CHAPTER 12

THE RODNEY DANGERFIELD
OF THE SCIENCES

Although there is a great public fascination with psychological topics, most judgments about the field and its accomplishments are resoundingly negative. Psychologists are aware of this image problem, but most feel that there is little they can do about it, so they simply ignore it. This is a mistake. As the mass media become more and more influential in determining public perceptions (for example, fictional TV “docudramas” become the true history for a public that does not read), ignoring psychology’s image problem threatens to make it worse.

Rodney Dangerfield is a popular comedian whose trademark is the plaintive cry, “I don’t get no respect!” In a way, this is a fitting summary of psychology’s status in the public mind. This chapter will touch on some of the reasons that psychology appears to be the Rodney Dangerfield of the sciences.

PSYCHOLOGY’S IMAGE PROBLEM

Some of the reasons for psychology’s image problem have already been discussed. For example, the Freud problem discussed in Chapter 1 undoubtedly contributes to the low esteem in which psychology is held. To the extent that the public knows about any reputable psychologists at all, Freud and Skinner are those psychologists. The distorted versions of their ideas that circulate among the public must contribute to the idea that psychology is a frivolous field indeed. There would appear to be little hope for a field when one of its most renowned scholars is said to have claimed that we have no minds and that we are just like rats. Of course, Skinner did not deny that we think, and many principles of operant conditioning that he developed from work with animals have been shown to generalize to human behavior. However, the public is little aware of any of these facts. Distorted ideas from Freudian doctrine also contribute to lowering the public esteem for psychology.
Psychology and Parapsychology

The layperson's knowledge of reputable psychological research, outside of the work of Freud and Skinner, is virtually nonexistent. One way to confirm this fact is to look in your local bookstore to see what material on psychology is available to the general public. Inspection will reveal that the material generally falls into three categories. First, there are a few classics (Freud, Skinner, Jung, Fromm, Erickson, etc.) heavily biased toward old-style psychoanalytic views that are totally unrepresentative of modern psychology.

The second class of material found in most stores might be called pseudoscience masquerading as psychology, that is, the seemingly never-ending list of so-called paranormal phenomena such as telepathy, clairvoyance, psychokinesis, precognition, reincarnation, biorhythms, astral projection, pyramid power, plant communication, and psychic surgery (Lilienfeld, Lohr, & Moirer, 2001). The presence of a great body of this material in the psychology sections of bookstores undoubtedly contributes to, and also reflects, the widespread misconception that psychologists are the people who have confirmed the existence of such phenomena. There is a bitter irony for psychology in this misconception. In fact, the relationship between psychology and the paranormal is easily stated. These phenomena are simply not an area of active research interest in modern psychology. The reason, however, is a surprise to many people.

The statement that the study of ESP and other paranormal abilities is not accepted as part of the discipline of psychology will undoubtedly provoke the ire of many readers. Surveys have consistently shown that more than 50 percent of the general public believes in the existence of such phenomena and often holds these beliefs with considerable fervor (National Science Board, 2001). Historical studies and survey research have suggested why these beliefs are held so strongly (Alcock, 1987; Bainbridge & Stark, 1980; Grimmer, 1992; Stanovich, 1989, 2004). A materialist culture tends to weaken the traditional religious beliefs of many people, who then seek some other form of transcendental outlet. Like most religions, many of the so-called paranormal phenomena seem to promise things such as life after death, and for some people, they serve the same need for transcendence. It should not be surprising, then, that the bearer of the bad tidings that research in psychology does not validate ESP is usually not greeted with enthusiasm. The statement that psychology does not consider ESP a viable research area invariably upsets believers and often provokes charges that psychologists are dogmatic in banishing certain topics from their discipline. Psychologists do not contribute to public understanding when they throw up their hands and fail to deal seriously with these objections. Instead, psychologists should give a careful and clear explanation of why such objections are ill founded. Such an explanation follows.

Scientists do not determine by edict which topics to investigate. No proclamation goes out declaring what can and cannot be studied. Areas of investigation arise and are expanded or terminated according to a natural se-
lection process that operates on ideas and methods. Those that lead to fruitful theories and empirical discoveries are taken up by a large number of scientists. Those that lead to theoretical dead ends or that do not yield replicable or interesting observations are dropped. This natural selection of ideas and methods is what leads science closer to the truth.

The reason that ESP, for example, is not considered a viable topic in contemporary psychology is simply that its investigation has not proved fruitful. Therefore, very few psychologists are interested in it. It is important here to emphasize the word contemporary, because the topic of ESP was of greater interest to psychologists some years ago, before the current bulk of negative evidence had accumulated. As history shows, research areas are not declared invalid by governing authorities; they are merely winnowed out in the competing environment of ideas.

ESP was never declared an invalid topic in psychology. The evidence of this fact is clear and publicly available (Alcock, 1990; Druckman & Swets, 1988; Hooft, 2000; Hyman, 1996; Marks, 2001; Milton & Wiseman, 1999). Many papers investigating ESP have appeared in legitimate psychological journals over the years. Parapsychologists who thrive on media exposure like to give the impression that the area is somehow new, thus implying that startling new discoveries are just around the corner. The truth is much less exciting.

The study of ESP is actually as old as psychology itself. It is not a new area of investigation. It has been as well studied as many of the currently viable topics in the psychological literature. The results of the many studies that have appeared in legitimate psychological journals have been overwhelmingly negative. After more than 90 years of study, there still does not exist one example of an ESP phenomenon that is replicable under controlled conditions. This simple but basic scientific criterion has not been met despite dozens of studies conducted over many decades. Many parapsychologists and believers themselves are even in agreement on this point (see Alcock, 1981, 1990; Druckman & Swets, 1988; Krippner, 1977). For this reason alone, the topic is now of little interest to psychology.

It is sometimes mistakenly suggested that scientists deny the existence of such phenomena because they violate currently accepted theories of nature. From our discussion of the scientific process in Chapters 1 and 2, it should be clear that this claim is false. All scientists are in the business of overturning currently accepted theories of nature, for it is only by changing and refining our current views, while maintaining, of course, the connectivity principle, that we can hope to get nearer the truth. When a new phenomenon contradicts currently accepted knowledge, scientists question it and seek alternative explanations for it. But this is not the reason that psychologists do not believe in the existence of ESP. The reason is simpler. There is just no scientific evidence for it. In short, there is no demonstrated phenomenon that needs scientific explanation (see Alcock, 1981, 1984, 1990; Hines, 1988; Humphrey, 1996; Hyman, 1992, 1996; Milton & Wiseman, 1999).
And now the irony. Psychologists have played a prominent role in attempts to assess claims of paranormal abilities. The importance of their contribution is probably second only to that of the professional magicians, who have clearly done the most to expose the fraudulent nature of most purported demonstrations of paranormal abilities (Randi, 1986, 1987). Many of the most important books on the state of the evidence on paranormal abilities have been written by psychologists.

