
Public Skepticism of Psychology

Why Many People Perceive the Study of Human Behavior as Unscientific

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Data indicate that large percentages of the general public regard psychology's scientific status with considerable skepticism. I examine 6 criticisms commonly directed at the scientific basis of psychology (e.g., psychology is merely common sense, psychology does not use scientific methods, psychology is not useful to society) and offer 6 rebuttals. I then address 8 potential sources of public skepticism toward psychology and argue that although some of these sources reflect cognitive errors (e.g., hindsight bias) or misunderstandings of psychological science (e.g., failure to distinguish basic from applied research), others (e.g., psychology's failure to police itself, psychology's problematic public face) reflect the failure of professional psychology to get its own house in order. I offer several individual and institutional recommendations for enhancing psychology's image and contend that public skepticism toward psychology may, paradoxically, be one of our field's strongest allies.

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Whenver we psychologists dare to venture outside of the hallowed halls of academia or our therapy offices to that foreign land called the “real world,” we are likely at some point to encounter a puzzling and, for us, troubling phenomenon. Specifically, most of us will inevitably hear the assertion from laypersons that psychology—which those of us within the profession generally regard as the scientific study of behavior, broadly construed—is in actuality not a science. Some outsiders go further, insinuating or insisting that much of modern psychology is pseudoscientific. Keith Stanovich (2009) dubbed psychology the “Rodney Dangerfield of the sciences” (p. 175) in reference to the late comedian famous for joking that “I don’t get no respect.” As Stanovich observed, “Most judgments about the field and its accomplishments are resoundingly negative” (p. 175).

Ironically, some of the harshest criticisms of contemporary psychology’s scientific status have come from within the ranks of psychology itself (see Gergen, 1973; S. C. Hayes, 2004; Lilienfeld, 2010; Lykken, 1968, 1991; G. A. Miller, 2004; Skinner, 1987). Specifically, many scholars within psychology have rued the extent to which (a) excessive reliance on statistical significance testing, (b) the propensity of psychologists to posit vague theoretical entities that are difficult to test, and (c) political correctness

and allied trends (see Redding & O’Donohue, 2009, and Tierney, 2011, for recent discussions) have retarded the growth of scientific psychology. Others (e.g., Dawes, 1994; Lilienfeld, Lynn, & Lohr, 2003; Thyer & Pignotti, in press) have assailed the scientific status of large swaths of clinical psychology, counseling psychology, and allied mental health disciplines, contending that these fields have been overly permissive of poorly supported practices. Still others (e.g., S. Koch, 1969; Meehl, 1978) have bemoaned the at times painfully slow pace of progress of psychology, especially in the “softer” domains of social, personality, clinical, and counseling psychology.

Such criticisms have been ego bruising to many of us in psychology. At the same time, they have been valuable and have spurred the field toward much needed reforms, such as greater emphasis on effect sizes and confidence intervals in addition to (or in lieu of) tests of statistical significance (Cohen, 1994; Meehl, 1978) and the development of criteria for, and lists of, evidence-based psychological interventions (Chambless & Hollon, 1998). Yet with the exception of criticisms of mental health practice, most of the concerns voiced by insiders probably overlap only minimally with those of outsiders.

Whatever the sources of the public’s skepticism of psychology’s scientific status, it is clear that such doubts are not new (Benjamin, 2006)—nor is our field’s deep-seated insecurity about how outsiders view it. As Coon (1992) remarked when explaining psychology’s conspicuous omission from Auguste Comte’s mid-19th-century hierarchy of the sciences,

It is well-known that whereas sociology sat at the peak of the Comtian hierarchy, psychology was not even on the pyramid, having been deemed incapable of becoming a science because its subject matter was unquantifiable and its methods mired in a metaphysical morass. Psychology has never quite lived this down and, as psychologists themselves like to say, has never recovered from its adolescent physics envy. (p. 143)

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Nevertheless, as a field, we have been reluctant to examine the reasons for the widespread and longstanding public skepticism of psychology (but see Benjamin, 1986, for a useful historical analysis), perhaps because we see little merit in these reasons. Nor have we invested much effort in generating potential solutions for enhancing our field's public image.

Yet we ignore negative lay perceptions of psychology at our peril. Public skepticism of psychology may render would-be mental health consumers reluctant to seek out our potentially valuable clinical services. Public skepticism may have also contributed indirectly to psychology's noticeable absence from some funding agencies' lists of Science, Technology, Engineering, and Mathematics (STEM) disciplines (Price, 2011). As a consequence of this lack of recognition, "Psychologists are often not eligible for targeted funding for education, professional training, and research that could contribute substantially to achieving STEM goals" (American Psychological Association [APA] 2009 Presidential Task Force Report on the Future of Psychology as a STEM Discipline, 2010, p. 6), such as programs for boosting psychology students' science and mathematics literacy. In addition, public skepticism of our field may foster the seemingly perpetual misunderstanding of psychological research by politicians, some of whom control the purse strings for federal funding of our research. Prompted in part by attacks from members of Congress against several federally funded studies on psychological topics (e.g., primate responses to inequity, the effects of retirement on marital quality), the APA Science Directorate (2006) prepared a "Self-Defense for the Psychological Scientist" pamphlet that provided researchers with advice for countering misperceptions of their studies by politicians. Certainly, the APA, the Association for Psychological Sci-

ence (APS), and other professional organizations are right to rebut such misunderstandings when they arise. Yet, with the exception of the APA Science Directorate pamphlet, such efforts are usually reactive rather than proactive. Moreover, these efforts may meet with mixed and at best short-term success, because they largely neglect to appreciate the underlying reasons for policymakers' skepticism.

Understanding why many nonpsychologists are skeptical of psychology is important for four reasons. First, such knowledge can forearm psychologists not only when fielding questions from skeptical relatives at holiday dinners ("But isn't what you're studying pretty obvious?") but, more crucially, when encountering resistance about psychological findings from students, therapy clients, and laypersons. In this way, such knowledge can equip psychologists with intellectual ammunition against misguided criticisms of their field. Second, such knowledge may allow psychologists to anticipate commonplace objections to psychological research from policymakers and help psychologists explain the pragmatic and theoretical significance of their research to outsiders. Third, such knowledge is valuable in its own right, because it sheds light on the psychological sources of resistance to the scientific study of human nature (see also Bloom & Weisberg, 2007, for a discussion of developmental precursors of resistance to science). In this respect, it may help us to grasp why so many educated individuals find psychology to be unscientific. Fourth, such knowledge may aid psychologists in crafting recommendations for counteracting public and policymakers' misunderstandings of psychology.

In this article, I pose two overarching questions: (a) Are the negative views of psychology's scientific status held by many outsiders warranted? (b) What are the principal sources of these views? I place primary emphasis on the psychological and sociological reasons for the public's skepticism of psychology's scientific status. Nevertheless, I acknowledge that these negative attitudes have deep historical roots, including early 20th-century psychologists' entanglements with the paranormal and spiritualism, which probably contributed to psychology's less than glowing public image (Benjamin, 2006; Coon, 1992). I conclude by proposing several individual and institutional recommendations for diminishing the widespread public skepticism of psychology as a scientific discipline. Before doing so, I canvass data on the prevalence of the public's skeptical attitudes toward psychology's scientific status, as such data offer us a glimpse of the magnitude of the problem we confront. As we will see, the data also provide us with tantalizing clues to the reasons for many laypersons' negative attitudes toward our field.

The Prevalence of Public Skepticism of Psychology

One need not look far and wide in the media to find examples of skepticism toward psychology. In 1982, a *New York Times* editorial subtitled "If This Is Consensus, Psychology Can't Be Much of a Science" (Wade, 1982, p. A28) reported on a *Psychology Today* survey that asked 11

psychologists to name the most significant psychological finding of the previous 15 years. The writer concluded wryly, "The results are astonishing; it would seem that there has been none" (Wade, 1982, p. A28). Observing that minimal consensus emerged among the psychologists who were queried, he further contended that this lack of agreement "evinces a serious problem in their [psychologists'] academic discipline" (Wade, 1982, p. A28). Some readers may also recall the time that David Stockman, President Reagan's Director of the Office of Management and Budget, derided psychology as a pseudoscience when justifying his administration's intention to slash behavioral science funding (Benjamin, 2003). More recently, other readers may remember when Dr. Laura Schlessinger (2000), better known as "Dr. Laura," proclaimed on her radio show, "Psychology has become a God to the general public. It is not science." Yet just how representative are these scattered assertions? The answer is sobering, at least to those of us in the field: Evidence suggests that serious doubts regarding psychology's scientific status are relatively widespread among the general public.

