Is the Pseudoscience Concept Useful for Clinical Psychology?

In this special section of *The Scientific Review of Mental Health Practice*, we examine the question of whether the concept of "pseudoscience" is useful for describing and understanding scientifically unsupported practices in clinical psychology and allied mental health disciplines. Although psychological authors have invoked the term "pseudoscience" with increasing frequency over the past decade, it has received little critical scrutiny. Some authors contend that this term is overused, hopelessly vague, or virtually meaningless; others contend that it is necessary for an adequate understanding of unscientific mental health practices.

In his target article, Harvard professor Richard McNally argues that the pseudoscience concept offers precious little above and beyond the more parsimonious concept of empirical support, and that clinical psychology would be better off without it. He goes further to contend that the concept is misleading and may distract researchers and clinicians from more critical issues, particularly those involving the scientific evidence for mental health claims. In response, psychologists James Herbert, William O'Donohue, and Scott Lilienfeld, along with philosopher of science Mario Bunge, maintain that the pseudoscience concept is indispensable for clinical psychology and related disciplines, and that abandoning this concept would be counterproductive. Finally, McNally responds to his four critics by reiterating his call for a renewed emphasis on the level of scientific support for mental health claims.

We hope that the readers of *The Scientific Review of Mental Health Practice* will benefit from this collegial and lively interchange, and that they will emerge with a heightened appreciation of the potential uses and misuses of the pseudoscience concept.

THE DEMISE OF PSEUDOSCIENCE

Richard J. McNally
Harvard University

Talented entrepreneurs have been developing and marketing novel therapeutic methods, some touted as veritable miracle cures for diverse complaints. This phenomenon has caught the attention of scientist-practitioners in psychology, many of whom criticize these approaches as "pseudoscientific." The purpose of this essay is to sketch a simpler, alternative approach to debunking dubious methods in clinical psychology. When therapeutic entrepreneurs make claims on behalf of their interventions, we should not waste our time trying to determine whether their interventions qualify as pseudoscientific. Rather, we should ask them: How do you know that your intervention works? What is your evidence?

Pseudoscience is like pornography: we cannot define it, but we know it when we see it.[1] Or so it seems. But on what basis do scholars identify pseudoscience in clinical psychology (Lilienfeld, Lynn, & Lohr, 2003)? Even if no sharp criterion distinguishes pseudoscience from genuine science, we still need a way to identify it—if we assume the concept of pseudoscience is meaningful. Accordingly, scholars have identified pseudoscience by either its practitioners, its theories, or its methods.

There is little question that certain figures in contemporary clinical psychology have been strongly identified with pseudoscience. But linking pseudoscience to its alleged practitioners has its limitations. Many of history's greatest scientists embraced ideas that clearly qualify as pseudoscientific, at least by today's standards. Not only did early modern astronomers moonlight as astrologers (Dear, 2001, p. 18; Heilbron, 1999, pp. 83–85), but scientific pioneers such as Boyle, Leibniz, and Newton credulously swallowed all kinds of bizarre tales about the natural world resembling those featured in tabloids sold today in supermarket checkout lines (Miller, 2000, p. 27).

A fascinating American case of the scientist doubling as pseudoscientist is that of Cotton Mather, the Puritan polymath perhaps best known for his notorious role in the Salem witch trials (Boyer & Nissenbaum, 1974, p. 9). Despite his "day job" as minister of Boston's First Church, he somehow found the time to publish enough outstanding research to earn election to England's prestigious Royal Society (Bremer, 1995, p. 197). In fact, Mather was nearly martyred for his scientifically prescient but unpopular promotion of the small-

1. I acknowledge the inspiration of Supreme Court Justice Potter Stewart's oft-paraphrased concurring opinion in the 1964 *Jacobellis v. Ohio* case. The Court was addressing whether an erotic film met the description of hard-core pornography. Stewart said: "I shall not today attempt further to define the kinds of material I understand to be embraced within that shorthand description; and perhaps I could never succeed in intelligibly doing so. But I know it when I see it, and the motion picture involved in this case is not that" (United States Reports, 1965, p. 197).

[On the very day that I finished this manuscript, I had occasion to speak to my good friend Carol Tavris. We discussed a forthcoming book on pseudoscience, and she seconded my ambivalence about the concept, noting that she had once likened its vagueness to that of pornography! When I expressed amazement at the coincidence of our both hitting upon the same analogy, she added that she had made this point in an American Psychological Society (APS) lecture a year or so ago. I suddenly had a recovered memory of having read quotes from her talk—including the analogy between pseudoscience and pornography—in the *APS Observer*. I had entirely forgotten the source of what I mistakenly thought was an original idea of mine! Tavris had said, "Pseudoscience is like pornography; we can't define it, but we know it when we see it" in her talk on the APS Presidential Symposium on Science and Pseudoscience, Denver Colorado, June, 3, 1999.]

Richard J. McNally, Department of Psychology, Harvard University.

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Correspondence concerning this article should be addressed to Richard J. McNally, Department of Psychology, Harvard University, 33 Kirkland Street, Cambridge, MA 02138. E-mail: rjm@wjh.harvard.edu.

pox inoculation. A fellow Bostonian, fearing that the vaccine would spread the dreaded disease, tossed a bomb through a window in Mather's house (p. 198). Despite his scientific achievements, Mather's impressive C.V. of 400-plus publications contains many curiosities, such as his article on two-headed snakes (Perry, 1984, p. 55) and his treatise entitled *Memorable Providences Relating to Witchcrafts and Possessions*, in which he described several bewitched children who could "fly like geese" by flapping their arms "like the wings of a bird" (Mather, quoted in Boyer & Nissenbaum, 1974, pp. 23–24). The upshot is that identifying pseudoscience by its practitioners fails because scientists and pseudoscientists have often been the very same people.

Another approach is to identify theories, rather than theorists, as pseudoscientific. Proclaiming falsifiability as the hallmark of science, Karl Popper (1976, pp. 41–43) consigned psychoanalysis,[2] Marxism, and Jungian depth psychology to the dustbin of pseudoscience because, he said, they did not generate falsifiable predictions. No matter what happened, no matter what the empirical observations turned out to be, advocates of these disciplines could always interpret the outcome as support for their theory.

But clearly this approach fails, too. If psychoanalysis were nothing but unfalsifiable pseudoscience, how has it been possible to test predictions derived from Freud's theory of repression (Holmes, 1990)? Indeed, historians of psychoanalysis have convincingly argued that Freud abandoned his early "seduction theory" because his clinical failures refuted predictions about the therapeutic benefits of recovering repressed memories of early childhood sexual abuse (Israëls & Schatzman, 1993).

Falsifiability is useless for distinguishing scientific theories from pseudoscientific ones because any theory, however bizarre, can be clarified, amended, or supplemented with auxiliary hypotheses to prevent its refutation. As Laudan (1996, pp. 218–219) pointed out, the falsifiability criterion renders "scientific" any crank claim made by flat-earthers, astrologers, creationists, or whomever, as long as they specify what would count as a falsifying observation. Falsifiability fails as a demarcation criterion because it is far too lenient.

Finally, one might identify certain methods as pseudoscientific. For example, even though a theory might be falsifiable, its advocates may act pseudoscientifically by engaging in ad hoc attempts to explain away theoretically embarrassing observations. From this Popperian[3] perspective, Herbert et al. (2000) have accused Francine Shapiro and other EMDR advocates of practicing pseudoscience. According to these critics, EMDR mavens do not behave like real scientists, who, according to Popperian dogma, derive bold conjectures from their theories and then relentlessly seek theoretical refutation by exposing these conjectures to risky empirical tests.

Although I share Herbert et al.'s (2000) concerns about the marketing of eye movements and other amusing exotica of the EMDR movement (McNally, 1999a, 1999b), I believe the accusation of pseudoscience misses the mark. After clearing away all the neurological mumbo-jumbo, one can see that EMDR theory is eminently falsifiable (McNally, 2001a), and if Shapiro's (1989) hypothesis about the curative powers of eye movement is not a Popperian "bold conjecture," then nothing is. Indeed, not only is EMDR theory falsifiable, it has already been repeatedly falsified, as a recent meta-analysis has shown (Davidson & Parker, 2001). Despite many attempts, researchers have been unable to demonstrate that eye movements possess therapeutic powers. In response to these disappointing findings, EMDR theorists have cheerfully reconceptualized placebo control manipulations (e.g., rhythmic tapping) as variant forms of EMDR, and it is this ad hoc maneuver that Herbert et al. find especially problematic.

But, as Putnam (1974) points out in his devastating critique[4] of Popper, scientists engage in these ad hoc maneuvers all the time. He illustrates this point with a historical example. Astronomers attempted to predict the orbit of Uranus by applying Newton's law of universal gravitation plus the auxiliary assumption that all planets in the solar system were known. Their observations, however, ran counter to prediction. Rather than admitting that Newton's theory was wrong, they brazenly engaged in an ad hoc gambit to save it from refutation. The astronomers simply assumed that there must be another planet lurking out there somewhere that was responsible for the aberrant observations. This maneuver is formally identical to Shapiro's concluding that all

2. For a truly ghastly specimen of psychoanalytic reasoning, see Freud's (1918/1955) famous case study of the "Wolfman." After reading this example of Freud's genius, one can easily understand Popper's contempt for psychoanalysis. Specifically, Freud begins with the assumption that his patient witnessed his parents having sexual intercourse. He then embarks on a wildly unrestrained interpretive exercise whereby every bit of evidence is twisted to fit his preordained conclusions.