The irony, then, is obvious. Psychology, the discipline that has probably contributed most to the accurate assessment of ESP claims, is the field that is most closely associated with such pseudosciences in the public mind. Psychology suffers greatly from this guilt-by-association phenomenon. As will be discussed in greater detail later, psychology is often the victim of a “double whammy.” Here is just one example. The assumption that anything goes in psychology, that it is a field without scientific mechanisms for deciding among knowledge claims, leads to its being associated with pseudosciences such as ESP. However, if psychologists ever become successful in getting the public to recognize these pseudosciences for what they really are, the pseudosciences’ association with psychology will be seen as confirmation that psychology is indeed not a science!

The Self-Help Literature

The third category in the bookstore psychology section is the so-called self-help literature. There are, of course, many different genres within this category (see Fried, 1994, 1998; Fried & Schultis, 1995; Paul, 2001; Santrock, Minnett, & Campbell, 1994). Some books are spiritually uplifting tracts written with the purpose of generally increasing feelings of self-worth and competence. Others attempt to package familiar bromides about human behavior in new ways. A few (but all too few) are authored by responsible psychologists writing for the general public. Many that are not in the latter category vie for uniqueness by presenting new “therapies” that are usually designed not only to correct specific behavioral problems but also to help satisfy general human wants (making more money, losing more weight, and having better sex are the “big three”), thereby ensuring larger book sales. These so-called new therapies are rarely based on any type of controlled experimental investigation. They usually rest on personal experience or on a few case histories, if the author is a clinician.

The many behavioral and cognitive therapies that have emerged after painstaking psychological investigation as having demonstrated effectiveness are usually poorly represented on the bookshelves. The situation is even worse in the electronic media. Radio and TV carry virtually no reports of legitimate psychology and instead present purveyors of bogus “therapies” and publicity-seeking media personalities who have no connection to the actual field of psychology. The main reason is that the legitimate psychological therapies do not claim to provide an instant cure or improvement, nor do they
guarantee success or claim a vast generality for their effects ("Not only will you quit smoking, but every aspect of your life will improve!"). Similarly with the Internet—the lack of peer review ensures that the therapies and cures that one finds there are often bogus.

The public is largely unaware that the publishing industry provides no quality controls for readers of the self-help literature. Fridson (1993) described the case of a writer of pop psychology who wrote best-selling books on careers. The writer had a column in *Forbes* magazine and appeared on television programs. It was then discovered that the writer did not have the Ph.D. degree that he claimed to have (he actually had a doctorate from an unaccredited correspondence school). Further investigation revealed that some of the 8,000 interviews that the author claimed to have conducted were fictitious. When questioned about the ethics of marketing misleading books by someone with bogus credentials, one of this author’s publishers said that “ninety-nine percent of what we do is based on faith” (p. 144). The reader who has digested this book will now have a valuable supplement to faith: knowledge of the logic of the empirical methods of modern science.

The self-help literature, which accounts for a substantial portion of the book market in the United States, has many unfortunate effects on the general perception of psychology. First, like the Freud problem, it creates confusion concerning the problems that dominate the attention of psychologists. For example, although a substantial number of psychologists are engaged in providing therapy for problems of obesity, of relationships, and of sexuality and also in researching these problems, the actual number is far less than that suggested by their representation in the self-help literature. This misrepresentation also contributes to the public’s view that most psychologists are engaged in the treatment of and research on abnormal behavior. In fact, most psychological research is directed at nonpathological behavior that is typical of all humans.

Beyond the content confusion, the self-help literature creates an inaccurate impression of the methods and goals of psychology. As we showed in Chapter 4, the science of psychology does not consider a few case studies, testimonials, and personal experiences—which are the database for most of the self-help “therapies”—adequate empirical evidence to support the efficacy of a therapy. The self-help literature misleads the public by implying that this is the type of database on which most psychological conclusions rest. As illustrated in Chapter 8, the confirmation of a theory must rest on many different types of evidence, and case studies yield the weakest type of data. It is a fundamental mistake to view such data as definitive proof of a particular theory or therapy.

**Recipe Knowledge**

Finally, the self-help literature creates confusion about the goals of psychology and about the type of knowledge that most psychological investigations seek. Psychologist Leigh Shaffer (1981) suggested that this literature strongly
implies that psychological researchers seek what has been termed *recipe knowledge*. Recipe knowledge is the knowledge of how to use something without knowledge of the fundamental principles that govern its functioning. For example, most people know many things about how to use a telephone. They know how to dial, how to get information, how to make long-distance connections, and so on. But many are completely ignorant of the physical principles on which the operation of the telephone is based. They do not know how it does what it does; they only know that they can make it work. This is recipe knowledge of the telephone. Our knowledge of many technological products in our society is also recipe knowledge.

Of course, this is not an entirely bad thing. Indeed, most technological products have been designed to be used without knowledge of all the principles that make them work. In fact, the idea of recipe knowledge provides one way of conceptualizing the difference between basic and applied research. The basic researcher seeks to uncover the fundamental principles of nature without necessarily worrying about whether they can be turned into recipe knowledge. The applied researcher is more interested in translating basic principles into a product that requires only recipe knowledge.

Most self-help literature provides only recipe knowledge about human behavior. It usually boils down to the form “Do X and you will become more Y,” or “Do Z and person A will react more B.” Now, there is nothing inherently wrong here, assuming, of course, that the recipes provided are correct (which is usually not a safe assumption). Many legitimate psychotherapies also provide much recipe knowledge. However, a problem arises when people mistakenly view recipe knowledge as the ultimate goal of all psychological research. Although a number of psychological researchers do work on turning basic behavioral principles into usable psychotherapeutic techniques, health-maintaining behavior programs, or models of efficient industrial organization, psychological research is largely basic research aimed at uncovering general facts and theories about behavior. Here we have another reason why psychological research may seem strange to the outsider. Investigations of basic principles often look very different from studies focused on developing applications.

We would consider it silly to walk into a molecular biology laboratory and ask a researcher whether we should take two or three aspirins for a headache. The reason is not that molecular biology has nothing to do with pain relief. Future developments in pain relievers will probably involve knowledge from this area of science. It is silly to ask this question because the molecular biologist is simply not working at the recipe level that deals with whether to take two aspirins or three. The researcher is concerned with fundamental facts about the molecular level of biological substances. These facts could lead to recipe knowledge in any number of areas, but the transformation to recipe knowledge will probably not be accomplished by the same investigator who uncovered the basic facts at the molecular level, nor
will it be accomplished by use of the same methods that led to the original discoveries.

