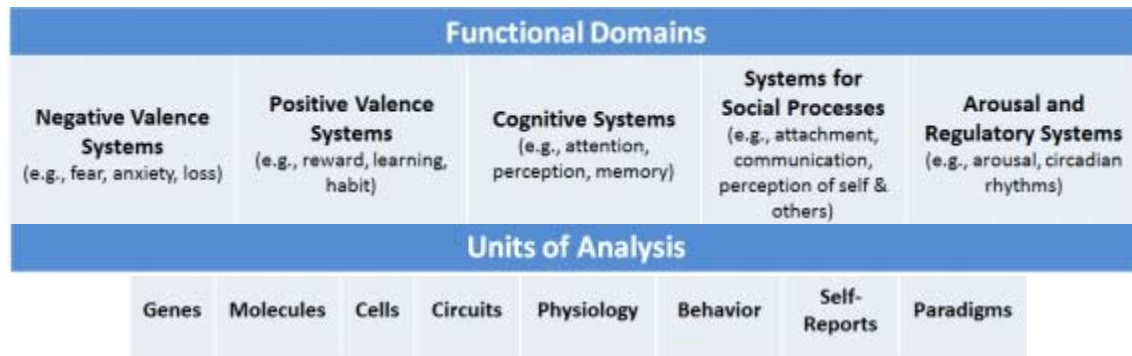
Area	Critiques	Proposed Changes
Concerns about the strength of treatment	<ul style="list-style-type: none"> • Inadequate attention to null or negative findings • Reliance on statistical, rather than clinical, significance • Inadequate attention to long-term outcomes • Potentially significant variability in study quality 	<ul style="list-style-type: none"> • Emphasize systematic reviews rather than individual studies • Separate strength of effect from strength of evidence • Grade quality of studies • Consider clinical significance in addition to statistical significance • Consider long-term efficacy in addition to short-term efficacy
Concerns about selecting among multiple treatment options	<ul style="list-style-type: none"> • Within a given EST category, there is little basis for choosing one over another • Lack of clarity about whether empirical support translates to a recommendation 	<ul style="list-style-type: none"> • Present quantitative information about treatment strength • Make specific recommendations based on clinical outcomes and the quality of the available research
Concerns about the relevance of findings	<ul style="list-style-type: none"> • Inadequate attention to functional outcomes • Inadequate attention to effectiveness in nonresearch settings or with diverse populations 	<ul style="list-style-type: none"> • Include functional or other health-related outcomes as well as symptom outcomes • Address generalization of research findings to nonresearch settings and diverse populations
Concern about unclear active treatment ingredients and the proliferation of manuals for specific diagnoses	<ul style="list-style-type: none"> • Listing of packaged treatments rather than empirically supported principles of change • Emphasis on specific psychiatric diagnoses 	<ul style="list-style-type: none"> • Evaluate and encourage dismantling research to identify empirically supported principles of change • De-emphasize diagnoses and emphasize syndromes/mechanisms of psychopathology

David F. Tolin, Dean McKay, Evan M. Forman, E. David Klonsky, Brett D. Thombs (2015), Empirically Supported Treatment: Recommendations for a New Model. *Clinical Psychology: Science and Practice*, 2, 1-22.



Tolin, D. F. (2014) Evidence-Based Practice: Three Legged Stool or Filter System? *The Clinical Psychologist*, 3 (67), 1-2.

NIMH Research Domain Criteria (RDoC)



Each domain reflects an aspect of human experience, and each includes within it several “constructs” which are recognized as artificial (human-imposed, not necessarily brain-based), as follows

Human experience	RDoC Domain	Constructs within this Domain
Negative	Negative “valence”	fear, anxiety, loss, frustration
Positive	Positive “valence”	reward, motivation, habit
Thinking	Cognition	attention, perception, cognitive control, memory
Social interaction	Social processes	attachment, communication, perception of self and others
Sleep	Arousal / regulatory	circadian rhythm, sleep and wakefulness

Each domain can vary from “normal” to “abnormal”. There are no official cut-offs to make “diagnoses” in this system.

Each domain can be approached on multiple levels of analysis:

- genes
- molecules
- cells
- circuits
- physiology
- behavior
- self-report
- experimental paradigms

Each construct above can be approached on each of these levels. Here’s the anxiety construct, for example (from the **matrix** on the RDoC page).