3. Some scholars interpret Popper as identifying certain practices as pseudoscientific (e.g., ad hoc falsification evasion) rather than certain theories as pseudoscientific (e.g., Cioffi, 1998, pp. 210–227).

4. Putnam is scarcely alone. Devastating critiques have been leveled against Popperian and neo-Popperian (e.g., Lakatos, 1970) philosophies of science in recent years (see, for example, Laudan, 1996; Sober, 1993, pp. 46–54; Stove, 2001).

kinds of rhythmic stimulation—bilateral eye movements, tapping, or whatever—are fungible and more effective than imaginal exposure minus rhythmic stimulation. Fortunately for the astronomers, they turned out to be right: Neptune was discovered. And Shapiro, one day, might also turn out to be right.

Of course, one might attempt to distinguish between legitimate ad hoc moves and illegitimate ones, condemning only the latter as pseudoscientific. One might argue that the astronomers were right to engage in ad hoc attempts to save Newton's theory; after all, it had a better prefalsification track record than Shapiro's. Unfortunately, this approach drains the ad hoc objection of its force; it renders it entirely parasitic on issues of evidential support. If we cannot tell whether an ad hoc move is justified without first examining a theory's track record, why not just cut to the chase and inspect the theory's evidential support (or lack thereof) without quibbling about ad hocness per se?

Not all psychologists who diagnose pseudoscience rely solely on Popper's falsifiability criterion. Lilienfeld (1998), for example, has endorsed Mario Bunge's seven hallmarks of pseudoscience. The more criteria met, the more likely the practice or theory qualifies as pseudoscience. The criteria are: (1) overuse of ad hoc hypotheses to escape refutation, (2) emphasis on confirmation rather than refutation, (3) absence of self-correction, (4) reversed burden of proof, (5) overreliance on testimonials and anecdotal evidence, (6) use of obscurantist language, and (7) absence of "connectivity" with other disciplines (p. 5). Of course, each of these individual criteria are fuzzy, too. For example, when does use of ad hoc hypotheses become "overuse," or reliance on anecdotes become "overreliance," or complex concepts become "obscurantist"? And as Foster and Huber (1999) have recently emphasized, first-rate science is strongly confirmationist. As they observed, authors of scientific papers are "much more likely to stress how well the data agree with some theory than how decisively they refute some theory" (p. 48).

One of Lilienfeld's (1998) chief concerns is educating the public about the hazards of pseudoscience. But if most people fail to grasp Popper's simple falsifiability criterion, what are the chances that John Q. Public will memorize and apply Bunge's seven complex criteria for diagnosing pseudoscience? The chances are not great, especially when one considers that most advocates of wacky therapies hold Ph.D.s in clinical psychology, making them far more educated than the average citizen.

The term "pseudoscience" has become little more than an inflammatory buzzword for quickly dismissing one's opponents in media sound-bites. This problem has been especially evident in debates about sociobiology and evolutionary psychology (Segerstråle, 2000, pp. 183, 329, and passim). In yet another example of terminological misuse, an erstwhile debunker of "snake oil" dismissed the work of Karl Lashley as "discredited pseudoscience" (Sarnoff, 2001, p. 28). To be sure, Lashley failed to locate the "engram" of memory, but does that make his efforts pseudoscientific?

Of course, merely because a term can be misused does not mean that it does not have its proper uses. Nevertheless, the pseudoscience concept generates more heat than light. As Laudan (1996) has said: "If we would stand up and be counted on the side of reason, we ought to drop terms like 'pseudo-science' and 'unscientific' from our vocabulary; they are just hollow phrases which do only emotive work for us" (p. 222).

I hasten to add that my ambivalence about the concept of pseudoscience should not be misunderstood as a defense of the psychologists, the theories, or the clinical practices justly criticized in this journal. EMDR, Thought Field Therapy (see McNally, 2001b), and all the rest rightly deserve critique, just not on the grounds of pseudoscience. There are much stronger grounds for critique. Rather than asking, Is this pseudoscience or genuine science? we should ask, What arguments and evidence support this clinical claim?[5] We should be con-

5. I am, of course, aware of Popper's (1959, pp. 27–42) critique of induction and related notions of empirical support, confirmation, etc. Indeed, his belief that science progresses via conjectures and refutations, not confirmation of predictions, arose as a response to Hume's (1739/2000) famous attempt to debunk inductive inference: "even after the observation of the frequent or constant conjunction of objects, we have no reason to draw any inference concerning any object beyond those of which we have had experience" (p. 95; emphasis in original).

Thus, a person who touches a flame and gets burned has "no reason" to infer that future flames will likewise be hot. One cannot validly deduce (in the logician's sense of valid deductive inference) a theory from the facts of observation. Moreover, Hume (1739/2000) added, any appeal to previous successful inductive inference presupposes the very principle under dispute, thereby leading to an infinite regress of justificatory explanations (p. 64).

According to Popper (1979), Hume provided "a simple, straightforward, logical refutation of any claim that induction could be a valid argument, or a justifiable way of reasoning" (p. 86). Agreeing with Hume's analysis, Popper argued that the invalidity of inductive inference means that observations can never "confirm" a theory's probable truth. Popper endeavored to ground scientific reasoning entirely on a deductive basis, claiming that we can falsify but never verify our hypotheses. Thankfully, he said, "a principle of induction is superfluous [in science]" (Popper, 1959, p. 29). We can get by with falsification even if confirmation is nothing but an illusion.

Few scientists take Popper very seriously. As Foster and Huber (1999) wrote: "Despite Popper's enormous prestige and the lip service that is often paid to his ideas, it is astounding how little influence he seems to have had on the practice of science" (p. 48).

cerned with belief-worthiness, epistemic warrant, evidential basis, empirical support (pick your favorite locution), rather than attempting to determine whether the theory or practice falls on the proper side of a demarcation criterion that separates science from pseudoscience. The problem with EMDR, for example, is not that Francine Shapiro is a pseudoscientist, or that EMDR theory is unfalsifiable, or that EMDR mavens make ad hoc moves when confronted with embarrassing data. The problem is that the central claim about the therapeutic powers of eye movement lacks any convincing empirical support.

In conclusion, when clinical psychologists make claims on behalf of their theories or interventions, we should ask them, "How do you know?" Or, we can paraphrase the immortal words of Cuba Gooding Jr.: "Show me the data! Show me the data!"

REFERENCES

Bremer, F. J. (1995). *The Puritan experiment: New England society from Bradford to Edwards* (rev. ed.). Hanover, NH: University Press of New England.

Boyer, P., & Nissenbaum, S. (1974). *Salem possessed: The social origins of witchcraft*. Cambridge, MA: Harvard University Press.

Cioffi, F. (1998). *Freud and question of pseudoscience*. Chicago, IL: Open Court.

Davidson, P. R., & Parker, K. C. H. (2001). Eye movement desensitization and reprocessing (EMDR): A meta-analysis. *Journal of Consulting and Clinical Psychology, 69*, 305–316.

Dear, P. (2001). *Revolutionizing the sciences: European knowledge and its ambitions, 1500–1700*. Princeton, NJ: Princeton University Press.

Foster, K. R., & Huber, P. W. (1999). *Judging science: Scientific knowledge and the federal courts*. Cambridge, MA: MIT Press.

Freud, S. (1955). From the history of an infantile neurosis. In J. Strachey (Ed. and Trans.), *The standard edition of the complete psychological works of Sigmund Freud* (Vol. 17, pp. 7–122). London: Hogarth Press. (Original work published 1918)

Harman, G. (1965). The inference to the best explanation. *Philosophical Review, 74*, 88–95.

Heilbron, J. L. (1999). *The sun in the church: Cathedrals as solar observatories*. Cambridge, MA: Harvard University Press.

Herbert, J. D., Lilienfeld, S. O., Lohr, J. M., Montgomery, R. W., O'Donohue, W. T., Rosen, G. M., et al. (2000). Science and pseudoscience in the development of eye movement desensitization and reprocessing: Implications for clinical psychology. *Clinical Psychology Review, 20*, 945–971.

Holmes, D. S. (1990). The evidence for repression: An examination of sixty years of research. In J. L. Singer (Ed.), *Repression and dissociation: Implications for personality theory, psychopathology, and health* (pp. 85–102). Chicago: University of Chicago.

Hume, D. (2000). *A treatise of human nature*. Oxford, England: Oxford University Press. (Original work published 1739)

Israëls, H., & Schatzman, M. (1993). The seduction theory. *History of Psychiatry, 4*, 23–59.

Josephson, J. R., & Josephson, S. G. (Eds). (1996). *Abductive inference: Computation, philosophy, technology*. Cambridge, England: Cambridge University Press.

Lakatos, I. (1970). Falsification and the methodology of scientific research programmes. In I. Lakatos & A. Musgrave (Eds.), *Criticism and the growth of knowledge* (pp. 91–196). London: Cambridge University Press.