Thus, because the self-help literature has led people to believe that most psychologists work at developing recipe knowledge, much of the basic research that psychologists conduct appears strange. What did Hecht’s data about subjects looking at red lights in a dark room have to do with anything in the real world? Well, on the surface, nothing. Hecht was interested in uncovering basic laws about the way the visual system adapts to darkness. The basic principles were eventually translated into recipe knowledge of how to deal with some specific problems, such as night blindness due to vitamin deficiency. However, this translation was not done by Hecht himself, and it did not come until several years later.

Thus, the self-help literature has two unfortunate side effects on the public perception of psychology. The range of problems addressed in this literature does not necessarily represent the focus of contemporary psychology. Instead, it reflects, quite naturally, what people want to read about. Students of psychology are often not sufficiently aware that publishing is a commercial endeavor. Market forces determine what appears on the bookshelves. However, the focus of science is not determined in the same way. In all sciences, and in psychology in particular, there is usually a gap between the ideas that are productive for scientists and those that can be packaged to sell to the public.

Finally, the self-help literature presents psychology as purely recipe knowledge. Although this is not entirely inaccurate, it does not reflect the large amount of basic research that goes on in psychology.

PSYCHOLOGY AND THE MEDIA

Some unfortunate consequences arise from the fact that most psychologists are well aware of their discipline’s image problem. Here we can see another example of the double whammies that plague the field. Many psychologists are very concerned about disassociating their field from the self-help books and pseudosciences that partially define it for the public. This concern has made many psychologists extremely wary about drawing firm conclusions regarding solutions to pressing human problems. This reluctance to claim special knowledge is virtually built into the training of research psychologists in most graduate schools. Research psychology is one of the most self-critical fields. The psychology journals are strewn with papers warning about all the critical methodological issues we have covered in previous chapters (the trap of inferring causation from correlation, the need for converging evidence, the need to test alternative explanations, and so on).

Thus, conservatism regarding the communication of psychological findings is deeply ingrained in most psychological researchers. Psychologists are, therefore, quite reluctant to claim that they have the answers to pressing social
problems. This reluctance is, of course, often well advised. The problems surrounding human behavior are complex, and it is not easy to study them. However, some unfortunate side effects result when psychology interacts with the media.

The peculiar logic of the media dictates that if the public is interested in a particular psychological question, the media will deliver a story whether or not there is one to tell. A scientist who tells a reporter, “I’m sorry, but that is a complex question, and the data are not yet in, so I wouldn’t want to comment on it,” by no means terminates the reporter’s search for an answer. The investigator will simply continue until he or she finds a scientist (or, often, in the case of psychology, anyone who can be quoted as an “authority”) who is less conservative about coming to conclusions (Murray, Schwartz, & Lichter, 2001; Ruscio, 2000).

Kelly, Rotton, and Culver (1985) discussed how this unfortunate “media logic” has worked to publicize so-called lunar effects, that is, the belief that the phases of the moon can affect human behavior, especially abnormal behavior. These researchers analyzed the results of 37 different studies and found that there was no evidence of lunar effects on human behavior. They found instead that it would make more sense to be studying “media effects,” that is, how the media can create a belief in nonexistent phenomena. Kelly et al. pointed directly to the media logic we have just discussed:

Newspapers, television programs, and radio shows favor individuals who claim that a full moon influences behavior . . . When reporters call us on the phone, they would probably be happier if we assure them by saying “The streets are full of loonies when the moon is full.” Unfortunately, when one scientist doesn’t give them a quotation that can be turned into an interesting headline, they can always find an “expert” who will provide the quotation they need. (p. 133)

Journalism professor Curtis MacDougall, in his book Superstition and the Press (1983), gave an example of media logic when he quoted a reporter who routinely wrote stories about psychic powers. When asked whether he actually thought that there was evidence indicating that such powers existed, the reporter replied, “I don’t have to believe in it. All I need is 2 PhDs who will tell me it’s so and I have a story” (p. 558). This example illustrates why reliance on peer-reviewed journals for scientific information has been emphasized in this book. It is the only protection that the consumer of scientific information has. Television talk shows, for example, contain no such protection. They work on exactly the logic in the MacDougall quote: If someone will say it, and people are interested in it, then it gets on television. Proof, truth, evidence, logic, justification, data, and so on have absolutely nothing to do with television talk shows, which are for entertainment, period. For information, look elsewhere.
The media selection process that presents science to the public has the following logic: Scientists who are cautious about stating opinions are rarely quoted. Only those more willing to go out on a limb become public figures. Again, this is not always a bad thing. For example, the late astronomer and television personality Carl Sagan sometimes went a little too far in his speculations in the opinion of his more conservative colleagues (Poundstone, 1999), but his vast contributions to the public understanding of astronomy undoubtedly more than compensated for any minor inaccuracies that he may have conveyed.

The situation in psychology, however, is entirely different. Most media psychologists, unlike Sagan, have absolutely no standing among researchers in their field. Psychology is so different due to a combination of factors. People want the answers to questions concerning human beings much more than they want the answers to questions about other aspects of nature. People want to know how to lose weight, whether psychological therapies actually work, or how to increase the academic achievement of their children much more than they want to know what is the composition of the rings of Saturn or whether a black hole in space is really possible. Combine this urgency with the reality that the answers to these complex questions are harder to come by, and psychology’s problem becomes clear. In other disciplines, the media selection process weeds out more conservative scientists and replaces them with scientists who are a little looser with their conclusions. Unfortunately, in psychology, the scientists are often weeded out altogether!

The scientifically justified conservatism of psychologists when faced by media representatives creates a void because a tentative statement does not make a story. But justified or not, the void does not remain a void for long. Into it rush all the self-help gurus and psychic charlatans who, bursting forth on TV and radio talk shows, become associated with psychology in the public mind. In short, the conservatism backfires. By exercising proper scientific caution in presenting the results of research to the public, psychology helps to create an image for itself that subsequently leads to its devaluation by both the public and other scientists.

An article in the APA Monitor (Azar, 1998) reported the statistic that the New York Times named a scientific source for its articles on the natural sciences 51.4 percent of the time, whereas a scientific source was given only 14.3 percent of the time for social science stories. Likewise, psychologist Bob Cialdini (1997) cited a study in which 80 percent of the time newspapers and news broadcasts referred to the authors of natural science studies as scientists, whereas this was true only 20 percent of the time for the authors of social science studies. Instead, 80 percent of the time authors of the latter type of study were referred to as writers or authors—thus, de-emphasizing the scientific basis of the study.

