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Can psychology become a science?

Scott O. Lilienfeld

Department of Psychology, Emory University, Room 473, 36 Eagle Row, Atlanta 30322, Georgia

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ABSTRACT

I am profoundly grateful to Tom Bouchard for helping me learn to think scientifically. Scientific thinking, which is characterized by a set of safeguards against confirmation bias, does not come naturally to the human species, as the relatively recent appearance of science in history attests. Even today, scientific thinking is in woefully short supply in many domains of psychology, including clinical psychology and cognate disciplines. I survey five key threats to scientific psychology – (a) political correctness, (b) radical environmentalism, (c) the resurrection of "common sense" and intuition as arbiters of scientific truth, (d) postmodernism, and (e) pseudoscience – and conclude that these threats must be confronted directly by psychological science. I propose a set of educational and institutional reforms that should place psychology on firmer scientific footing.

1. Can psychology become a science?

When I entered graduate school in psychology at the University of Minnesota in the Fall of 1982, I was a bright-eyed, bushy-tailed 21 year-old eager to learn about the mysteries of the mind. I was brimming with energy, intellectually curious, and deeply in love with psychology. Yet despite my undergraduate education at a superb institution, Cornell University, something important was conspicuously absent from my intellectual repertoire, although I did not realize it at the time. I had not learned how to think.

As one symptom of my dysrationalia, to use Stanovich's (2009) term, I confidently held a host of profoundly misguided beliefs about individual differences. Among other things, I was certain that:

- Genetic influences on most psychological traits are trivial.
- Genes and environments always interact.
- Genes and environments cannot be separated.
- IQ tests are invalid for predicting cognitive performance.
- IQ tests are strongly biased against minorities.

At the time, it never occurred to me that some of these beliefs were not only poorly supported, but contradictory. For example, it never crossed my mind that if one cannot separate the influences of genes and environments, there is no way of ascertaining whether genes and environments interact statistically. Nor did it cross my mind that for IQ tests to be biased against certain subgroups, they would need to possess above zero validity for at least one subgroup.

Of course, a naïve graduate student can perhaps be forgiven for such logical errors, especially one embarking on his training nearly three decades ago. Yet as Faulkner (1951) noted, "The past is never dead. In fact, it's not even past." Even today, in the pages of our journals and newsletters, we can find similar misunderstandings of individual differences psychology. Witness, for example, two recent passages from the pages of the *APS Observer*, the newsletter of the Association for Psychological Science:

> "...partitioning the determinants of behavioral characteristics into separate genetic versus environmental causes is no more sensible than asking which areas of a rectangle are mostly due to length and which to width" (Mischel, 2005, p. 3).
> "...this approach [traditional behavior genetics] does not escape the nature–nurture dichotomy, and it perpetuates the idea that genetic and environmental factors can be accurately quantified and their relative influence on human development measured...genes and environment are always interacting, and it would be impossible to consider one without the other" (Champaigne, 2009, p. 2).

Both quotations confuse the transaction between genes and environment *within* individuals with the separate influences of genes and environment *across* individuals (Rowe, 1987). Mischel's assertion, like many others in the literature (e.g., Ferris, 1996; LeDoux, 1998), implies erroneously that one cannot examine the question of whether good quarterbacks are more important to a football team's success than are good receivers, because quarterbacks "depend on" receivers to function, and vice versa. Yet it is

E-mail address: slilien@emory.edu

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entirely possible to partition sources of variance across individuals even when these sources "depend on" each other within individuals (Waldman, 2007). Chaimpaigne's claim exemplifies the same error, and compounds it by asserting simultaneously that (a) genes and environments always interact, but that (b) one cannot separate or quantify the relative influences of genes and environments, despite the fact that one cannot ascertain whether genes and environments interact statistically without separating them as sources of variance. Incidentally, I strongly suspect that as a beginning graduate student, I would have found both of the aforementioned quotations persuasive, in part because they dovetailed with my own biases against genetic influences, or at least genetic main effects, on behavior.

It was not until my second year of graduate school at Minnesota, when I enrolled in Tom Bouchard's course on individual differences, that I first began to learn to think scientifically – that is, to try to put aside my biases in an effort to align my beliefs more closely with reality (in this respect, I am an unabashed adherent of the correspondence theory of truth; O'Connor, 1975). Tom taught me that political correctness has no place in science: The desire to discover the truth must trump the desire to feel comfortable (see also Sagan, 1995). Tom also taught me that we must be courageous in facing up to evidence, regardless of where it leads us, and that as scientists we must prepare to have our preconceptions challenged, even shattered. More than anything, Tom inculcated in me a profound appreciation for intellectual honesty, which B.F. Skinner (1953) regarded as the "opposite of wishful thinking" (p. 12). For this wisdom, which I have always tried to take to heart as a researcher and teacher, I will forever be grateful.

