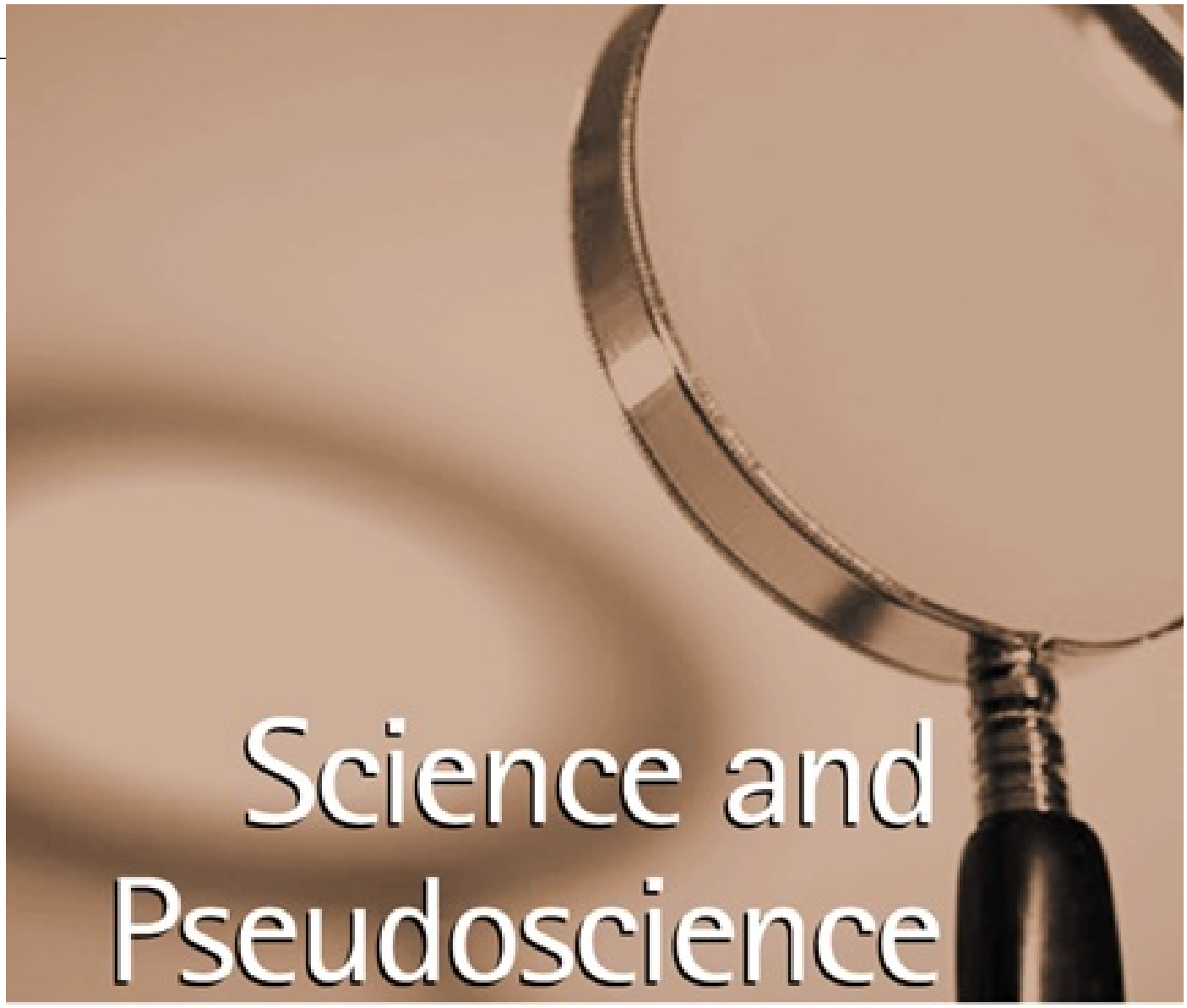

Science and Pseudoscience in Clinical Psychology

Edited by
Scott O. Lilienfeld
Steven Jay Lynn
Jeffrey M. Lohr

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Foreword by
CAROL TAVRIS

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About the Editors

Scott O. Lilienfeld, PhD, is Associate Professor of Psychology at Emory University in Atlanta. He has authored or coauthor approximately 100 articles and book chapters on such topics as the assessment and causes of personality disorders, the relation personality traits to anxiety disorders, conceptual issues in psychiatric classification and diagnosis, projective testing, multiple personal disorder, and the problem of pseudoscience in clinical psychology. He is founder and editor of the new journal *The Scientific Review Mental Health Practice*. Dr. Lilienfeld also serves on the editorial boards of several major journals, including the *Journal of Abnorm Psychology*, *Psychological Assessment*, *Clinical Psychology: Science and Practice*, *Clinical Psychology Review*, the *Journal of Clini Psychology*, *Skeptical Inquirer*, and *The Scientific Review of Alternative Medicine*. He is past president of the Society for a Science of Clini Psychology, and he was the recipient of the 1998 David Shakow Award for Early Career Contributions to Clinical Psychology fro Division 12 (Society for Clinical Psychology) of the American Psychological Association.

Steven Jay Lynn, PhD, ABPP (Clinical, Forensic), is a licensed clinical psychologist and Professor of Psychology at the State University New York at Binghamton. A former president of American Psychological Association's Division 30 (Psychological Hypnosis), he is recipient of the division's award for distinguished contributions to scientific hypnosis. Dr. Lynn is a fellow of many profession organizations and an advisory editor to many professional journals, including the *Journal of Abnormal Psychology*. He is also the author 11 books and more than 200 articles and chapters on hypnosis, memory, fantasy, victimization, and psychotherapy.

Jeffrey M. Lohr, PhD, is Professor of Psychology at the University of Arkansas– Fayetteville. He has been a licensed psychologist Arkansas with a part-time independent practice since 1976. Dr. Lohr's research interests focus on anxiety disorders, domestic violence, a the efficacy of psychosocial treatments. His teaching interests include abnormal psychology, behavior modification and therapy, resear methods, and professional issues in mental health practice.

Contributors

Timothy Anderson, PhD, Department of Psychology, Ohio University, Athens, Ohio

Laura Arnstein, MA, Department of Psychology, State University of New York at Binghamton, Binghamton, New York

Patricia A. Boyle, PhD, Department of Psychiatry and Human Behavior, Brown University, Providenc Rhode Island

Howard N. Garb, PhD, Behavioral Health, VA Pittsburgh Healthcare System, Pittsburgh, Pennsylvania

John P. Garske, PhD, Department of Psychology, Ohio University, Athens, Ohio

Jennifer Gillis, MA, Department of Psychology, State University of New York at Binghamton, Binghamton, New York

Richard Gist, PhD, Department of Psychology, Johnson County Community College, Overland Park, Kansas

Russell E. Glasgow, PhD, AMC Cancer Research Center, Denver, Colorado

Tammy R. Hammond, MS, Department of Psychology, State University of New York at Binghamton, Binghamton, New York

G. Perry Hill, PhD, School of Allied Health Professionals, Louisiana State University Medical Center, Shreveport, Louisiana

Wayne Hooke, MA, private practice, Vancouver, Washington, and Department of Psychology, Portlan Community College, Portland, Oregon

John Hunsley, PhD, School of Psychology, University of Ottawa, Ottawa, Ontario, Canada

Irving Kirsch, PhD, Department of Psychology, University of Connecticut, Storrs, Connecticut

Elisa Krackow, MS, Department of Psychology, State University of New York at Binghamton, Binghamton, New York