Con-struct	Genes	Mole-cules	Cells	Cir-cuits	Physiology	Beha-vior	Self-report	Paradigm
An-xiety	corticotropin re-leasing factor (CRF)	CRF, cor-tisol	Pit-uitary cells	BNST	Average cortisol and ACTH levels; startle	Avoid-ance	Beck Anxiety Inventory	Contextual threat, dark-ness,

Addendum

My colleague, Thomas J. Bouchard, Jr., on reading a draft of this article faulted me for what he saw as a major inconsistency between my neo-Popperian emphasis on falsifiability and my positive assessment of Freud. There is no denying that for such a quantitatively oriented product of the "dust-bowl empiricist" tradition as myself, I do have a soft spot in my heart (Minnesota colleagues would probably say in my head) for psychoanalysis. So, the most honest and straightforward way to deal with Bouchard's complaint might be simply to admit that the evidence on Freud is inadequate and that Bouchard and I are simply betting on different horses. But I cannot resist the impulse to say just a bit more on this vexatious question, because while I am acutely aware of a pronounced (and possibly irrational) difference in the "educated prior" I put on Freud as contrasted with rubber band theory or labeling theory or whatever, I am not persuaded that my position is as grossly incoherent as it admittedly appears. Passing the question whether attempts to study psychoanalytic theory by the methods of experimental or differential psychology have on the whole tended to support rather than refute it (see, e.g., Fisher & Greenberg, 1977; Rapaport, 1959; Sears, 1943; Silverman, 1976), my own view is that the best place to study psychoanalysis is the psychoanalytic session itself, as I have elsewhere argued in a far too condensed way (Meehl, 1970/1973e).

I believe that some aspects of psychoana-

Meehl, P.E. (1978). Theoretical risks and tabular asterisks: Sir Karl, Sir Ronald, and the slow progress of soft psychology. *Journal of Consulting & Clinical Psychology*, 46, 806-834.

lytic theory are not presently researchable. Because the intermediate technology required—which really means instruments-cum-theory—does not exist. I mean auxiliaries and methods such as a souped-up, highly developed science of psycholinguistics, and the kind of mathematics that is needed to conduct a rigorous but clinically sensitive and psychoanalytically realistic job of theme tracing in the analytic protocol. This may strike some as a kind of cop-out, but I remind you that Lakatos, Kuhn, Feyerabend, and others have convincingly made the point that there are theories in the physical and biological sciences that are untestable when first propounded because the theoretical and technological development necessary for making certain kinds of observations bearing on them had not taken place. It is vulgar positivism (still held by many psychologists) to insist that any respectable empirical theory must be testable, if testable means *definitively testable right now*.

But I do think that there is another class of consequences of psychoanalytic theory, close to the original "clinical connections" alleged by Freud, Ferenczi, Jones, Abraham, and others that does not involve much of what Freud called *the witch metapsychology*, where no complicated statistics are needed, let alone the invention of any new formal modes of protocol analysis. Here the problem is mainly that *none of us has bothered to carry out some relatively simple-minded kinds of analyses on a random sample of psychoanalytic protocols collected from essentially naive patients to whom no interpretations have as yet been offered*. This second category is, in my view, a category of research studies that we could have done, but have not done. *Example*: We can easily ascertain whether manifest dream content of a certain kind is statistically associated (in the simple straightforward sense of a patterned fourfold table) with such and such kinds of thematic material in the patient's subsequent associations to the dream. I would not even object to doing significance tests on a batch of such tables, but to explain why would unduly enlarge what is already an addendum.

I cheerfully admit, in this matter, to the presence of a large distance between my subjective personalistic probability (based on my

experiences as analyst and practitioner of psychoanalytic therapy) and the present state of the "intersubjective public evidence." That is what I mean by saying that Bouchard and I are betting on different horses. But one must distinguish, as I know from subsequent conversations that he does, between a criticism (a) that what is proper evidence *does* presently exist and is *adverse* to a conjecture and (b) an anti-Popperian claim that falsifiability in principle does not matter. If I thought (as does Popper) that Freudian theory was in principle not falsifiable, then I would have to confess to a major inconsistency. But I do think it is falsifiable, although I agree that *some parts* of it cannot *at present* be tested because of the primitive development of the auxiliary theories and the measurement technologies that would be jointly necessary.

