



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

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## 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 Clinical Psychology*—in action. A pediatric psychologist, a woman with a PhD in clinical psychology from a prestigious university, was testifying about the reasons for her sure and certain diagnosis that the defendant was a “Munchausen by proxy” mother, and that the woman’s teenage son was not in fact ill with an immune disorder but rather was “in collusion” with his disturbed mother to produce his symptoms.

No one disputes that some mothers have induced physical symptoms in their children and subjected them to repeated hospitalizations; some cases have been captured on hospital video cameras. There is a term for this cruel behavior; we call it child abuse. When the child dies at the hands of an abusive parent, we have a term for that, too; we call it murder. But many clinicians suffer from syndromophilia. 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, and train others to be ever alert for signs of it. No new disorder is “rare” to these experts; it is “mistaken” for something else or “underdiagnosed.” Munchausen by proxy (MBP; factitious disorder by proxy in the appendix to DSM-IV) is the latest trendy disorder to capture clinical and media attention (Mart, 1999; see also [Chapter 4](#)). Experts all over the country are training nurses, physicians, and clinicians to be on the lookout for Munchausen mothers, and here was one of them. I watched as this clinical psychologist—I’ll call her Dr. X—revealed the pseudoscientific assumptions, methods, and ways of thinking that 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 apart from the problems of reliability and validity with these tests (see [Chapter 3](#)), no one has any idea whether real MBP mothers have any characteristic mental disorder, any more than we know whether child-abusing fathers do. Moreover, evidence of a “mental disorder” in this defendant would not reliably indicate that she *was* an MBP mother anyway.

- Dr. X knew nothing about the importance of testing clinical assumptions empirically, let alone of operationally defining her terms. What does “in collusion” mean? How does a MBP mother’s behavior differ from that of any mother of a chronically sick child, or, for that matter, from that of any loving mother?

- Dr. X knew nothing about confirmation bias (see [Chapter 2](#)) or the principle of falsifiability, and how these might affect clinical diagnosis. Once she decided this mother was a “classic Munch.,” as she wrote in her notes, that was that. Nothing the mother did or said could change her mind. This is because, she testified, Munchausen mothers are so deceptively charming, educated, and persuasive. Nothing the child said could change her mind. This is because, she said, he naturally wants to remain with his mother, in spite of her abusiveness. No testimony from immunologists that the child really does not have an immune disorder could change her mind. This is because, she explained, Munchausen mothers force doctors to impose treatments on their children by interpreting “borderline” medical conditions as problems needing intervention.

- Dr. X understood nothing about the social psychology of diagnosis: for example, how a rare problem, such as “dissociative identity disorder” (see [Chapter 5](#)) or “Munchausen by proxy” syndrome, becomes overreported when clinicians start looking for it everywhere and are rewarded with fame and 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 avoid false negatives (failing to identify mothers who are harming their children), clinicians might significantly boost the rate of false positives (mistakenly labeling mothers as having MBP syndrome). “This disorder destroys families,” she said, apparently without pausing to consider that mistaken diagnoses do the same.

In short, this clinical psychologist received a PhD without having acquired a core understanding of the basic principles of critical and scientific thinking. Many teachers lament the woeful lack of scientific education of their undergraduate students, who are expected to digest a vast assortment of facts and bits of knowledge but rarely have learned how to think about them. But this problem is also widespread in 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 strong conclusions? Are there alternative hypotheses that I have not considered? Why are so many diagnoses of mental illness based on consensus—a group vote—rather than on empirical evidence, and what does this process reveal about problems of reliability and validity in diagnosis? An ethnographic study of the training of psychiatrists showed that psychiatric residents learn how to make quick diagnoses, prescribe medication, and, in a dwindling number of locations, do psychodynamic talk therapy, but rarely do they learn to be skeptical, ask questions, analyze research, or consider alternative explanations or treatments (Luhmann, 2000).

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

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

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

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

In psychology, this divergence in goals and training was present at the conception. Empirical psychology and psychoanalysis were born of different fathers in the late 19th century, and never grew along. Throughout the 20th century, they quarreled endlessly over fundamental assumptions about the meaning of science and truth. How do we know what is true? What kind of evidence is required to support a hypothesis? To early psychoanalysts, “science” had nothing to do with controlled experiments, 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 and literature, and of people’s behavior. To empirically minded psychologists, the idea that analysts could claim to be doing science while chucking out the cardinal rules of the scientific method—replicable findings, verifiable data, objective confirmation of evidence, and the concerted effort to control for prejudices and any other possible sources of bias—was alarming. When psychoanalysis first became popular in the United States in the 1920s, many scientific psychologists regarded it as a popular craze, something on a par with mind reading or phrenology, which would blow over. John Watson called it “voodooism.” “Psychoanalysis attempts to creep in wearing the uniform of science,” wrote another critic.