Catherine M. Lee, PhD, School of Psychology, University of Ottawa, Ottawa, Ontario, Canada

Scott O. Lilienfeld, PhD, Department of Psychology, Emory University, Atlanta, Georgia

Stephen A. Lisman, PhD, Department of Psychology, State University of New York at Binghamton, Binghamton, New York

Timothy Lock, PhD, Department of Psychology, State University of New York at Binghamton, Binghamton, New York

Elizabeth F. Loftus, PhD, Department of Psychology, University of Washington, Seattle, Washington

Jeffrey M. Lohr, PhD, Department of Psychology, University of Arkansas, Fayetteville, Arkansas

Steven Jay Lynn, PhD, Department of Psychology, State University of New York at Binghamton, Binghamton, New York

James MacKillop, MA, Department of Psychology, State University of New York at Binghamton, Binghamton, New York

Joseph T. McCann, PsyD, JD, United Health Services Hospitals and State University of New York Upstate Medical University–Clinical Campus, Binghamton, New York

Timothy E. Moore, PhD, Department of Psychology, Glendon College/York University, Toronto, Ontario, Canada

Abraham Nievod, PhD, JD, private practice, Berkeley, California

Raymond G. Romanczyk, PhD, BCBA, Department of Psychology, State University of New York at Binghamton, Binghamton, New York

Gerald M. Rosen, PhD, private practice, Seattle, Washington

Deborah Rosenbaum, MA, Department of Psychology, State University of New York at Binghamton, Binghamton, New York

Kelley L. Shindler, BA, Department of Psychology, State University of New York at Binghamton, Binghamton, New York

Margaret Thaler Singer, PhD, Department of Psychology, University of California, Berkeley, Californi

Latha V. Soorya, MA, Department of Psychology, State University of New York at Binghamton, Binghamton, New York

David F. Tolin, PhD, Anxiety Disorders Center, The Institute of Living, Hartford, Connecticut

Harald Walach, PhD, Institute of Environmental Medicine and Department of Epidemiology, University Hospital, Freiburg, Germany

Daniel A. Waschbusch, PhD, Department of Psychology, Dalhousie University, Halifax, Nova Scotia, Canada

Allison Weinstein, MA, Department of Psychology, State University of New York at Binghamton, Binghamton, New York

Nona Wilson, PhD, Department of Counselor Education, University of Wisconsin, Oshkosh, Wisconsi

James M. Wood, PhD, Department of Psychology, University of Texas, El Paso, Texas

The Widening Scientist–Practitioner Gap

A View from the Bridge

CAROL TAVRIS

I was sitting in a courtroom, watching the title of this book—*Science and Pseudoscience in Clinic Psychology*—in action. A pediatric psychologist, a woman with a PhD in clinical psychology from prestigious university, was testifying about the reasons for her sure and certain diagnosis that th defendant was a "Munchausen by proxy" mother, and that the woman's teenage son was not in fact i with an immune disorder but rather was "in collusion" with his disturbed mother to produce h symptoms.

No one disputes that some mothers have induced physical symptoms in their children and subject them to repeated hospitalizations; some cases have been captured on hospital video cameras. There is term for this cruel behavior; we call it child abuse. When the child dies at the hands of an abusi parent, we have a term for that, too; we call it murder. But many clinicians suffer from syndromophili They have never met a behavior they can't label as a mental disorder. One case is an oddity, two is coincidence, and three is an epidemic.

Once a syndrome is labeled, it spawns experts who are ready and willing to identify it, treat it, ar train others to be ever alert for signs of it. No new disorder is "rare" to these experts; it is "mistaken" fo something else or "underdiagnosed." Munchausen by proxy (MBP; factitious disorder by proxy in th appendix to DSM-IV) is the latest trendy disorder to capture clinical and media attention (Mart, 199 see also Chapter 4). Experts all over the country are training nurses, physicians, and clinicians to be o the lookout for Munchausen mothers, and here was one of them. I watched as this clinical psycholog —I'll call her Dr. X—revealed the pseudoscientific assumptions, methods, and ways of thinking th have become common in clinical practice, as this volume will consider in depth:

- Dr. X relied on projective tests to determine that the mother had psychological problems. Quite apa from the problems of reliability and validity with these tests (see Chapter 3), no one has any id whether real MBP mothers have any characteristic mental disorder, any more than we know wheth child-abusing fathers do. Moreover, evidence of a "mental disorder" in this defendant would not reliab indicate that she *was* an MBP mother anyway.
- Dr. X knew nothing about the importance of testing clinical assumptions empirically, let alone operationally defining her terms. What does "in collusion" mean? How does a MBP mother's behavio differ from that of any mother of a chronically sick child, or, for that matter, from that of any lovin mother?

- Dr. X knew nothing about confirmation bias (see Chapter 2) or the principle of falsifiability, an how these might affect clinical diagnosis. Once she decided this mother was a "classic Munch.," as sh wrote in her notes, that was that. Nothing the mother did or said could change her mind. This because, she testified, Munchausen mothers are so deceptively charming, educated, and persuasiv Nothing the child said could change her mind. This is because, she said, he naturally wants to remai with his mother, in spite of her abusiveness. No testimony from immunologists that the child really d have an immune disorder could change her mind. This is because, she explained, Munchausen mothe force doctors to impose treatments on their children by interpreting "borderline" medical conditions problems needing intervention.
- Dr. X understood nothing about the social psychology of diagnosis: for example, how a ra problem, such as "dissociative identity disorder" (see Chapter 5) or "Munchausen by proxy" syndrom becomes overreported when clinicians start looking for it everywhere and are rewarded with fam acclaim, and income when they find it (Acocella, 1999).

- Dr. X understood nothing about the problem of error rates (Mart, 1999): that in their zeal to avo false negatives (failing to identify mothers who are harming their children), clinicians might significant boost the rate of false positives (mistakenly labeling mothers as having MBP syndrome). "This disord destroys families," she said, apparently without pausing to consider that mistaken diagnoses do the sam

In short, this clinical psychologist received a PhD without having acquired a core understanding the basic principles of critical and scientific thinking. Many teachers lament the woeful lack of scien education of their undergraduate students, who are expected to digest a vast assortment of facts and bi of knowledge but rarely have learned how to think about them. But this problem is also widespread graduate clinical psychology programs and psychiatric residencies, where students can earn a PhD or a MD without ever having considered the basic epistemological assumptions and methods of the profession (see also Chapter 16): What kinds of evidence are needed before we can draw stron conclusions? Are there alternative hypotheses that I have not considered? Why are so many diagnoses mental illness based on consensus—a group vote—rather than on empirical evidence, and what does th process reveal about problems of reliability and validity in diagnosis? An ethnographic study of th training of psychiatrists showed that psychiatric residents learn how to make quick diagnoses, prescri medication, and, in a dwindling number of locations, do psychodynamic talk therapy, but rarely do the learn to be skeptical, ask questions, analyze research, or consider alternative explanations or treatmen (Luhrmann, 2000).