2. The unnatural nature of scientific thinking

Why did I begin this article by presenting misguided statements by myself and other psychologists? To make a straightforward point: Scientific thinking does not come naturally to any of us. In many respects, science is "uncommon sense," because it requires us to set aside our gut hunches and intuitions *in lieu* of convincing data (Cromer, 1993; McCauley, 2000; Wolpert, 1993). Even many great thinkers have failed to grasp this profound truth. Huxley (1902), Darwin's "bulldog," wrote that "science is nothing but trained and organized common sense" and mathematician-philosopher Whitehead (1916) wrote that "science is rooted in the whole apparatus of commonsense thought."

In contrast, other scholars, including eminent psychologists, have offered a diametrically opposed perspective, one more consonant with that I present here. Titchener (1929) maintained that "common sense is the very antipodes of science," and Skinner (1971) asked rhetorically, "What, after all, have we to show for non-scientific or prescientific good judgment, or common sense, or the insights gained from personal experience? (p. 160)." Skinner's characteristically blunt answer: "It is science or nothing" (p. 160). As Cromer (1993) noted, "All non-scientific systems of thought accept intuition, or personal insight, as a valid source of ultimate knowledge...Science, on the other hand, is the rejection of this belief, and its replacement with the idea that knowledge of the external world can come only from objective investigation (p. 21)."

Cromer's insightful observation helps to explain why science is a relatively recent development in history. Science requires us to override more automatic, effortless, and intuitive modes of thinking with more controlled, effortful, and reflective modes of thinking (Stanovich, 2009). According to many scholars, science arose only once in world history, namely in ancient Greece, reappearing in full-fledged form in the European enlightenment (Wolpert, 1993). Even the concept of control groups, which we take for granted today, did not emerge in psychology until the early 20th century (Dehue, 2000). The necessity of control groups is decidedly unintuitive, as these groups are designed to eliminate alternative explanations that lie outside of our immediate sensory awareness. Our commonsense realism or "naïve realism" – the seductive but erroneous belief that the world is exactly as we see it (Ross & Ward, 1996) – tells us that if a group of depressed clients improves following therapy, we can conclude that the therapy worked. Our naïve realism assures us that "we have seen the change with our own eyes" and that "seeing is believing." Yet these conclusions are erroneous, because they do not control for a host of rival explanations that lurk in the causal background, such as regression to the mean, placebo effects, spontaneous remission, effort justification, and the like (Lilienfeld, Lohr, & Olatunji, 2008).

3. What is science, anyway?

Up to this point, I have said little or nothing about what science is. Some scholars insist that any attempt to define science is doomed to fail, as the specific methodological procedures used in one domain (e.g., astronomy) often bear little or no superficial resemblance to the procedures used in others (e.g., psychology; Bauer, 1992). Yet this argument overlooks the possibility that certain higher-order epistemic commonalities cut across most or all scientific domains.

I side with several authors who maintain that science is a set of systematic safeguards against confirmation bias, that is, the tendency to seek out evidence consistent with our hypotheses and to deny, dismiss, or distort evidence that runs counter to them (Hart et al., 2009; Nickerson, 1998; see also Lilienfeld, Ammirati, & Landfield, 2009). Nobel-prize winning physicist Feynman's (1985) aphorism that the essence of science is "bending over backwards to prove ourselves wrong" succinctly embodies this view, as does Skinner's (1953) conclusion that science mandates a "willingness to accept facts even when they are opposed to wishes" (p. 12). This emphasis on disconfirmation rather than confirmation accords with Popperian and neo-Popperian views of the philosophy of science (Meehl, 1978), which underscore the need to subject our most cherished hypotheses to the risk of falsification. More broadly, this emphasis dovetails with the point that science is a prescription for humility (McFall, 1996) and a method of "arrogance control" (Tavris & Aronson, 2007). The adoption of scientific procedures, such as control groups, is an explicit acknowledgement that our beliefs could be wrong (Sagan, 1995), as these procedures are designed to protect us from fooling ourselves.