Sometimes, when reporting on “psychological” topics, the media dispense with even the pretense of scientific validity. Consider an article in the
June 20, 1994, issue of *Newsweek*. After spending one column hyping a new therapy, “eye movement desensitization and reprocessing” quoting another news magazine that called the therapy “a miracle cure,” the *Newsweek* article stated, “Unfortunately, the excitement is based largely on testimonials [see Chapter 4]. Proponents have yet to show scientifically that EMDR has unique advantages over other forms of therapy” (Cowley & Biddle, 1994, p. 70). Then what, one may ask, is the rationale for publishing this story? To emphasize how bad the media problem is for psychology, we need only ponder the source of the report of this unvalidated therapy. *Newsweek* magazine is not a supermarket tabloid. One may rightfully ask: *What possible purpose is served by printing a story on an unvalidated therapy in a national magazine that will be read by millions of people*, many of whom are desperately seeking cures for a variety of maladies? Two years after the *Newsweek* article appeared, psychologist Scott Lilienfeld (1996) reviewed the evidence on the EMDR treatment. Most of the evidence consists of uncontrolled case reports of effectiveness (see Chapter 4). The experimental studies with proper control groups show no indication that this treatment is effective.

**PSYCHOLOGY AND OTHER DISCIPLINES**

Psychology, of course, does not have a monopoly on studying behavior. Many other allied disciplines, using a variety of different techniques and theoretical perspectives, also contribute to our knowledge. Many problems concerning behavior call for an interdisciplinary approach. However, a frustrating fact that most psychologists must live with is that when work on an interdisciplinary problem is publicized, the contributions of psychologists are often usurped by other fields.

There are many examples of scientific contributions by psychologists that have been ignored, minimized, or partially attributed to other disciplines. For instance, the first major survey of the evidence on television’s effects on children’s behavior was conducted under the aegis of the U.S. surgeon general, so it is not surprising that the American Medical Association (AMA) passed a resolution to reaffirm the survey’s findings of a suggested causal link and to bring the conclusions more publicity. Again, there is nothing wrong here, but an unintended consequence of the repeated association of the findings on televised violence with the AMA is that it has undoubtedly created the impression that the medical profession conducted the scientific research that established the results. In fact, the vast majority of the research studies on the effects of television violence on children’s behavior were conducted by psychologists.

One of the reasons that the work of psychologists is often ascribed to other disciplines is that the word *psychologist* has, over the years, become ambiguous. Many research psychologists commonly append their research spe-
cialty to the word *psychologist* when labeling themselves, calling themselves, for example, physiological psychologists, cognitive psychologists, industrial psychologists, or neuropsychologists. Some use a label that does not contain a derivative of the word *psychology* at all, for example, neuroscientist, cognitive scientist, sociobiologist, artificial intelligence specialist, and ethologist. Both of these practices—in conjunction with the media’s bias that “psychology isn’t a science”—lead to the misattribution of the accomplishments of psychologists: The work of physiological psychologists is attributed to biology, the work of cognitive psychologists is attributed to computer science, the work of industrial psychologists is attributed to engineering and business, and so on. Psychology won’t be helped by the fact that one of its most brilliant contemporary researchers, Daniel Kahneman, received the 2002 Nobel Prize in economics! Of course, no Nobel Prize is given in psychology (MacCoun, 2002).

Psychologist Frederick King (1993), the director of the Yerkes Primate Research Center at Emory University, told of taking time to explain to a reporter the importance of animal models in the study of human neurological disorders. After listening to the long explanation by King, who had contributed for years to the research literature on the neurological and behavioral problems of epilepsy, the reporter asked, “How do you know anything about epilepsy? You’re just a psychologist.”

In the late 1970s, several cases involving the use of standardized tests were adjudicated in the courts. One such case, *PASE vs. Hannon*, involved the issue of cultural bias in intelligence tests. The judge in the case felt that the only way to arrive at a decision was to inspect each test item himself and to trust his own intuition. He had no reservations about his ability to make an accurate judgment and, in his legal opinion, cited his own view of each question on the tests involved (Bersoff, 1981, 1982). He concluded that eight items in one test and a single item in another may have been biased. The judge did not realize that the issue in question is an empirical one that can be answered by use of the scientific method. Personal opinion is not only irrelevant but may be extremely misleading. The determination of bias in test items involves complex statistical procedures and extensive data collection. Psychologists have been prominent in collecting the necessary data and developing the necessary statistical techniques for their evaluation.

Ironically, given the judge’s action, research has in fact revealed that the layperson’s intuitive judgment about which items are culturally biased is often markedly inaccurate. Many items that are judged to be fair are in fact biased in various ways, and many that are thought to be unfair are actually statistically unbiased (Sandoval & Miille, 1980). For example, the Wechsler Adult Intelligence Scale has been criticized in Canada because some of the items on one of its subscales (Information) clearly seem to be biased in favor of U.S. citizens. One item, for instance, asks the respondent to name four men who have been president of the United States since 1950. Thus, some of the
items have been “Canadianized” for administration in Canada (Violato, 1984, 1986). The “presidents” item, for example, was changed to “Name four men who have been prime minister of Canada since 1900.” However, there was one little problem with this obvious, “commonsense” change: Canadian citizens do better on the presidents version than on the prime ministers version.

OUR OWN WORST ENEMIES

Lest it appear that we are blaming everyone else for psychology’s image problems, it is about time that we acknowledge the contribution of psychologists themselves to confusion about their field. There are very few rewards for the legitimate psychologist who tries to communicate actual psychology to the public. However, the American Psychological Association and the American Psychological Society are making more efforts to facilitate public communication. The APS has started a new journal for this purpose: Psychological Science in the Public Interest. Psychology needs to make much more of an effort in this area. Otherwise, we will have only ourselves to blame for the misunderstanding of our discipline.

Past APA president Ronald Fox (1996) spoke of psychology’s communication problems in a recent presidential address and how we have brought some of these communication problems on ourselves:

Some practitioners who are appearing in the mass media are behaving in ways that are unprofessional, marginally ethical at best, and downright embarrassing to a majority of their peers. . . . Our discipline lacks effective measures for responding to irresponsible and outrageous public claims. . . . Too often in today’s world, the public is treated to the views and opinions of charlatans (as observed on a recent TV talk show in which a psychologist claimed to have helped dozens of patients remember traumas suffered in past lives), rather than rational practitioners. (pp. 779–780)

And, finally, there is the phenomenon of antiscientific attitudes within parts of psychology itself (Coan, 1997; Watters & Ofshe, 1999). For example, some groups of psychotherapists have traditionally resisted scientific evaluations of their treatments. Columnist and psychotherapist Charles Krauthammer (1985) wrote of how this attitude presents a serious threat to the integrity of psychotherapy. First, there is the proliferation of therapies that has occurred because of a reluctance to winnow out those that do not work. Such a proliferation not only removes a critical consumer protection but also promotes confusion in the field: “Psychotherapy has come upon this state of confusion because . . . it permits too few deaths among its schools. It is incapable of killing its own. Psychotherapy is dying of dilution.” Krauthammer was here lamenting how the failure to use the falsification strategy stymies scientific progress.
Finally, Krauthammer pointed to the inconsistency of a therapeutic community that, on one hand, argues against scientific evaluation because it is “more art than science,” in the common phrase, but is still greatly concerned about what he called the 800-pound gorilla: reimbursement for services by government and private health insurers. Krauthammer exposed the inconsistency of these attitudes within the psychotherapy community: “As long as psychotherapies resist pressure to produce scientific evidence that they work, the economic squeeze will tighten. After all, if psychotherapy is really an art, it should be supported by the National Endowment for the Humanities, not by Medicare.” Consistent with this sentiment, in their review of psychotherapy outcome research Kopta, Lueger, Saunders, & Howard (1999) argued, “The effectiveness of specific psychological treatments must be empirically validated to justify reimbursement by insurance and managed care companies and by government agencies that are demanding more accountability” (p. 442).