I am neither an academic nor a clinician, but as a social psychologist by training and a writer l profession, I have long been interested in the influence of psychological theories on society (and in th influence of societal events on psychological theories). Because psychotherapists of all kinds are the on who get public attention—they tend to be the ones who are writing advice columns, writing pop-psyc books, going on talk shows, and testifying as experts in court cases—the public is largely ignorant of th kind of research done by psychological scientists on clinical issues or any other psychological topic. Thu I have been especially interested in the schism between psychological science and much of clinic practice, and its implications for individuals and for the larger culture.[1]

And what a schism it has become. I have been keeping a list of the widely held beliefs, promoted b many clinicians and other psychotherapists, that have been discredited by empirical evidence. Here is ju a sampling:

- Almost all abused children become abusive parents.
- Almost all children of alcoholics become alcoholic.
- Children never lie about sexual abuse.
- Childhood trauma invariably produces emotional symptoms that carry on into adulthood.
- Memory works like a tape recorder, clicking on at the moment of birth.
- Hypnosis can reliably uncover buried memories.
- Traumatic experiences are usually repressed.
- Hypnosis reliably uncovers accurate memories.
- Subliminal messages strongly influence behavior.
- Children who masturbate or "play doctor" have probably been sexually molested.
- If left unexpressed, anger builds up like steam in a teapot until it explodes.
- Projective tests like the Rorschach validly diagnose personality disorders, most forms psychopathology, and sexual abuse.

All of these mistaken ideas can have, and have had, devastating consequences in people's lives. In tha same courtroom, I heard a social worker explain why she had decided to remove a child from h mother's custody: The mother had been abused as a child, and "we all know" that this is a major ris factor for the mother's abuse of her own child one day. Obviously no one had taught this social worke about disconfirming cases. In fact, longitudinal studies find that although being abused as a chi increases the risk of becoming an abusive parent, the large majority of abused children—about two thir —do not become abusive parents (Kaufman & Zigler, 1987).

Of course, there has always been a gap between psychological science and clinical practice. In mar ways, it is no different from the natural tensions that exist between researchers and practitioners in an field—medicine, engineering, education, psychiatry, physics—when one side is doing research and th other is working in an applied domain: their goals and training are inherently quite different. The goal o psychotherapy, for example, is to help the suffering individual who is sitting there; the goal o psychological research is to explain and predict the behavior of people in general. That is why mar therapists maintain that research methods and findings capture only a small, shriveled image of the re person (Edelson, 1994). Therapy, they note, was helping people long before science or psychology wer invented. Professional training, therefore, should teach students how to do therapy, not how to d science.

In psychology, this divergence in goals and training was present at the conception. Empiric psychology and psychoanalysis were born of different fathers in the late 19th century, and never g along. Throughout the 20th century, they quarreled endlessly over fundamental assumptions about th meaning of science and truth. How do we know what is true? What kind of evidence is required t support a hypothesis? To early psychoanalysts, "science" had nothing to do with controlled experiment interviews, or statistics (Hornstein, 1992). In constructing what they saw as a "science of the mind psychoanalysts relied solely on their own interpretations of cases they saw in therapy, of myths an literature, and of people's behavior. To empirically minded psychologists, the idea that analysts coul claim to be doing science while chucking out the cardinal rules of the scientific method—replicab findings, verifiable data, objective confirmation of evidence, and the concerted effort to contr prejudices and any other possible sources of bias—was alarming. When psychoanalysis first becam popular in the United States in the 1920s, many scientific psychologists regarded it as a popular craz something on a par with mind reading or phrenology, which would blow over. John Watson called "voodooism." "Psychoanalysis attempts to creep in wearing the uniform of science," wrote another criti

at the time, "and to strangle it from the inside" (quoted in Hornstein, 1992). Replace *psychoanalysis* that ~~sentence with eye movement desensitization and reprocessing (EMDR) or thought field~~ therap (TFT), and the attitude is just as prevalent today among psychological scientists.

By the 1960s and 1970s, as the popularity of psychoanalysis was waning, new therapies we emerging. It was easy to tell how pseudoscientific *they* were. Unlike the Freudians, who said you neede to be in treatment for 5 years, these new guys were offering miracle therapies that promised to cure yo in 5 days, 5 minutes, or 5 orgasms.

In the heyday of the countercultural revolution, these therapies multiplied like rabbits. Martin Gross book *The Psychological Society* (1978) included marathon therapy, encounter therapy, nude therap crisis therapy, primal-scream therapy, electric sleep therapy, body-image therapy, deprivation therapy expectation therapy, alpha-wave therapy, "art of living" therapy, "art of loving" therapy, and "do it now therapy. In the 1980s, pop therapy had gone high-tech. Electrical gizmos promised to get both halves your brain working at their peak (Chance, 1989): the Graham Potentializer, the Tranquilite, th Floatarium, the Transcutaneous Electro-Neural Stimulator, the Brain SuperCharger, and the Who Brain Wave Form Synchro-Energizer.

At first, most psychological scientists paid as little heed to the explosion of post-Freudian pop therapi and technologies as they had to psychoanalysis. These therapies were a blot on the landscape psychology, perhaps, but a benign nuisance; the worst thing that consumers might suffer was a loss money and dignity.

But by the mid-1980s North America was in the midst of three social contagions, which some ca hysterical epidemics or moral panics (Jenkins, 1998; Showalter, 1997): recovered-memory therapy (s Chapter 8), the daycare sex-abuse scandals, and multiple personality disorder (now officially calle dissociative identity disorder in DSM-IV; see Chapter 5). All three phenomena had been fomented b the erroneous and scientifically unvalidated claims of psychotherapists, using subjective and unreliab methods. Moreover, many of the people making these claims were psychiatrists and clinic psychologists, along with social workers and generic "psychotherapists" who had taken a weekend cour somewhere on child abuse. Hadn't they taken Psychology 101? Had no one taught them about contr groups, memory, child development, the limitations of hypnosis?

Apparently not. Poole, Lindsay, Memon, and Bull (1995) found that large minorities of registere psychotherapists in the United States and England were using subjective, highly influential techniqu such as hypnosis, dream analysis, and guided imagery related to abuse situations to "un-cover" repress memories of childhood sexual abuse. Replications of this study in the United States and Canada ha found that the percentages have not declined appreciably in recent years (Katz, 2001; Nunez, Poole, Memon, in press; Polusny & Follette, 1996; see also Chapter 8).

And when Michael Yapko (1994) surveyed nearly 1,000 members of the American Association Marriage and Family Therapists, he found that more than half believed that "hypnosis can be used recover memories from as far back as birth"; one third agreed that "the mind is like a compute accurately recording events that actually occurred"; and one fourth of them—this is scary—agreed th "someone's feeling certain about a memory means the memory is likely to be correct." None of thes statements is true; on the contrary, they are belied by extensive research on the normal processes memory confabulation, distortion, and error (Brainerd, Reyna, & Brandse, 1995; Garry, Manning, Loftus, 1996; Loftus & Ketcham, 1994; Schacter, 1996). There was no difference between MAs an PhDs in their endorsement of these items.

The recovered-memory movement showed in glaring lights how far apart empirical psychology an clinical psychology had grown. After World War II, the two sides had tried forging an alliance: Th

"scientist-practitioner model" would govern the training of clinical psychologists, who would draw o
the most relevant findings of research psychology in diagnosing and treating clients. This harmoniou
ideal is still in place in a number of clinical psychology programs across the United States and Canad
where students learn research methods, the empirical findings on cognitive processes as well as on ment
disorders and psychopathology, and the data on the assessment of therapeutic methods and outcome
But like the Ten Commandments, the scientist-practitioner model has been easier to preach than to obe
The inherent tensions between the two sides grew, and by the early 1990s, researchers and clinician
were speaking openly of the "scientist–practitioner gap" (Persons, 1991).