As we all know, scientists are hardly immune from confirmation bias (see Kelley and Blashfield (2009), for a striking illustration in the domain of sex differences research). Mahoney (1977) asked 75 journal reviewers who held strong behavioral orientations to evaluate simulated manuscripts that featured identical research designs but different results. In half of the cases, the results were consistent with traditional behavioral views (reinforcement strengthened motivation), whereas in the other half of the cases, the results were inconsistent with traditional behavioral views (reinforcement weakened motivation). Even though the Introduction and Method sections of the articles were identical, Mahoney found that reviewers were much more likely to evaluate the study positively if it confirmed their views (quotations from the reviewers included "A very fine study" and "An excellent paper") than disconfirmed them (quotations from the reviewers included "A serious, mistaken paper" and "There are so many problems with this paper, it is difficult to know where to begin"). Still, because scientific methods themselves minimize the risk of confirmation bias, the inevitable shortcomings of the peer review process (e.g., Peters

& Ceci, 1982) tend to be corrected over time by the force of consistently replicated findings (Lykken, 1968).

4. The troubling state of science in clinical psychology and cognate fields

A few months ago, while attending a small psychology conference, I saw an intriguing talk on the prevalence of psychological misconceptions among undergraduates, not coincidentally an interest of mine (Lilienfeld, Lynn, Ruscio, & Beyerstein, 2010). One of the survey items the authors had presented to their subjects was "Psychology is a science," with a "False" answer ostensibly representing a misconception. Yet while listening to this talk, I found it difficult not to wonder, "Had I been a subject in this study, how would I have answered this question?" Even with the benefit of several months of hindsight, I am not certain, because some domains of psychology are clearly scientific, others less so, and still others blatantly pseudoscientific (Lilienfeld, Lynn, Namy, & Woolf, 2009). This realization forms the basis for the title of this article, which raises the question of whether we can place psychology on sturdier scientific footing.

In my own field of mental health research and practice, the state of science can most charitably be described as worrisome, perhaps more accurately as dismaying (e.g., Dawes, 1994; Lilienfeld, Lynn, & Lohr, 2003; Sarnoff, 2001; Singer & Lalich, 1996). In some domains of clinical practice, there is an indifference to scientific research, in others an outright antipathy. One recent anecdote from a bright student who completed her Ph.D. last year highlights this point. She was participating in a discussion on a listserv dedicated to "energy therapies" – treatments that supposedly cure psychological ailments by unblocking clients' invisible energy fields – and was tactfully raising a number of questions concerning the conspicuous absence of evidence for these interventions. The response from one listserv member was illustrative: "My understanding was that this was a clinical list, not a list about scientific evidence." Apparently, this participant found the intrusion of scientific questions into a clinical discussion unwelcome.

Of course, anecdotes are useful for illustrative, but not probative, purposes, so one can legitimately ask whether the scientific foundations of clinical psychology and cognate fields are as rickety as I have claimed. In fact, there is plentiful evidence for the "scientist-practitioner gap" (Fox, 1996), the deep chasm between research evidence and clinical practice underscored by the listserv participant's response. Consider, for example, recent survey data on the use – in some cases, nonuse – of interventions among mental health professionals:

- Most clients with depression and panic attacks do not receive scientifically supported treatments, such as behavioral, cognitive-behavioral, and interpersonal therapies (Kessler et al., 2001).
- Most therapists who treat clients with eating disorders do not administer scientifically supported psychotherapies, such as the three mentioned immediately above (Mussell et al., 2000).
- Most therapists who treat obsessive–compulsive disorder do not administer the clear treatment of choice based on the scientific literature, namely, exposure and response prevention; increasing numbers are administering energy therapies and other treatments devoid of scientific support (Freiheit, Vye, Swan, & Cady, 2004).
- About one-third of children with autism and autism-spectrum disorders receive non-scientific interventions, such as sensory-motor integration training and facilitated communication (Levy & Hyman, 2003).
- Over 70,000 mental health professionals have been trained in eye movement desensitization and reprocessing (EMDR), a treatment for anxiety disorders based on the scientifically unsupported notion that lateral eye movements facilitate the cognitive processing of traumatic memories (see Herbert et al., 2000).