As Wood, Jones, and Benjamin (1986; see also Benjamin, 1986) noted, several early studies (e.g., Guest, 1948; Withey, 1959) suggested that many laypersons hold negative views of psychology. In contrast, some recent surveys have yielded more encouraging results. Kabatznick (1984) found that only about 25% of individuals, including business people, mall shoppers, and physical and biological scientists, hold mostly negative views of psychologists. Wood et al. surveyed 201 members of the community from four major metropolitan areas (Los Angeles, Milwaukee, Houston, and Washington, DC). They found that 8.5% of participants had neither "favorable" nor "somewhat favorable" views of psychology; 15.5% disagreed that psychology is a science. Eighty-three percent of respondents, however, believed that daily life experiences afford them adequate training in psychology, suggesting that many laypersons do not appreciate the crucial role of formal scientific training in understanding human behavior.

Other data suggest more troubling trends. Janda, England, Lovejoy, and Drury (1998) contacted a randomly selected community sample of 141 adults in Virginia, 100 of whom agreed to participate. Participants rated psychology and sociology significantly lower than the five other disciplines examined (biology, chemistry, economics, medicine, and physics) in their "contribution of the discipline to society" (p. 141); the mean score for psychology was 4.94 ($SD = 1.46$) on a 7-point scale. They also rated psychology ($M = 5.08$, $SD = 1.45$), sociology, and economics lower than the other professions in terms of expertise—namely, the difference between what an ostensible expert in the field as opposed to the average person knows about the subject matter. Of 27 spontaneous comments by participants, 25 pertained to psychology, 24 of which were deemed "clearly negative" (p. 141). The authors noted,

Many of the negative comments had as their theme that at least some of what psychologists have to say cannot be believed and that people should rely instead on their common sense. A few

respondents had much stronger views, suggesting that psychology was responsible for creating problems for our society. (Janda et al., 1998, p. 141)

In a second study, Janda et al. (1998) surveyed 72 participants who were faculty members at a university in Virginia and asked them to rate the same variables as in the previous study. For the variables of contribution to the discipline ($M = 5.06$, $SD = 0.96$) and expertise ($M = 4.72$, $SD = 1.24$), psychology ranked lower than the other five disciplines (only sociology did not differ significantly from psychology); the authors did not separate out these ratings by area of faculty expertise.

The most recent large-scale survey, the APA Benchmark Study (Penn, Schoen and Berland Associates, 2008), sampled 1,000 adults across the United States. The findings of this important study are multifaceted and give psychologists ample reasons to both cheer and moan. On the relatively positive side, the researchers found that 82% agreed that psychological research helps to improve people's lives either "somewhat or a lot," with 16% disagreeing (see also Mills, 2009).

On the mostly negative side, only a minority of participants appeared to view psychology as scientific. On a forced-choice question, only 30% agreed that "psychology attempts to understand the way people behave through scientific research," whereas 52% agreed that "psychology attempts to understand the way people behave by talking to them and asking them why they do what they do" (Penn, Schoen and Berland Associates, 2008, p. 29). Others voiced doubts about psychology's scientific standards: 41% believed that psychology is less rigorous than medical research and 31% that it is less rigorous than economic research, with 11% in both cases stating that it is "a lot less rigorous." Even more striking was the finding that few participants seemed aware of psychology's impact on myriad applied domains. When offered a choice among multiple professions (e.g., psychologists, psychiatrists, lawyers, businesspersons, priests and other religious figures), only 22% selected psychologists when asked which profession is best suited to reducing divorce rates, with 37% selecting religious figures. Only 12% selected psychologists when asked which profession is best suited to addressing physical health problems, such as obesity and smoking (60% selected physicians); 11% selected psychologists when asked which profession is best suited to improving organizational productivity and morale (37% selected businesspersons); and 2% selected psychologists when asked which profession is best suited to understanding economic problems (57% selected economists and 15% selected businesspersons). Most disconcerting of all, 1% (!) selected psychologists when asked which profession is best suited to confronting the problems posed by climate change, with 44% selecting engineers and 16% economists. Finally, although 62% agreed that the federal government should spend more money on psychological research, 29% disagreed, with 13% opining that the government should spend "a lot" less money on such research.

Few studies have examined the attitudes of students from nonpsychological disciplines toward psychology. Martin, Sadler, and Baluch (1997) administered attitudinal questions to 193 students drawn from various majors at a British university. They found that engineering students were significantly less likely than students in other fields (e.g., sociology, business, English) to regard psychology as either a science or a social science. On average, students across disciplines tended to view psychology as a social science but not as a genuine science, although the authors did not report means for these comparisons (but see Bartels, Hinds, Glass, and Ryan, 2009, who found no significant differences among students from different majors in the view that psychology is a science).

In sum, survey data indicate that although most members of the public hold generally positive views of psychology's scientific status, nontrivial minorities do not hold such views. Hefty percentages view psychology as less valuable to society than a number of other disciplines, including physics, business, medicine, and engineering (Janda et al., 1998). Moreover, many laypersons view psychology as largely nonscientific and as lacking in scientific rigor. Consistent with the results of earlier surveys (Wood et al., 1986), the data from the APA Benchmark Study (Penn, Schoen and Berland Associates, 2008) suggest that many members of the public appear unaware of the breadth of psychology's current and potential contributions to society (e.g., Zimbardo, 2004), including its applications to physical health, worker productivity, economic problems, and the environment.

Six Common Criticisms of the Scientific Basis of Psychology and Six Rebuttals

To what extent is the public's skepticism of our field merited? Perhaps the best means of addressing this question is to examine the most prevalent criticisms of psychology's scientific basis and to evaluate how well they withstand careful scrutiny. Here I evaluate the merits of six widely voiced criticisms.

"Psychology Is Merely Common Sense"

As Stanovich (2009) observed, the claim that psychology is scant more than common sense is among the most ubiquitous criticisms of our field (see also Chabris & Simons, 2010; Furnham, 1988; Kelley, 1992). Some popular websites and newspapers have recently introduced regular columns titled "The Duh Files," consisting of ostensibly obvious findings (e.g., that television shows featuring sex are viewed more often than other television shows, that women can tolerate pain better than men can), many of which derive from psychological research (e.g., see <http://thebadmomclub.com/2010/04/from-the-duh-files-women-can-tolerate-pain-more.html>; <http://haigmedia.blogspot.com/2008/04/more-from-duh-files.html>). Moreover, so-called "folk knowledge" or "fireside inductions" (Meehl, 1971), such as "opposites attract," "familiarity breeds contempt," or "we use only 10% of our brain power," are omnipresent in

popular culture, even though all are contradicted by controlled research (Lilienfeld, Lynn, Ruscio, & Beyerstein, 2010).

In recent years, several high-profile radio hosts (e.g., Prager, 2002; Schlessinger, 2000) and writers have suggested that common sense should almost always trump scientific findings in psychology and allied fields. In a widely discussed *New York Times* editorial, influential science journalist John Horgan (2005) argued that common sense should be an arbiter of the value of scientific theories in numerous disciplines, including psychology and neuroscience. Horgan wrote, "I have also found common sense—ordinary, nonspecialized knowledge and judgment—to be indispensable for judging scientists' pronouncements" (p. A34). For Horgan, it is "only sensible to doubt" (p. A34) findings that clash with our intuitions. Even some psychology scholars (e.g., Kluger & Tikochinsky, 2001; Redding, 1998) have suggested that psychological findings that contravene popular wisdom should be treated with considerable skepticism.

The view that accurate psychological knowledge is merely common sense is also pervasive in the legal arena. In one striking example, in the 2007 trial of former White House aide I. Lewis (Scooter) Libby, Judge Reginald Walton disallowed expert testimony on the malleability of human memory (from psychologist Robert Bjork of the University of California at Los Angeles) on the grounds that the fallibility of memory is a "commonplace matter of course" for jurors and that jurors can safely rely on their "common sense" to ascertain how memory works (see Kassam, Gilbert, Swencionis, & Wilson, 2009, p. 552).

Contradicting this position, scores of psychology findings violate popular wisdom; in a recent book, my colleagues and I (Lilienfeld et al., 2010) collected over 300 examples. Here, for example, is a sampling of the prevalence of psychological misconceptions derived from surveys of undergraduates in North American psychology classes, followed in parentheses by the percentage of participants who endorsed each misconception:

- Expressing pent-up anger reduces anger (66%; Brown, 1983).
- Strange behaviors are especially likely during full moons (65%; G. W. Russell & Dua, 1983).
- People with schizophrenia have multiple personalities (77%; Vaughan, 1977).
- Human memory operates like a tape recorder (27%; Lenz, Ek, & Mills, 2009).
- The polygraph test is a highly accurate detector of lies (45%; Taylor & Kowalski, 2004).
- Hypnotized people act like robots and blindly follow the suggestions of hypnotists (44%; Green, Page, Rasekhy, Johnson, & Bernhardt, 2006).
- On a multiple-choice test, one should stick with one's original answer, even if a different answer seems correct (75%; Kruger, Wirtz, & Miller, 2005).