Laudan, L. (1996). *Beyond positivism and relativism: Theory, method, and evidence*. Boulder, CO: Westview Press.

Lilienfeld, S. O. (1998). Pseudoscience in contemporary clinical psychology: What it is and what we can do about it. *The Clinical Psychologist, 51*(4), 3–9.

Lilienfeld, S. O., Lynn, S. J., & Lohr, J. M. (Eds.). (2003). *Science and pseudoscience in contemporary clinical psychology*. New York: Guilford Press.

McNally, R. J. (1999a). EMDR and mesmerism: A comparative historical analysis. *Journal of Anxiety Disorders, 13*, 225–236.

McNally, R. J. (1999b). On eye movements and animal magnetism: A reply to Greenwald's defense of EMDR. *Journal of Anxiety Disorders, 13*, 617–620.

Of course, no scientist believes that infallible truth can be deduced or derived from observational data. Yet merely because an inference is formally invalid by a logician's criteria does not mean that it is unlikely to be true. Indeed, scientists make abductive inferences ("the inference to the best explanation"; Harman, 1965; Josephson & Josephson, 1996) all the time in their efforts to explain their data. Stove (2001) provides a homely example of the kind of reasoning common in science, historical scholarship, and police investigation, but offensive to deductivists like Hume and Popper: "The canary was alive and well when we left the room an hour ago; but it is dead now. Gas from the oven was leaking into the room during that time. So, if nothing else caused the canary's death, the gas did" (p. 136).

The inference about the cause of the canary's death is formally invalid, but not unreasonable. Moreover, it is not unreasonable to believe that all flames are hot even though one cannot deductively derive this conclusion from having gotten one's fingers burned a few times. Scientific reasoning is not confined to formal deductive inference. Popperian concerns about inductive (in)validity should not obscure the fact that science progresses by "getting it right," confirming hypotheses as well as by falsifying them. Therefore, a failure to test and confirm a theory is a good reason for suspending belief in its truth. On these grounds, we can easily criticize targets hitherto condemned on grounds of pseudoscience.

McNally, R. J. (2001a). How to end the EMDR controversy. *Psicoterapia Cognitiva e Comportamentale, 7*, 153–154.

McNally, R. J. (2001b). Tertullian's motto and Callahan's method. *Journal of Clinical Psychology, 57*, 1171–1174.

Miller, P. N. (2000). *Peiresc's Europe: Learning and virtue in the seventeenth century*. New Haven, CT: Yale University Press.

Perry, L. (1984). *Intellectual life in America: A history*. New York: Franklin Watts.

Popper, K. (1959). *The logic of scientific discovery*. New York: Harper & Row.

Popper, K. (1976). *Unended quest: An intellectual autobiography*. LaSalle, IL: Open Court.

Popper, K. R. (1979). *Objective knowledge: An evolutionary approach* (rev. ed.). Oxford, England: Oxford University Press.

Putnam, H. (1974). The "corroboration" of theories. In P. A. Schilpp (Ed.), *The philosophy of Karl Popper* (pp. 221–240). LaSalle, IL: Open Court.

Sarnoff, S. K. (2001). *Sanctified snake oil: The effect of junk science on public policy*. Westport, CT: Praeger.

Segerstråle, U. (2000). *Defenders of the truth: The battle for science in the sociobiology debate and beyond*. Oxford, England: Oxford University Press.

Shapiro, F. (1989). Efficacy of the eye movement desensitization procedure in the treatment of traumatic memories. *Journal of Traumatic Stress, 2*, 199–223.

Sober, E. (1993). *Philosophy of biology*. Boulder, CO: Westview Press.

Stove, D. (2001). *Scientific irrationalism: Origins of a postmodern cult*. New Brunswick, NJ: Transaction Publishers.

United States Reports. (1965). *Cases adjudged in the Supreme Court at October term, 1963* (Vol. 378). Washington, DC: United States Government Printing Office.

THE CONCEPT OF PSEUDOSCIENCE AS A PEDAGOGICAL HEURISTIC

James D. Herbert
Drexel University

McNally criticizes the concept of pseudoscience and offers the idea of *evidential warrant*, or analysis of the empirical data bearing on a claim, as a simple alternative for evaluating the scientific status of mental health practices. Despite its appeal, there are both theoretical and practical problems with evidential warrant. Most importantly, it rests on the dubious assumption that nonscientists have the resources and skills to locate and interpret the scientific literature. The concept of pseudoscience has value as a heuristic device to encourage healthy skepticism among nonscientists regarding questionable practices in mental health.

I find myself in agreement with much of what my friend and colleague Rich McNally has to say in his provocative essay on the utility of the "pseudoscience" concept (McNally, 2003). Specifically, he correctly notes that the concept fails when applied to individuals and even to specific theories. He convincingly refutes a strict Popperian reliance on falsification and deduction as sine qua non of science. He also rightly notes that labels such as "pseudoscience" and its cousins (e.g., "junk science," "fringe science") can be inappropriately yet conveniently misused to dismiss one's opponents and to stifle debate. To put his arguments in context, it is also noteworthy that McNally is no apologist for dubious theories and practices in mental health. He has, in fact, been a vocal critic of some of the more egregious practices in our field, and serves on the editorial board of this journal.

James D. Herbert, Department of Psychology, Drexel University.

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Correspondence concerning this article should be addressed to James D. Herbert, Department of Psychology, Drexel University, Mail Stop 988, 245 N. 15th Street, Philadelphia, PA 19102-1192. E-mail: james.herbert@drexel.edu.

McNally's central thesis boils down to this: There are various philosophical and practical problems with the concept of pseudoscience, all of which can be eliminated by a focus on what he terms *evidential warrant*, i.e., a direct examination of the empirical evidence relating to specific claims. He proposes that it would be more straightforward to examine such evidence directly than to evaluate a claim in light of characteristics deemed pseudoscientific.

This is where we disagree. The sources of my dissent are threefold: (1) McNally gives far too much credit to the ability of nonscientists to locate and interpret scientific data; (2) the legitimacy of scientific theories rests on more than direct empirical support; and (3) finite resources require prioritization of scientific efforts, thereby precluding direct examination of the evidence for all claims.

First and foremost, it should be noted that the concept of pseudoscience is most useful as a heuristic to educate the public and nonscientist professionals about dubious practices rather than as a scientific category per se. It is unlikely that scientists would find much value in arguing among themselves over whether or not a particular practice meets diagnostic criteria for pseudoscience. But such discussions might be quite helpful to nonscien-

tists. Educational efforts using the pseudoscience concept can be used to engender a sense of healthy skepticism among the public regarding questionable claims and practices. In the mental health arena, for example, people can be encouraged to tread cautiously when confronted with claims that appear too good to be true, that are declared to be revolutionary, that are aggressively promoted through testimonials via nontraditional media, that claim validity by virtue of not having been disproved, that are accompanied by defensive dismissals of criticisms of established scientists, and that cost a lot of money (Herbert et al., 2000; Lilienfeld, 1998). This is not, of course, an exhaustive list, and none of these features is necessarily singularly problematic. But the more of these features characterize a given theory or technique, the more useful the educational value of the concept of pseudoscience.

It is worth noting that McNally's position logically requires rejection of the concept of "science" as well. After all, there are no hard-and-fast criteria distinguishing science from other methods of intellectual inquiry. Again, this is not necessarily a problem for scientists, who are less interested in essentialistic philosophical questions than with simply getting on with their work. Like the notion of pseudoscience, the primary value of the concept of science is pedagogical.

McNally's alternative would be to ask the public to examine the evidence directly—"Show me the data!" What he fails to appreciate is that the average citizen simply does not have the time, the resources, and, most importantly, the skills to locate, examine, and interpret the data bearing on most questions. Consider, for example, the case of eye movement desensitization and reprocessing (EMDR) discussed by McNally. As McNally rightly notes, most psychological scientists have now concluded that controlled research has convincingly demonstrated that the defining ingredient of EMDR—bilateral eye movements—is superfluous to any effects produced by the intervention (e.g., Davidson & Parker, 2001; Devilly, 2002; Herbert et al., 2000). This has not deterred the proponents of EMDR, however, who continue to promote the approach, highlighting the eye movement component as aggressively as ever to the public. Does McNally really expect the average citizen to conduct a comprehensive search of the scientific literature and to digest the dozens of papers on this topic? How many members of the public have the statistical background required to interpret empirical psychological studies, or even quantitative reviews of such studies (e.g., meta-analytic reviews)?

Moreover, with respect to mental health, this problem extends to many—and probably most—practicing professionals as well. There is mounting evidence that most mental health professionals pay surprisingly little attention to the empirical scientific literature on assessment and intervention methods in their clinical work (Barlow, Levitt, & Bufka, 1999; Cohen, Sargent, & Sechrest, 1986; Goisman, Warshaw, & Keller, 1999; Harwell et al., 2001; Herbert, 2003; Sanderson, 2002). Although it would be desirable for clinicians to have the resources, time, and skills to obtain and digest the scientific literature bearing on their work, it is simply naive to believe that this will happen anytime soon.

In contrast, the concept of pseudoscience provides a useful pedagogical tool for heightening skepticism among both professionals and the public at large. No specialized knowledge is necessary to recognize characteristics of pseudoscience.