Noted clinician Don Peterson (1995) also concurred with Krauthammer’s warning. He says bluntly that clinicians who argue that “the empirical evidence is all negative, but my experience tells me otherwise, so I refuse to change my practice” (p. 977) [will] “get no sympathy from me” (p. 977). In short, Peterson argued that clinical psychologists must be responsive to scientific evidence or risk being seen as “an irresponsible guild” (p. 977) and losing society’s support.

Some readers of the first few editions editions of this book commented that they thought I had “let psychologists get off too easily” by not emphasizing more strongly that unprofessional behavior and antiscientific attitudes among psychologists themselves contribute greatly to the discipline’s image problem. My task of providing more balance on this point was made easier by the publication, in 1994, of Robyn Dawes’s House of Cards: Psychology and Psychotherapy Built on Myth. If anyone doubts that psychologists themselves have contributed greatly to the field’s dilemmas, they need only read this book. In this courageous work, Dawes did not hesitate to air psychology’s dirty linen and, at the same time, to argue that the scientific attitude toward human problems that is at the heart of the true discipline of psychology is of great utility to society (although its potential is still largely untapped). For example, Dawes argued that “there really is a science of psychology that has been developed with much work by many people over many years, but it is being increasingly ignored, derogated, and contradicted by the behavior of professionals—who, of course, give lip service to its existence” (p. vii).

What Dawes (and many others, see Dineen, 1996; Lilienfeld, 1998; Lilienfeld et al., 2000; Mook, 2001; Watters & Ofshe, 1999) objected to is the historical trend of the field to become more concerned about so-called guild issues, such as licensure, than about scientific issues—a trend observable in the behavior of its oldest organization, the American Psychological Association. In principle, there is nothing wrong with imposing licensure requirements that protect the unique expertise that the study of psychology
conveys. But the field does seem to have lost sight of what is unique about it. Recall our discussion at the very beginning of this book. The unique aspect of the field of psychology is that it brings the tools of the scientific method to the study of behavior. In contrast, the study of psychology does not confer any specific “intuitive” powers to “read people.” Indeed, there has been voluminous research on just this point. Recall the discussion of actuarial versus clinical prediction in the previous chapter. The research of 40 years on this topic has consistently indicated that, in predicting just about any relevant psychological outcome, a quantification of relevant variables makes better predictions of human behavior than do trained clinicians (Dawes et al., 1989; Faust et al., 1988; Goldberg, 1959, 1968, 1991; Meehl, 1954; Sawyer, 1966). Furthermore, even in the realm of psychotherapeutic outcome itself (rather than the prediction of outcomes), research evidence indicates that neither the training leading to licensure as a psychologist nor years of experience as a clinical psychologist lead to a better therapeutic outcome. Much research has been conducted on this issue, and it consistently indicates that the therapeutic results of nonpsychologist practitioners (e.g., social work professionals) are equal to those of licensed clinical psychologists (Landman & Dawes, 1982; Smith et al., 1980). Indeed, the results of completely uncredentialed paraprofessionals are equal to those of licensed clinical psychologists (Berman & Norton, 1985; Christensen & Jacobson, 1994). Finally, there is no evidence that experience in psychotherapy leads to better outcomes. Years of experience as a psychotherapist are uncorrelated with therapeutic outcome (Christensen & Jacobson, 1994; Landman & Dawes, 1982; Smith et al., 1980).

If the field of psychology were true to its principles of relying on empirical evidence to guide its actions, then it would be publicizing such findings and, thus, helping the nation to find lower-cost ways to meet its mental health needs. As Dawes (1994) argued:

These results have very strong implications for public policy in the mental health area. We should not be pouring out resources and money to support high-priced people who do not help others better than those with far less training would, and whose judgments and predictions are actually worse than the simplest statistical conclusion based on “obvious” variables. Instead, we should take seriously the findings that the effectiveness of therapy is unrelated to the training or credentials of the therapist. We should take seriously the findings that the best predictors of future behavior are past behavior and performance on carefully standardized tests, not responses to inkblot tests or impressions gained in interviews, even though no prediction is as good as we might wish it to be. The conclusion is that in attempting to alleviate psychological suffering, we should rely much more than we do on scientifically sound, community-based programs and on “paraprofessionals,” who can have extensive contact with those suffering at no greater expense than is currently incurred by paying those claiming to be experts. (p. 5)
Instead of following this course, the field of psychology justifies licensure requirements based on the scientific status of psychology and then uses licensure to protect the unscientific behavior of psychological practitioners. For example, one thing that a well-trained psychologist should know is that we can be reasonably confident only in aggregate predictions. In contrast, predicting the behavior of particular individuals is fraught with uncertainty (see Chapters 10 and 11) and is something no competent psychologist should attempt without the strongest of caveats, if at all. As Dawes (1994) noted:

A mental health expert who expresses a confident opinion about the probable future behavior of a single individual (for example, to engage in violent acts) is by definition incompetent, because the research has demonstrated that neither a mental health expert nor anyone else can make such a prediction with accuracy sufficient to warrant much confidence. (Professionals often state that their professional role “requires” them to make such judgments, however much they personally appreciate the uncertainty involved. No, they are not required—they volunteer.) (p. vii)

In short, the American Psychological Association has fostered an ethos surrounding clinical psychology that suggests that psychologists can be trained to acquire an “intuitive insight” into the behavior of individual people that the research evidence does not support. When pushed to defend licensure requirements as anything more than restraint of trade, however, the organization uses its scientific credentials as a weapon (one president of the APA, defending the organization from attack, said “Our scientific base is what sets us apart from the social workers, the counselors, and the Gypsies”; Dawes, 1994, p. 21). But the very methods that the field holds up to justify its scientific status have revealed that the implication that licensed psychologists have a unique “clinical insight” is false. It is such intellectual duplicity on the part of the APA that spawned Dawes’s book and that in part led to the formation of the American Psychological Society in the 1980s by psychologists tired of an APA that was more concerned about Blue Cross payments than with science.