Today, however, calling it a "gap" is like saying there is an Israeli– Arab "gap" in the Middle East. It i
a war, involving deeply held beliefs, political passions, views of human nature and the nature
knowledge, and—as all wars ultimately involve—money, territory, and livelihoods. Anyone who h
disputed the accuracy of recovered memories of sex abuse or who has publicly questioned any of th
many popular but unvalidated therapies (e.g., EMDR, facilitated communication [FC], critical incide
stress debriefing [CISD], or "rebirthing") or projective tests (e.g., the Rorschach) knows the inflammato
nature of such criticism and the invective with which it will be received. In 1993, I wrote an essay for th
New York Times Book Review, pointing out the scientifically unfounded, indeed often preposterou
claims about memory and trauma that characterized popular books on recovered memories of ince
such as *The Courage to Heal* and *Secret Survivors*. I didn't say anything that you wouldn't learn in Psyc
101, yet the *Book Review* received dozens of irate letters from psychiatrists, social workers, and clinic
psychologists. One feminist psychiatrist accused me of writing a "malicious screed," while anothe
clinician, representing the consensus of the letter writers, said my essay placed me "directly on the side
the molesters, rapists, pedophiles and other misogynists."

The current war between psychological scientists and clinicians—as opposed to the normal squabblin
between researchers and practitioners that had been going on for decades—stems from several econom
and cultural forces. One has been the rapid proliferation of psychotherapists of all kinds. Many a
graduated from "freestanding" schools, unconnected to university psychology departments, where th
typically learn only to do therapy—and sometimes only a vague kind of psychodynamic therapy at tha
Others take brief certification courses in hypnotherapy or various counseling programs, and the
promote themselves as experts in a particular method. Because so many kinds of therapy are no
competing in the marketplace of treatments, and because of the economic challenges posed by manage
care, these specialties have become precious sources of income to many therapists. People who earn the
living from giving Rorschach workshops, TFT training, setting up crisis-intervention program
administering projective tests, or diagnosing sexual abuse are not going to be receptive to eviden
questioning the validity of their methods or assumptions.

In North America today, entire industries sail under the flags of pseudoscience, and there is a cultur
reason for their popularity as well as an economic one. Cross-cultural psychologists have studied ho
cultures differ in their need for certainty and tolerance of ambiguity, and hence, for example, wheth
they are willing to try to live with life's inherent uncertainties or pass laws to try to reduce or elimina
them (Cvetkovich & Earle, 1994; Hofstede & Bond, 1988). The United States is a culture that has a lo
tolerance for uncertainty; hence our attraction to "zero tolerance" policies that fruitlessly attempt
eradicate drug abuse and to "abstinence only" sex-education programs that fruitlessly attempt
eradicate sex among teenagers.

In such a culture pseudoscience is particularly attractive, because pseudoscience by definition promis
certainty, whereas science gives us probability and doubt. Pseudoscience is popular because it confirm
what we believe; science is unpopular because it makes us question what we believe. Good science, li

good art, often upsets our established ways of seeing the world. Bruce Rind and his colleagues discover this to their dismay when they published their meta-analysis suggesting that child sexual abuse, careful defined, does not inevitably produce severe psychopathology in adulthood (Rind, Tromovitch, Bauserman, 1998). Did the public rise as one to praise them for this scientific "reassurance" that mo people survive terrible experiences? Hardly. Instead, Congress passed a resolution condemning the research, and an odd consortium of religious conservatives and recovered-memory psychotherapis mobilized an attack on the researchers' motives, methods, and findings (Rind, Tromovitch, Bauserman, 2000).

Longing for certainty about difficult problems, the public turns to psychologists who will give them *t* answer: Which parent should get custody? Is this rapist cured? Is this child's terrible accusatio accurate? What therapy can make me better, fast? Scientists speak in the exasperating language probability: "It is likely that…."How much more appealing are the answers of clinicians who a prepared to say, with certainty: "This mother is paranoid, believing that her husband is out to get her "This rapist is definitely cured," "Children never lie about sexual abuse," or "Thought field therapy can fix you in 5 minutes."

Pseudoscientific programs, potions, and therapies have always been an entrenched part of America culture, along with moonshine and Puritanism. The cultural mix of pragmatism, an optimistic belief th anything can be changed and improved, and impatience with anything that takes much time has create a long-standing market for instant solutions. All a clever entrepreneur has to do is apply a formu historically guaranteed to be successful: (Quick Fix + Pseudoscientific Gloss) Credulous Public = Hig Income. That is why, when TFT, FC, neurolinguistic programming, and rebirthing have traveled th route of electric sleep therapy and the Transcutaneous Electro-Neural Stimulator, new miracle therapi with different acronyms will rise to take their place. It's the American way.

Pseudoscientific therapies will always remain with us, because there are so many economic an cultural interests promoting them. But their potential for harm to individuals and society is growin which is why it is more important than ever for psychological scientists to expose their pretensions an dangers. As Richard McNally says, the best way to combat pseudoscience is to do good science. Indee good psychological science has already helped slow, if not yet overturn, the hysterical epidemics of ou recent history that wrought so much harm. Psychological science has given us a better understanding memory, of the processes of influence and suggestibility in therapy that create such iatrogenic disorde as "multiple personalities," and of better ways of interviewing children and assessing their accounts an memories (Poole & Lamb, 1998). Good psychological science has helped clinicians develop the mo effective interventions for specific problems. Research has distinguished therapeutic techniques that a merely ineffective from those that are harmful—such as rebirthing, in which therapists in Colorad smothered a 10-year-old girl to death as they supposedly helped her to be "reborn" (see Chapter 7), an CISD programs, which can actually delay a victim's recovery from disasters and traumas (see Chapter this volume).

Yet the essential difference between scientific psychology and psychotherapy will always remain, too "In therapy, the trick is to tell stories that satisfy; in science the trick is to tell stories that predict," say Michael Nash (personal communication). "A story that is satisfying—a compelling narrative that mak our lives meaningful—need not be true in some objective sense. So therapists are right when they say th research can't help individuals learn to live with suffering, resolve moral dilemmas, or make sense o their lives. But they must be disabused of the notion that their clients' stories are literally true, or th they have no part in shaping them."

The scientist–practitioner gap, then, may not matter much in the subjective, immeasurable process o

helping a client find wisdom and a story that satisfies. But it does matter in the practice of incompeter coercive, or harmful therapy. And it matters profoundly when therapists step outside their bound claiming expertise and *certainty* in domains in which unverified clinical opinion can ruin lives, ar where knowledge of good psychological science can save them.

REFERENCES

Acocella, J. (1999). *Creating hysteria: Women and multiple personality disorder*. San Francisco: Jossey-Bass.

Brainerd, C. J., Reyna, V. F., & Brandse, E. (1995). Are children's false memories more persistent than their true memories? *Psychologi Science, 6*, 359–364.

Chance, P. (1989, November). The other 90%. *Psychology Today*, pp. 20–21.

Cvetkovich, G. T., & Earle, T. C. (1994). Risk and culture. In W. J. Lonner & R. Malpass (Eds.), *Psychology and culture* (pp. 217–224). Bosto Allyn & Bacon.

Edelson, M. (1994). Can psychotherapy research answer this psychotherapist's questions? In P. F. Talley, H. H. Strupp, & S. F. Butler (Eds. *Psychotherapy research and practice: Bridging the gap* (pp. 60–87). New York: Basic Books.

Garry, M., Manning, C. G., & Loftus, E. F. (1996). Imagination inflation: Imagining a childhood event inflates confidence that it occurre *Psychonomic Bulletin and Review, 3*, 208–214.