Aside from practical concerns, the concept of evidential warrant is not without conceptual problems. The idea begs the question of what constitutes evidence. McNally is not clear on this point. He appears to suggest that evidence consists of empirical data derived from scientific research. This ignores, however, other criteria relevant to evaluating the legitimacy of scientific theories and practices. The well-known physicist Stephen Hawking (2001) notes that there is no more direct data to support some of the most interesting theories in contemporary theoretical physics (e.g., string theory, brane theory) than there is to support astrology. What distinguishes the two is that the former theories are consistent with other established data and theories, whereas the latter is not. Of course, McNally could expand his notion of evidential warrant to include such considerations, but in so doing he moves away from a strict "Show me the data!" approach to consideration of precisely some of the criteria that characterize pseudoscience (in this case, the notion of "connectivity," or the degree to which a theory is consistent with established observations).

Finally, implicit in McNally's concept of evidential warrant is the idea that all claims are equally deserving of empirical investigation. In essence, we are asked to reserve a priori judgment on the scientific legitimacy of a claim or practice pending review of the relevant empirical data. But not all claims are created equal. Suppose I propose that dyeing aspirin pink greatly enhances its analgesic effects by correcting deficiencies in patients' plasma levels of pink bile. Should I really expect scientists to seriously entertain my claim and to devote precious resources to evaluate it? In fact, scientists simply ignore such claims all the time. We live in a world of finite resources, requiring the prioritization of scientific

efforts. By necessity, many claims go untested. The features associated with the concept of pseudoscience provide useful criteria for deciding which claims merit attention and which do not.

In conclusion, evaluating the scientific legitimacy of questionable, unorthodox, or otherwise dubious claims is tricky business. Scientists must strike a delicate balance between open-mindedness to new innovations on the one hand, and healthy skepticism on the other. This balance is especially relevant to contemporary mental health, in which a maturing scientific culture increasingly competes with a host of questionable claims and practices for the attention of professionals and the public at large. Although the concept of pseudoscience is not without problems, it provides a useful tool to foster healthy skepticism about potentially harmful practices while simultaneously encouraging awareness of scientific developments in mental health.

REFERENCES

Barlow, D. H., Levitt, J. T., & Bufka, L. F. (1999). The dissemination of empirically supported treatments: A view to the future. *Behaviour Research and Therapy, 37*, 147–162.

Cohen, L. H., Sargent, M. M., & Sechrest, L. B. (1986). Use of psychotherapy research by professional psychologists. *American Psychologist, 41*, 198–206.

Davidson, P. R., & Parker, K. C. H. (2001). Eye movement desensitization and reprocessing (EMDR): A meta-analysis. *Journal of Consulting and Clinical Psychology, 69*, 305–316.

Devilly, G. J. (2002). Eye movement desensitization and reprocessing: A chronology of its development and scientific standing. *The Scientific Review of Mental Health Practice, 1*(2), 113–138.

Goisman, R. M., Warshaw, M. G., & Keller, M. B. (1999). Psychosocial treatment prescriptions for generalized anxiety disorder, panic disorder, and social phobia, 1991–1996. *American Journal of Psychiatry, 156*, 1819–1821.

Harwell, V. D., Herbert, J. D., Brandsma, L. L., Dalrymple, K., Cardiaciotto, L., & Crittenden, K. (2001, November). *Research utilization among various mental health professionals*. Paper presented at the meeting of the Association for Advancement of Behavior Therapy, Philadelphia, PA.

Hawking, S. (2001).*The universe in a nutshell.* New York: Bantam Books.

Herbert, J. D. (2003). The science and practice of empirically supported treatments. *Behavior Modification, 27*(3), 412–430.

Herbert, J. D., Lilienfeld, S. O., Lohr, J. M., Montgomery, R. W., O'Donohue, W. T., Rosen, G. M., et al. (2000). Science and pseudoscience in the development of eye movement desensitization and reprocessing: Implications for clinical psychology. *Clinical Psychology Review, 20*, 945–971.

Lilienfeld, S. O. (1998). Pseudoscience in contemporary clinical psychology: What it is and what we can do about it. *The Clinical Psychologist, 51*, 3–9.

McNally, R. J. (2003). The demise of pseudoscience. *The Scientific Review of Mental Health Practice, 2*(2), 97–101.

Sanderson, W. C. (2002). Are evidence-based psychological interventions practiced by clinicians in the field? *Medscape Mental Health, 7*, 1–3.

PSEUDOSCIENCE IS A LEGITIMATE CONSTRUCT

William O'Donohue
University of Nevada, Reno

Because science has such positive connotations in many circles, many try to make their efforts fall into this domain. However, some of these efforts are bad science and some are pseudoscience. The construct "pseudoscience" is defined and defended.

McNally (2003) relies upon an alluring but problematic argument strategy. The metastructure of his negative argument regarding pseudoscience appears to be:

1. The concept "pseudoscience" cannot be clearly defined or demarcated.
2. If something cannot clearly be defined or demarcated, then it ought to be eliminated, or at least avoided.
3. Therefore, the concept of "pseudoscience" ought to be eliminated.

This is a logically valid argument. Nevertheless, it is unsound given the falseness of premises 1 and 2.

One obvious and, on first appearance, successful rhetorical strategy is to demand clarity of your opponent's terms. If one sets the bar for such clarity sufficiently high, one can conclude that the term in question is insufficiently clear and therefore ought to be avoided or that some knowledge claim involving it is obviated. But however rhetorically successful this semantic move may be, it is generally problematic as it can too easily lead to general skepticism. What terms are clear? Are "anxiety," "reinforcer," or "plausible rival hypothesis" sufficiently clear? Quine's (1980) semantic underdetermination thesis involves several arguments. First, the significance of a word (or even sentence) is not built individually by its own individual basic observations. Thus, no word or sentence can be reduced to sense experience. Second, with proper shifts in the meanings of other words, all words or sentences can be changed to accord with experience. Quine's positive doctrine is that words and sentences have meanings corporately—taken as a whole—with other words and sentences in the web, and various modifications can be made in this system to still leave coherence and correspondence.

Wittgenstein (1958) also has cast doubt on this Socratic "unless you give me necessary and sufficient conditions definitions we are in big trouble" move. He famously suggested that the denotations of words often simply share "family resemblances." He asserted that "we see a complicated network of similarities overlapping and criss-crossing; sometimes overall similarities, sometimes similarities of detail" (p. 32). Words need not have sharp boundaries or necessary or sufficient conditions in order to be meaningful and useful. The notion that if something is not clear then it ought to be eliminated would do injury to more modern notions of "family resemblances," "fuzzy definitions," and "language games" in understanding the use of language.

Thus, one problem with McNally's argument is that he is not clear on what is "sufficiently clear" for an acceptable definition. Note that by McNally's criteria the concept of "science" is also lost. He might be happy

William O'Donohue, Nicholas Cummings Professor of Organized Behavioral Healthcare Delivery, University of Nevada, Reno.

Correspondence concerning this article should be addressed to William O'Donohue, Department of Psychology, University of Nevada, Reno, Reno, NV 89557. E-mail: billodonohue@earthink.net.

with this result, simply asking all to quit quibbling about the applications of these words and ask instead, What's your evidence? But I am not so sanguine. McNally's critical rationalism is certainly to be applauded. Asking for warrants for claims is generally a good idea. However, McNally's suggestion that this move is sufficient to eliminate the concept of pseudoscience is too broad. I can ask my daughter to produce the evidence for the claim that her sister hit her, but I am not thereby conducting science or evaluating her scientific behavior. I can ask my mechanic for evidence that he actually replaced my allegedly faulty starter, but again I am not conducting science nor evaluating his scientific behavior. I am rationally evaluating claims, but I am not engaging in science. Critical rationalism is the general category, science the species. McNally is making what logicians term a *category error*. That is, he is confusing a construct as belonging to one logical type that actually belongs to another, more abstract level (Ryle, 1949).

Let me now turn to my positive argument. McNally is correct in that pseudoscience may be overused and at times a simpler approach is called for. Such concepts as "bad science," "problematic evidence," "no evidence," and "ad hoc strategem" can do the work of some of the uses of the pseudoscience concept. But let me sketch out what I believe is a legitimate use of the pseudoscience concept:

1. The process or claim bears the trappings of science (e.g., measurement, conducted at an "institute," that contains scientific sounding components, such as neurological mechanisms and even control groups). On appearance there is the "smell" of science.
2. But there is not the *substance* of science.

Feynman's (1998) construct of "cargo cult science" fits nicely here. Feynman tells of indigenous people who, during World War II, saw planes and runways, who later built façade planes and runways in an attempt to bring the same rewards. Here the concept of pseudoscience captures a very similar phenomenon: the façade of science is constructed but the substance is missing. The pseudoscience concept is critical is because science has enormous prestige and persuasive potential. And those wanting to persuade others exploit these attributes. We have all seen advertisements for the "science" of weight loss, hair replacement, and breast and penis enlargement. These advertisements often feature researchers in white coats, "scientific" institutes, and technical sounding gibberish. Calling these claims pseudoscientific denotes that although they have the appearance of science, they lack the substance.