The field's tepid and at times antagonistic response to the movement to develop a list of empirically supported therapies (ESTs), interventions demonstrated to work for specific disorders in randomized control trials, is sobering. Although some therapists and researchers have embraced the push to develop a list of ESTs, which almost surely reduce the risk of harm to clients (Lilienfeld, 2007), others have been sharply resistant to the effort to place the field of psychotherapy on more solid scientific footing (Baker, McFall, & Shoham, 2009). For example, some critics have pointed out that a number of studies on which the EST list is based are methodologically imperfect (e.g., Westen, Novotny, & Thompson-Brenner, 2004). Other authors have contended that the EST list is inherently flawed because it is based on groups, not individuals. For example, the American Psychological Association's current Director of Professional Practice wrote that "We have to realize the limitations of science in regard to the generalization of research results to the individual patient. Studies do not always take into account or offer a good match for the complexity of the patient's problems or the diversity of factors in a patient such as cultural background, lifestyles choices, values, or treatment preferences" (Nordal, 2009). Both arguments neglect the crucial point that science aims to reduce human error by minimizing confirmation bias. Hence, any list that increases the field's ratio of scientifically supported to unsupported interventions is a step in the right direction, just so long as it is regarded as fallible, provisional, and open to revision (Chambless & Ollendick, 2001). Moreover, many psychology training programs, including those in clinical, counseling, and school psychology, seem reluctant to place constraints on which interventions their students can learn and administer, even though many of these interventions are lacking in scientific support. The results of one survey revealed that 72% of internships accredited by the American Psychological Association offered less than 15 h of training in ESTs (Hays et al., 2002); the remaining time is presumably spent on learning about "nonspecific" therapy techniques (e.g., rapport, empathy) and treatments boasting less research support than ESTs. Still another showed that among clinical Ph.D. programs, only 34% and 53% required training in behavioral and cognitive-behavioral therapies, respectively. The corresponding numbers among social work programs were 13% and 21%, respectively (Weissman et al., 2006). These percentages are troubling given that behavioral and cognitive-behavioral therapies are among the best supported treatments for mood, anxiety, and eating disorders, and occupy the lion's share of ESTs (Hunsley & DiGuilio, 2002).

5. Five threats to scientific psychology

I hope that I have by now persuaded the reader that all is not well in the field of psychology (see also Dawes, 1994; Lilienfeld et al., 2003; Lykken, 1991; Meehl, 1978), especially clinical psychology and allied fields. Given increasing evidence that some psychotherapies, such as crisis debriefing for traumatized individuals and Scared Straight programs for conduct disordered adolescents, appear to make some clients worse (Lilienfeld, 2007), the marginal state of science in these fields is unacceptable.

I contend that there are five major threats to scientific psychology; I will review each in turn. Despite their superficial differences, all of these threats share a higher-order commonality: a failure to control for confirmation bias. As a consequence, all are marked by an absence of essential safeguards against the all-too-human propensity to see what we wish to see. To place psychology on more solid scientific footing, we must acknowledge these threats and confront them directly. These threats are too often ignored within academia, largely because researchers nestled safely within the confines of the Ivory Tower understandably prefer to concentrate on their research and grant-writing (Bunge, 1984). Yet such neglect has come at a serious cost, as it has allowed dubious science, nonscience, and even pseudoscience to take root and flourish in many quarters.

5.1. Political correctness

Political correctness is the ruling of certain scientific questions as "out of bounds" merely because they offend our political sensibilities (see Satel, 2000). Regrettably, as noted by Koocher (2006), many people use "behavioral science as a rationale to promote or oppose political and social policy agendas" (p. 5). The threats posed by political correctness to psychology stem from both the extreme political left and extreme political right (Hunt, 1999), and pervade a host of domains, including individual and group differences in intelligence (Gottfredson, 2009), recovered memories of childhood trauma (Loftus, 1993), the effects of day care on child development (Belsky, 1986), and the potential impact of child sexual abuse (CSA) on adult psychopathology (Lilienfeld, 2002a).

One case with which I was closely involved helps to illustrate these threats. In 1998, one of the premier journals of the American Psychological Association (APA), *Psychological Bulletin*, published a meta-analysis that revealed only weak correlations between a history of CSA and subsequent psychopathology in college students. Not long after this article, authored by Rind, Tromovitch, and Bauserman (1998), appeared in print, it was roundly condemned by critics on the political left, who argued that it understated the grave threat to victims of CSA, and on the political right, including radio personality Dr. Laura Schlessinger ("Dr. Laura"), who argued that it represented an explicit effort by the APA to normalize pedophilia (Lilienfeld, 2002a). The article suffered the further indignity of becoming the first scientific article to be condemned by the United States Congress, with a vote of 355 to 0 (with 13 members abstaining) in the House of Representatives. Under intense pressure from members of Congress, the APA, under the leadership of then Chief Executive Officer Raymond Fowler, essentially apologized for publishing the article, stating that Rind et al.'s conclusions "should have caused us to evaluate the article based on its potential for misinforming the public policy process. This is something we failed to do, but will do in the future" (Fowler, 1999, p. 1).