Gross, M. (1978). *The psychological society*. New York: Random House.

Hofstede, G., & Bond, M. H. (1988). The Confucius connection: From cultural roots to economic growth. *Organizational Dynamics, 18*, 21.

Hornstein, G. (1992). The return of the repressed: Psychology's problematic relations with psychoanalysis, 1909–1960. *Americ Psychologist, 47*, 254–263.

Jenkins, P. (1998). *Moral panic: Changing concepts of the child molester in modern America*. New Haven, CT: Yale University Press.

Katz, Z. (2001). Canadian psychologists' education, trauma history, and the recovery of memories of childhood sexual abuse (Doctor dissertation, Simon Fraser University, 2001). *Dissertations Abstracts International, 61*, 3848.

Kaufman, J., & Zigler, E. (1987). Do abused children become abusive parents? *American Journal of Orthopsychiatry, 57*, 186–192.

Loftus, E. F., & Ketcham, K. (1994). *The myth of repressed memory*. New York: St. Martin's Press.

Luhrmann, T. M. (2000). *Of two minds: The growing disorder in American psychiatry*. New York: Knopf.

Mart, E. (1999). Problems with the diagnosis of factitious disorder by proxy in forensic settings. *American Journal of Forensic Psycholo 17*, 69–82.

Nunez, N., Poole, D. A., & Memon, A. (in press). Psychology's two cultures revisited: Implications for the integration of science wi practice. *Scientific Review of Mental Health Practice*.

Persons, J. B. (1991). Psychotherapy outcome studies do not accurately represent current models of psychotherapy: A proposed reme *American Psychologist, 46*, 99–106.

Polusny, M. A., & Follette, V. M. (1996). Remembering childhood sexual abuse: A national survey of psychologists' clinical practices, belie and personal experiences. *Professional Psychology: Research and Practice, 27*, 41–52.

Poole, D. A., & Lamb, M. E. (1998). *Investigative interviews of children*. Washington, DC: American Psychological Association.

Poole, D. A., Lindsay, D. S., Memon, A., & Bull, R. (1995). Psychotherapy and the recovery of memories of childhood sexual abuse: U.S. a British practitioners' opinions, practices, and experiences. *Journal of Consulting and Clinical Psychology, 63*, 426–437.

Rind, B., Tromovitch, P., & Bauserman, R. (1998). A meta-analytic examination of assumed properties of child sexual abuse using colle samples. *Psychological Bulletin, 124*, 22–53.

Rind, B., Tromovitch, P., & Bauserman, R. (2000). Condemnation of a scientific article: A chronology and refutation of the attacks and discussion of threats to the integrity of science. *Sexuality and Culture, 4*, 1–62.

Schacter, D. L. (1996). *Searching for memory: The brain, the mind, and the past*. New York: Basic Books.

Showalter, E. (1997). *Hystories: Hysterical epidemics and modern culture*. New York: Columbia University Press.

Tavris, C. (1993, January 3). Beware the incest-survivor machine. *The New York Times Book Review*, pp. 1, 16–18.

Yapko, M. (1994). *Suggestions of abuse: True and false memories of childhood sexual trauma*. New York: Simon & Schuster.

[1] Of course, there are many clinical psychologists who are also scientists, and who do both clinical work and research with equal skill; t other contributors to this volume are examples. However, the very reason for this book is that scientifically minded clinicians are becomi a rapidly dwindling minority within their profession.

Preface

This book is likely to make a number of readers angry. Some readers will probably object to portions o the book on the grounds that their cherished clinical techniques or brands of psychotherapy have bee targeted for critical examination. For them, this book may be a bitter pill to swallow. Other readers w probably be deeply disturbed, even incensed, by the growing proliferation of questionable an unvalidated techniques in clinical psychology. For them, this will be a book that is long overdue. If w manage to leave readers in both camps at least a bit distressed, we will have been successful, because w will have gotten their attention.

Our purpose in this edited volume is to subject a variety of therapeutic, assessment, and diagnost techniques in clinical psychology to incisive but impartial scientific scrutiny. We have elected to focus o techniques that are novel, controversial, or even questionable, but that are currently influential an widely used. By providing thoughtful evaluations of clinical techniques on the boundaries of prese scientific knowledge, we intend to assist readers with the crucial goal of distinguishing science fro pseudoscience in mental health practice.

As will become clear throughout the book, unscientific and otherwise questionable techniques ha increasingly come to dominate the landscape of clinical psychology and allied fields. Survey data sugge that for many psychological conditions, including mood and anxiety disorders, patients are more like to seek out and receive scientifically unsupported than supported interventions. Yet no book exists t help readers differentiate techniques within clinical psychology that are ineffective, undemonstrated, o harmful from those that are grounded solidly in scientific evidence.

This book is the first major volume devoted exclusively to distinguishing scientifically unsupported fro scientifically supported practices in modern clinical psychology. Many readers may find this fa surprising. Nevertheless, as we point out later in the book (Chapter 16)), the field of clinical psycholog has traditionally been reluctant to subject novel and controversial methods to careful scientif evaluation. This reluctance has left a major gap, and to a substantial extent our book will fill it.

We have urged the authors of each chapter to be as objective and dispassionate as possible. I addition, we have encouraged them to be not only appropriately critical when necessary, b constructive. To this end, each chapter features both a discussion of which clinical techniques a ineffective, unvalidated, or undemonstrated, and also a discussion of which techniques are empirical supported or promising. Our mission is not merely to debunk—although in certain cases debunking is needed activity in science—but to enlighten. Not all methods that are novel or superficially implausib are necessarily worthless or ineffective. Reflexive dismissal of the new and untested is as ill advised as blind acceptance. We have tried to ensure that our authors avoid both errors.

This book should be of considerable interest to several audiences: (1) practicing clinicians across th spectrum of mental health professions, including clinical psychology, psychiatry, social work, counselin and psychiatric nursing; (2) academicians and researchers whose work focuses on psychopathology an its diagnosis and treatment; (3) current and would-be consumers of mental health treatment technique (4) educated laypersons interested in mental illness; and (5) graduate students and advanc

undergraduates wishing to learn more about the science and pseudoscience of clinical psychology. Wit
respect to the latter group, this book is suitable as either a primary or supplemental text for graduate an
advanced undergraduate courses in clinical psychology, psychotherapy, and assessment. In addition, th
book should be of interest to lawyers, educators, physicians, nurses, and others whose work bears o
clinical psychology and allied mental health disciplines.

Although many of the chapters deal with conceptually and methodologically challenging issues, w
have tried to keep technical language to a minimum. In addition, each of the major chapters of the boo
contains a glossary of key concepts and terms that should prove useful to readers unfamiliar with ea
major content area.

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throughout the writing and production of this book. We dedicate this book to them.

Scott O. Lilienfe
Steven Jay Lyn
Jeffrey M. Lo

Contents

Science and Pseudoscience in Clinical Psychology

Initial Thoughts, Reflections, and Considerations

SCOTT O. LILIENFELD
STEVEN JAY LYNN
JEFFREY M. LOHR

As Bob Dylan wrote, "The times they are a-changin'." Over the past several decades, clinical psycholog and allied disciplines (e.g., psychiatry, social work, counseling) have borne witness to a virtual se change in the relation between science and practice. A growing minority of clinicians appear to be basi their therapeutic and assessment practices primarily on clinical experience and intuition rather than c research evidence. As a consequence, the term "scientist–practitioner gap" is being invoked wit heightened frequency (see foreword to this volume by Carol Tavris; Fox, 1996), and concerns that th scientific foundations of clinical psychology are steadily eroding are being voiced increasingly in mar quarters (Dawes, 1994; Kalal, 1999; McFall, 1991). It is largely these concerns that have prompted us compile this edited volume, which features chapters by distinguished experts across a broad spectrum areas within clinical psychology. Given the markedly changing landscape of clinical psychology, w believe this book to be both timely and important.