This discussion, of course, does involve an understanding what the substance of science is. This issue is not easy but my foregoing remarks about the permissible looseness of constructs are relevant here. Lilienfeld's (1998) criteria for pseudoscience, modeled largely after those of Bunge (1984), do a nice job of defining some decent boundaries between pseudoscience and science. His features of pseudoscience (e.g., ad hoc moves in the face of anomalies, a confirmationist emphasis, absence of self-correction, reliance on testimonials, obscurantist language, and failure to connect with established sciences) accord well with those offered by prominent philosophers of science who have attempted to capture the nature of the scientific enterprise. His criteria also shed light on the key dimensions of the concept of pseudoscience.

REFERENCES

Bunge, M. (1984, Fall). What is pseudoscience? *Skeptical Inquirer, 9*, 36–46.

Feynman, R. (1998). *Surely you're joking, Mr. Feynman*. New York: Basic Books.

Lilienfeld, S. O. (1998). Pseudoscience in contemporary clinical psychology: What it is and what we can do about it. *The Clinical Psychologist, 51*(4), 3–9.

McNally, R. J. (2003). The demise of pseudoscience. *The Scientific Review of Mental Health Practice, 2*(2), 97–101.

Quine, W. V. O (1980). Two dogmas of empiricism. In W. V. O. Quine, *From a logical point of view*. Cambridge, MA: Harvard University Press.

Ryle, G. (1949). *The concept of the mind*. London: Hutchinson.

Wittgenstein, L. (1958). *Philosophical investigations*. New York: Macmillan.

PSEUDOSCIENCE IS ALIVE AND WELL

Scott O. Lilienfeld
Emory University

Steven J. Lynn
Binghamton University

Jeffrey M. Lohr
University of Arkansas

In contrast to McNally, we contend that the concept of pseudoscience is meaningful and useful for researchers, clinicians, and mental health consumers. This concept denotes a "syndrome" of covarying characteristics exhibited by research programs that aspire toward scientific status but that possess only its superficial trappings. Hence, the signs of pseudoscience provide extremely helpful warning signs for individuals who are evaluating the plausibility of novel and controversial mental health claims. Moreover, McNally's proposal to abandon the pseudoscience concept neglects to distinguish between appropriate and inappropriate uses of ad hoc hypotheses. We conclude that the pseudoscience concept is necessary to separate individuals who merely advance false claims (which almost all scientists do on occasion) from individuals who advance false claims but who do not "play by the rules" of science.

In his engaging and thought-provoking commentary "The Demise of Pseudoscience," Richard McNally (2003) suggests that the concept of pseudoscience has, on balance, done more harm than good and should probably be scheduled for a long-overdue retirement. We hope that McNally's provocative essay will serve as a much-needed stimulus for further discussion regarding the utility, or lack thereof, of the pseudoscience concept.

According to McNally, the term "pseudoscience" is all too frequently used as a conversation stopper: a means of stigmatizing claims that are not to one's liking. Regrettably, we suspect that McNally's sober assessment of the typical use of this term is largely correct. Moreover, McNally maintains that well-respected scientists sometimes make use of the same techniques, such as ad hoc immunizing tactics, frequently used by ostensible pseudoscientists. For example, he notes that when astronomers posited the existence of a planet outside the orbit of Uranus (which turned out, of course, to be Neptune), they engaged in an ad hoc maneuver designed to salvage their gravitational models from falsification. McNally concludes that we should focus exclusively on the question of whether there is adequate empirical support for a psychological claim—"Show me the data!"—and leave it at that. That is, absence of sufficient empirical warrant, not pseudoscience, is the critical problem with many or most of the techniques critically examined in this journal.

Our views differ from those of McNally in several ways, although we of course agree with him that the level of empirical support for claims is crucial. Unlike McNally, who contends that the sun has set on the concept of pseudoscience, we maintain that this concept remains immensely useful and cannot simply be dispensed with. This concept—not to mention the troubling epistemic practices associated with it—is alive and well.

EVIDENCE VERSUS THE HANDLING OF EVIDENCE

In particular, McNally's arguments do not distinguish between the *evidence for claims* and the *ways in which the proponents of a research program handle the*

Scott O. Lilienfeld, Department of Psychology, Emory University; Steven J. Lynn, Department of Psychology, Binghamton University; Jeffrey M. Lohr, Department of Psychology, University of Arkansas.

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Correspondence concerning this article should be addressed to Scott O. Lilienfeld, Department of Psychology, Room 206, 532 North Kilgo Circle, Emory University, Atlanta, GA 30322. E-mail: slilien@emory.edu.

evidence concerning these claims. We are inclined to believe (and we suspect that McNally would concur with us) that claims per se are neither scientific nor pseudoscientific (see Weiner, Spielberger, & Abeles, 2002, for a misunderstanding of this point in the context of the Rorschach inkblot test). As a consequence, it is inappropriate to brand specific techniques (e.g., a form of psychotherapy, an assessment instrument) pseudoscientific (cf., Herbert et al., 2000). Instead, the concept of pseudoscience applies to the ways in which proponents of a claim handle evidence, particularly evidence that contradicts their claim. In this respect, we depart from McNally's exclusive focus on the truth value of claims. The concept of pseudoscience remains meaningful because there exists a constellation of covarying features, including the overuse of ad hoc hypotheses, absence of self-correction, reversal of the burden of proof, and lack of connectivity with established scientific disciplines (Bunge, 1984; Lilienfeld, 1998), that typifies certain research programs characterized by the appearance of science, but not its substance.

Putting it somewhat differently, by abandoning the concept of pseudoscience, one is left with only the distinction between claims that do and do not possess empirical support. But because scientists are often wrong, sometimes egregiously so (see Youngson, 1998, for striking examples), one is thereby left with no means of distinguishing researchers who "play by the rules" of science from those that do not. Some proponents of mistaken claims eventually, albeit often reluctantly, acknowledge that their claims are either erroneous or in need of revision, whereas others cling stubbornly to such claims despite overwhelming evidence to the contrary. The former are adhering to the canons of scientific methodology, even though their initial claims were incorrect. In contrast, the latter often exemplify the core features of pseudoscience, particularly an absence of self-correction.

The Utility of the Pseudoscience Concept

The distinction between these two types of researchers is useful and important, because the characteristic features of pseudoscience can assist us with differentiating science from the superficial appearance of science. In other words, because the features of pseudoscience tend to covary as a loose "syndrome," one or more of these features can be thought of as helpful warning signs to consumers in the general public, practitioners, and researchers of the potential presence of other features. Nevertheless, as McNally warns us, the precise demarcation line between these two areas is inherently fuzzy. This is because science and pseudoscience are almost certainly open (fuzzy) concepts with indefinite boundaries (see Pap, 1953; Rosch, 1973). As a consequence, debates concerning whether certain research programs on the "borderlands" of science (Shermer, 2001) are scientific or pseudoscientific are unlikely to prove fruitful, as the distinction between science and pseudoscience is unlikely to be clear-cut.

Nevertheless, this distinction is likely to be no less clear-cut than that between evidentiary support and its absence. Although McNally appears to imply that determining whether a claim possesses strong empirical support is more straightforward than determining of whether a research program is pseudoscientific, we are doubtful. One has only to look at the recent and highly acrimonious debates concerning which psychotherapies should be regarded as "empirically supported" (see Chambless & Ollendick, 2001) to appreciate how vehemently even well-informed scientists can disagree about the level of scientific support for a claim (see also Lilienfeld, Wood, & Garb, 2000, for a discussion of controversies concerning the scientific status of projective techniques). Moreover, as Meehl (1991) noted, a variety of factors (e.g., problematic auxiliary hypotheses, low statistical power, experimenter error, publication bias favoring positive over negative findings, editorial bias, use of pilot studies to decide whether additional studies are worth pursuing) conspire to render many or most research literatures in the "soft" areas of psychology (e.g., clinical, personality, social) extremely difficult to interpret. Thus, although the distinction between science and pseudoscience is somewhat fuzzy around the edges, it is unlikely to be any fuzzier than the distinction between evidentiary warrant and its absence.

Scientists and Ad Hoc Maneuvers: Make Not a Mockery of Honest Ad Hockery

We have argued that the characteristics of pseudoscience can be viewed as helpful warning signs that something is seriously amiss in the conduct of a research program. But do not scientists occasionally engage in pseudoscientific tactics? Certainly. The common features of pseudoscience are only probabilistically useful in distinguishing genuine science from its intellectual impostures (Herbert et al., 2000; Lilienfeld, 1998).

Nevertheless, several of the distinctions between scientific and pseudoscientific tactics are, to invert an overused phrase, more real than superficial. Take, for

instance, McNally's example of astronomers who invoked the existence of Neptune to rescue their gravitational theories from falsification. As McNally correctly notes, this maneuver was decidedly ad hoc in character. Did these astronomers therefore engage in tactics that are essentially equivalent to those used by the proponents of eye movement desensitization and reprocessing (EMDR), who attempted to explain away the therapeutic inertness of eye movements by conceptualizing placebo control conditions (e.g., a fixed eye control condition) as variants of EMDR (see Herbert et al., 2000)? McNally appears to believe so.