Erroneous beliefs about psychology are widespread in the general population too. Surveys of laypersons reveal

that (a) about 50% believe that schizophrenia is synonymous with a split personality (H. Stuart & Arboleda-Florez, 2001; Wahl, 1987); (b) 72% believe that subliminal advertising is effective in persuading people to purchase products (Rogers & Smith, 1993); and (c) 40% believe that listening to Mozart's music enhances intelligence (Chabris & Simons, 2010). These and a host of other data on the prevalence of psychological misconceptions (e.g., Furnham, 2002) call into question the recommendation (e.g., Kluger & Tikochinsky, 2001) to privilege common sense over scientific findings when distinguishing facts from fictions concerning human nature (Lilienfeld, 2010).

"Psychology Does Not Use Scientific Methods"

As many philosophers of science have noted, the belief that there is a monolithic scientific method is almost surely a myth (Bauer, 1992). Still, many philosophers of science concur that a higher order commonality in epistemic approach cuts across most or all scientific disciplines. Specifically, different sciences, despite their surface diversity, are marked by (a) a willingness to root out error in one's web of beliefs and (b) the implementation of procedural safeguards against confirmation bias—the deeply ingrained tendency to seek out evidence consistent with one's hypotheses and to deny, dismiss, or distort evidence that is not (Gilovich, 1991; Lilienfeld, 2010; Tavris & Aronson, 2007).

From this perspective, many areas of psychology are every bit as scientific as traditional "hard" sciences, including physics and chemistry. In such domains as social and cognitive psychology, for example, the use of randomized control groups and blinded observations are *de rigueur*; the same holds for the use of randomized controlled trials, placebo control groups, and blinded designs in clinical psychology, counseling psychology, and allied fields (Kazdin, 2003). Moreover, subdisciplines of psychology that investigate individual differences, such as the study of intellectual aptitudes, vocational interests, personality, and psychopathology, rely on sophisticated statistical methods, including correlational, multiple regression, and structural equation modeling techniques, to distinguish genuine covariation in nature from "illusory correlation" (Chapman & Chapman, 1967, p. 194)—namely, the perception of statistical associations in their absence. And longitudinal designs are partial controls against retrospective memory bias and hindsight bias, both of which can distort the recollection of previously collected information (Ruspini, 2002).

These and other methodological and statistical procedures are sophisticated, albeit fallible, safeguards against manifold sources of human error, especially confirmation bias (O'Donohue, Lilienfeld, & Fowler, 2007). Blinded observations, for example, prevent investigators' preconceptions from inadvertently influencing their ratings; and meta-analytic procedures decrease (although by no means eliminate; see Ghaemi, 2009) the chances that investigators' biases will influence their integration and interpretation of ambiguous bodies of literature.

"Psychology Cannot Yield Meaningful Generalizations Because Everyone Is Unique"

Each of us, even those of us who are monozygotic (identical) twins, is unquestionably unique. Some critics have invoked this uniqueness to argue that psychology cannot yield meaningful generalizations across individuals. For example, in an effort to explain why the self-help program Alcoholics Anonymous (AA) does not help all problem drinkers, *New York Times* columnist David Brooks (2010) wrote,

Each member of an A.A. group is distinct. Each group is distinct. Each moment is distinct. There is simply no way for social scientists to reduce this kind of complexity into equations and formula [*sic*] that can be replicated one place after another. (para. 8)

Brooks's (2010) argument reflects a pervasive misunderstanding. Each person's uniqueness does not necessarily undermine the efficacy of psychological interventions across most or even virtually all individuals, because unique variables may be largely or entirely irrelevant to the underlying mechanisms of the treatment in question (Hill, 1962). Put in statistical terms, the unique attributes of each individual may not interact statistically with the intervention but may be swamped out by its main effects. This state of affairs holds in medicine: To take merely one example, although all individuals with melanoma are surely unique, 90% or more of cases of this form of skin cancer are largely curable with early surgery (Berwick, 2010).

A similar misconception arises frequently in the context of criticisms of psychiatric classification. Many have attacked the use of psychiatric diagnoses, such as schizophrenia, major depression, and obsessive-compulsive disorder, as pigeonholing, because such diagnoses ostensibly disregard crucial differences among individuals within each category (e.g., B. Miller, 2007). But psychiatric diagnoses, like medical diagnoses, do not imply that all individuals within a category are alike in all ways. They imply only that they are alike in *one* crucial way—namely the core signs and symptoms that constitute the category (Lilienfeld & Landfield, 2008).

"Psychology Does Not Yield Repeatable Results"

How replicable (repeatable) are the results of psychology compared with those of the hard sciences? Larry Hedges (1987) decided to find out. He compared the replicability of findings in particle physics, ostensibly one of the most rigorous domains of physics, with those of several areas in psychology, including the effect of teacher expectations on students' IQ scores, gender differences in verbal and spatial ability, the effects of desegregation on educational achievement, and the validity of student course evaluations. Using various statistical metrics of consistency, Hedges found that the results of particle physics studies aimed at estimating the mass or lifetime of stable subatomic particles (e.g., the muon) were in general no more consistent than those of

psychology. Hedges's findings suggest that the claim that psychology's results are far less dependable than those of physics are not supported by data.

Still, we should not overstate the implications of Hedges's (1987) findings. As Hedges acknowledged, he did not sample randomly within either physics or psychology, so his results may be unrepresentative of the domains within these broad fields. Nevertheless, Hedges observed that the results of studies in several other domains of physics, including the estimation of chemical and thermodynamic constants, appear to be about equally consistent (or inconsistent, depending on whether one chooses to view the glass as half full or half empty) as those within many domains of psychology.

That said, George Howard (1993) pointed out that there is one respect in which psychology is undeniably softer than physics: the ability to generate successful predictions (see also Pigliucci, 2010). Because psychology's "causal density" (Manzi, 2010, para. 10)—the sheer number of causal variables—tends to be much higher than that of physics, its capacity to produce successful predictions about human actions, such as voting behavior, the recidivism of released prison inmates, or the outcome of psychotherapy, is usually modest (see Cohen, 1990; Meehl, 1978). Still, even in these and numerous other domains, psychology's ability to generate successful predictions far exceeds chance (Meyer et al., 2001).

"Psychology Cannot Make Precise Predictions"

Extending Howard's (1993) argument, some critics have decried the fact that psychology's predictions are highly probabilistic. Such probabilistic predictions, the argument continues, render psychology unscientific. To take merely one example, an Amazon.com reviewer of a book on psychological misconceptions that I coauthored (Lilienfeld et al., 2010) ridiculed the notion that "sociologists, psychologists [*sic*], anthropologists, etc [*sic*] actually are engaged in scientific pursuits because they can hook numbers up to their assertions, even though the conclusions are spurious at best" (Leach, 2010). He seized on the fact that across studies, estimates of the percentage of children who suffer negative psychological aftereffects following divorce range from 15% to 25%. The reviewer continued,

They [the authors] expose the "myth" that divorce has a deleterious [*sic*] effect on the children of divorced parents. They cite two "well designed" experiments, in fact surveys of such children, one that shows 15% of such children suffer ill effects and another that shows 25% suffer. GOOD GRIEF!! How could such "experiments" produce such widely variable results?? If Gallileo [*sic*] had such results when he rolled his round balls down his inclined planes he certainly would have concluded that his notions of the constancy of gravity were all wet. (Leach, 2010)

The reviewer's comments imply that any discipline that yields a fairly wide range of results in its predictions is of negligible scientific value. Yet as Paul Meehl (1978) reminded us, statistical associations in psychology, in contrast to those in physics, tend to be highly stochasticological

(probabilistic), in part because these associations are often context dependent (see also Cronbach, 1975). For example, the relation between divorce and negative outcomes in children is almost certainly conditional on such variables as the severity of conflict between parents, the degree of psychopathology in one or both parents, the emotional resilience of the children, the race and culture of the family, and so on. Even such figures as the heritability of measured intelligence can vary markedly across samples and populations; for example, intelligence appears to be substantially less heritable in poor than in rich samples (Turkheimer, Haley, Waldron, D'Onofrio, & Gottesman, 2003). As a consequence, it is probably impossible to assign firm numerical values to such statistics as the percentage of children who suffer ill effects following divorce, heritability of IQ, or correlation between impulsivity and physical aggression.

Hempel's (1942, p. 42; see also Hempel & Oppenheim, 1948) concept of the "explanation sketch" and von Bertalanffy's (1972) concept of the "explanation in principle" are relevant in this regard. Many psychological theories are useful, albeit incomplete, explanations of natural phenomena. This incompleteness stems in part from the enormous number of moderating variables in any given case, as well as from a lack of knowledge of the values of these variables. As Hempel (1942) observed, the "filling-out [of the explanation sketch] requires further empirical research, for which the sketch suggests the direction" (p. 42).