Some might contend that the problem of unsubstantiated treatment techniques is not new and has fact dogged the field of clinical psychology virtually since its inception. To a certain extent, they would b correct. Nevertheless, the growing availability of information resources (some of which have also becom misinformation resources), including popular psychology books and the Internet, the dramatic upsur in the number of mental health training programs that do not emphasize scientific training (Beyerstei 2001), and the burgeoning industry of fringe psychotherapies, have magnified the gulf between scient and practitioner to a problem of critical proportions.

THE SCIENTIST–PRACTITIONER GAP AND ITS SOURCES

What are the primary sources of the growing scientist–practitioner gap? As many authors have not (see Lilienfeld, 1998, 2001, for a discussion), some practitioners in clinical psychology and related ment health disciplines appear to making increased use of unsubstantiated, untested, and otherwi questionable treatment and assessment methods. Moreover, psychotherapeutic methods of unknown doubtful validity are proliferating on an almost weekly basis. For example, a recent and highly selecti sampling of fringe psychotherapeutic practices (Eisner, 2000; see also Singer & Lalich, 1996) include neurolinguistic programming, eye movement desensitization and reprocessing, Thought Field Therap

Emotional Freedom Technique, rage reduction therapy, primal scream therapy, feeling therapy, Buddh psychotherapy, past lives therapy, future lives therapy, alien abduction therapy, angel therapy, rebirthin Sedona method, Silva method, entity depossession therapy, vegetotherapy, palm therapy, and a pletho of other methods (see also Chapter 7).

Moreover, a great deal of academic and media coverage of such fringe treatments is accompanied b scant critical evaluation. For example, a recent edited volume (Shannon, 2002) features 23 chapters c largely unsubstantiated psychological techniques, including music therapy, homeopathy, breath wor therapeutic touch, aromatherapy, medical intuition, acupuncture, and body-centered psychotherapie Nevertheless, in most chapters these techniques receive minimal scientific scrutiny (see Corsini, 2001, f a similar example).

Additional threats to the scientific foundations of clinical psychology and allied fields stem from th thriving self-help industry. This industry produces hundreds of new books, manuals, and audiotap each year (see Chapter 14), many of which promise rapid or straightforward solutions to complex li problems. Although some of these self-help materials may be efficacious, the overwhelming majority them have never been subjected to empirical scrutiny. In addition, an ever-increasing contingent of sel help "gurus" on television and radio talk shows routinely offer advice of questionable scientific validity a receptive, but often vulnerable, audience of troubled individuals (see Chapter 15).

Similarly questionable practices can be found in the domains of psychological assessment an diagnosis. Despite well-replicated evidence that statistical (actuarial) formulas are superior to clinic judgment for a broad range of judgmental and predictive tasks (Grove, Zald, Lebow, Snitz, & Nelso 2000), most clinicians continue to rely on clinical judgment even in cases in which it has been shown be ill advised. There is also evidence that many practitioners tend to be overconfident in their judgmen and predictions, and to fall prey to basic errors in reasoning (e.g., confirmatory bias, illusory correlatio in the process of case formulation (Chapter 2). Moreover, many practitioners base their interpretation on assessment instruments (e.g., human figure drawing tests, Rorschach Inkblot Test, Myers–Brigg Type Indicator, anatomically detailed dolls) that are either highly controversial or questionable from scientific standpoint (see Chapter 3).

Still other clinicians render confident diagnoses of psychiatric conditions, such as dissociative identi disorder (known formerly as multiple personality disorder), whose validity remains in dispute (s Chapter 5, but see also Gleaves, May, & Cardena, 2001, for a different perspective). The problem o questionable diagnostic labels is especially acute in courtroom settings, where psychiatric labels unknown or doubtful validity (e.g., road rage syndrome, sexual addiction, premenstrual dysphor disorder) are sometimes invoked as exculpatory defenses (see Chapter 4).

STRIKING A BALANCE BETWEEN EXCESSIVE OPEN-MINDEDNESS AND EXCESSIVE SKEPTICISM

It is critical to emphasize that at least some of the largely or entirely untested psychotherapeuti assessment, and diagnostic methods reviewed in this volume may ultimately prove to be efficacious valid. It would be a serious error to dismiss any untested techniques out of hand or antecedent to pri critical scrutiny. Such closed-mindedness has sometimes characterized debates concerning the efficacy novel psychotherapies (Beutler & Harwood, 2001). Nevertheless, a basic tenet of science is that t burden of proof always falls squarely on the claimant, not the critic (see Shermer, 1997). Consequentl it is up to the proponents of these techniques to demonstrate that they work, not up to the critics of the

techniques to demonstrate the converse.

As Carl Sagan (1995b) eloquently pointed out, scientific inquiry demands a unique mix of ope mindedness and penetrating skepticism (see also Shermer, 2001). We must remain open to novel an untested claims, regardless of how superficially implausible they might appear at first blush. At the sam time, we must subject these claims to incisive scrutiny to ensure that they withstand the crucible rigorous scientific testing. As space scientist James Oberg observed, keeping an open mind is a virtue b this mind cannot be so open that one's brains fall out (Sagan, 1995a; see also Chapter 9). Although th requirement to hold all claims to high levels of critical scrutiny applies to all domains of science, suc scrutiny is especially crucial in applied areas, such as clinical psychology, in which erroneous claims ineffective practices have the potential to produce harm.

WHY POTENTIALLY PSEUDOSCIENTIFIC TECHNIQUES CAN BE HARMFUL

Some might respond to our arguments by contending that although many of the techniques reviewed this book are either untested or ineffective, most are likely to prove either efficacious or innocuous. Fro this perspective, our emphasis on the dangers posed by such techniques is misplaced, becau unresearched mental health practices are at worst inert.

Nevertheless, this counterargument overlooks several important considerations. Specifically, there a at least three major ways in which un-substantiated mental health techniques can be problemat (Lilienfeld, 2002; see also Beyerstein, 2001). First, some of these techniques may be harmful per se. Th tragic case of Candace Newmaker, the 10-year-old Colorado girl who was smothered to death in 200 by therapists practicing a variant of rebirthing therapy (see Chapter 7), attests to the dangers implementing untested therapeutic techniques (see Mercer, in press). There is also increasing reason t suspect that certain suggestive techniques (e.g., hypnosis, guided imagery) for unearthing purported repressed memories of childhood trauma may exacerbate or even produce psychopathology b inadvertently implanting false memories of past events (see Chapters 7 and 8). Even the use of facilitate communication for infantile autism (see Chapter 13) has resulted in erroneous accusations of chi abuse against family members. Moreover, there is accumulating evidence that certain widely use treatment techniques, such as critical incident stress debriefing (see Chapter 9), peer group interventio for adolescents with conduct disorders (Dishion, McCord, & Poulin, 1999), and certain self-he programs (Rosen, 1987; see Chapter 14) can be harmful. Consequently, the oft-held assumption th "doing something is always better than doing nothing" in the domain of psychotherapy is likely to b mistaken. As psychologist Richard Gist reminds us, doing something is not license to do anything.