The controversy did not end there. A year later, I submitted an article to the APA journal *American Psychologist*, recounting the story of the Rind et al. controversy and criticizing what I viewed as the APA's failure to stand behind the peer review process that led to the publication of Rind et al.'s article. Although my article was formally accepted by action editor Nora Newcombe following several rounds of peer review, this decision was overturned by *American Psychologist* editor Richard McCarty, who solicited a new round of peer review without informing the author or action editor (the article was eventually published following a large outcry by the APA membership; see Lilienfeld, 2002a). In his conversation with me, McCarty – whose actions as head of the APA Science Directorate were among those I had criticized in the article – justified his decision to unaccept my already accepted article on the grounds that APA must be circumspect about the published information it disseminates to its wide and diverse membership (Lilienfeld, 2002b). Notably, McCarty did not take issue with the substance of my conclusions; instead, he feared that many members of APA might find the message of my article to be unpalatable. When our field's leading professional organizations capitulate to political correctness, the scientific integrity of our discipline is seriously undermined.

5.2. Radical environmentalism

Our field has come a long way from Watson's (1930) rash speculation that he could take "a dozen healthy infants, well-formed, and my own specified world to bring them up in and I'll guarantee to take any one at random and train him to become any type of specialist I might select – doctor, lawyer, artist, merchant-chief and, yes, even beggar-man and thief, regardless of his talents, penchants, tendencies, abilities, vocations, and race of his ancestors" (p. 182). Today, the notions that virtually all important human individual differences are (a) at least partly heritable (Bouchard, Lykken, McGue, Segal, & Tellegen, 1990) and (b) not infinitely malleable are taken for granted in most psychology departments, an undeniable sign of intellectual progress.

Yet in its subtler forms, radical environmentalism remains alive and well today. Journalist Gladwell's (2009) popular book, *Outliers*, offers a telling example. Gladwell's core thesis is that intellectual ability is relevant to real-world success up to only a modest threshold. After that, the major determinants are luck, opportunity, and being in the right place at the right time. For Gladwell, innate talent is typically of minimal importance to great social achievements; the primary contributors are environmental, especially those that afford extended practice. For example, Gladwell attributed the enormous success of *The Beatles*, considered by most experts to be the most influential rock band of all time, less to the individual talents of its four members (which Gladwell acknowledged) than to the fact that their extended gigs in Hamburg, Germany, afforded them thousands of hours of intense practice. Indeed, Gladwell devoted relatively little space to the possibility that the causal arrow is reversed: High levels of talent may lead to extended practice more than the converse.

Said Gladwell in an interview, "Outliers aren't outliers. People who seem like they are off on their own, having achieved extraordinary things, are actually very ordinary….they are there… because of a whole series of circumstances and environments and cultural legacies that really implicate us all." Yet Gladwell's assertions fly in the face of data demonstrating a consistent link between exceptional intellectual aptitude in early adolescence and both creative and occupational accomplishment in adulthood (Lubinski, Benbow, Webb, & Bleske-Rechek, 2006), and the conspicuous absence of a threshold effect in the association between intellectual aptitude scores and real-world achievement (Sackett, Borneman, & Connelly, 2008).

We can also still witness the undercurrents of radical environmentalism in many domains of academic psychology and psychiatry. One still popular variant of radical environmentalism is the "trauma-centric" view of psychopathology (Giesbrescht, Lynn, Lilienfeld, & Merckelbach, 2010), which posits that childhood trauma, especially child sexual or physical abuse, is of overriding causal importance for a wide variety of mental illnesses. For example, one prominent set of authors referred to a "continuum of trauma-spectrum psychiatric disorders" (p. 368) and presented a diagram (p. 369) displaying causal linkages between childhood trauma and 16 psychopathological phenomena, including depression, anxiety, panic attacks, somatization, identity disturbance, and dissociative symptoms. (Bremner, Vermetten, Southwick, Krystal, & Charney, 1998). Ross and Pam (2005) went further, claiming that "serious chronic childhood trauma is the overwhelming major driver of psychopathology in Western civilization" (p. 122). These and many other authors (e.g., Gleaves, 1996) presume the correlation between early sexual or physical abuse and later psychopathology to be strictly environmental, often with no acknowledgement of potential genetic confounds (DiLalla & Gottesman, 1991; Lilienfeld et al., 1999). Few would dispute the claim that early abuse, when severe or repeated, can exert detrimental effects on later personal adjustment. Nevertheless, the increasing recognition that mainstream psychology has underestimated both childhood and adult resilience in the face of stressors (Garmezy, Masten, & Tellegen, 1984; Paris, 2000) should remind us that the causal associations between early abuse and later maladjustment are likely to be far more complex than implied by the trauma-centric view.