Scott Lilienfeld (1998), the winner of the David Shakow Award for early career contributions to clinical psychology, reiterated all of these points in his award acceptance speech, warning that “we in clinical psychology seem to have shown surprisingly little interest in doing much about the problem of pseudoscience that has been festering in our own backyards” (p. 3). Lilienfeld (1998) listed several categories of pseudosciences that have flourished in clinical psychology during the 1990s, including:

1. Unvalidated and bizarre treatments for trauma
2. Demonstrably ineffective treatments for autism such as facilitated communication (see Chapter 6)
3. The continued use of inadequately validated assessment instruments (e.g., many projective tests)
4. Subliminal self-help tapes
5. Use of highly suggestive therapeutic techniques to unearth memories of child abuse

Lilienfeld quoted noted clinical researcher Paul Meehl’s (1993), “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” (p. 728; see also, Mahrer, 2000). In the same essay, Meehl warned that if clinical psychology does not adopt a thoroughly scientific stance, clinicians risk becoming little more than “well-paid soothsayers.” Meehl was here referring to the tendency—discussed in Chapter 11—for clinicians to imply, contrary to the empirical evidence, that they have “special” knowledge of people that goes beyond general behavioral trends that are publicly available as replicable scientific knowledge. Arguing that the clinical psychologist must, if anything, be more concerned that knowledge be empirically and publicly verified, Meehl (1993) warns that “it is absurd, as well as arrogant, to pretend that acquiring a PhD somehow immunizes me from the errors of sampling, perception, recording, retention, retrieval, and inference to which the human mind is subject” (p. 728).

Psychologist John Perez (1999) argued that the points raised by Dawes, Lilienfeld, and Meehl really amount to the argument that the advocacy efforts of clinical psychology should be in the aid of clients rather than in aid of the rights of clinicians to carry on with whatever treatments they want. As Perez (1999) put it, “We must decide if we want to foster an environment in which clinicians can practice whatever they want, even in the absence of scientific evidence that what they practice actually works. Conversely, we may choose to protect the rights of clients to receive the most effective treatments available” (pp. 205–206).

Nevertheless, the field of psychology continues to be debased by people who hang out a shingle, call themselves psychologists, and fail to adhere to the strictures of the discipline by practicing pseudoscience. The field deserves no sympathy when it fails to police itself and to rule out unscientific practice. For example, the National Post of Toronto reports a story of a psychotherapist in California treating depression with astrological charts—a patent absurdity (people can consult him over the phone for $150 an hour). The writer of the story tells us that “while the psychology profession might desire to purge itself of the likes of Perry, it may not be so simple. Perry holds a PhD in clinical psychology, and much of his training in astrological psychology was based on the works of Carl Gustav Jung . . . who is still taught in colleges and universities today.” It turns out that this person is licensed to practice therapy by the Board of Behavioral Science Examiners in Sacramento and is a member of the California Association of Marriage and Family Therapists (Milstone, 2000).
Things may be looking up, however. In 2002 a new journal was started: *The Scientific Review of Mental Health Practice* (Lilienfeld, 2002). The journal is dedicated to research that tries to distinguish scientific from pseudoscientific treatments, and it has been endorsed by the Council for Scientific Mental Health Practice. Even more heartening are indications that at least some psychological organizations are showing the fortitude required to police clinical practice and to rid psychological practice of its ultimately destructive “anything goes” attitude. Lilienfeld and Lohr (2000) report on how the Arizona Board of Psychological Examiners sanctioned a psychologist who attempted to treat phobias with a pseudoscientific treatment that involved tapping body parts in a predetermined order. Needless to say, there are no controlled studies of the efficacy of this treatment, and the Arizona Board ordered the therapist to stop using it and put him on probation—an all too rare example of a psychological organization policing the pseudoscience that is practiced by its clinical members.

In short, psychology has a kind of Jekyll and Hyde personality. Extremely rigorous science exists right alongside pseudoscientific and antiscientific attitudes. This Jekyll and Hyde aspect of the discipline was clearly apparent in the recovered-memory–false-memory debate of the early to mid-1990s (Garry et al., 1999; Loftus, 1997; Loftus & Guyer, 2002; Loftus & Ketcham, 1994; Pezdek & Banks, 1996; Pezdek & Hodge, 1999; Shermer, 1997). Many cases were reported of individuals who had claimed to remember instances of child abuse that had taken place decades earlier but had been forgotten. Many of these memories occurred in the context of therapeutic interventions. It is clear that some of these memories were induced by the therapy itself (Campbell, 1998; Loftus & Guyer, 2002; Piper, 1998). Some people insisted that such memories were never to be trusted; others insisted that they were always to be trusted. In the emotionally charged atmosphere of such an explosive social issue, psychologists provided some of the more balanced commentary and, most important, some of the more dispassionate empirical evidence on the issue of recovered or false memories (Bremner, Shobe, & Kihlstrom, 2000; Clancy, Schacter, McNally, & Pitman, 2000; Pezdek & Banks, 1996). Here we have the Jekyll and Hyde feature of psychology in full-blown form. Some of the cases of therapeutically induced false memories—and, hence, of the controversial phenomenon itself—were caused by incompetent and scientifically ignorant therapists who were psychologists. On the other hand, whatever uncertain partial resolution of the controversy we do have is in large part due to the painstaking efforts of research psychologists who studied the relevant phenomena empirically. Psychology is contributing to a problem and helping to solve it at the same time!

I hope this section has helped to dispel the notion that I wish to “let psychology off the hook” with the use of my Rodney Dangerfield joke to title this chapter. In his book on research methods, psychologist Douglas Mook (2001) referred to my use of the Dangerfield joke and commented that “often indeed,
psychology gets no respect; but sometimes, too, it is respected more than is warranted and for the wrong reasons” (p. 473). I agree completely with this sentiment. Mook is right that the student of psychology needs to understand the paradoxes that surround the discipline. As I have presented it in this book, as the science of human behavior, the discipline of psychology often gets too little respect. But the face that psychology often presents to the public—that of a clinician claiming “unique” insight into people that is not grounded in research evidence—psychology often gets too much respect. The discipline is often represented to the public by segments of psychology that do not respect its really unique defining feature—that it validates statements about human behavior by employing the methods of science.

ISN’T EVERYONE A PSYCHOLOGIST?
IMPLICIT THEORIES OF BEHAVIOR

We all have theories about human behavior. It is hard to see how we could get through life if we did not. In this sense, we are all psychologists. It is very important, though, to distinguish between this individual psychology and the type of knowledge produced by the science of psychology. The distinction is critical because the two are often deliberately confused in popular writings about psychology, as we shall see.