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 therapy (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 were emerging. It was easy to tell how pseudoscientific *they* were. Unlike the Freudians, who said you needed to be in treatment for 5 years, these new guys were offering miracle therapies that promised to cure you in 5 days, 5 minutes, or 5 orgasms.

In the heyday of the countercultural revolution, these therapies multiplied like rabbits. Martin Gross's book *The Psychological Society* (1978) included marathon therapy, encounter therapy, nude therapy, 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 of your brain working at their peak (Chance, 1989): the Graham Potentializer, the Tranquilite, the Floatarium, the Transcutaneous Electro-Neural Stimulator, the Brain SuperCharger, and the Whole Brain Wave Form Synchro-Energizer.

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

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

Apparently not. Poole, Lindsay, Memon, and Bull (1995) found that large minorities of registered psychotherapists in the United States and England were using subjective, highly influential techniques such as hypnosis, dream analysis, and guided imagery related to abuse situations to “un-cover” repressed memories of childhood sexual abuse. Replications of this study in the United States and Canada have 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 of Marriage and Family Therapists, he found that more than half believed that “hypnosis can be used to recover memories from as far back as birth”; one third agreed that “the mind is like a computer accurately recording events that actually occurred”; and one fourth of them—this is scary—agreed that “someone's feeling certain about a memory means the memory is likely to be correct.” None of these statements is true; on the contrary, they are belied by extensive research on the normal processes of memory confabulation, distortion, and error (Brainerd, Reyna, & Brandse, 1995; Garry, Manning, Loftus, 1996; Loftus & Ketcham, 1994; Schacter, 1996). There was no difference between MAs and PhDs in their endorsement of these items.

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

“scientist-practitioner model” would govern the training of clinical psychologists, who would draw on the most relevant findings of research psychology in diagnosing and treating clients. This harmonious ideal is still in place in a number of clinical psychology programs across the United States and Canada where students learn research methods, the empirical findings on cognitive processes as well as on mental disorders and psychopathology, and the data on the assessment of therapeutic methods and outcomes. But like the Ten Commandments, the scientist-practitioner model has been easier to preach than to obey. The inherent tensions between the two sides grew, and by the early 1990s, researchers and clinicians 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 is a war, involving deeply held beliefs, political passions, views of human nature and the nature of knowledge, and—as all wars ultimately involve—money, territory, and livelihoods. Anyone who has disputed the accuracy of recovered memories of sex abuse or who has publicly questioned any of the many popular but unvalidated therapies (e.g., EMDR, facilitated communication [FC], critical incident stress debriefing [CISD], or “rebirthing”) or projective tests (e.g., the Rorschach) knows the inflammatory nature of such criticism and the invective with which it will be received. In 1993, I wrote an essay for the *New York Times Book Review*, pointing out the scientifically unfounded, indeed often preposterous claims about memory and trauma that characterized popular books on recovered memories of incest such as *The Courage to Heal* and *Secret Survivors*. I didn’t say anything that you wouldn’t learn in Psych 101, yet the *Book Review* received dozens of irate letters from psychiatrists, social workers, and clinical psychologists. One feminist psychiatrist accused me of writing a “malicious screed,” while another clinician, representing the consensus of the letter writers, said my essay placed me “directly on the side of the molesters, rapists, pedophiles and other misogynists.”

The current war between psychological scientists and clinicians—as opposed to the normal squabbling between researchers and practitioners that had been going on for decades—stems from several economic and cultural forces. One has been the rapid proliferation of psychotherapists of all kinds. Many have graduated from “freestanding” schools, unconnected to university psychology departments, where they typically learn only to do therapy—and sometimes only a vague kind of psychodynamic therapy at that. Others take brief certification courses in hypnotherapy or various counseling programs, and then promote themselves as experts in a particular method. Because so many kinds of therapy are now competing in the marketplace of treatments, and because of the economic challenges posed by managed care, these specialties have become precious sources of income to many therapists. People who earn their living from giving Rorschach workshops, TFT training, setting up crisis-intervention programs, administering projective tests, or diagnosing sexual abuse are not going to be receptive to evidence questioning the validity of their methods or assumptions.