Psychology is far from the only scientific discipline that trades in highly probabilistic predictions. Many sophisticated natural sciences, like meteorology and seismology, yield highly probabilistic predictions with wide confidence intervals (Sherden, 1998). This fact does not obviate these disciplines' scientific status, because some of the operative causal variables are unknown, and the precise levels of many of the operative causal variables that are known are unknown at any given moment. One might reasonably contend that the same principle holds even in more traditionally "deterministic" sciences (see Nisbett, Fong, Lehman, & Cheng, 1987, p. 630), such as physics and chemistry. Even Nobel Prize-winning physicist Richard Feynman (1995) acknowledged this point:

Physics has given up. We do not know how to predict what would happen in a given circumstance, and we believe now that it is impossible—that the only thing that can be predicted is the probability of different events. (p. 135)

Berscheid (1986) quoted Gombrich (1979) in advancing similar arguments with respect to natural scientists' ability to generate precise predictions regarding individual events:

I ask you to imagine the response of an expert in thermodynamics if I were to ask him or her when the pot of stew sitting on my stove is going to boil, or the response of a classical physicist if we were to ask him or her to plot the fall of a single snowflake. (p. 284)

"Psychology Is Not Useful to Society"

The results of the APA Benchmark Study (Penn, Schoen and Berland, Associates, 2008) reviewed earlier suggest that many laypersons do not recognize psychology's applicability to many traditionally nonpsychological domains, such as physical disease and crime prevention. As I also noted earlier, other survey data (e.g., Janda et al., 1998) show that many laypersons and academicians doubt the contribution of psychology to society; most find psychology considerably less valuable than other disciplines, including biology, chemistry, physics, and economics.

In all fairness, one could make a reasonable argument that these fields have made more significant and enduring contributions to society than has psychology. Nevertheless, claims that psychology has proven largely useless to society are clearly unwarranted. The contributions of psychological science to contemporary society are far too myriad to enumerate and could easily occupy an entire issue of *American Psychologist*, but a handful are worth listing here (also see <http://www.decadeofbehavior.org/specialpublications.cfm> and Lilienfeld, Lynn, Namy, & Woolf, 2011, and Zimbardo, 2004, for selective summaries):

- Operant conditioning techniques derived from the laboratory have proven useful across a variety of domains, including teaching autistic individuals language, managing the behavior of children with conduct disorders, and training animals (Grasha, 1997).
- Psychology has been at the forefront of the construction and validation of aptitude tests used to measure intelligence and other abilities, standardized tests for college and graduate admission, and personnel selection tests for employees (Zimbardo, 2004).
- Research from applied social psychology has helped to reform eyewitness lineups to minimize error. As a consequence of this research, police departments in the United States are increasingly turning to sequential lineups (in which eyewitnesses are shown one suspect at a time) in lieu of more traditional simultaneous lineups (in which eyewitnesses are shown all suspects at the same time; Wells, Memon, & Penrod, 2006).
- Work on the psychology of human memory has helped triers of fact to appreciate that memory is much more malleable than previously assumed and has exerted a substantial impact on legal decisions (Loftus, 1997).
- Research by psychologists in social cognition has revolutionized economic models and moved economics away from standard rational choice models (which assume that people rationally weigh the costs and benefits of financial decisions) to better supported models that acknowledge that financial decision making is influenced by a plethora of biases (such as overweighting losses under certain conditions of risk and underweighting losses under other conditions; Ariely, 2008; Tversky & Kahneman, 1992).

- Perception researchers have helped to improve the safety of vehicles and apparatuses; for example, research showing that lime-yellow objects are easier than red objects to detect in the dark has led to a gradual change in the color of fire engines (Solomon & King, 1995).

Public Skepticism Toward Psychology: Eight Sources

In all likelihood, the sources of public skepticism toward psychology are multifarious. In this section, I offer my candidates for eight prime culprits, although this list is surely not exhaustive. As we will discover, some of these sources reflect cognitive errors or public misunderstandings of science, psychological science in particular. Yet several other sources, especially the first two, point to systemic difficulties within professional psychology itself and underscore the need to get our own house in order.

Psychology's Failure to Police Itself

Like many unjustified beliefs (Lilienfeld et al., 2010), the belief that psychology is nonscientific probably contains a kernel of truth. The general public can hardly be blamed for holding a negative view of certain domains of psychology because our field has been slow to police its own questionable practices (Baker, McFall, & Shoham, 2009; Benjamin, 2003; Tavris, 2003). Indeed, survey data from both psychotherapy clients and practitioners suggest that questionable science is thriving in some sectors of psychology, especially clinical and educational practice (Gambrill, 2005; Lilienfeld et al., 2003; see also Lilienfeld, Wood, & Garb, 2006, for survey data on the prevalence of questionable assessment practices).

Studies show that approximately one third of children with autistic disorder receive scientifically unsupported interventions, such as sensory-motor integration therapy and facilitated communication (Levy & Hyman, 2003); most people with clinical depression or panic attacks are receiving scientifically unsupported interventions for these conditions, such as acupuncture, herbal remedies, and yoga (Kessler et al., 2001); about a fourth of licensed clinical psychologists use suggestive techniques, such as repeated prompting and cueing, hypnosis, and guided imagery, to "recover" purported memories of past abuse, even though these techniques are associated with a heightened risk of false memories (Polusny & Follette, 1996; Poole, Lindsay, Memon, & Bull, 1995); and half or more of clinicians who treat patients with obsessive-compulsive disorder or post-traumatic stress disorder (PTSD) do not use exposure-based therapies, the clear scientific interventions of choice for these conditions (Becker, Zayfert, & Anderson, 2004; Freiheit, Vye, Swan, & Cady, 2004). A recent survey revealed that approximately 90% of psychologists treating PTSD in the Veterans Administration system do not use any of the evidence-based treatments recommended by the U.S. government (M. Russell & Silver, 2007).

Although approximately 3,500 self-help books are published each year, only about 5% of them are subjected

to scientific testing (Arkowitz & Lilienfeld, 2006b). Some of these books dispense scientifically grounded advice and have been shown in meta-analyses to be efficacious for anxiety, depression, and other problems (Gould & Clum, 1993; Hirai & Clum, 2006). In contrast, many best-selling self-help books rest on feeble scientific foundations (Rosen, Glasgow, & Moore, 2003). For example, a number of popular self-help books provide readers with checklists that purport to contain specific signatures of symptoms (e.g., giving too much to others in relationships, concerns about body image, frequent daydreaming) for ascertaining whether they were sexually abused in childhood (Woodiwiss, 2009). Nevertheless, these checklists barely distinguish sexually abused from nonabused women at better than chance levels (Emery & Lilienfeld, 2004). Other self-help books advance claims that go well beyond available data. Psychologist John Gray's enormously popular *Men Are From Mars, Women Are From Venus* book series (e.g., Gray, 1992), which in aggregate has sold over 40 million copies and was second only to the Bible in book sales during the 1990s, implies that men and women differ so vastly in their communication styles that we can regard them metaphorically as inhabiting different planets (Lilienfeld et al., 2010). Yet meta-analyses show that gender differences in most communicative variables, such as frequency of interruptions, amount of self-disclosure, and sheer verbal productivity, are at best small in magnitude (Hyde, 2005; see also Zimmerman, Haddock, & McGeorge, 2001).

Despite the insinuation of dubious science into much of mental health practice, some psychologists, including a number of the field's chief spokespersons, have resisted the movement to establish criteria and lists for empirically supported therapies (ESTs), treatments that have been demonstrated to work in replicated controlled trials (Chambless & Ollendick, 2001). In all fairness, some criticisms of the EST movement may well have merit, such as the assertions that current EST lists (a) probably include some interventions that have not been tested against rigorous control conditions (Westen, Novotny, & Thompson-Brenner, 2004; but see Weisz, Weersing, & Henggeler, 2005), (b) rest on an overly simple dichotomization of treatments as either empirically supported or not (Arkowitz & Lilienfeld, 2006a), (c) overemphasize the potency of specific relative to nonspecific effects in psychotherapy (Wampold, 2001), (d) do not identify which features of treatments exert specific therapeutic effects (J. D. Herbert, 2003), (e) emphasize psychological techniques at the expense of underlying principles of therapeutic efficacy (Rosen & Davison, 2003), and (f) neglect to consider the theoretical plausibility of treatments (David & Montgomery, in press). Such criticisms do not reflect a rejection of scientific evidence per se or imply that alternative approaches to knowledge (e.g., clinical intuition) are as valuable as science in ascertaining treatment efficacy (see O'Donohue et al., 2007). To the contrary, they are signs of healthy debate in the field regarding the best means of operationalizing evidence-based practice (Westen, Novotny, & Thompson-Brenner, 2005).