In what ways is our personal psychological knowledge different from the knowledge gained from a scientific study of behavior? We have already discussed several. Much of our personal psychological knowledge is recipe knowledge. We do certain things because we think they will lead others to behave in a certain way. We behave in particular ways because we think that certain behavior will help us achieve our goals. But it is not the mere presence of recipe knowledge that distinguishes personal psychology from scientific psychology (which also contains recipe knowledge). Psychotherapies, to varying degrees, provide recipe knowledge about which behaviors and environments will lead to the solution of personal problems or to a more fulfilling life. The main difference here is that the science of psychology seeks to validate its recipe knowledge empirically.

Scientific evaluation is systematic and controlled in ways that individual validation procedures can never be. Indeed, psychological research on decision making has indicated that humans have difficulty detecting correlations in their behavioral environment that run counter to their accepted beliefs (see Baron, 2000). We see what we want to see. Psychologists have uncovered many of the reasons (Kunda, 1999; Lassiter, Geers, Munhall, Ploutz-Snyder, & Breitenbecher, 2002; Stanovich, 1999, in press), but they need not concern us here. Even if we wanted to evaluate personal recipe knowledge on an individual basis, built-in biases that make us less than adequate observers of behavioral phenomena would make it extremely difficult. The scientific method
has evolved to avoid the biases of any single human observer. The implication here is a simple one. The recipe knowledge generated by the science of psychology is more likely to be accurate because it has undergone validation procedures more stringent than those to which personal recipe knowledge is exposed.

As discussed earlier in this chapter, the differences between personal and scientific psychologies go beyond the validation of recipe knowledge. Science always aspires to more than recipe knowledge of the natural world. Scientists seek more general, underlying principles that explain why the recipes work. But many people desire no more insight into human behavior than that provided by recipe knowledge. Indeed, the idea of investigating the underlying causes of behavior actually frightens some people, and they actively avoid such knowledge.

The personal psychologies of some people, however, are similar to scientific psychology in seeking more basic psychological principles and theories. These personal theories, though, often depart from scientific theories in important ways. We have already mentioned that they are often unfalsifiable. Rather than being coherently constructed, many people’s personal psychological theories are merely a mixture of platitudes and clichés, often mutually contradictory, that are used on the appropriate occasion. They reassure people that an explanation does exist and, furthermore, that the danger of a seriously contradictory event—one that would deeply shake the foundations of a person’s beliefs—is unlikely to occur. Somehow the net of clichés will be stretched, no matter how far, to explain whatever happens. As discussed in Chapter 2, although these theories may indeed be comforting, comfort is all that theories constructed in this way provide. In explaining everything post hoc, these theories predict nothing. By making no predictions, they tell us nothing. Theories in the discipline of psychology must meet the falsifiability criterion, and in doing so, they depart from the personal psychological theories of many laypeople. Theories in psychology can be proved wrong, and, therefore, they contain a mechanism for growth and advancement that is missing from many personal theories.

Some personal theories, however, are not merely unfalsifiable, jerry-built sets of clichés. Some people do hold to implicit psychological theories that are structured in a reasonably coherent manner and that are indeed potentially falsifiable. However, a personal psychological theory of this type still suffers from some of the same difficulties as personal recipe knowledge. Although the theory may be coherent and potentially falsifiable, there remains the problem of evaluating the data that could lead to revision of the theory. An individual’s observations are simply not structured in the controlled manner of the scientific method. They are subject to biased interpretations and are acquired in an intermittent way that leads to errors in probabilistic reasoning (see Chapter 10). Indeed, we saw in Chapter 6 that people’s intuitive theories about motion in the natural world are often very wrong, despite years of
experience with moving objects. In the more variable area of human behavior, the situation is bound to be much worse.

THE SOURCE OF RESISTANCE TO SCIENTIFIC PSYCHOLOGY

For the reasons we just discussed, it is important not to confuse the idea of a personal psychological theory with the knowledge generated by the science of psychology. Such a confusion is often deliberately fostered to undermine the status of psychology in the public mind. The idea that “everyone’s a psychologist” is true if it is understood to mean simply that we all have implicit psychological theories. But it is often subtly distorted to imply that psychology is not a science.

We discussed in Chapter 1 why the idea of a scientific psychology is threatening to some people. A maturing science of behavior will change the kinds of individuals, groups, and organizations that serve as sources of psychological information. It is natural that individuals who have long served as commentators on human psychology and behavior will resist any threatened reduction in their authoritative role. Chapter 1 described how the advance of science has continually usurped the authority of other groups to make claims about the nature of the world. The movement of the planets, the nature of matter, and the causes of disease were all once the provinces of theologians, philosophers, and generalist writers. Astronomy, physics, medicine, genetics, and other sciences have gradually wrested these topics away and placed them squarely within the domain of the scientific specialist.

Many religions, for example, have gradually evolved away from claiming special knowledge of the structure of the universe. The titanic battles between science and religion have passed into history, with the exception of some localized flare-ups such as the creationism issue. Scientists uncover the structure of the natural world. Many religions provide commentary on the implications of the uses of these discoveries, but they no longer contest with scientists for the right to determine what the discoveries are. The right to adjudicate claims about the nature of the world has unquestionably passed to scientists.

The issue, then, is the changing criteria of belief evaluation. Few newspaper editorials ever come out with strong stands on the composition of the rings of Saturn. Why? No censor would prevent such an editorial. Clearly the reason it is not written is that it would be futile. Society knows that scientists, not editorial writers, determine such things. Only a hundred years ago, newspapers and preachers in the pulpit did comment vociferously on the origins of species in the animal kingdom. These comments have largely disappeared because science has destroyed the conditions that would allow them to be believed by rational thinkers. Psychology threatens to destroy those con-
ditions in another large domain of nature. A hundred years from now, newspaper editorials entitled “Heredity or Environment in Early Childhood Growth?” may sound as silly and dated as Archbishop Ussher’s statement that the world was created in 4004 B.C.

Some people find it difficult to accept such a state of affairs when it comes to psychology. They cling tenaciously to their right to declare their own opinions about human behavior even when these opinions contradict the facts. Of course, the correct term here is really not right, because, obviously, in a free society, everyone has the right to voice opinions, regardless of their accuracy. It is important to understand that what many people want is much more than simply the right to declare their opinions about human behavior. What they really want is the conditions that are necessary for what they say to be believed. When they make a statement about human psychology, they want the environment to be conducive to the acceptance of their beliefs. This is the reason that there are always proponents of the “anything-goes” view of psychology, that is, the idea that psychological claims cannot be decided by empirical means and are simply a matter of opinion. But science is always a threat to the “anything-goes” view, because it has a set of strict requirements for determining whether a knowledge claim is to be believed. Anything does not go in science. This ability to rule out false theories and facts accounts for scientific progress.