A final point on this subject is one that I hesitate to include because it is very difficult to explain in the present state of philosophy of science, and I could be doing my main thesis damage by presenting a cursory and somewhat dogmatic statement of it. Nevertheless, having made the above statements about psychoanalytic theory and having contrasted it favorably with some of the (to me, trivial and flabby) theories in soft psychology, I fear I have an obligation to say it, however ineptly. Once one sees that it is inappropriate to conflate the concepts *rational* and *statistical*, then it is a fuzzy open question, in the present state of the metatheoretician's art, just when a mass of nonquantitative converging evidence can be said to have made a stronger case for a conjecture than the weak kinds of nonconverging quantitative evidence usually represented by the significance testing tradition. I say "when" rather than "whether," because it is blindingly obvious that *sometimes* qualitative evidence of certain sorts is superior in its empirical weight to what a typical social, personality, or clinical psychologist gets in support of a substantive theory by the mere refutation of the null hypothesis. Take, for instance, the evidence in a well-constructed criminal case, such as the evidence that Bruno Hauptmann was the kidnapper of the Lindbergh baby. I do not see how anybody who reads the trial transcript of the Hauptmann

case could have a reasonable doubt that he was guilty as charged. Yet I cannot recall any of the mass of data that convicted him as being of a quantitative sort (one cannot fairly except the serial numbers on the gold notes, they being not "measures" but "football numbers").

All of us believe a lot of things that we would not have the vaguest idea how to express as a probability value (*pace* strong Bayesians!) or how to compute as an indirect test of statistical significance. I believe, for instance, that Adolf Hitler was a schizotype; I do not believe that Kaspar Hauser was the son of a prince; I believe that the domestic cat probably was evolved from *Felis lybica* by the ancient Egyptians; I hold that my sainted namesake wrote the letter to the Corinthians but did not write the letter to the Hebrews; I am confident that my wife is faithful to me; and so forth. The point is really a simple one—that there are many areas of both practical and theoretical inference in which nobody knows how to calculate a numerical probability value, and nobody knows how to state the manner or degree in which various lines of evidence converge on a certain conjecture as having high verisimilitude. There are propositions in history (such as, "Julius Caesar crossed the Rubicon") that we all agree are well corroborated by the available documents but without any *t* tests or the possibility of calculating any, whereas Fisbee's theory of social behavior is only weakly corroborated by the fact that he got a significant *t* test when he compared the boys and the girls or the older kids and the younger kids on the Hockheimer-Sedlitz Communication Scale. Now I consider my betting on the horse of psychoanalysis to be in the same kind of ball park as my beliefs about Julius Caesar or the evolution of the cat. But, I repeat, this may be a terribly irrational leap of faith on my part. For the purposes of the present article and Bouchard's criticism of it, I hope it is sufficient to say that one could arguably hold that significance testing in soft psychology is a pretentious endeavor that falls under a tolerant neo-Popperian criticism, and could nevertheless enter his personalistic prediction that *when adequate tests become available to us, a sizable portion of psychoanalytic theory will escape refutation*. So I do not think I am actually contradicting myself, but I am personalistically betting on the outcome of a future horse race.

SundayReview | CONTRIBUTING OP-ED WRITER

Redefining Mental Illness

JAN. 17, 2015

T. M. Luhrmann

TWO months ago, the British Psychological Society released a remarkable document entitled “Understanding Psychosis and Schizophrenia.” Its authors say that hearing voices and feeling paranoid are common experiences, and are often a reaction to trauma, abuse or deprivation: “Calling them symptoms of mental illness, psychosis or schizophrenia is only one way of thinking about them, with advantages and disadvantages.”

The report says that there is no strict dividing line between psychosis and normal experience: “Some people find it useful to think of themselves as having an illness. Others prefer to think of their problems as, for example, an aspect of their personality which sometimes gets them into trouble but which they would not want to be without.”

The report adds that antipsychotic medications are sometimes helpful, but that “there is no evidence that it corrects an underlying biological abnormality.” It then warns about the risk of taking these drugs for years.

And the report says that it is “vital” that those who suffer with distressing symptoms be given an opportunity to “talk in detail about their experiences and to make sense of what has happened to them” — and points out that mental health services rarely make such opportunities available.