But as Meehl (1990) pointed out, useful guidelines exist for distinguishing legitimate from illegitimate uses of ad hoc hypotheses in science. In particular, it is often quite appropriate to invoke an ad hoc hypothesis in the face of negative data when the substantive theory in question already possesses a strong track record of previously corroborated predictions (that is, a well-corroborated theory has what Meehl playfully calls "money in the bank"). This is especially the case when the theory has survived numerous risky Popperian tests. Because such a theory has already proven its mettle in previous tests, its proponents are frequently justified in invoking ad hoc hypotheses to account for negative findings.

In addition, ad hoc hypotheses are more likely to be legitimate when they connect up with already established scientific findings and principles. In such cases, the ad hoc hypotheses tend to rest on firmer epistemic footing and to bear a closer relation to the substantive theory of interest. In many developed sciences, for example, ad hoc hypotheses are often difficult to separate from the substantive theory itself (see also Meehl, 1978). Ad hoc hypotheses concerning gravitation, as discussed by McNally, are a good example.

In contrast, it is usually problematic to invoke ad hoc hypotheses in the absence of an established track record of successfully corroborated predictions. In addition, ad hoc hypotheses that are invoked largely "out of thin air" and that bear little or no relation to the substantive theory itself tend to rest on much shakier scientific footing (Meehl, 1978). For example, the ad hoc claim that a fixed eye movement condition, initially conceptualized as a control condition with which to compare EMDR, is merely a variant of EMDR, lacks connectivity (Bunge, 1967) with well-established scientific principles. Much like magicians pulling rabbits out of hats, researchers in pseudoscientific research programs tend to "tack on" whatever ad hoc hypotheses strike them as convenient for rescuing their pet claims from refutation.

Moreover, as Lakatos noted, in scientific research programs ad hoc hypotheses (what he termed "strategic retreats") often strengthen the theory's content, enhance the theory's capacity to generate successful predictions, or both. In contrast, in pseudoscientific research programs ad hoc hypotheses typically do neither. In Lakatosian terms, the former research programs tend to be *progressive*—hypotheses, including ad hoc hypotheses, anticipate novel findings. In contrast, the latter research programs tend to be *degenerative*, i.e., ad hoc hypotheses are constructed only in response to novel (and anomalous) findings (Lakatos, 1970). Consequently, the proponents of the latter programs are much like the Red Queen in *Alice Through the Looking-Glass* (Carroll, 1872), who is always "running just to keep in the same place" (see also Herbert et al., 2000).

Thus, not all ad hoc hypotheses are created equal, and some are far more legitimate than others. As philosopher of science Clark Glymour was fond of saying, "Make not a mockery of honest ad hockery" (see also Meehl, 1990, for a discussion of the distinction between "honest" and "dishonest" ad hoc hypotheses in science).

CONCLUDING THOUGHTS

All that said, we share McNally's serious and trenchantly stated concerns regarding the misuse of the pseudoscience concept. As McNally observes, it is all too easy to cavalierly dismiss the arguments of one's intellectual opponents by slapping them with the pejorative label of pseudoscience. Nevertheless, we must recall the basic principle of *abusus non tollit usum* (viz., the abuse of a concept does not invalidate its proper use). The problem rightly identified by McNally—and the misuses of the pseudoscience concept that he justifiably decries—largely dissolves once one distinguishes between factual claims, on the one hand, and the ways in which proponents of these claims deal with evidence, on the other. The pseudoscience concept applies to the latter, not the former (see also Lilienfeld, 1998).

REFERENCES

Bunge, M. (1967). *Scientific research*. New York: Springer.

Bunge, M. (1984, Fall). What is pseudoscience? *Skeptical Inquirer, 9*, 36–46.

Carroll, L. (1872). *Through the looking-glass and what Alice found there*. London: Macmillan.

Chambless, D. L., & Ollendick, T. H. (2001). Empirically supported psychological interventions: Controversies and evidence. *Annual Review of Psychology, 52*, 685–716.

Herbert, J. D., Lilienfeld, S. O., Lohr, J. M., Montgomery, R. W., O'Donohue, W. T, Rosen, G. M., et al. (2000). Science and pseudoscience in the development of eye movement desensitization and reprocessing. *Clinical Psychology Review, 20*, 945–971.

Lakatos, I. (1970). Falsification and the methodology of scientific research programmes. In I. Lakatos & A. Musgrave (Eds.), *Criticism and the growth of knowledge* (pp. 91–195). Cambridge, England: Cambridge University Press.

Lilienfeld, S. O. (1998). Pseudoscience in contemporary clinical psychology: What it is and what we can do about it. *The Clinical Psychologist, 51*, 3–9.

Lilienfeld, S. O., Wood, J. M., & Garb, H. N. (2000). The scientific status of projective techniques. *Psychological Science in the Public Interest, 2*, 27–66.

McNally, R. J. (2003). The demise of pseudoscience. *The Scientific Review of Mental Health Practice, 2*(2), 97–101.

Meehl, P. E. (1978). Theoretical risks and tabular asterisks: Sir Karl, Sir Ronald, and the slow progress of soft psychology. *Journal of Consulting and Clinical Psychology, 46*, 816–834.

Meehl, P. E. (1990). Appraising and amending theories: The strategy of Lakatosian defense and two principles that warrant it. *Psychological Inquiry, 1*, 108–141.

Meehl, P. E. (1991). Why summaries of research on psychological theories are often uninterpretable. In R. E. Snow & D. E. Wiley (Eds.), *Improving inquiry in social science: A volume in honor of Lee J. Cronbach* (pp. 13–59). Hillsdale, NJ: Erlbaum.

Pap, A. (1953). Reduction sentences and open concepts. *Methodos, 5*, 3–30.

Rosch, E. (1973). Natural categories. *Cognitive Psychology, 4*, 328–350.

Shermer, M. (2001). *The borderlands of science: Where sense meets nonsense*. New York: Oxford University Press.

Weiner, I. B., Spielberger, C. D., & Abeles, N. (2002). Scientific psychology and the Rorschach Inkblot Method. *The Clinical Psychologist, 55*, 7–12.

Youngson, R. (1998). *Scientific blunders: A brief history of how wrong scientists can sometimes be*. New York: Carroll & Graf.

THE PSEUDOSCIENCE CONCEPT, DISPENSABLE IN PROFESSIONAL PRACTICE, IS REQUIRED TO EVALUATE RESEARCH PROJECTS: A REPLY TO RICHARD J. MCNALLY

Mario Bunge
McGill University

All a responsible craftsman needs to know about a theory or a method is whether it "works." However, meeting this condition is insufficient to do scientific research, whether basic or applied. The reason is that, since the point of much empirical research is to produce data capable of supporting or undermining the item under scrutiny, such data are not available at the time of evaluating the research project. To accomplish this task, and thus make an intelligent decision concerning the worth and viability of an empirical research project, investigators use some more or less explicit notion of science—or its fake impersonator, pseudoscience. Now, given the complexity of science, it is unlikely that such notion can be characterized by a single attribute, such as confirmability, refutability, explanatory power, or formalizability. Any suitable characterization of science will involve a whole battery of criteria—such as the one proposed earlier by the present author. A handful of examples in several fields are briefly examined. The upshot is that a realistic philosophy of science can pay its way in encouraging promising if initially empirically weak research projects, and in discouraging wasting talent and funds in speculations that exhibit only some of the trappings of genuine science.

The aim of the present paper is to examine the theses of Professor McNally (2003), that the concept of pseudoscience is undefinable, and that it is dispensable anyway, because all we need to know about a theory or a procedure is whether it enjoys empirical support. I claim that both theses are false. Worse yet, they are misleading because, in the absence of an explicit and adequate characterization of science (and its opposite, nonscience), any number of bogus theories and practices may pass through the gates of the citadel of science. Just think of creationist cosmology, "scientific creationism," genetic determinism, psychoanalysis, or the use of inkblots for personality diagnosis and of hypnosis for memory recovery.

Mario Bunge, Frothingham Professor of Logic and Metaphysics, Department of Philosophy, McGill University.

Correspondence concerning this article should be addressed to Mario Bunge, Department of Philosophy, McGill University, 855 Sherbrooke St. W, Montreal, PQ, Canada H3A 2T3.

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True, Karl Popper's equation of scientificity with refutability is inadequate, not only because, as McNally asserts, scientists are anxious to have their views confirmed rather than falsified, but also because a high degree of corroboration is an indicator of truth—though not the only one.

I used these and other arguments in several face-to-face discussions with Popper, 4 decades ago, as well as in a number of publications (e.g., Bunge, 1967, 1973, 1983). And I have defended the use of ad hoc hypotheses of a certain kind to save theories from apparently adverse evidence. These are what I call bona fide ad hoc hypotheses: they are not only fertile but also precise, independently testable, and compatible with the bulk of background knowledge. Well-known historical examples are Harvey's hypothesis of the then-invisible capillaries connecting the ends of arteries with the beginnings of veins, Maxwell's displacement currents, Cajal's neural circuits, and Hebb's cell assemblies.