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

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

good art, often upsets our established ways of seeing the world. Bruce Rind and his colleagues discovered this to their dismay when they published their meta-analysis suggesting that child sexual abuse, carefully 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 most people survive terrible experiences? Hardly. Instead, Congress passed a resolution condemning the research, and an odd consortium of religious conservatives and recovered-memory psychotherapists 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 the answer: Which parent should get custody? Is this rapist cured? Is this child’s terrible accusation accurate? What therapy can make me better, fast? Scientists speak in the exasperating language of probability: “It is likely that...” How much more appealing are the answers of clinicians who are 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 American culture, along with moonshine and Puritanism. The cultural mix of pragmatism, an optimistic belief that anything can be changed and improved, and impatience with anything that takes much time has created a long-standing market for instant solutions. All a clever entrepreneur has to do is apply a formula historically guaranteed to be successful: (Quick Fix + Pseudoscientific Gloss) Credulous Public = High Income. That is why, when TFT, FC, neurolinguistic programming, and rebirthing have traveled the route of electric sleep therapy and the Transcutaneous Electro-Neural Stimulator, new miracle therapies 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 and cultural interests promoting them. But their potential for harm to individuals and society is growing, which is why it is more important than ever for psychological scientists to expose their pretensions and dangers. As Richard McNally says, the best way to combat pseudoscience is to do good science. Indeed, good psychological science has already helped slow, if not yet overturn, the hysterical epidemics of our recent history that wrought so much harm. Psychological science has given us a better understanding of memory, of the processes of influence and suggestibility in therapy that create such iatrogenic disorders as “multiple personalities,” and of better ways of interviewing children and assessing their accounts and memories (Poole & Lamb, 1998). Good psychological science has helped clinicians develop the most effective interventions for specific problems. Research has distinguished therapeutic techniques that are merely ineffective from those that are harmful—such as rebirthing, in which therapists in Colorado smothered a 10-year-old girl to death as they supposedly helped her to be “reborn” (see [Chapter 7](#)), and CISD programs, which can actually delay a victim’s recovery from disasters and traumas (see [Chapter 10](#) in 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,” says Michael Nash (personal communication). “A story that is satisfying—a compelling narrative that makes our lives meaningful—need not be true in some objective sense. So therapists are right when they say that research can’t help individuals learn to live with suffering, resolve moral dilemmas, or make sense of their lives. But they must be disabused of the notion that their clients’ stories are literally true, or that they have no part in shaping them.”

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

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

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

# Preface

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

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

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

*This book is the first major volume devoted exclusively to distinguishing scientifically unsupported from scientifically supported practices in modern clinical psychology.* Many readers may find this fact surprising. Nevertheless, as we point out later in the book ([Chapter 16](#)), the field of clinical psychology has traditionally been reluctant to subject novel and controversial methods to careful scientific 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. In addition, we have encouraged them to be not only appropriately critical when necessary, but also constructive. To this end, each chapter features both a discussion of which clinical techniques are ineffective, unvalidated, or undemonstrated, and also a discussion of which techniques are empirically supported or promising. Our mission is not merely to debunk—although in certain cases debunking is a needed activity in science—but to enlighten. Not all methods that are novel or superficially implausible 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 the spectrum of mental health professions, including clinical psychology, psychiatry, social work, counseling, and psychiatric nursing; (2) academicians and researchers whose work focuses on psychopathology and its diagnosis and treatment; (3) current and would-be consumers of mental health treatment techniques; (4) educated laypersons interested in mental illness; and (5) graduate students and advanced

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

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

We are grateful to a number of individuals who have helped to bring this book to fruition. In particular, we thank Jim Nageotte and Kitty Moore at The Guilford Press, whose advice, assistance, and moral support throughout the project have been invaluable. We also thank Richard McNally, David Tolin, James Herbert, Jerry Davison, Jerry Rosen, Richard Gist, Grant Devilly, Robert Montgomery, John W. Bush, Liz Roemer, Ron Kleinknecht, Carol Tavris, and a host of other colleagues and friends whose ideas have helped to inform and shape this book. Finally, we thank our spouses, Lori, Fern, and Marjorie Beth, for their support, patience, and forbearance—which at times bordered on the irrational—throughout the writing and production of this book. We dedicate this book to them.