This is a radically different vision of severe mental illness from the one held by most Americans, and indeed many American psychiatrists. Americans think of schizophrenia as a brain disorder that can be treated only with

medication. Yet there is plenty of scientific evidence for the report's claims.

Moreover, the perspective is surprisingly consonant — in some ways — with the new approach by our own National Institute of Mental Health, which funds much of the research on mental illness in this country. For decades, American psychiatric science took diagnosis to be fundamental. These categories — depression, schizophrenia, post-traumatic stress disorder — were assumed to represent biologically distinct diseases, and the goal of the research was to figure out the biology of the disease.

That didn't pan out. In 2013, the institute's director, Thomas R. Insel, announced that psychiatric science had failed to find unique biological mechanisms associated with specific diagnoses. What genetic underpinnings or neural circuits they had identified were mostly common across diagnostic groups. Diagnoses were neither particularly useful nor accurate for understanding the brain, and would no longer be used to guide research.

And so the institute has begun one of the most interesting and radical experiments in scientific research in years. It jettisoned a decades-long tradition of diagnosis-driven research, in which a scientist became, for example, a schizophrenia researcher. Under a program called **Research Domain Criteria**, all research must begin from a matrix of neuroscientific structures (genes, cells, circuits) that cut across behavioral, cognitive and social domains (acute fear, loss, arousal). To use an example from the program's website, psychiatric researchers will no longer study people with anxiety; they will study fear circuitry.

Our current diagnostic system — the main achievement of the biomedical revolution in psychiatry — drew a sharp, clear line between those who were sick and those who were well, and that line was determined by science. The system started with the behavior of persons, and sorted them into types. That approach sank deep roots into our culture, possibly because sorting ourselves into different kinds of people comes naturally to us.

The institute is rejecting this system because it does not lead to useful research. It is starting afresh, with a focus on how the brain and its trillions of synaptic connections work. The British Psychological Society rejects the

centrality of diagnosis for seemingly quite different reasons — among them, because defining people by a devastating label may not help them.

Both approaches recognize that mental illnesses are complex individual responses — less like hypothyroidism, in which you fall ill because your body does not secrete enough thyroid hormone, and more like metabolic syndrome, in which a collection of unrelated risk factors (high blood pressure, body fat around the waist) increases your chance of heart disease.

The implications are that social experience plays a significant role in who becomes mentally ill, when they fall ill and how their illness unfolds. We should view illness as caused not only by brain deficits but also by abuse, deprivation and inequality, which alter the way brains behave. Illness thus requires social interventions, not just pharmacological ones.

ONE outcome of this rethinking could be that talk therapy will regain some of the importance it lost when the new diagnostic system was young. And we know how to do talk therapy. That doesn't rule out medication: while there may be problems with the long-term use of antipsychotics, many people find them useful when their symptoms are severe.

The rethinking comes at a time of disconcerting awareness that mental health problems are far more pervasive than we might have imagined. The World Health Organization estimates that one in four people will have an episode of mental illness in their lifetime. Mental and behavioral problems are the biggest single cause of disability on the planet. But in low- and middle-income countries, about four of five of those disabled by the illnesses do not receive treatment for them.

When the United Nations sets its new Sustainable Development Goals this spring, it should include mental illness, along with diseases like AIDS and malaria, as scourges to be combated. There is much we still do not know about mental illness, and much we can do to improve its care. But we know enough to do something, and to accept that knowing more and doing more should be a fundamental commitment.

Correction: January 25, 2015

An opinion article about mental illness last Sunday incorrectly referred to a group that recently issued a report on schizophrenia. It is the British Psychological Society, not the British Psychological Association.

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Mind Games: Psychological Warfare Between Therapists and Scientists

By CAROL TAVRIS

Recently, while lecturing to a large group of lawyers, judges, mediators, and others involved in the family-court system in Los Angeles, I asked how many knew what a "social psychologist" was. Three people shyly raised their hands. That response was typical, and it's the reason I don't tell people anymore that I'm a social psychologist: They think I'm a therapist who gives lots of parties. If I tell them I'm a psychological scientist, they think I'm a pompous therapist, because everyone knows that "psychological science" is an oxymoron.