Second, even psychotherapies that are by themselves innocuous can indirectly produce harm b depriving individuals of scarce time, financial resources, or both. Economists refer to this side effect "opportunity cost." As a consequence of opportunity cost, individuals who would otherwise use the time and money to seek out demonstrably efficacious treatments may be left with precious little of eithe Such individuals may therefore be less likely to obtain interventions that could prove beneficial.

Third, the use of unsubstantiated techniques eats away at the scientific foundations of the professio of clinical psychology (Lilienfeld, 1998; McFall, 1991). As one of us (Lilienfeld, 2002) recently observed

> Once we abdicate our responsibility to uphold high scientific standards in administering treatments, our scientific credibility and influence are badly damaged. Moreover, by continuing to ignore the imminent dangers posed by questionable mental health techniques, we send an implicit message to our students that we are not deeply committed to anchoring our discipline in scientific evidence or to combating potentially unscientific practices. Our students will most likely follow in our footsteps and continue to turn a blind eye to the widening gap between scientist and practitioner, and between research evidence and clinical work. (p. 9)

In addition, the promulgation of treatment and assessment techniques of questionable validity ca undermine the general public's faith in the profession of clinical psychology, and lead citizens to plac less trust in the assertions of clinical researchers and practitioners.

THE DIFFERENCES BETWEEN SCIENCE AND PSEUDOSCIENCE: A PRIMER

One of the major goals of this book is to distinguish scientific from pseudoscientific claims in clinic psychology. To accomplish this goal, however, we must first delineate the principal differences betwee scientific and pseudoscientific research programs. As one of us has noted elsewhere (Lilienfeld, 1998 science probably differs from pseudoscience in degree rather than in kind. Science and pseudoscience ca be thought of as Roschian (Rosch, 1973) or open (Meehl & Golden, 1982; Pap, 1953) concepts, whic possess intrinsically fuzzy boundaries and an indefinitely extendable list of indicators. Nevertheless, th fuzziness of such categories does not mean that distinctions between science and pseudoscience a fictional or entirely arbitrary. As psychophysicist S. S. Stevens observed, the fact that the preci boundary between day and night is indistinct does not imply that day and night cannot be meaningful differentiated (see Leahey & Leahey, 1983). From this perspective, pseudosciences can be conceptualize as possessing a fallible, but nevertheless useful, list of indicators or "warning signs." The more suc warning signs a discipline exhibits, the more it begins to cross the murky dividing line separating scien from pseudoscience (see also Herbert et al., 2000). A number of philosophers of science (e.g., Bung 1984) and psychologists (e.g., Ruscio, 2001) have outlined some of the most frequent features pseudoscience. Among these features are the following (for further discussions, see Herbert et al., 200 Hines, 1988; Lilienfeld, 1998):

1. *An overuse of ad hoc hypotheses designed to immunize claims from falsification.* From a Popperia or neo-Popperian standpoint (see Popper, 1959) assertions that could never in principle be falsified a un-scientific (but see McNally, in press, for a critique of Popperian notions). The repeated invocation o ad hoc hypotheses to explain away negative findings is a common tactic among proponents pseudoscientific claims. Moreover, in most pseudosciences, ad hoc hypotheses are simply "pasted on" t plug holes in the theory in question. When taken to an extreme, ad hoc hypotheses can provide a impenetrable barrier against potential refutation. For example, some proponents of eye moveme desensitization and reprocessing (EMDR) have argued that negative findings concerning EMDR a almost certainly attributable to low levels of fidelity to the treatment procedure (see Chapter 9). But th have typically been inconsistent in their application of the treatment fidelity concept (Rosen, 1999).

It is crucial to emphasize that the invocation of ad hoc hypotheses in the face of negative evidence sometimes a legitimate strategy in science. In scientific research programs, however, such maneuvers ten to enhance the theory's content, predictive power, or both (see Lakatos, 1978).

2. *Absence of self-correction.* Scientific research programs are not necessarily distinguished fro pseudoscientific research programs in the verisimilitude of their claims, because proponents of bo programs frequently advance incorrect assertions. Nevertheless, in the long run most scientific researc programs tend to eliminate these errors, whereas most pseudoscientific research programs do no Consequently, intellectual stagnation is a hallmark of most pseudoscientific research programs (Rusci 2001). For example, astrology has changed remarkably little in the past 2,500 years (Hines, 1988).

3. *Evasion of peer review.* On a related note, many proponents of pseudoscience avoid subjecting the work to the often ego-bruising process of peer review (Ruscio, 2001; see also Gardner, 1957, fo

illustrations). In some cases, they may do so on the grounds that the peer review process is inherent biased against findings or claims that contradict well-established paradigms (e.g., see Callahan, 2001 for an illustration involving Thought Field Therapy; see also Chapter 9). In other cases, they may avo the peer review process on the grounds that their assertions cannot be evaluated adequately usi standard scientific methods. Although the peer review process is far from flawless (see Peters & Ce 1982, for a striking example), it remains the best mechanism for self-correction in science, and assis investigators in identifying errors in their reasoning, methodology, and analyses. By remaining large insulated from the peer review process, some proponents of pseudoscience forfeit an invaluab opportunity to obtain corrective feedback from informed colleagues.

4. *Emphasis on confirmation rather refutation.* The brilliant physicist Richard Feynman (198 maintained that the essence of science is a bending over backwards to prove oneself wrong. Bartl (1962) similarly maintained that science at its best involves the maximization of constructive criticis Ideally, scientists subject their cherished claims to grave risk of refutation (Meehl, 1978; see also Rusci 2001). In contrast, pseudo-scientists tend to seek only confirming evidence for their claims. Because determined advocate can find at least some supportive evidence for virtually any claim (Popper, 1959 this confirmatory hypothesis-testing strategy is not an efficient means of rooting out error in one's web beliefs.

Moreover, as Bunge (1967) observed, most pseudosciences manage to reinterpret negative anomalous findings as corroborations of their claims (see Herbert et al., 2000). For example, proponen of extrasensory perception (ESP) have sometimes interpreted isolated cases of worse than chan performance on parapsychological tasks (known as "psi missing") as evidence of ESP (Gilovich, 199 Hines, 1988).

5. *Reversed burden of proof.* As noted earlier, the burden of proof in science rests invariably on th individual making a claim, not on the critic. Proponents of pseudoscience frequently neglect th principle and instead demand that skeptics demonstrate beyond a reasonable doubt that a claim (e.g., a assertion regarding the efficacy of a novel therapeutic technique) is false. This error is similar to th logician's ad ignorantium fallacy (i.e., the argument from ignorance), the mistake of assuming that claim is likely to be correct merely because there is no compelling evidence against it (Shermer, 1997 For example, some proponents of unidentified flying objects (UFOs) have insisted that skeptics accou for every unexplained report of an anomalous event in the sky (Hines, 1988; Sagan, 1995a). But becau it is essentially impossible to prove a universal negative, this tactic incorrectly places the burden of pro on the skeptic rather than the claimant.

6. *Absence of connectivity*. In contrast to most scientific research programs, pseudoscientific researc programs tend to lack "connectivity" with other scientific disciplines (Bunge, 1983; Stanovich, 2001). other words, pseudosciences often purport to create entirely new paradigms out of whole cloth rath than to build on extant paradigms. In so doing, they often neglect well-established scientific principles hard-won scientific knowledge. For example, many proponents of ESP argue that it is a genui (although heretofore undetected) physical process of perception, even though reported cases of ES violate almost every major law of physical signals (e.g., ESP purportedly operates just as strongly fro thousands of miles away as it does from a few feet away). Although scientists should always remain op to the possibility that an entirely novel paradigm has successfully overturned all preexisting paradigm they must insist on very high standards of evidence before drawing such a conclusion.