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# Science and Pseudoscience in Clinical Psychology

## *Initial Thoughts, Reflections, and Considerations*

SCOTT O. LILIENFELD

STEVEN JAY LYNN

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

Some might contend that the problem of unsubstantiated treatment techniques is not new and has in fact dogged the field of clinical psychology virtually since its inception. To a certain extent, they would be correct. Nevertheless, the growing availability of information resources (some of which have also become misinformation resources), including popular psychology books and the Internet, the dramatic upsurge in the number of mental health training programs that do not emphasize scientific training (Beyerstein, 2001), and the burgeoning industry of fringe psychotherapies, have magnified the gulf between scientist 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 noted (see Lilienfeld, 1998, 2001, for a discussion), some practitioners in clinical psychology and related mental health disciplines appear to be making increased use of unsubstantiated, untested, and otherwise questionable treatment and assessment methods. Moreover, psychotherapeutic methods of unknown or doubtful validity are proliferating on an almost weekly basis. For example, a recent and highly selective sampling of fringe psychotherapeutic practices (Eisner, 2000; see also Singer & Lalich, 1996) included neurolinguistic programming, eye movement desensitization and reprocessing, Thought Field Therapy,

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

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

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

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

Still other clinicians render confident diagnoses of psychiatric conditions, such as dissociative identity disorder (known formerly as multiple personality disorder), whose validity remains in dispute (see [Chapter 5](#), but see also Gleaves, May, & Cardena, 2001, for a different perspective). The problem of questionable diagnostic labels is especially acute in courtroom settings, where psychiatric labels of unknown or doubtful validity (e.g., road rage syndrome, sexual addiction, premenstrual dysphoric 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 psychotherapeutic, assessment, and diagnostic methods reviewed in this volume may ultimately prove to be efficacious and valid. It would be a serious error to dismiss any untested techniques out of hand or antecedent to prior critical scrutiny. Such closed-mindedness has sometimes characterized debates concerning the efficacy of novel psychotherapies (Beutler & Harwood, 2001). Nevertheless, a basic tenet of science is that the burden of proof always falls squarely on the claimant, not the critic (see Shermer, 1997). Consequently, 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 open-mindedness and penetrating skepticism (see also Shermer, 2001). We must remain open to novel and untested claims, regardless of how superficially implausible they might appear at first blush. At the same time, we must subject these claims to incisive scrutiny to ensure that they withstand the crucible of rigorous scientific testing. As space scientist James Oberg observed, keeping an open mind is a virtue but this mind cannot be so open that one's brains fall out (Sagan, 1995a; see also [Chapter 9](#)). Although the requirement to hold all claims to high levels of critical scrutiny applies to all domains of science, such scrutiny is especially crucial in applied areas, such as clinical psychology, in which erroneous claims and 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 in this book are either untested or ineffective, most are likely to prove either efficacious or innocuous. From this perspective, our emphasis on the dangers posed by such techniques is misplaced, because unresearched mental health practices are at worst inert.