In fact, in many states, I cannot call myself a psychologist at all -- the word is reserved for someone who has an advanced degree in clinical psychology and a license to practice psychotherapy. That immediately rules out the many other kinds of psychologists who conduct scientific research in their respective specialties, including child development, gerontology, neurobiology, emotions, sleep, behavioral genetics, memory and cognition, sexual behavior and attitudes, trauma, learning, language, . . . and social psychology, the study of how social situations and other people affect every human activity from love to war.

For the public, however, the word "psychologist" has only one meaning: psychotherapist. It is true that *clinical* psychologists practice therapy, but many psychologists are not clinicians, and most therapists are not clinical psychologists. The word "psychotherapist" is completely unregulated. It includes people who have advanced training in psychology, along with those who get a "certification" in some therapeutic specialty; clinical social workers; marriage, family, and child counselors; psychoanalysts and psychiatrists; and countless others who have no training in anything. Starting tomorrow, I could package and market my own highly effective approach, Chocolate Immersion Therapy, and offer a weekend workshop to train neophytes (\$395, chocolate included). I could carry out any kind of unvalidated, cockamamie therapy I wanted, and I would not be guilty of a single crime. Unless I described myself as a psychologist.

As a result of such proliferation of psychotherapists, the work of psychological scientists who do research and teach at colleges and universities tends to be invisible outside the academy. It is the psychotherapists who get public attention, because they turn up on talk shows, offer advice in books and newspaper columns, and are interviewed in the aftermath of every disaster or horrible crime -- for example, speculating on the motives and childhoods of the Washington snipers. Our society runs on the advice of mental-health professionals, who are often called upon in legal settings to determine whether a child has been molested, a prisoner up for parole is still dangerous, a defendant is lying or insane, a mother is fit to have custody of her children, and on and on. Yet while the public assumes, vaguely, that therapists must be "scientists" of some sort, many of the widely accepted claims promulgated by therapists are based on subjective clinical opinions and have been resoundingly disproved by empirical research conducted by psychological scientists. Here are a few examples that have been shown to be false:

- Low self-esteem causes aggressiveness, drug use, prejudice, and low achievement.
- Abused children almost inevitably become abusive parents, causing a "cycle of abuse."
- Therapy is beneficial for most survivors of disasters, especially if intervention is rapid.
- Memory works like a tape recorder, clicking on at the moment of birth; memories can be accurately retrieved through hypnosis, dream analysis, or other therapeutic methods.

- Traumatic experiences, particularly of a sexual nature, are typically "repressed" from memory, or split off from consciousness through "dissociation."
- The way that parents treat a child in the first five years (three years) (one year) (five minutes) of life is crucial to the child's later intellectual and emotional success.

Indeed, the split between the research and practice wings of psychology has grown so wide that many psychologists now speak glumly of the "scientist-practitioner gap," although that is like saying there is an "Arab-Israeli gap" in the Middle East. It is a war, involving deeply held beliefs, political passions, views of human nature and the nature of knowledge, and -- as all wars ultimately do -- money and livelihoods. The war spilled out of academic labs and therapists' offices and into the public arena in the 1980s and '90s, when three epidemics of hysteria caught fire across the country: the rise of claims of "repressed memories" of childhood sexual abuse; the growing number of cases of "multiple-personality disorder" (MPD), from a handful before 1980 to tens of thousands by 1995; and the proliferation of day-care sex-abuse scandals, which put hundreds of nursery-school teachers in prison on the "testimony" of 3 and 4-year-old children.

All three epidemics were fomented and perpetuated by the mistaken beliefs of psychotherapists: that "children never lie about sexual abuse"; that childhood trauma causes the personality to "split" into several or even thousands of identities; that if you don't remember being sexually abused in childhood, that's evidence that you were; that it is possible to be raped by your father every day for 16 years and to "repress" the memory until it is "uncovered" in therapy; that hypnosis, dream analysis, and free association of fantasies are reliable methods of "uncovering" accurate memories. (On the contrary, such techniques have been shown to increase confabulation, imagination, and memory errors, while inflating the belief that the retrieved memories are accurate.) The epidemics began to subside as a result of the painstaking research of psychological scientists.