5.3. The resurrection of "common sense" and intuition as arbiters of scientific truth

There are multiple definitions of common sense, but the one closest to that I present here is the form of reasoning that Meehl (1971) described as comprising "fireside inductions": "commonsense empirical generalizations about human behavior which we accept on the culture's authority plus introspection plus anecdotal evidence from ordinary life. Roughly, the phrase 'fireside inductions' designates here what everybody (except perhaps the skeptical social scientist) believes about human conduct" (p. 66). Just how accurate are fireside inductions in everyday psychology?

The past several years have seen a parade of popular books touting the benefits of common sense and gut hunches in decision-making, most notably Gladwell's (2005) *Blink*: *The Power of Thinking Without Thinking* and psychologist Gigerenzer's (2007) *Gut Feelings: The Intelligence of the Unconscious*. Such books surely have some merit, as there is increasing scientific evidence that "rapid cognition" and split-second intuitions can be helpful for certain kinds of decisions, including affective preferences (Lehrer, 2009). Nevertheless, many of these books may leave readers with the impression that commonsense judgments are generally superior to scientific evidence for most purposes, including adjudicating complex disputes regarding the innermost workings of the human mind.

Indeed, over the past decade, psychology has witnessed a comeback of the once popular but largely discredited view that common sense and intuition can be extremely useful, even decisive, in the evaluation of psychological theories. In an article in a law journal, Redding (1998) argued that "Common sense and intuition serve as 'warning signals' about the likely validity or invalidity of particular research findings" (p. 142). Accordingly, we should raise questions about psychological findings that conflict with conventional wisdom. In a widely discussed editorial in the *New York Times* entitled "In Defense of Common Sense," prominent science writer John Horgan (2005) called for a return to common sense in the evaluation of scientific theories, including those in psychology and neuroscience. Theories that conflict with intuition, Horgan insisted, should be viewed with grave suspicion. He wrote that "I have also found common sense – ordinary, nonspecialized knowledge and judgment – to be indispensable for judging scientists' pronouncements." And in an interview with Science News (2008), Gigerenzer contended that "fast and frugal heuristics demonstrate that there's a reason for trusting our intuitions…We need to trust our brains and our guts."

These arguments have found their way into the pages of leading psychological journals. In an article in *Psychological Bulletin*, Kluger and Tikochinsky (2001) argued for the "resurrection of commonsense hypotheses in psychology" (p. 408). They reviewed nine domains, such as the association between personality and job performance, the relation between attitudes and behavior, the congruence between self and observer reports, the validity of graphology (handwriting analysis) and personality, alternative remedies for cancer, and the existence of Maslow's (1943) hierarchy of needs, in which commonsense judgments (e.g., that attitudes predict behavior) were seemingly overturned by research, only to be later corroborated by better conducted research. Yet most of these domains, such as the relation between self and observer reports and Maslow's need hierarchy, are not among those in which the average layperson holds strong intuitions. Moreover, several of the associations Kluger and Tikochinsky put forth as well supported or promising, such as the validity of graphology or the efficacy of alternative remedies for cancer, have not in fact been corroborated by well-conducted research (Della Sala, 2007; Lilienfeld et al., 2010).

More important, common sense inductions about the natural world have a decidedly checkered history. For centuries, people assumed that the world was flat and that the sun revolved around the earth because their naïve realism and raw sensory impressions told them so. In addition, many survey studies demonstrate convincingly that large proportions of undergraduates in psychology courses (who are probably better informed psychologically than the average layperson) evince a host of misconceptions about human nature (Lilienfeld et al., 2010). Many of these misconceptions seem to fit Meehl's (1971) definition of fireside inductions and accord with the average person's intuitive judgments. Below is a small sampling of such misconceptions derived from survey studies, followed by the percentage of college students (or in the case of the final misconception, college-educated people) who endorsed each misconception:

- Opposites tend to attract in romantic relationships (77%) (McCutcheon, 1991).
- Expressing pent-up anger reduces anger (66%) (Brown, 1983).
- Odd behaviors are especially likely to occur during full moons (65%) (Russell & Dua, 1983).
- People with schizophrenia have multiple personalities (77%) (Vaughan, 1977).
- Most people use only 10% of their brain power (59%) (Herculano-Houzel, 2002).