Obviously, the failure of Popper's definition of sci-

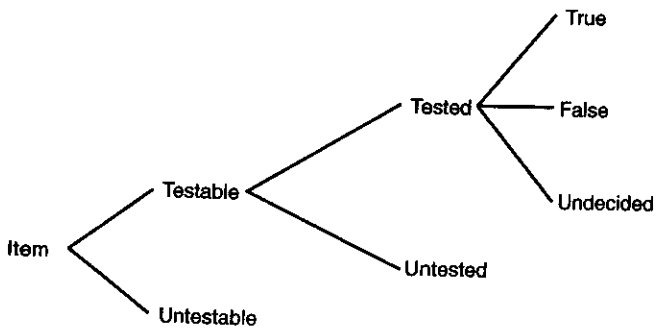
ence does not entail the failure of every attempt to sketch that strange and complex animal, such as my own (e.g., Bunge, 1983), adopted by Lilienfeld (1998) and others. McNally finds it fuzzy, perhaps because he has not consulted the original. For example, my exactness requirement is anything but imprecise, since it consists in demanding that the key concepts of a theory be well defined—unlike, say, the concepts of information in cognitive psychology, and subjective utility in neoclassical microeconomics. And my external consistency requirement is equally transparent, as it consists in the compatibility of the item under examination with the bulk of the antecedent knowledge, in particular the one gained in adjoining research fields—such as neuroscience and sociology in the case of psychology. I have used my characterization of pseudoscience to indict a number of popular theories in physics, astronomy, biology, psychology, and social science (e.g., Bunge, 1962, 1985, 1996, 1998, 1999; Bunge & Ardila, 1987; Mahner & Bunge, 1997). However, my aim here is not to defend my own characterization of science, but to argue for the need of some explicit and refined notion of it.

It may well be that some people, like Richard McNally and Carol Tavris, may know pseudoscience when they see it. But—as McNally himself notes—other scientists are not equally fortunate. For example, some cosmologists are mesmerized by Hawking's speculation on the origin of the universe, although it contradicts all of the well-corroborated conservation laws; many biologists believe Dawkins's genetic determinism, despite the well-known facts that genes are impotent without enzymes and that the availability of the latter at the right moment depends critically upon the state of the environment; many cognitive psychologists claim that all mental processes are algorithmic, even though feelings, emotions and creative processes are anything but rule directed; and thousands of social scientists craft rational-choice models, which include conceptually fuzzy and empirically lame concepts such as those of subjective probability and subjective utility. All these students and their many readers might have benefited from an explicit and demanding definition of the concept of pseudoscience. Besides, since intuition is preanalytic, it is bound to go occasionally wrong, and in any case it cannot be refined. To keep our pseudoscience detector in good shape, we must examine and fine-tune it once in a while.

Without an explicit and somewhat refined concept of scientificity, we cannot distinguish it from those of explanatory power and empirical confirmation, let alone that of factual truth. And the distinction between truth and scientificity is necessary because there are plenty of truths that owe nothing to science, such as "Foxes are hunters" and "Rabbits are the prey to foxes." It is only when we wish to explain and predict with some accuracy the oscillations of the populations of predators and preys that we must set up and solve the corresponding equations.

Likewise, there are plenty of scientific propositions that, not having yet been tested, cannot be said to be either true or false. For example, Maxwell's theory of electromagnetic waves, though certainly scientific—since it was part of his electrodynamic theory, which enjoyed solid empirical support—was experimentally confirmed by Hertz only several years after Maxwell's death. And Einstein's hypothesis of gravitational waves, formulated 8 decades ago, is still in empirical limbo. Is it worth it to go on devising and constructing ingenious and extremely expensive gravity detectors? All theoretical physicists seem to believe so, even though no one doubts that the outcome will be positive, since the hypothesis in question is a component of a theory that has been confirmed by dozens of qualitatively different observations and experiments. In short, that is a scientific hypothesis in search of a long-overdue truth certificate. Hundreds of physicists worldwide are currently working on the hypothesis, and more than $300 million have been spent in the construction of 3 huge detectors of those waves in the United States and Germany (Brumfield, 2002). Obviously, they would not be doing all this if they believed that science is nothing but data gathering and hunting.

In sum, hypotheses and methods, whether or not scientific, can be partitioned into the following classes:



Now, the predicate "is true" (or its technological counterpart "is effective") is applicable to only a subset of all the items in question. These are those that, being testable, have already been put to the test with reasonably certain (positive or negative) results. Hence, we

need a narrower category to include only those items involved in foreseeable and ongoing but unfinished research. This category is that of scientific items, typically hypotheses and methods—precise, scrutable, compatible with the bulk of antecedent knowledge, etc.

Scientists are expected to figure out, work out, or test original scientific hypotheses and methods, not just any outlandish speculations or groundless procedures. And a scientific hypothesis, unlike conjectures of other kinds, is precise rather than vague, empirically testable (confirmable or falsifiable by data, directly or via some theory) rather than inscrutable, and compatible with the bulk of relevant antecedent knowledge rather than in conflict with it. Much the same holds for methods or techniques, except that in this case effectiveness is substituted for truth. For example, intercessory prayer has recently been shown to be medically ineffective (Posner, 2002)—as was to be expected on the strength of the external compatibility criterion.

Before endeavoring to check the truth of a hypothesis, or the validity of a method, a scientist evaluates its potential, for it would be foolish to invest time and resources testing every possible fantasy. The referees who review a research proposal do the same: They, too, check whether the proposal (a) is scientific rather than pseudoscientific, (b) is feasible with the means at hand or proposed, (c) is original, and (d) promises to deliver interesting or practically valuable findings.

Whereas points (b) to (c) require technical expertise, point (a) calls for some methodological sophistication rather than just a good "nose" for detecting hoax or fraud. Regrettably, some current research projects are methodologically naive, hence a waste of time and resources. Here are some examples of contemporary research that I deem pseudoscientific: (a) work on the many-worlds interpretation of quantum mechanics, which postulates the existence of parallel universes inaccessible from ours; (b) the crafting of mathematically sophisticated cosmological models involving not just an initial explosion but also initial nothingness, i.e., creation of matter ex nihilo—as if nothingness could expand; (c) the postulation of mathematically unspecified morphogenetic fields allegedly guiding organ specialization from the outside, rather than as a result of intermolecular and intercellular forces of various kinds; (d) the search for a subcellular (e.g., nanotubular) seat of consciousness, as if mental processes did not involve whole systems of neurons acting synchronically; (e) the search for the adaptive features of incapacitating diseases, such as multiple sclerosis and schizophrenia, predicated by the so-called evolutionary medicine; (f) the design of algorithms producing law statements from data—as if premises could flow unambiguously from conclusions; (g) the mathematical embellishments of neoclassical microeconomics, which ignores time and macroeconomic parameters, hence economic disequilibria; and (h) the crafting of game-theoretic models of political conflict that eschew all the economic and cultural dimensions, and moreover adjust the entries of the payoff matrix so as to obtain the desired result. Whereas some of these projects are pseudoscientific for involving untestable assumptions, others fall into the same category for being hilariously at with variance neighboring fields or with reality.

In conclusion, the practitioner of a craft, such as clinical psychology, maintenance engineering, or management science, may not need a sophisticated philosophy of science because he does not engage in scientific research: He needs to know only whether the ideas he puts into practice have been proved true or efficient. By contrast, the researcher needs an explicit and rather sophisticated philosophy of science, if only to avoid engaging in or supporting pseudoscience—a clear risk given that science is a complex system, some of whose features can easily be mimicked when taken in isolation from the others.

William James might call this the cash value of philosophy of science. An economist might call opportunity cost that incurred by the ignorance of that branch of philosophy. In a few cases, such as that of the National Center for Complementary and Alternative Medicine in the National Institutes of Health, we know this cost: $10 million per year. Is it an exaggeration to guess that the worldwide investment in pseudoscientific research, for lack of clear scientificity criteria, attains several hundred million dollars per year?

REFERENCES

Brumfield, G. (2002). To catch a wave. *Nature, 417*, 482–484.

Bunge, M. (1962). Cosmology and magic. *The Monist, 44*, 116–141.

Bunge, M. (1967). *Scientific research* (2 Vols). New York: Springer-Verlag.

Bunge, M. (1973). *Method, model, and matter.* Boston: D. Reidel [Kluwer].

Bunge, M. (1983). *Treatise on basic philosophy, Vol. 6: Understanding the world.* Boston: D. Reidel [Kluwer].

Bunge, M. 1985. *Treatise on basic philosophy, vol. 7, parts I and II: Philosophy of science and technology.* Boston: D. Reidel [Kluwer].

Bunge, M. (1996). *Finding philosophy in social science*. New Haven CT: Yale University Press.

Bunge, M. (1998). *Social science under debate*. Toronto, ON: University of Toronto Press.

Bunge, M. (1999). *The sociology-philosophy connection*. New Brunswick, NJ: Transaction.

Bunge, M., & Ardila, R. (1987). *Philosophy of psychology*. New York; Springer-Verlag.

Lilienfeld, S. O. (1998). Pseudoscience in contemporary clinical psychology: What it is and what we can do about it. *The Clinical Psychologist, 51*(4), 3–9.

Mahner, M., & Bunge, M. (1997). *Foundations of Biophilosophy*. New York: Springer-Verlag.

McNally, R. J. (2003). The demise of pseudoscience. *The Scientific Review of Mental Health Practice, 2*(2), 97–101.