But psychotherapeutic nonsense is a Hydra: Slay one set of mistaken ideas, and others take their place. Recovered-memory therapy may be on the wane, but "rebirthing" techniques and forms of "restraint therapy" -- physically abusive practices that supposedly help adopted or troubled children form attachments to their parents -- are on the rise. In Colorado, 10-year-old Candace Newmaker was smothered to death during rebirthing, a procedure in which she was expected to fight her way through a "birth canal" of suffocating blankets and pillows. The two therapists convicted in Candace's death are now serving time in prison, but efforts in Colorado to prohibit all forms of "restraint therapy" were defeated by protests from "attachment therapists" in the state and throughout the country. After Candace's death, one member of the Colorado Mental Health Grievance Board noted with dismay that her hairdresser's training took 1,500 hours, whereas anyone could take a two-week course and become "certified" in rebirthing. Yet the basic premise -- that children can recover from trauma, insecure attachment, or other psychological problems by "reliving" their births or being subjected to punitive and coercive restraints -- has no scientific validity whatsoever.

To understand how the gap between psychological scientists and clinicians grew, it is necessary to understand a little about therapy and a little about science, and how their goals and methods diverged. For many years, the training of most clinical psychologists was based on a "scientist practitioner" model. Ideally, clinicians would study the research on human behavior and apply relevant findings to their clinical practice. Clinical psychologists who are educated at major universities are still trained in this model. They study, for example, the origins of various mental disorders and the most effective ways to treat them, such as cognitive-behavior therapy for anxiety, depression, eating disorders, anger, and obsessive-compulsive disorder.

They have also identified which interventions are unhelpful or potentially harmful. For example, independent assessments of a popular post-trauma intervention called Critical Incident Stress Debriefing have found that most survivors benefit just as much by talking with friends and other survivors as with debriefers. Sometimes CISD even slows recovery, by preventing victims from drawing on their own wellsprings of resilience. And, sometimes, it harms people -- for example, by having survivors ventilate their emotions without also learning good methods of coping with them.

Unfortunately, the numbers of scientifically trained clinicians have been shrinking. More and more therapists are getting their degrees from "free-standing" schools, so called because they are independent of research institutions or academic psychology departments. In these schools, students are trained only to do therapy, and they do not necessarily even learn which kinds of therapy have been shown to be most effective for particular problems. Many of the schools are accredited by the American Psychological Association, and their graduates learn what they need to know to pass state licensing examinations. But that does not mean that the graduates are scientifically knowledgeable. For example, the Rorschach Inkblot Test has been resoundingly discredited as a reliable means of diagnosing most mental disorders or emotional problems; it usually reveals

more about the clinician administering it than about the individual taking it. I call it the Dracula of psychological tests, because no one has been able to drive a stake through the cursed thing's heart. Many clinicians love it; it is still widely used; and it still turns up on licensing exams.

Of course, tensions exist between researchers and practitioners in any field -- medicine, engineering, education. Whenever one group is doing research and the other is working in an applied domain, their interests and training will differ. The goal of the clinician, in psychology or medicine, is to help the suffering individual; the goal of the psychological or medical researcher is to explain and predict the behavior or course of illness in people in general. That is why many clinicians argue that empirical research cannot possibly capture the complex human beings who come to their offices. Professional training, they believe, should teach students empathy and appropriate therapeutic skills. Good therapy depends on the therapist's insight and experience, not on knowledge of statistics, the importance of control groups, and the scientific method.

I agree that therapy often deals with issues on which science is silent: finding courage under adversity, accepting loss, making moral choices. My clinician friends constantly impress me with their deep understanding of the human condition, which is based on seeing the human condition sobbing in their offices many times a week. Nor am I arguing that psychological scientists, or any other kind, are white knights with a special claim to intellectual virtue. They, too, wrangle over data, dispute each other furiously in print and public, and have plenty of vested interests and biases. (For example, many scientists and consumer advocates are concerned about the growing co-optation of scientific investigators by the pharmaceutical industry -- which now finances the majority of studies of treatments for mental disorders and sexual problems -- because the result has been a pro-drug bias in research.)

It is not that I believe that science gives us ultimate truths about human behavior, while clinical insight is always foolish and wrong. Rather, I worry that when psychotherapists fail to keep up with basic research on matters on which they are advising their clients; when they fail to learn which methods are most appropriate for which disorders, and which might be harmful; when they fail to understand their own biases of perception and do not learn how to correct them; when they fail to test their own ideas empirically before running off to promote new therapies or wild claims -- then their clients and the larger public pay the price of their ignorance.