7. *Overreliance on testimonial and anecdotal evidence.* Testimonial and anecdotal evidence can b quite useful in the early stages of scientific investigation. Nevertheless, such evidence is typically muc more helpful in the context of discovery (i.e., hypothesis generation) than in the context of justificati

(i.e., hypothesis testing; see Reichenbach, 1938). Proponents of pseudoscientific claims frequently invo reports from selected cases (e.g., "This treatment clearly worked for Person X, because Person improved markedly following the treatment") as a means of furnishing dispositive evidence for the claims. For example, proponents of certain treatments (e.g., secretin) for autistic disorder (see Chapt 13) have often pointed to uncontrolled case reports of improvement as supportive evidence.

As Gilovich (1991) observed, however, case reports almost never provide sufficient evidence for claim, although they often provide necessary evidence for this claim. For example, if a new form psychotherapy is efficacious, one should certainly expect at least some positive case reports improvement. But such case reports do not provide adequate evidence that the improvement w attributable to the psychotherapy, because this improvement could have been produced by a host other influences (e.g., placebo effects, regression to the mean, spontaneous remission, maturation; s Cook & Campbell, 1979).

8. *Use of obscurantist language*. Many proponents of pseudoscience use impressive sounding or high technical jargon in an effort to provide their disciplines with the superficial trappings of science (see va Rillaer, 1991, for a discussion of "strategies of dissimulation" in pseudoscience). Such language may b convincing to individuals unfamiliar with the scientific underpinnings of the claims in question, an may therefore lend these claims an unwarranted imprimatur of scientific legitimacy.

For example, the developer of EMDR explained the efficacy of this treatment as follows (see als Chapter 9):

> [The] valences of the neural receptors (synaptic potential) of the respective neuro networks, which separately store various information plateaus and levels of adaptive information, are represented by the letters Z through A. It is hypothesized that the high-valence target network (Z) cannot link up with the more adaptive information, which is stored in networks with a lower valence. That is, the synaptic potential is different for each level of affect held in the various neuro networks.... The theory is that when the processing system is catalyzed in EMDR, the valence of the receptors is shifted downward so that they are capable of linking with the receptors of the neuro networks with progressively lower valences.... (Shapiro, 1995, pp. 317–318)

9 . *Absence of boundary conditions*. Most well-supported scientific theories possess bounda conditions, that is, well-articulated limits under which predicted phenomena do and do not apply. I contrast, many or most pseudoscientific phenomena are purported to operate across an exceedingly wi range of conditions. As Hines (1988, 2001) noted, one frequent characteristic of fringe psychotherapies that they are ostensibly efficacious for almost all disorders regardless of their etiology. For example, som proponents of Thought Field Therapy (see Chapter 9) have proposed that this treatment is beneficial f virtually all mental disorders. Moreover, the developer of this treatment has posited that it is efficaciou not only for adults but for "horses, dogs, cats, infants, and very young children" (Callahan, 2001b, 1255).

10. *The mantra of holism*. Proponents of pseudoscientific claims, especially in organic medicine an mental health, often resort to the "mantra of holism" (Ruscio, 2001) to explain away negative finding When invoking this mantra, they typically maintain that scientific claims can be evaluated only with the context of broader claims and therefore cannot be judged in isolation. For example, some proponen of the Rorschach Ink-blot Test have responded to criticisms of this technique (see Chapter 3) by asserti that clinicians virtually never interpret results from a Rorschach in isolation. Instead, in actual practi clinicians consider numerous pieces of information, only one of which may be a Rorschach protoc There are two major difficulties with this line of reasoning. First, it implies that clinicians can effectivel integrate in their heads a great deal of complex psychometric information from diverse sources, a clai that is doubtful given the research literature on clinical judgment (see Chapter 2). Second, by invoki the mantra of holism, proponents of the Rorschach and other techniques can readily avoid subjecti

their claims to the risk of falsification. In other words, if research findings corroborate the validity of specific Rorschach index, Rorschach proponents can point to these findings as supportive evidence, but these findings are negative, Rorschach proponents can explain them away by maintaining that "clinician never interpret this index in isolation anyway" (see Merlo & Barnett, 2001, for an example). This "head I win, tails you lose" reasoning places the claims of these proponents largely outside of the boundaries science.

We encourage readers to bear in mind the aforementioned list of pseudoscience indicators (see Rusci 2001, for other useful indicators) when evaluating the claims presented in this volume. At the same tim we remind readers that these indicators are only probabilistically linked to pseudoscientific researc programs. Scientists, even those who are well trained, are not immune from such practices. In scientif research programs, however, such practices tend to be weeded out eventually through the slow b steady process of self-correction. In contrast to sciences, in which erroneous claims tend to be gradual ferreted out by a process akin to natural selection (e.g., see Campbell's [1974] discussion of evolutionar epistemology), pseudosciences tend to remain stagnant in the face of contradictory evidence.

CONSTRUCTIVE EFFORTS TO ADDRESS THE PROBLEM

Until fairly recently the field of clinical psychology has shown relatively little interest in addressing th threats posed by pseudoscientific or otherwise questionable practices. Paul Meehl (1993), perhaps t foremost clinical psychologist of the latter half of the 20th century, observes,

> It is absurd, as well as arrogant, to pretend that acquiring a Ph.D. somehow immunizes me from the errors of sampling, perception, recording, retention, retrieval, and inference to which the human mind is subject. In earlier times, all introductory psychology courses devoted a lecture or two to the classic studies in the psychology of testimony, and one mark of a psychologist was hard-nosed skepticism about folk beliefs. It seems that quite a few clinical psychologists never got exposed to this basic feature of critical thinking. My teachers at [the University of] Minnesota … shared what Bertrand Russell called the dominant passion of the true scientist—the passion not to be fooled and not to fool anybody else … all of them asked the two searching questions of positivism: "What do you mean?" "How do you know?" If we clinicians lose that passion and forget those questions, we are little more than be-doctored, well-paid soothsayers. I see disturbing signs that this is happening and I predict that, if we do not clean up our clinical act and provide our students with role models of scientific thinking, outsiders will do it for us. (pp. 728–729)

Nevertheless, the past decade has witnessed a number of constructive efforts to address the problem posed by questionable and potentially pseudoscientific methods in clinical psychology. Two of the efforts have originated within the American Psychological Association (APA), an organization that ha been chastised for turning a blind eye to the festering problem of pseudoscience within clinic psychology (Lilienfeld, 1998). First, Division 12 of the APA has advanced a set of criteria for empirical supported treatments (ESTs) for adult and childhood disorders, along with provisional lists therapeutic techniques that satisfy these criteria (see Chambless & Ollendick, 2001, for a thoughtf review). Vigorous and healthy debate surrounds the criteria established for identifying ESTs as well a the current list of ESTs (Herbert, 2000; see also Chapter 6 for a critique of the current status of ESTs Despite this controversy, it seems clear that the increasing push toward ESTs reflects a heightene emphasis on distinguishing interventions that are scientifically supported from those whose support negligible or nonexistent. Second, there is suggestive evidence that certain APA committees have begu to move in the direction of addressing the threats posed by unsubstantiated psychotherapies. F example, several years ago, the APA Continuing Education (CE) Committee turned down workshops o Thought Field Therapy for CE credit on the grounds that the scientific evidence for this treatment wa