Nevertheless, several other criticisms of the EST criteria and lists appear to reflect a partial or wholesale rejection of the primacy of scientific evidence in ascertaining therapeutic efficacy. A former president of APA, Ronald Fox (2000), wrote, "Psychologists do not have to apologize for their treatments. Nor is there any actual need to prove their effectiveness" (p. 5). Another ex-APA President, Ronald Levant (2004), argued against ESTs on the grounds that clinical experience and intuition should be accorded equal status with the best available scientific evidence when deciding which psychological treatments to administer: His preferred operationalization of evidence-based practice "does not imply that one component is privileged over another" (p. 223; but see the APA Presidential Task Force on Evidence-Based Practice, 2006). In another critique of ESTs, Bohart (2005) argued that clinical psychology should make room for "alternative forms" of knowledge, including "practical wisdom" (p. 48).

From the perspective of Fox, Levant, Bohart, and others (see also Hunsberger, 2007, and Hoshmand & Polkinghorne, 1992), the EST movement is misguided because it places undue constraints on subjective clinical judgment. Moreover, these critics contend, the EST movement implies erroneously that scientific evidence should be weighted more heavily than clinical intuition when making treatment decisions. Yet decades of research show that error rates typically increase when practitioners routinely override well-established data with their informal impressions (Dawes, Faust, & Meehl, 1989). The critics' arguments overlook the crucial point that science, although imperfect, is our best set of safeguards against manifold sources of human error, including confirmation bias, hindsight bias, and illusory correlation (O'Donohue et al., 2007). They also neglect the history of medicine, which shows that exclusive reliance on clinical experience has often resulted in suboptimal and at times disastrous practices (Grove & Meehl, 1996).

Still other critics contend that ESTs are inherently limited because all clients are different and therefore cannot be expected to respond equally well to the same treatment (e.g., Nordal, 2009). Yet this argument disregards the point that, just as in medicine, some evidentiary basis for generalization to the individual clinical case is surely better than none (Dawes et al., 1989). Science is by no means infallible, but it is our best means of reducing uncertainty in our clinical inferences (McFall, 2000). Moreover, meta-analyses designed to detect moderators of treatment response are more likely than informal clinical impressions to yield accurate information about who is most likely to benefit from a given intervention.

The Problematic Public Face of Psychology

To most Americans, the public face of psychology is not represented by psychological researchers or scientifically minded psychotherapists (Stanovich, 2009). Instead, to most Americans, psychology's public face is represented largely by such media personalities as Dr. Phillip McGraw ("Dr. Phil") and Dr. Laura Schlessinger (who is not even a psychologist; her PhD is in physiology, although she holds

a certificate degree in marriage, family, and child counseling), both of whom routinely dispense confident and unqualified psychological advice on the basis of minimal clinical information (Arkowitz & Lilienfeld, 2009). Dr. Phil has also advanced a number of claims that run counter to scientific evidence (see also Lilienfeld, 2002, for a discussion of Dr. Laura's questionable views of psychological research and meta-analysis). Among other things, on his television show Dr. Phil has endorsed the polygraph test as a "foolproof" technique for ascertaining whether an individual is a sexual predator (Furedy, 2005) and claimed that electroencephalograph biofeedback is a recommended treatment for attention-deficit/hyperactivity disorder despite a striking paucity of evidence that this treatment is more efficacious than placebo (Barkley, 2006). Yet despite Dr. Phil's endorsement of several nonscientific claims, in 2006, the APA selected Dr. Phil as its invited speaker to highlight the effective communication of psychology to the public (Meyers, 2006). The APA presented Dr. Phil with a Presidential citation that read, "Your work has touched more Americans than any other living psychologist" (Meyers, 2006, para. 9).

Furthermore, in sharp contrast to the late decades of the 19th and early decades of the 20th centuries, when many prominent psychologists (e.g., John B. Watson, William James, E. L. Thorndike, Mary Whiton Calkins, and Hugo Munsterberg; see Benjamin, 2006) wrote for popular magazines, including *Harpers*, *Atlantic Monthly*, and *Popular Science Monthly*, few scientific psychologists today write for the general public. The number of psychologists authoring articles for popular magazines plummeted by 300% between the 1870s and 1930s and has not rebounded since (Benjamin, 2006). The primary popular magazine whose title contains the word *psychology*, *Psychology Today*, has a readership of more than 3 million people. Initiated in 1967, its early issues featured scientifically grounded and entertaining articles by eminent research psychologists, including Stanley Milgram, Philip Zimbardo, Nathan Azrin, Hans Eysenck, and David Lykken. Yet beginning in the 1970s and extending into the 1980s, *Psychology Today* shifted in content and style to appeal to a more general audience, featuring articles on such pop psychology topics as love, relationships, work, and happiness, most of them written by nonexperts from a largely nonscientific perspective (Benjamin & Bryant, 1997). Despite a brief and ill-fated attempt by the APA to rehabilitate the magazine in 1983 (Pallak & Kilburg, 1986), *Psychology Today's* lack of scientific rigor persists today. Moreover, arguably the lone high-quality psychology magazine geared to the general public, *Scientific American Mind*, which launched in 2004, does not even contain the word *psychology* in its title or its subtitle (*Behavior, Brain Science, Insights*), perhaps reflecting a desire to appeal to more scientifically inclined readers.

Confusion Between Psychologists and Psychotherapists

There is reason to believe that many laypersons regard psychology largely as a helping profession, not as a sci-

tific discipline (Hartwig & Delin, 2003). The findings here are more fragmentary, dated, and mixed than we might like, but those that are available support this concern. Using content analyses of essays describing various professions, Webb and Speer (1986) reported that although undergraduate students and their parents hold a generally positive view of psychology, they perceive psychologists as extremely similar to psychiatrists ($r = .98$) and as extremely dissimilar ($r = .11$) to scientists. In addition, they found that whereas scientists were seen as tough minded and as focused on normality, psychologists were seen as tender minded and as focused on abnormality. In their discussion, Webb and Speer commented on the high prevalence of what Korn and Lewandowski (1981, p. 149) called "the clinical bias"—the erroneous assumption that most psychologists are therapists—among members of the general public. Findings by Rosenthal, McKnight, and Price (2001) bear out the existence of this bias, at least among college students. They found that undergraduates estimated that 67% of psychologists are clinical, counseling, or school psychologists when the actual figure was 50%, and that undergraduates estimated that 56% of psychologists are in private practice when the actual figure was 39% (but see Rosenthal, Soper, Rachal, McKnight, & Price, 2004, who did not find evidence for such overestimation).

Potentially contributing to the confusion of psychology with psychotherapy are findings pointing to "role diffusion" (Schindler, Berren, Hannah, Beigel, & Santiago, 1987, p. 372) in the public perception of different therapeutic professionals. Data show that many, although by no means all (see Wood et al., 1986), laypersons confuse psychologists with psychiatrists, social workers, counselors, and other psychotherapists. In a survey of over 1,000 Americans, Farberman (1997) found that large percentages "cannot tell one mental health professional from another" (p. 128); J. L. Wong (1994) similarly reported that only half of a sample of 286 college students and staff felt that they could distinguish among psychologists, psychiatrists, and psychoanalysts. Such confusion may stem in part from misleading coverage by the entertainment media. For example, many films refer to psychologists and psychiatrists interchangeably or blur the boundaries between psychologists and psychiatrists by depicting the former as prescribing medication (Schneider, 1987; von Sydow & Reimer, 1998).

On the one hand, the overestimation of the proportion of psychologists who are psychotherapists, as well as the confusion between psychologists and other mental health professionals, may not necessarily be problematic given survey data that most laypersons perceive psychotherapy as helpful (Hartwig & Delin, 2003; J. L. Wong, 1994). On the other hand, to the extent that laypersons overestimate the proportion of psychologists who are professional helpers, they almost certainly underestimate the proportion of psychologists engaged in other pursuits, including basic and applied scientific research.

Since 1996, the APA has embarked on a large-scale public education campaign to destigmatize mental illness, enhance the reputation of psychotherapy in the eyes of the

general public, and offer laypersons information about how and where to obtain help for psychological problems (<http://www.apa.org/practice/programs/campaign/background.aspx>). There is much to commend in this effort. It is not clear, however, whether this campaign will enhance public understanding of the differences between practicing and research psychologists or heighten the public's appreciation of the value of psychological science. To accomplish these goals, APA may need to prescribe alternative remedies, which I address later (see the Concluding Thoughts and Recommendations section).