For present purposes, I am going to do an end run around the centuries-old debate about defining science, and focus on two core elements of the scientific method. These elements are central to the training of all scientists, but they are almost entirely lacking in the training of most psychotherapists, including clinical psychologists. The first is skepticism: a willingness to question received wisdom. The second is a reliance on gathering empirical evidence to determine whether a prediction or belief is valid. You don't get to sit in your chair and decide that autism is caused by cold, rejecting, "refrigerator" mothers, as Bruno Bettelheim did. But legions of clinicians (and mothers) accepted his cruel and unsubstantiated theory because he was, well, Bruno Bettelheim. It took skeptical scientists to compare the mothers of autistic children with those of healthy children, and to find that autism is not caused by anything parents do; it is a neurological disorder.

The scientific method is designed to help investigators overcome the most entrenched human cognitive habit: the *confirmation bias*, the tendency to notice and remember evidence that confirms our beliefs or decisions, and to ignore, dismiss, or forget evidence that is discrepant. That's why we are all inclined to stick to a hypothesis we believe in. Science is one way of forcing us, kicking and screaming if necessary, to modify our views. Most scientists regard a central, if not defining, characteristic of the scientific method to be what Karl Popper called "the principle of falsifiability": For a theory to be scientific, it must be falsifiable -- you can't show me just those observations that confirm it, but also those that might show it to be wrong, false. If you can twist any result of your research into a confirmation of your hypothesis, you aren't thinking scientifically. For that reason, many of Freud's notions were unfalsifiable. If analysts saw evidence of "castration anxiety" in their male patients, that confirmed Freud's theory of its universality; if analysts didn't see it, Freud wrote, they lacked observational skills and were just too blind or stubborn to see it. With that way of thinking, there is no way to disconfirm the belief in castration anxiety.

Yet many psychotherapists perpetuate ideas based only on confirming cases -- the people they see in therapy -- and do not consider the disconfirming cases. The popular belief in "the cycle of abuse" rests on cases of abusive parents who turn up in jail or therapy and who report that they were themselves victims of abuse as children. But scientists would want to know also about the disconfirming cases: children who were beaten but did not grow up to mistreat their children (and, therefore, did not end up in therapy or jail), and people who were not beaten and then did grow up to be abusive parents. When the researchers Joan Kaufman and Edward Zigler reviewed longitudinal studies of the outcomes of child abuse, they found that although being abused does considerably increase the risk of becoming an abusive parent, more than 70 percent of all abused

children do not mistreat their offspring -- hardly an inevitable "cycle."

Practitioners who do not learn about the confirmation bias and ways to counteract it can make devastating judgments in court cases. For example, if they are convinced that a child has been sexually molested, they are often unpersuaded by the child's repeated denials; such denials, they say, are evidence of the depth of the trauma. Sometimes, of course, that is true. But what if it isn't? In the Little Rascals day-care-abuse case in North Carolina, one mother told reporters that it took *10 months* before her child was able to "reveal" the molestation. No one at the time considered the idea that the child might have been remarkably courageous to persist in telling the truth for so long.

Because many therapists tend not to be as deeply imbued with the spirit of skepticism as scientists are (or are supposed to be), it is common for many of them to place their faith in the leader of a particular approach, and to set about trying to do what the school's founder did -- rather than to raise too many questions about the founder's methods or the validity of the founder's theories. If you go off to become certified in Eye Movement Desensitization and Reprocessing (EMDR), invented by Francine Shapiro while she was walking in the woods one day, you are unlikely to ask, "Why, exactly, does waving your finger in front of someone's eyes realign the halves of the brain and reduce anxiety?" Scientific studies of this method show that the successful ingredient in EMDR is an old, tried-and-true technique from behavior therapy: exposing people to a thought or situation that makes them anxious, until the feeling subsides. The eye movements that are supposedly essential, the clinical scientist Scott O. Lilienfeld concluded, do not constitute "anything more than pseudoscientific window dressing."