In short, a lot of the resistance to scientific psychology is due to what might be termed conflict of interest. As discussed in earlier chapters, many pseudosciences are multimillion-dollar industries that thrive on the fact that the public is unaware that statements about behavior can be empirically tested (there are 20 times more astrologers in the United States than astronomers; Gilovich, 1991, p. 2). The public is also unaware that many of the claims that are the basis of these industries (such as astrological prediction, subliminal weight loss, biorhythms, the administration of laetrile, and psychic surgery) have been tested and found to be false. A subcommittee of the U.S. Congress has estimated that $10 billion is spent annually on medical quackery, an amount that dwarfs the sum that is spent on legitimate medical research (Eisenberg et al., 1993; U.S. Congress, 1984).

Many purveyors of pseudosciences and bogus therapies depend on an atmosphere of anything goes surrounding psychology. It provides a perfect environment for feeding on public gullibility, because the public has no consumer protection if anything goes. As attorney Peter Huber (1990) argued, “[At] the fringes of science and beyond . . . assorted believers in homeopathic medicine and the curative powers of crystals and pyramids . . . must discredit orthodox science to build their own cases for unorthodox nostrums” (p. 97). Those selling pseudoscience have a vested interest in obscuring the fact that there are mechanisms for testing behavioral claims. As Michael Ghiselin (1989) warned, “What is going on here is quite straightforward. People are trying to sell a given point of view. Those who know how to evaluate the
product are not the same as those to whom it is being marketed” (p. 139). In
the domain of behavioral claims and therapies, psychologists are the ones
who “know how to evaluate the product.” This is why the pseudoscience in-
dustry continues to oppose the authority of scientific psychology to pass judg-
ment on behavioral claims. However, the purveyors of pseudoscience often
do not need to do direct battle with psychology. They simply do an end run
around psychology and go straight to the media with their claims. The media
make it very easy for cranks, quacks, and pseudoscientists to do an end run
around scientific psychology. The plethora of other talk shows that have in-
undated the airwaves in the 1990s do not ask their guests to produce their bib-
liographies of scientific research. If these guests are “interesting,” they are
simply put on the show.

Folk wisdom often contains a lot of wishful thinking: People want to be-
lieve that the world is the way they wish it to be rather than the way it is. Sci-
ence often has the unenviable task of having to tell the public that the nature
of the world is somewhat different from how they wish it to be (“No, that fast-
food lunch is not good for your health”). The media, which could help in this
situation (by telling people what is true rather than what they want to hear),
only make it worse with their focus on what will “entertain” rather than on
what will inform.

Science, then, does rule out the special-knowledge claims of those
proposing statements that do not meet the necessary tests. In this book, we
have briefly touched on what are considered adequate and inadequate tests
in science. Introspection, personal experience, and testimonials are all con-
sidered inadequate tests of claims about the nature of human behavior. Thus,
it should not be surprising that conflict arises because these are precisely the
types of evidence that nonpsychologist commentators have been using to
support their statements about human behavior since long before a discipline
of psychology existed.

However, it should not be thought that I am recommending a dour,
spoilsport role for the science of psychology. Quite the contrary. The actual
findings of legitimate psychology are vastly more interesting and exciting
than the repetitious gee-whiz pseudoscience of the media. Furthermore, it
should not be thought that scientists are against fantasy and imagination.
Again, on the contrary, scientists have nothing against fantasy, imagination, and
flights of fancy—in their proper contexts. Peter Medawar (1990) addressed
this point:

I am quite a believer in hot air in its proper place. I believe that most people psy-
chologically need to be what Paul Jennings calls “bunkrapt.” (You may remem-
ber Paul Jennings’ typewriter when he was trying to write “bankrupt” wrote
“bunkrapt.”) Everybody needs to be bunkrapt, and I prefer to be bunkrapt by
listening to Wagner’s music dramas or reading Tolkien’s novels. It must not spill
over into science. (p. 5)
If we stop and think for a minute, most of us would agree with Medawar’s point. We want fancy and fantasy when we go to the movies or the theater—but not when we go to the doctor’s office, buy insurance, register our children for child care, fly in an airplane, or have our car serviced. We could add to this list going to a psychotherapist, having our learning-disabled child tested by a school psychologist, or taking a friend to suicide-prevention counseling at the university psychology clinic. Psychology, like other sciences, must remove fantasy, unfounded opinion, “common sense,” commercial advertising claims, the advice of gurus, testimonials, and wishful thinking from its search for the truth.

It is difficult for a science to have to tell parts of society that their thoughts and opinions are needed—but not here. Psychology is the latest of the sciences to be in this delicate position. The difference in time period for psychology, however, is relevant. Most sciences came of age during periods of elite control of the structures of society, when the opinion of the ordinary person made no difference. Psychology, on the other hand, is emerging in a media age of democracy and ignores public opinion at its own peril. Many psychologists are now taking greater pains to remedy the discipline’s lamentable record in public communication. As more psychologists take on a public communication role, the conflicts with those who confuse a personal psychology with scientific psychology are bound to increase.

Not everyone is a physicist, even though we all hold intuitive physical theories. But in giving up the claim that our personal physical theories must usurp scientific physics, we make way for a true science of the physical universe whose theories, because science is public, will be available to us all. Likewise, everyone is not a psychologist. But the facts and theories uncovered by the science of psychology are available to be put to practical ends and to enrich the understanding of all of us.

THE FINAL WORD

We are now at the end of our sketch of how to think straight about psychology. It is a rough sketch, but it can be of considerable help in comprehending how the discipline of psychology works and in evaluating new psychological claims. Our sketch has revealed that:

1. Psychology progresses by investigating solvable empirical problems. This progress is uneven because psychology is composed of many different subareas, and the problems in some areas are more difficult than in others.
2. Psychologists propose falsifiable theories to explain the findings that they uncover.
3. The concepts in the theories are operationally defined, and these definitions evolve as evidence accumulates.

4. These theories are tested by means of systematic empiricism, and the data obtained are in the public domain, in the sense that they are presented in a manner that allows replication and criticism by other scientists.

5. The data and theories of psychologists are in the public domain only after publication in peer-reviewed scientific journals.

6. What makes empiricism systematic is that it strives for the logic of control and manipulation that characterizes a true experiment.

7. Psychologists use many different methods to arrive at their conclusions, and the strengths and weaknesses of these methods vary.

8. Most often, conclusions are drawn only after a slow accumulation of data from many experiments.

9. The behavioral principles that are eventually uncovered are almost always probabilistic relationships.

In 1961, British psychologist Donald Broadbent made a statement that is just as relevant today as it was then. For us, it could serve as a condensed summary of how to think straight about psychology:

We end then upon a note of doubt, with no certainty about the beliefs which future psychologists will hold. This is as it should be. Nobody can grasp the nature of things from an armchair, and until fresh experiments have been performed we do not know what their results will be. The confident dogmatisms about human nature which fall so readily from pulpits, newspaper editorials, and school prize-givings are not for us. Rather, we must be prepared to live with an incomplete knowledge of behavior but with confidence in the power of objective methods to give us that knowledge some day. (pp. 200–201)