Hindsight Bias

As I noted earlier, many laypersons appear to view most psychological knowledge as obvious. Although there are probably several reasons for this belief (Stanovich, 2009), a crucial one on which I focus here is hindsight bias (the "I knew it all along" effect), the tendency to perceive outcomes as foreseeable once we know them. In the case of psychological knowledge, hindsight bias often takes the form of the "feeling of obviousness" (L. Y. Wong, 1995, p. 504). Once we learn of a psychological finding, it frequently appears self-evident (Gage, 1991; Kelley, 1992; Myers, 1994). Because we humans are meaning-seeking organisms (Shermer, 2002), we almost always manage to concoct a plausible explanation of a psychological finding after the fact.

In a clever study, Baratz (1983) asked undergraduates to read 16 pairs of statements describing psychological findings and asked them to evaluate how likely they would have been to predict each finding; each pair consisted of a finding and its opposite. For example, participants read, "People who go to church regularly tend to have more children than people who go to church infrequently," but they also read, "People who go to church infrequently tend to have more children than people who go to church regularly"; they read, "Single women express more distress over their unmarried status than single men do," but they also read, "Single men express more distress over their unmarried status than single women do" (see Gage, 1991, p. 14). Across the board, most participants rated each statement as what "I would have predicted." Baratz's findings suggest that many people find psychological findings commonsensical only because they judge them retrospectively as obvious once they learn about them (see L. Y. Wong, 1995, for comparable findings in the domain of educational psychology research).

The Illusion of Understanding

To many people, psychology seems "easier" than physics, chemistry, and other hard sciences. Why? Frank Keil and his colleagues (Keil, Lockhart, & Schlegel, 2010, p. 4) asked children to rate the difficulty of several questions from five disciplines—physics, chemistry, biology, economics, and psychology—that a sample of adults had ranked previously as equally difficult to answer. Questions from physics included "How does a top stay spinning upright?" and "Why does light travel faster than sound?" whereas those from psychology included "Why is it hard to

understand two people talking at once?" and "Why can children learn new languages more easily than adults?" Despite these questions' being matched for difficulty by adults, children in the second, fourth, sixth, and eighth grades (but not kindergartners) rated psychological phenomena easier to explain than those in the natural sciences and, in many cases, economics. It is interesting that, in a second study, children from kindergarten to the eighth grade, but not adults, found questions within psychology that deal with neuroscience (e.g., "How does your brain know when to have you wake up?") more difficult than those in other psychological domains, such as personality (e.g., "Why do some people sometimes lie about something bad they did?") and cognitive psychology (e.g., "How do you recognize yourself in the mirror?"; p. 6). So even within psychology, children perceive domains closer to hard sciences (namely, neuroscience) as more difficult than those closer to soft sciences.

As Keil and his colleagues (2010) observed, there is probably no objective benchmark for ascertaining which scientific disciplines are more inherently difficult than others. Yet it is intriguing that even preschool children display a pronounced bias toward perceiving psychological phenomena as easier to explain than other sciences. The reasons for this proclivity are unclear, although Keil and collaborators conjectured that psychological phenomena may seem easier because they are (a) more subjectively immediate and (b) easier to control. With respect to immediacy, we have direct contact with our behaviors, thoughts, memories, and emotions, whereas we do not have such contact with our genes, let alone our subatomic particles. We also have a great deal of experience interacting with others and with anticipating and interpreting their behaviors. As a consequence, by drawing on the familiarity heuristic (W. Herbert, 2010), we may come to confuse familiarity with comprehensibility. With respect to controllability, we can exert direct influence over our actions and often over our thoughts and feelings. In turn, we may confuse the ability to control a phenomenon with the ability to understand it (Keil et al., 2010).

Greedy Reductionism

We humans are "cognitive misers" (Fiske & Taylor, 1984, p. 12; see also Ruscher, Fiske, & Schnake, 2000, p. 241, for a discussion of humans as "motivated tacticians"), meaning that we tend to seek explanatory simplicity. This propensity is by and large adaptive, as it helps us to make sense of our often confusing everyday worlds. But it can lead us astray when it causes us to oversimplify reality. One likely manifestation of cognitive miserliness is a preference for parsimonious explanations. But as the scientific guideline of Occam's razor reminds us, we should generally select the simplest explanation only when it accounts for the evidence as well as alternative explanations (Uttal, 2003).

In this regard, the past several decades have witnessed a pronounced increase in the popularity of reductionist explanations of human behavior, especially those striving to reduce all psychological phenomena to neuroscience (Lilienfeld, 2007). The seductive appeal of reductionist

explanations probably stems in part from their seeming simplicity (Jacquette, 1994). In turn, such explanations have probably fostered the impression in the eyes of educated laypersons that neuroscience is more “scientific” than psychology. Weisberg, Keil, Goodstein, Rawson, and Gray (2008, p. 471) showed that merely inserting the words “brain scans indicate” (along with a few phrases of accompanying neuroscientific explanation) into logically flawed interpretations of psychological findings can render undergraduates—but not neuroscience experts—significantly more likely to find these interpretations persuasive (see also McCabe & Castel, 2008). If such findings prove generalizable to the general public, they suggest that brain imaging findings may often be held with a certain reverence in the minds of laypersons.

Critical to understanding the perils of a *neurocentric* view of psychology—one that regards neuroscience as inherently the most important level of explanation for understanding behavior—is the distinction between *constitutive* and *eliminative* reductionism, the latter termed *greedy reductionism* by Daniel Dennett (1995, p. 82). Constitutive reductionism, a relatively noncontroversial credo subscribed to by virtually all scientific psychologists, posits only that all “mind stuff” is ultimately “brain stuff” (or at least central nervous system stuff) at some level and that everything mental is ultimately material. In contrast, the far more radical program of eliminative reductionism proposes that the neural level of explanation will eventually gobble up all higher levels of explanation, including the psychological, much like a greedy computerized Pac-Man cannibalizing everything in its path. From the standpoint of the eliminative reductionist, psychology will eventually be rendered superfluous as a subject matter, because advances in neuroscience will one day allow scientists to translate all thoughts, emotions, and behaviors into strictly neural language.

In accord with the tenets of eliminative reductionism, some skeptics of psychology’s scientific status have gone further, arguing that this field affords few insights that cannot be reduced to more fundamental levels of analysis. As G. A. Miller (2010) observed in a provocative analysis brimming with dozens of vivid examples, such pronouncements have become customary in the popular press. As one illustration, Miller cited a *New York Times* op-ed piece on Abraham Lincoln’s depression: “Lincoln suffered from recurring episodes of what would now be called depression from early childhood onward. In light of what we know today, an effort to link them to emotional disappointments rather than to a chemical imbalance seems quaint rather than scientific” (Schreiner, 2006, p. A19). The reasoning here implies that depression is necessarily better viewed at the level of a chemical imbalance than at the level of a psychological dysfunction. Even setting aside nagging scientific questions concerning the chemical imbalance model of depression (see Kirsch, 2010; Lacasse & Leo, 2005), the notion that depression is a chemical imbalance at one level of analysis in no way precludes the possibility that it is a psychological disorder at a different level (Lilienfeld, 2007).

Endorsements of eliminative reductionism (see also Guze, 1989) also neglect the possibility of *emergent properties*, higher level capacities that result from interactions among lower order elements (Chalmers, 2006; Meehl & Sellars, 1956). Traffic jams and crystals, for example, are emergent phenomena that cannot be reduced entirely to their lower order constituents: The whole differs from the sum of its parts (Calvin, 1996). Even if emergent properties do not exist in psychology—and some scholars doubt they do (Churchland, 1984)—we are still a long way away from a full explanatory reduction of human psychological capacities to the neural level of analysis. For the foreseeable future, the psychological level of explanation will offer indispensable contributions to the scientific understanding of thinking, feeling, and acting (Lilienfeld, 2007).

As Kendler (2005) observed, this level is also likely to be the most fruitful for the lion’s share of psychological interventions. He noted that brain scans are of scant help when counseling a client who is struggling with a potential career change from scientific research to the priesthood. Instead, constructs at the psychological level of explanation, such as vocational interests, personality traits, perceived social pressures, and emotional conflicts, are far more germane to treatment planning in such a case.

The Scientific Impotence Excuse

As we have seen, some psychological findings conflict sharply with our commonsense intuitions: Similar, not opposites, tend to attract; when it comes to bystander intervention in emergencies, there is typically danger rather than safety in numbers; and expressing pent-up anger typically increases, not decreases, hostility (Lilienfeld et al., 2010). When people are confronted with findings that challenge their preconceptions, they occasionally are willing to forsake their beliefs. But more often than not, they respond by dismissing a scientific approach to the questions at hand, a reaction that Munro (2010) called the *scientific impotence excuse*.