Similarly, most clinicians are not trained to be skeptical of what a client says or to demand corroborating evidence. Why would they be? A client comes to see you complaining that he has a terrible mother; are you going to argue? Ask to meet the mother? Some clinicians, notably those who practice cognitive-behavior therapy, would, indeed, ask you for the evidence that your mother is terrible and also invite you to consider other explanations of her behavior; but most do not. As the psychiatrist Judith Herman explained in a PBS *Frontline* special on recovered memory: "As a therapist, your job is not to be a detective; your job is not to be a fact-finder; your job is not to be a judge or a jury; and your job is also not to make the family feel better. Your job is to help the patient make sense out of her life, make sense out of her symptoms . . . and make meaning out of her experience."

That remark perfectly summarizes the differing goals of most clinicians and scientists. Clinicians are certainly correct that most of the time it is not possible to corroborate a client's memory anyway, and that it isn't their job to find out what "really" happened in the client's past. Scientists, though, have shown that memories are subject to distortion. So, if the client is going to end up suing a parent for sexual abuse, or if the therapist's intervention ends up causing a devastating family rift, a little detective work seems called for. Detective work is the province of scientists, who are trained *not* to automatically believe what someone says or what someone claims to remember, but to ask, "Where's the evidence?"

For psychological scientists, clinical insight is simply not sufficient evidence. For one thing, the clinician's observations of clients will be inherently limited if they overlook comparison groups of people who are not in therapy. For example, many clinicians invent "checklists" of "indicators" of some problem or disorder -- say, that "excessive" masturbation or bed-wetting are signs of sexual abuse or, my favorite, that losing track of time or becoming engrossed in a book is a sign of multiple-personality disorder. But, before you can say that bed-wetting or masturbation is an indicator that a child has been sexually abused, what must you know? Many psychotherapists cannot give you the simple answer: You must know the rates of bed-wetting and masturbation among all children, including nonabused ones. In fact, many abused children have no symptoms, and many nonabused children wet their beds, masturbate, and are fearful in new situations.

Throughout the 1980s and '90s, many therapists routinely testified in court that they could magically tell, with complete certainty, that a child had been sexually abused because of how the child played with anatomically correct dolls, or because of what the child revealed in drawings. The plausible assumption is that very young children may reveal feelings in their play or drawings that they cannot express verbally. But while such tests may have a therapeutic use, again the scientific evidence is overwhelming that they are worthless for assessment or diagnostic purposes. How do we know that? Because when scientists compared the doll play of abused children to that of control groups of nonabused children, they found that such play is not a valid way of determining whether a child has been sexually abused. The doll's genitals are pretty interesting to all kids.

Likewise, psychological scientists who study children's cognitive development empirically have examined the belief held by many psychotherapists that "children never lie" about sexual abuse. Scientists have shown in dozens of experiments that children often do tell the truth, but that they also lie, misremember, and can be influenced to make false allegations -- just as

adults do. Researchers have shown, too, that adults often misunderstand and misinterpret what children say, and they have identified the conditions that increase a child's suggestibility and the interviewing methods virtually guaranteed to elicit false reports. Those conditions and methods were present in the interrogations of children by social workers, therapists, and police officers in all of the sensational cases of day-care hysteria of the 1980s and '90s. And those coercive practices continue in many jurisdictions today where child-protection workers have not been trained in the latest research.

I fear that the scientist-therapist gap is a done deal. There are too many economic and institutional supports for it, in spite of yearly exhortations by every president of the American Psychological Association for "unity" and "cooperation." That's why, in the late 1980s, a group of psychological scientists formed their own organization, the American Psychological Society, to represent their own scientific interests. Every year, the APA does something else to rile its scientific members while placating its therapist members -- like supporting prescription-writing privileges for Ph.D. psychologists and approving continuing-education programs for unvalidated methods or tests -- and so, every year, more psychological scientists leave the APA for the APS.

But to the public, all this remains an internecine battle that seems to have no direct relevance. That's the danger. Much has been written about America's scientific illiteracy, but social-scientific illiteracy is just as widespread and in some ways even more pernicious. People can deny evolution or fail to learn basic physics, but such ignorance rarely affects their personal lives. The scientific illiteracy of psychotherapists has torn up families, sent innocent defendants to prison, cost people their jobs and custody of their children, and promoted worthless, even harmful, therapies. A public unable to critically assess psychotherapists' claims and methods for scientific credibility will be vulnerable to whatever hysterical epidemic comes along next. And in our psychologically oriented culture, there will be many nexts. Some will be benign; some will merely cost money; and some will cost lives.

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