Nevertheless, this counterargument overlooks several important considerations. Specifically, there are at least three major ways in which un-substantiated mental health techniques can be problematic (Lilienfeld, 2002; see also Beyerstein, 2001). First, some of these techniques may be harmful per se. The tragic case of Candace Newmaker, the 10-year-old Colorado girl who was smothered to death in 2007 by therapists practicing a variant of rebirthing therapy (see [Chapter 7](#)), attests to the dangers of implementing untested therapeutic techniques (see Mercer, in press). There is also increasing reason to suspect that certain suggestive techniques (e.g., hypnosis, guided imagery) for unearthing purported repressed memories of childhood trauma may exacerbate or even produce psychopathology by inadvertently implanting false memories of past events (see [Chapters 7 and 8](#)). Even the use of facilitated communication for infantile autism (see [Chapter 13](#)) has resulted in erroneous accusations of child abuse against family members. Moreover, there is accumulating evidence that certain widely used treatment techniques, such as critical incident stress debriefing (see [Chapter 9](#)), peer group interventions for adolescents with conduct disorders (Dishion, McCord, & Poulin, 1999), and certain self-help programs (Rosen, 1987; see [Chapter 14](#)) can be harmful. Consequently, the oft-held assumption that “doing something is always better than doing nothing” in the domain of psychotherapy is likely to be 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 by depriving individuals of scarce time, financial resources, or both. Economists refer to this side effect as “opportunity cost.” As a consequence of opportunity cost, individuals who would otherwise use their time and money to seek out demonstrably efficacious treatments may be left with precious little of either. 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 profession 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 can undermine the general public's faith in the profession of clinical psychology, and lead citizens to place 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 clinical psychology. To accomplish this goal, however, we must first delineate the principal differences between 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 can be thought of as Roschian (Rosch, 1973) or open (Meehl & Golden, 1982; Pap, 1953) concepts, which possess intrinsically fuzzy boundaries and an indefinitely extendable list of indicators. Nevertheless, the fuzziness of such categories does not mean that distinctions between science and pseudoscience are fictional or entirely arbitrary. As psychophysicist S. S. Stevens observed, the fact that the precise boundary between day and night is indistinct does not imply that day and night cannot be meaningfully differentiated (see Leahey & Leahey, 1983). From this perspective, pseudosciences can be conceptualized as possessing a fallible, but nevertheless useful, list of indicators or "warning signs." The more such warning signs a discipline exhibits, the more it begins to cross the murky dividing line separating science from pseudoscience (see also Herbert et al., 2000). A number of philosophers of science (e.g., Bunge, 1984) and psychologists (e.g., Ruscio, 2001) have outlined some of the most frequent features of pseudoscience. Among these features are the following (for further discussions, see Herbert et al., 2000; Hines, 1988; Lilienfeld, 1998):

1. *An overuse of ad hoc hypotheses designed to immunize claims from falsification.* From a Popperian or neo-Popperian standpoint (see Popper, 1959) assertions that could never in principle be falsified are unscientific (but see McNally, in press, for a critique of Popperian notions). The repeated invocation of ad hoc hypotheses to explain away negative findings is a common tactic among proponents of pseudoscientific claims. Moreover, in most pseudosciences, ad hoc hypotheses are simply "pasted on" to plug holes in the theory in question. When taken to an extreme, ad hoc hypotheses can provide an impenetrable barrier against potential refutation. For example, some proponents of eye movement desensitization and reprocessing (EMDR) have argued that negative findings concerning EMDR are almost certainly attributable to low levels of fidelity to the treatment procedure (see [Chapter 9](#)). But they 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 is sometimes a legitimate strategy in science. In scientific research programs, however, such maneuvers tend 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 from pseudoscientific research programs in the verisimilitude of their claims, because proponents of both programs frequently advance incorrect assertions. Nevertheless, in the long run most scientific research programs tend to eliminate these errors, whereas most pseudoscientific research programs do not. Consequently, intellectual stagnation is a hallmark of most pseudoscientific research programs (Ruscio, 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 their work to the often ego-bruising process of peer review (Ruscio, 2001; see also Gardner, 1957, for

illustrations). In some cases, they may do so on the grounds that the peer review process is inherently 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 avoid the peer review process on the grounds that their assertions cannot be evaluated adequately using standard scientific methods. Although the peer review process is far from flawless (see Peters & Ceci, 1982, for a striking example), it remains the best mechanism for self-correction in science, and assists investigators in identifying errors in their reasoning, methodology, and analyses. By remaining largely insulated from the peer review process, some proponents of pseudoscience forfeit an invaluable opportunity to obtain corrective feedback from informed colleagues.

4. *Emphasis on confirmation rather refutation.* The brilliant physicist Richard Feynman (1988) maintained that the essence of science is a bending over backwards to prove oneself wrong. Bartlett (1962) similarly maintained that science at its best involves the maximization of constructive criticism. Ideally, scientists subject their cherished claims to grave risk of refutation (Meehl, 1978; see also Rusconi, 2001). In contrast, pseudo-scientists tend to seek only confirming evidence for their claims. Because a 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 of 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, proponents of extrasensory perception (ESP) have sometimes interpreted isolated cases of worse than chance performance on parapsychological tasks (known as "psi missing") as evidence of ESP (Gilovich, 1991; Hines, 1988).

5. *Reversed burden of proof.* As noted earlier, the burden of proof in science rests invariably on the individual making a claim, not on the critic. Proponents of pseudoscience frequently neglect this principle and instead demand that skeptics demonstrate beyond a reasonable doubt that a claim (e.g., an assertion regarding the efficacy of a novel therapeutic technique) is false. This error is similar to the logician's ad ignorantium fallacy (i.e., the argument from ignorance), the mistake of assuming that a 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 account for every unexplained report of an anomalous event in the sky (Hines, 1988; Sagan, 1995a). But because it is essentially impossible to prove a universal negative, this tactic incorrectly places the burden of proof on the skeptic rather than the claimant.