These and other survey findings (Lilienfeld, 2005a; Lilienfeld et al., 2010) raise serious questions about the increasingly fashionable notion that commonsense judgments about human nature tend to be accurate. They also offer no support for the contention (e.g., Kluger & Tikochinsky, 2001; Redding, 1998) that when scientific findings conflict with common sense, we should cast our lots with the latter.

5.4. Postmodernism

Postmodernism is not easily defined, but it is generally regarded as a reaction against the view that scientific methods, whatever their imperfections as safeguards against error, can bring us closer to a view of objective reality (Gross & Levitt, 1994). Some variants of postmodernism even deny that such a reality exists. As Bunge (1994) and others have observed, postmodernism and allied movements, such as poststructuralism, share several key tenets, including (a) a deep mistrust of reason, especially that imparted by science and logic; (b) a rejection of science as a "privileged" means of acquiring knowledge, along with the belief that other means of knowledge acquisition are equally valid; (c) pessimism about scientific progress; (d) subjectivism, and the accompanying notion that the world is largely socially constructed; and (e) extreme relativism, along with a denial of universal scientific truths. Although some authors (e.g., Mooney & Kirshenbaum, 2009) have argued that postmodernism poses scant threat to science, this position neglects the influence of postmodern views on clinical practice (e.g., see Herbert et al., 2000 for a discussion of postmodernism's impact on the marketing of EMDR and related therapies).

In particular, postmodernism has been associated with an increased acceptance of the role of "clinical experience" and "subjective judgment" in acquiring knowledge in clinical settings. Unquestionably, clinical experience is an invaluable source of rich hypotheses to be tested in more rigorous investigations. But if the last several decades of research in clinical judgment and prediction have taught us anything, it is that such experience is often clouded by a host of biases (e.g., confirmation bias, hindsight bias) and heuristics (e.g., availability, representativeness) that often hinder its accuracy (Dawes, Faust, & Meehl, 1989; Garb, 1998). As a consequence, clinical experience tends to be markedly limited in usefulness for Reichenbach's (1938) "context of justification," that is, systematic hypothesis testing.

This critical point has been neglected by many authors writing in the pages of *American Psychologist* and other prominent publications. Tsoi-Hoshmand and Polkinghorne (1992) argued that clinical intuition should be placed on a par with scientific evidence in the training of psychotherapists. They advocated for an epistemology called "practicing knowledge," contending that "in relating theory to practice, research typically served as gatekeeper for entry into a discipline's body of knowledge," but in "practicing knowledge, however, the test for admission is carried out through reflective thought" (p. 62). In this model, subjective judgment derived from the clinical setting trumps well-replicated scientific evidence. In another article in the *American Psychologist*, Hunsberger (2007) similarly wrote that:

"Subjective knowledge and skills are at the core of psychology...To preserve clinical psychology's vital subjective essence, I suggest that the American Psychological Association (APA) not only should make a place at psychology's policymaking table for 'clinical expertise' but should prioritize clinical and subjective sources of data – the essence of the psychological – and set policies to ensure that objective data, such as behaviors and DSM diagnoses, are considered in their 'subjective context' (p. 615).

Readers who wonder whether these quotations reflect fringe views in the field should inspect the recent APA Statement of Evidence-Based Practice (APA Presidential Task Force on Evidence-Based Practice, 2006), which is intended to serve as the field's authoritative guide to clinical practice. This statement acknowledges that clinical experience sometimes conflicts with scientific evidence and asserts that "In a given clinical circumstance, psychologists of good faith and good judgment may disagree about how best to weigh different forms of evidence; over time we presume that systematic and broad empirical inquiry...will point the way toward best practice in integrating best evidence" (p. 280; see also Nordal, 2009). That is, no inherent priority should be accorded to scientific evidence above clinical judgment, and practitioners should feel free to use their own discretion in deciding which to weight more heavily.

5.5. Pseudoscience

We can think of pseudoscience as nonscience masquerading as genuine science; pseudoscience possesses many of the superficial trappings of science without its substance. Pseudoscience is marked by several key features, such as an overreliance on *ad hoc* immunizing tactics (escape hatches or loopholes) to avoid falsification, emphasis on confirmation rather than falsification, an absence of self-correction, overuse of anecdotal and testimonial evidence, evasion of peer review as a safeguard against error, and use of hypertechnical language devoid of substance (Lilienfeld et al., 2003; Ruscio, 2006). Overall, most pseudosciences lack the safeguards against confirmation bias that mark mature sciences. As a consequence, they resemble degenerating research programs in the sense delineated by Lakatos (1978), that is, domains of inquiry that are continually invoking *ad hoc* hypotheses in a desperate effort to explain away negative results.