In two investigations, Munro (2010) presented undergraduates with brief descriptions of studies that either confirmed or disconfirmed their stereotypes about homosexuality—specifically, their beliefs about whether homosexuality is a mental illness. When participants read information that contradicted their preexisting beliefs, they became more likely to conclude that scientific methods are inadequate for addressing questions concerning homosexuality. Moreover, this belief in scientific impotence generalized to an unrelated topic, namely, whether science can inform the question of whether the death penalty should be retained. So, participants whose beliefs were disconfirmed appeared to become more skeptical of scientific methods in general. If these results extend to other psychological domains, they raise the possibility that when scientific findings run counter to folk psychological beliefs, many people may conclude that scientific approaches are simply not up to the task of shedding light on human nature.

Failure to Distinguish Basic From Applied Research

Between 1975 and 1989, Wisconsin Senator William Proxmire became famous—or infamous—for his Golden Fleece Awards, satirical “honors” bestowed on researchers who he felt had flagrantly wasted taxpayers’ money (Benson, 2006; Shaffer, 1977). A number of Golden Fleece recipients were psychologists who had obtained federal funding for research that Proxmire deemed risible. Some might reasonably contend that a few of Proxmire’s awards were well deserved, such as a study of how long male drivers honked their horns at women wearing miniskirts of differing lengths as a function of drivers’ stress levels (see Atkinson, 1984, for a discussion).

Yet several other Golden Fleece Awards (see Hunt, 1999, for other Congressional misunderstandings of psychological research) reflected a widespread logical error: a failure to distinguish *basic* research—research designed to uncover fundamental scientific principles—from *applied* research—research designed to solve practical, real-world problems (Hoffman & Deffenbacher, 1993). Basic research frequently entails the use of scientific *models*, which examine phenomena that are not the focus of interest per se but that allow investigators to isolate potential causal variables of interest.

When laypersons or politicians neglect to appreciate the distinction between basic and applied research, they may conclude erroneously that researchers are interested in the topic of study per se rather than using this topic as a model system for investigating deeper psychological principles. This error, in turn, can mislead them into concluding that potentially important psychological research is frivolous. In 1980, Proxmire bestowed the Golden Fleece on psychologist Robert Kraut for his work on “why bowlers smile” (Kraut, 2006, para. 1; Kraut & Johnston, 1979). In fact, Kraut was not interested in the smiling behavior of bowlers (or hockey fans and pedestrians, whom he also studied) themselves. Instead, he used bowlers, among other groups, as real-world models for understanding the effects of social stimuli on emotional expressions. Ironically, Kraut and Johnston’s (1979) study, which demonstrated that smiling often serves more of a communicative than an emotional function, is a citation classic (an article cited more than 100 times in the literature) that helped to give birth to the now burgeoning field of evolutionary psychology (Diener, 2006). As another example, in 1988, Proxmire awarded the Golden Fleece to psychologist Michael Domjan for his now classic work on the mating habits of Japanese quail (Domjan & Hall, 1986). But Domjan was not interested in the reproductive habits of Japanese quail per se; he instead used quail as a model species to better understand the mechanisms of classical (Pavlovian) conditioning of sexual behavior in general, a topic of considerable theoretical and practical interest.

More recently, in 2003, the research of psychologist J. Michael Bailey of Northwestern University drew the ire of 20 Republican members of Congress. Bailey’s \$147,000 National Institutes of Health (NIH) grant study, which had

already been approved following peer review, asked participants to observe sexually arousing film stimuli and was intended to investigate the hypothesis that females’ sexual arousal is less tied to their sexual orientation than males’. Congressman Dave Weldon (R, Florida) complained that NIH was using its “money to pay women to watch pornography” (“Lawmakers Assail NIH Funding for Sexual-Arousal Conference,” 2003, para. 10), and the other 19 representatives similarly demanded an explanation for why NIH was funding studies involving salacious films (“University Investigates Ethics of Sex Researcher,” 2003). Putting aside questions concerning the scientific merits of Bailey’s research, such criticisms miss the point: that Bailey was using sexually arousing stimuli as a means of testing basic questions regarding sex differences in sexual arousal—differences that may hold significant implications for high-risk behaviors and their prevention. In July of 2003, a bill to strip funding for Bailey’s study and three others was defeated in Congress by the razor-thin margin of 212 to 210.

Concluding Thoughts and Recommendations

Although most laypersons view psychology at least somewhat positively, sizeable minorities do not (Benjamin, 1986), and many doubt its scientific status. Survey data suggesting improvement in the public opinion of psychology over the past few decades are encouraging, although such data indicate that our field still faces an uphill battle in the effort to assuage widespread doubts among laypersons. Many people continue to have a poor understanding of psychology’s scientific worth, especially its contributions to society and applicability to a broad array of everyday problems (Janda et al., 1998; Penn, Schoen, and Berland Associates, 2008). Regrettably, Wood et al.’s (1986) 25-year-old conclusion that most laypersons have “virtually no understanding of the impact of psychology on their lives” (p. 949) seems to hold today.

Some of the public skepticism toward psychology’s scientific status is unwarranted and is rooted in misunderstandings of both scientific epistemology and psychological knowledge. In particular, such skepticism largely neglects the fact that psychology relies on scientific methods—systematic tools designed to compensate for confirmation bias—and has generated a host of replicable findings in sensation and perception, learning theory, memory, emotion, social psychology, and clinical psychology, among other domains. In these subdisciplines, psychology has yielded helpful applications that many of us take for granted, such as the safety of appliances and vehicles, aptitude testing, political polling, eyewitness identification, financial decision making, and effective psychotherapy (Zimbardo, 2004).

What Are We Doing Wrong?

Still, psychologists should curb the facile temptation to place all of the blame for their field’s tarnished image on widespread public misunderstanding. At least some of psy-

chology's negative reputation appears to be deserved, as large pockets of the field, especially those pertaining to psychotherapy, remain mired in unscientific practices (Dawes, 1994; Lilienfeld et al., 2003). As Meehl (1993) noted, professional psychology has failed to "clean up [its] clinical act and provide . . . students with role models of scientific thinking" (p. 729). Moreover, the continued resistance on the part of some of our field's leaders to adopting evidence-based practices may hamper efforts to enhance psychology's blemished public image. Perhaps understandably, many laypersons are less familiar with psychology's scientific accomplishments (Zimbardo, 2004) than with its highly visible public embarrassments, such as the recovered memory debacle of the 1990s, which spotlighted many psychologists' use of suggestive and potentially harmful therapeutic techniques (Garry & Hayne, 2006). Moreover, the public face of psychology is often represented not by psychological scientists but by flashy media personalities who have routinely put forth psychological claims that have minimal scientific grounding (Stanovich, 2009).

Exacerbating the problem, many psychological researchers, practitioners, and teachers have been reluctant to devote any of their time to disseminating good science to the public, combating bad science, and correcting misconceptions of their field (Benjamin, 2003). When it comes to confronting the threats posed by questionable science or pseudoscience, academicians have typically stayed out of the fray, preferring to concentrate on their research, grant seeking, and teaching (Lilienfeld, 1998). This reluctance to confront pseudoscience in the public arena is understandable given the mounting pressures on university and college psychologists to publish peer-reviewed articles and obtain federal funding, but it has come at a dear cost: their field's poor public image.

All of this leads to several concluding recommendations, some individual, others institutional. The two sets of recommendations are by no means mutually exclusive, but because they differ in emphasis and strategy, I separate them here.

Individual-Level Recommendations

First, psychologists must play a more active role in educating laypersons about their discipline's scientific side and in confronting their discipline's smaller but more publicly conspicuous nonscientific side (see Olson, 2009, for recommendations for communicating science to laypersons). To do so, they must be willing to venture out occasionally of their laboratories, therapy rooms, and classrooms to communicate scientific psychology to the public. For example, perhaps because pharmacological companies are blessed with much larger advertising budgets than psychologists, the public probably underestimates the efficacy of evidence-based psychotherapies relative to medications for depression and several other conditions (Nordal, 2010). Clinical psychologists therefore have a valuable role to play in educating mental health consumers about the substantial research base for psychotherapy.

Just as important, within academia, administrators and department chairs must be willing to reward, not punish, faculty who take the time and effort to disseminate psychological science to the public. Although there are few formal data in this regard, most suggest that a negative attitude toward popularization is prevalent in academia. In a national survey of 287 social and physical scientists (percentages for these two groups were not separated in the analyses), Dunwoody and Ryan (1985) found that 67% agreed that "scientists are not rewarded within the scientific community for having their work reported in the popular media" (p. 32), and 47% agreed that "scientists who allow their work to be publicized in the popular media are more likely to be criticized than praised by their peers" (p. 32). More recently, in their critique of the "anti-popularization sentiment" (p. 77) common in the science world, Mooney and Kirshenbaum (2009) offered a number of examples of the "Carl Sagan phenomenon," in reference to the eminent Cornell University astrophysicist, science writer, and media personality who was "persecuted during his lifetime by much of the scientific establishment for being too much of a 'popularizer'" (p. 77).