Posner, G. (2000). Another controversial effort to establish the medical efficacy of intercessory prayer. *The Scientific Review of Alternative Medicine, 4*(1), 15–17.

PSEUDOSCIENCE RESURGENT? A REPLY

Richard J. McNally
Harvard University

The commentators believe that the concept of pseudoscience is useful for criticizing questionable claims and practices in clinical psychology. In contrast, I believe that evidential warrant (or lack thereof) provides a more straightforward means for criticizing them. Attempting to diagnose pseudoscience is an unnecessary and roundabout exercise that ultimately boils down to questions of evidential warrant, anyway. Therefore, rather than asking "Is this claim pseudoscientific?" we can simply ask "What is the evidence for this claim?"

I am grateful for the opportunity to reply to the critiques of my article on the concept of pseudoscience (McNally, 2003). The commentators and I share concerns about certain practices in clinical psychology. In fact, I have cowritten articles with several of them in which we have spelled out these concerns (e.g., Rosen, Lohr, McNally, & Herbert, 1998; Rosen, McNally, et al., 1998).

Given that we agree about so much, what is the basis for our disagreement about pseudoscience? In a nutshell, we agree about strategy but disagree about tactics. We share the same goal but differ regarding the means to achieve it. Our shared goal has been to alert the public and other mental health professionals about problematic practices in clinical psychology. As for tactics, the commentators believe that diagnosing pseudoscience can be an effective means of achieving this goal, whereas I regard this approach as a superfluous distraction from the real issue: the absence of evidential warrant for these practices. Rather than wasting our time trying to determine whether something meets complex criteria for pseudoscience, why not just cut to the chase and scrutinize the relevant evidence? If a clinical claim or practice lacks evidential support, what more do we gain by affixing the label of pseudoscience to it? If evidential support is lacking, why should we care whether it does or does not qualify as pseudoscience? The purpose of my rejoinder is to clarify several issues and to address objections expressed by the commentators.

Contrary to O'Donohue's (2003) reading of my article, I do not believe that only sharply defined concepts are useful in science. Indeed, like O'Donohue, I endorse Wittgenstein's family resemblance approach to defining concepts (see, for example, McNally, 1994, p. 198). Moreover, I agree with Herbert (2003) and Lilienfeld, Lynn, and Lohr (2003) that what counts as evidence is itself often a contentious issue. So, if the concepts of evidential warrant and pseudoscience both lack clear boundaries, why should one prefer the former to the latter as a means of scrutinizing questionable practices in clinical psychology? Apart from the fact that a simple fuzzy concept is preferable to a complex fuzzy one, evaluating evidence is a more straightforward activity than attempting to determine whether something counts as pseudoscience.

Attempts to diagnose pseudoscience are parasitic on examining evidence. Therefore, if deciding whether a practice or claim counts as pseudoscientific ultimately rests on whether it enjoys empirical support, then the diagnostic preliminaries are superfluous. For example,

Richard J. McNally, Department of Psychology, Harvard University.

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Correspondence concerning this article should be addressed to Richard J. McNally, Department of Psychology, Harvard University, 33 Kirkland Street, Cambridge, MA 02138. E-mail: rjm@wjh.harvard.edu

O'Donohue (2003) characterizes pseudoscience as anything that possesses the trappings of science—measurement procedures or neurological terminology, for example—but lacks the substance of science. As O'Donohue concludes, "Calling these claims pseudoscientific denotes that although they have the appearance of science, they lack the substance" (p. 106). But what is this "substance" if not evidential support? If substance is what distinguishes science from pseudoscience, why not streamline our critique by simply focusing on substance (i.e., evidence)?

Also, Lilienfeld et al.'s (2003) distinction between legitimate and illegitimate ad hoc maneuvers ultimately boils down to questions of evidence. According to Lilienfeld et al., an ad hoc adjustment is legitimate if the theory has an established track record of empirical success, but illegitimate (pseudoscientific) if it does not. But if decisions about the legitimacy of an ad hoc adjustment rest on a theory's preexisting empirical support—its track record—then evaluation can proceed on the basis of evidence alone without any need to diagnose pseudoscience as such. Likewise, the connectivity (or lack thereof) of a clinical claim or practice to established theories rests on the evidential basis of these theories.

Two issues raised by Herbert (2003) require clarification. First, contrary to his reading of my article, I do not expect laypersons themselves to sift through the evidence bearing on a clinical claim or practice. As Herbert rightly emphasizes, they are ill-equipped to do so. My point is that psychologists can best educate the public by summarizing the relevant data rather than by denouncing the claims as pseudoscientific. Herbert lists several features of clinical innovations that should incite skepticism among laypersons: claims that seem too good to be true, that are allegedly revolutionary breakthroughs, that cost a lot of money, and so forth. However, none of these features is problematic if the data support the claim. Accordingly, teaching the public to ask "What is the evidence?" is a more straightforward pedagogical approach than teaching the public to diagnose pseudoscience.

Second, contrary to Herbert's (2003) reading of my article, I do not believe "that all claims are equally deserving of empirical investigation" (p. 103). In an article critical of Thought Field Therapy, I suggested, following Kitcher (1982, pp. 166–169), that clinical claims can be assigned to one of three categories:

> The first category includes theories having considerable empirical support. The second includes promising theories that have much less support than their rivals, but are nevertheless capable of explaining certain otherwise puzzling phenomena. Only theories in the first and second categories deserve our attention. The third category includes the residue. These theories have so little support that busy scientists can simply ignore them. (McNally, 2001, p. 1173).

So, Herbert and I actually agree that "not all claims are created equal" (Herbert, 2003, p. 103). Where we differ is that I rely on evidential warrant as the guideline for determining whether certain ones deserve our attention, whereas he believes that distinguishing between science and pseudoscience can help us prioritize our attention and efforts.

Like Herbert, Bunge (2003) believes that diagnosing pseudoscience can help us steer scarce resources away from worthless projects. Bunge says that referees who review research proposals check whether the proposal is scientific rather than pseudoscientific, in addition to determining whether it is feasible, original, and likely to yield practical or theoretical benefits. Having served as a referee on panels reviewing research proposals submitted to the National Institute of Mental Health, I can say that I have never encountered anyone who applied criteria of pseudoscience to evaluate a proposal.

Finally, I was surprised by the scientists whose work is dismissed as pseudoscientific by Bunge here (2003) and elsewhere (1996). Among them are the late psychologist Richard J. Herrnstein, economist Gary S. Becker, biologist Richard Dawkins, and cosmologist Stephen W. Hawking. The work of these men may be flawed in various ways, but tarring it with the pseudoscience label seems pointless and inflammatory. Likewise, Bunge (2003) criticizes certain notions as pseudoscientific merely because they were later refuted by empirical research. For example, some psychologists hypothesized that hypnosis aids memory retrieval. As it turns out, the evidence failed to support this conjecture (see Lynn, Lock, Myers, & Payne, 1997). But the failure of this hypothesis to gain empirical support does not mean that it was pseudoscientific; it merely means that it was wrong.

In conclusion, the best way to debunk bunk in clinical psychology is to examine the relevant evidence. Attempts to diagnose pseudoscience is an unnecessary and roundabout way of achieving the same goal.

REFERENCES

Bunge, M. (1996). In praise of intolerance to charlatanism in academia. In P. R. Gross, N. Levitt, & M. W. Lewis

(Eds.), *The flight from science and reason* (pp. 96–115). New York: New York Academy of Sciences.

Bunge, M. (2003). The concept of pseudoscience, dispensable in professional practice, is required to evaluate research projects: A reply to Richard J. McNally. *The Scientific Review of Mental Health Practice*, 2(2), 111–114.

Herbert, J. D. (2003). The concept of pseudoscience as a pedagogical heuristic. *The Scientific Review of Mental Health Practice*, 2(2), 102–104.

Kitcher, P. (1982). *Abusing science: The case against creationism*. Cambridge, MA: MIT Press.

Lilienfeld, S. O., Lynn, S. J., & Lohr, J. M. (2003). Pseudoscience is alive and well. *The Scientific Review of Mental Health Practice*, 2(2), 107–110.

Lynn, S. J., Lock, T. G., Myers, B., & Payne, D. G. (1997). Recalling the unrecallable: Should hypnosis be used to recover memories in psychotherapy? *Current Directions in Psychological Science*, 6, 79–83.

McNally, R. J. (1994). *Panic disorder: A critical analysis*. New York: Guilford Press.

McNally, R. J. (2001). Tertullian's motto and Callahan's method. *Journal of Clinical Psychology*, 57, 1171–1174.

McNally, R. J. (2003). The demise of pseudoscience. *The Scientific Review of Mental Health Practice*, 2(2), 97–101.

O'Donohue, W. (2003). Pseudoscience is a legitimate construct. *Scientific Review of Mental Health Practice*, 2(2), 105–106.

Rosen, G. M., Lohr, J. M., McNally, R. J., & Herbert, J. D. (1998). Power Therapies, miraculous claims, and the cures that fail. *Behavioural and Cognitive Psychotherapy*, 26, 97–99.

Rosen, G. M., McNally, R. J., Lohr, J. M., Devilly, G. J., Herbert, J. D., & Lilienfeld, S. O. (1998, October). A realistic appraisal of EMDR. *California Psychologist*, 31, 25, 27.