6. *Absence of connectivity.* In contrast to most scientific research programs, pseudoscientific research programs tend to lack "connectivity" with other scientific disciplines (Bunge, 1983; Stanovich, 2001). In other words, pseudosciences often purport to create entirely new paradigms out of whole cloth rather than to build on extant paradigms. In so doing, they often neglect well-established scientific principles and hard-won scientific knowledge. For example, many proponents of ESP argue that it is a genuine (although heretofore undetected) physical process of perception, even though reported cases of ESP violate almost every major law of physical signals (e.g., ESP purportedly operates just as strongly from thousands of miles away as it does from a few feet away). Although scientists should always remain open to the possibility that an entirely novel paradigm has successfully overturned all preexisting paradigms, 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 be quite useful in the early stages of scientific investigation. Nevertheless, such evidence is typically much more helpful in the context of discovery (i.e., hypothesis generation) than in the context of justification.

(i.e., hypothesis testing; see Reichenbach, 1938). Proponents of pseudoscientific claims frequently invoke reports from selected cases (e.g., “This treatment clearly worked for Person X, because Person X 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 [Chapter 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 a claim, although they often provide necessary evidence for this claim. For example, if a new form of psychotherapy is efficacious, one should certainly expect at least some positive case reports of improvement. But such case reports do not provide adequate evidence that the improvement was attributable to the psychotherapy, because this improvement could have been produced by a host of other influences (e.g., placebo effects, regression to the mean, spontaneous remission, maturation; see Cook & Campbell, 1979).

8. *Use of obscurantist language.* Many proponents of pseudoscience use impressive sounding or highly technical jargon in an effort to provide their disciplines with the superficial trappings of science (see van Rillaer, 1991, for a discussion of “strategies of dissimulation” in pseudoscience). Such language may be convincing to individuals unfamiliar with the scientific underpinnings of the claims in question, and 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 also [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 boundary conditions, that is, well-articulated limits under which predicted phenomena do and do not apply. In contrast, many or most pseudoscientific phenomena are purported to operate across an exceedingly wide range of conditions. As Hines (1988, 2001) noted, one frequent characteristic of fringe psychotherapies is that they are ostensibly efficacious for almost all disorders regardless of their etiology. For example, some proponents of Thought Field Therapy (see [Chapter 9](#)) have proposed that this treatment is beneficial for virtually all mental disorders. Moreover, the developer of this treatment has posited that it is efficacious not only for adults but for “horses, dogs, cats, infants, and very young children” (Callahan, 2001b, p. 1255).

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

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

We encourage readers to bear in mind the aforementioned list of pseudoscience indicators (see Rusconi 2001, for other useful indicators) when evaluating the claims presented in this volume. At the same time, we remind readers that these indicators are only probabilistically linked to pseudoscientific research programs. Scientists, even those who are well trained, are not immune from such practices. In scientific research programs, however, such practices tend to be weeded out eventually through the slow but steady process of self-correction. In contrast to sciences, in which erroneous claims tend to be gradually ferreted out by a process akin to natural selection (e.g., see Campbell’s [1974] discussion of evolutionary 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 the threats posed by pseudoscientific or otherwise questionable practices. Paul Meehl (1993), perhaps the 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 beddoctored, 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 has been chastised for turning a blind eye to the festering problem of pseudoscience within clinical psychology (Lilienfeld, 1998). First, Division 12 of the APA has advanced a set of criteria for empirically supported treatments (ESTs) for adult and childhood disorders, along with provisional lists of therapeutic techniques that satisfy these criteria (see Chambless & Ollendick, 2001, for a thoughtful review). Vigorous and healthy debate surrounds the criteria established for identifying ESTs as well as 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 heightened emphasis on distinguishing interventions that are scientifically supported from those whose support is negligible or nonexistent. Second, there is suggestive evidence that certain APA committees have begun to move in the direction of addressing the threats posed by unsubstantiated psychotherapies. For example, several years ago, the APA Continuing Education (CE) Committee turned down workshops on Thought Field Therapy for CE credit on the grounds that the scientific evidence for this treatment was



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