Still, there are encouraging signs that the trend away from the popularization of psychology by psychological scientists may gradually be reversing. In recent years, a number of prominent academic psychologists, such as Steven Pinker (2002), Daniel Gilbert (2006), Daniel Ariely (2008), and Christopher Chabris and Daniel Simons (Chabris & Simons, 2010), have written successful trade books for the general public that portray psychological findings in an accurate and engaging fashion. Perhaps the tide is turning, but it is too early to tell.

Second, when explaining psychological findings to the general public, psychologists must remain cognizant of the fact that many misconstruals of the subject matter stem from what we might term *understandable misunderstandings*. Because psychology is part and parcel of our everyday lives and is subjectively immediate (Keil et al., 2010), many laypersons assume it to be intuitively obvious. Yet such intuitiveness is frequently deceptive, because it may be associated with various illusions of understanding marked by the sense that we comprehend things better than we do (Chabris & Simons, 2010). For example, Rozenblit and Keil (2002; see also Lawson, 2006) found that many people believe they understand the workings of everyday appliances, such as toilets, zippers, and sewing machines, far better than they actually do. If such findings generalize to the human mind, they may suggest a similar tendency on the part of many laypersons to be much more confident in their understanding of basic psychological principles than is warranted.

In any case, when attempting to rebut criticisms of our work in the public square, we must recognize that merely pointing out the merits of our research projects will often be insufficient. We must also be prepared to acknowledge the understandable bases for skepticism of our investigations, such as the views that psychology is mostly commonsense knowledge or usually trivial in real-world importance. For example, when confronting policymakers'

claims that “this is all obvious,” we may need to explain that many psychological findings seem self-evident but only in retrospect, and remind them of the scores of counterintuitive results yielded by psychological research. When addressing concerns that our findings are exceedingly unlikely to be pragmatically useful, we may need to remind policymakers that basic psychological research on learning, memory, sensation, perception, and the like has led to unanticipated benefits across a host of applied domains, and we should be prepared to illustrate this point with easily grasped examples. Or when responding to complaints that our research is frivolous, we must be prepared to explain the distinction between basic and applied research and to note that psychological scientists frequently use model systems as vehicles for understanding much broader psychological phenomena. More generally, rather than reflexively viewing policymakers’ mistrust of psychological research as stemming from “anti-intellectualism” (e.g., Shaffer, 1977, p. 814), it may be more profitable to conceive of it as a gap in understanding regarding how psychological scientists approach, obtain, and apply knowledge. This gap, it is worth noting, is attributable in part to our field’s collective failure to effectively articulate the methods and fruits of psychological science to the public.

Third, although thoughtful debates concerning the best means of operationalizing evidence-based practice (J. D. Herbert, 2003; Westen et al., 2005) should continue, practitioners within the applied fields of psychology (e.g., clinical, counseling, school) would be well advised to become less tolerant of pseudoscience and more willing to ground their practices in replicated research evidence. Perhaps running counter to the stereotypes of some academicians, surveys of practicing clinicians offer reason for cautious optimism in this regard. Such surveys reveal that large pluralities or even majorities are favorable to the concept of evidence-based treatments, including manualized therapies, and to incorporating at least some of them in their practices (e.g., Addis & Krasnow, 2000; McGovern, Fox, Xie, & Drake, 2004), although many are not persuaded that they can readily apply these interventions to their everyday clinical work. Therefore, clinical researchers must be willing to spend more of their time communicating their findings regarding therapeutic efficacy to practitioners and to addressing practitioners’ concerns regarding the transportability of psychotherapies to real-world practice settings. If they were to do so, they might often find a more receptive audience than they anticipated.

Institutional-Level Recommendations

These individual-level recommendations, although important, are probably not sufficient to address the problem of public skepticism toward psychology’s scientific basis. An APA Presidential Task Force recently outlined several recommendations for enhancing the “recognition of psychology as a science and as a STEM discipline” (APA 2009 Presidential Task Force on the Future of Psychology as a STEM Discipline, 2010, p. 15), including public education campaigns to better inform laypersons regarding the ap-

plied value of psychological science, capitalizing on news events that highlight psychology’s contributions to society, increasing collegial interchanges between psychologists and scientists in traditional STEM disciplines, and expanding advocacy efforts to include psychology in STEM training programs. I echo all of these recommendations but wish to go further in proposing that our professional organizations take the lead in bringing about three more fundamental changes in the profession.

First, APA, APS, and other major professional organizations must step up their efforts to include psychological scientists—including researchers, scientifically rigorous practitioners, and teachers—in regular media coverage. As it now stands, television stations typically turn not to research psychologists or scientifically minded psychotherapists but to physicians (e.g., Dr. Sanjay Gupta, Dr. Drew Pinsky) or pop psychologists to comment on psychological news stories. To the extent that we can encourage scientifically informed psychologists to serve as media point persons for psychology news stories, the public perception of our discipline as a science may be markedly enhanced.

Second, professional organizations must help the general public to better grasp the distinctions between psychology and allied professions. As noted earlier, data on role diffusion (Schindler et al., 1987) suggest that the public often perceives psychology as similar to psychiatry and related practice fields and does not appreciate psychological science’s unique contribution to alleviating mental health ailments or broader societal problems. In response to these misperceptions, professional organizations must continually underscore the point that trained psychologists are virtually unparalleled among rival professions in one crucial respect: our ability to apply scientific reasoning and rigorous methodology to assessing, evaluating, and alleviating human problems, whether they be mental health difficulties, such as depression or anxiety disorders, or broader societal difficulties, such as prejudice or blind obedience (see also N. Hayes, 1996; McFall, 1991). To do so, professional organizations must focus squarely on making clearer distinctions among helping professions and whenever possible avoid blurring them. For the past 15 years or so, APA has made achieving prescriptive authority a major goal for practicing psychologists. The merits or demerits of this proposal aside (see McGrath, 2010, and R. B. Stuart & Heiby, 2007, for arguments, both pro and con), APA must ensure that in pursuing prescription privileges and other practice rights, it does not inadvertently cloud the already murky distinctions among mental health professions, especially psychology and psychiatry, in the public eye.

Third, professional psychological organizations need to be much clearer about not only what they are for, but also what they are against. By doing so, they can help to forge a more cohesive scientific identity in the public eye (see also Dawes, 1995, on the distinction between *hortatory* and *minatory* standards of psychological practice). Specifically, APA, APS, and other organizations need to play a more active public role in distancing themselves from the plethora of therapeutic and assessment fads that

are either poorly supported by scientific evidence or that blatantly contradict such evidence (Dawes, 1994; Lilienfeld et al., 2003). The social psychological literature on persuasion reminds us that by establishing unambiguous comparisons to alternatives ("We support X, but we oppose Y"; see Pratkanis, 2007), organizations can sharpen their message. In the case of psychology, professional organizations would enhance their scientific credibility—and more important, ultimately enhance the perceived legitimacy of psychology at large—by adopting not only visible public stances for science but also visible public stances against questionable science and blatant nonsense.

Final Thoughts: Viewing Public Skepticism as Our Ally

Rather than viewing public skepticism of psychology as our enemy, we might instead best regard it as our stalwart ally. Such skepticism may allow us to anticipate potential objections to our research by laypersons and policymakers and to make a more compelling case for our field's long-term social import. In this way, we can harness public skepticism as an opportunity to help us become more effective communicators of psychological science. A better understanding of the reasons for the public skepticism of psychology may also afford us a valuable window into the sources of deep-seated misconceptions regarding human nature and point us toward educational interventions to alleviate them.

Finally, public skepticism of psychology may provide us with a much needed impetus toward getting our clinical house in order and winnowing out the elements of our profession that are scientifically dubious, some of which have tarnished our hard-fought credibility. In this respect, public skepticism may be an imperfect but nonetheless informative barometer of our field's scientific status. Just as former New York City Mayor Ed Koch (1981) became famous for asking his constituents "How'm I doing?" as a means of gauging his performance, we as a field should continually be asking the general public "How are we doing?" and be prepared to take their critical feedback to heart if their answers are not to our liking.

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Correction to Nisbett et al. (2012)

In the article “Intelligence: New Findings and Theoretical Developments,” by Richard E. Nisbett, Joshua Aronson, Clancy Blair, William Dickens, James Flynn, Diane F. Halpern, and Eric Turkheimer (*American Psychologist*, Vol. 67, No. 2, pp. 130–159; this issue), two correlational values are incorrect in the 10th line on p. 134. The relevant sentences should read, “It appears, for example, that socioeconomic differences in intelligence are not as pronounced in modern Europe as they are in the United States. In the Turkheimer et al. (2003) study, the correlation of SES with IQ was .46; in Asbury et al. (2005), it was about .2.”

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