As we have already seen, pseudoscience is alive and well in many domains of psychology, including clinical psychology (Lilienfeld et al., 2003). One troubling case in point is the enduring popularity of facilitated communication (FC). FC is premised on the scientifically unsupported notion that children with autism are intellectually and emotionally normal, but suffer from a motor impairment (developmental apraxia) that prevents them from articulating words or using keyboards without assistance (Lilienfeld, 2005b). With the help of a "facilitator" who offers subtle resistance to their hand movements, proponents of FC claim, mute or severely linguistically impaired children with autism can type out complete sentences using a computer keyboard, typewriter, or letter pad. Yet numerous controlled studies demonstrate that FC is ineffective (Jacobson, Foxx, & Mulick, 2005), and that its apparent efficacy is due to the well documented ideomotor ("Oujia board") effect (Wegner, 2002), in which facilitators unknowingly guide children's fingers to the letters they have in mind. In addition to gratuitously raising and then dashing the hopes of the parents of children with autism and other developmental disabilities, FC has compounded the problem by yielding numerous uncorroborated allegations of sexual and physical abuse against these parents (Herbert, Sharp, & Gaudiano, 2002).

Yet despite being convincingly debunked by the scientific community, FC is far from dead, and actually appears to be mounting a comeback in many quarters. As of several years ago, FC was being used by approximately 200 children in Whittier, California (Rubin & Rubin, 2005), and was featured prominently and uncritically in a 2005 Academy-Award nominated documentary ("Autism is a World"). In November, 2009, much of the news media enthusiastically presented video footage of a 46 year old Belgian man, who had lain unresponsive in a coma for 23 years, using FC to type out sentences. In addition, the number of positive mentions of FC in the media has skyrocketed in recent years (Wick & Smith, 2006), despite the noteworthy absence of any new evidence supporting its efficacy.

6. Constructive remedies: placing psychology on firmer scientific footing

These five serious threats to scientific psychology notwithstanding, there is reason for cautious optimism. In particular, I contend that with the proper educational and institutional reforms, we should be able to combat these threats and place the field of psychology on firmer scientific grounds.

In particular, I argue that the current trend toward allowing clinical psychology programs to select their own training models and evaluating how well they hew to these models has been a grievous error (Lilienfeld, 1998). Instead, formal training in scientific thinking should be required for all graduate students in psychology, including students in clinical psychology and allied fields. Specifically, students in all domains of psychology must come to appreciate their own fallibility and propensity toward biases, including confirmation bias and hindsight bias (Arkes, Wortmann, Saville, & Harkness, 1981), and taught that scientific methods (e.g., randomized controlled designs, blinded designs) are essential, albeit imperfect, safeguards against manifold sources of error. Coursework in clinical judgment and prediction (e.g., Garb, 1998; Ruscio, 2006) should likewise be required for all graduate students in mental health fields and integrated throughout all phases of students' didactic and practicum work. Accrediting bodies must make formal training in scientific thinking a desideratum for graduate training in mental health disciplines.

As Meehl reminded us, learning about the history of errors in other sciences, such as physics and medicine, can also help students to appreciate that scientific methods are the best available tools for overcoming such errors and minimizing confirmation bias. Science is, after all, a discipline of corrected mistakes (Wood & Nezworski, 2005). Such knowledge should help make graduate students better researchers, practitioners, and teachers, namely, those who are self-critical, epistemically humble, and continually trying to root out errors in their web of beliefs.

Educational reform, essential as it is, is not enough; institutional reform is also sorely needed. Academics must be encouraged to combat threats against science, as well as to disseminate high quality science to the popular media (Lilienfeld et al., 2003; Mooney & Kirshenbaum, 2009). To do so, college and university departments must come to regard the accurate and thoughtful popularization of science as a valued aspect of academic service and to reward such service. Regrettably, the ability to convey psychological science to the general public without oversimplifying its findings and implications is rarely emphasized in graduate training. In this respect, Tom Bouchard has been a role model for his fellow academics in his willingness to both speak out against unsupported claims (e.g., radical environmentalism) and educate the public about the scientific and social implications of behavior genetic findings.

So, to return to the question constituting this article's title, "Can psychology become a science?" my answer is straightforward and optimistic. With the implementation of these educational and institutional reforms, and with the encouragement of more teachers and mentors like Tom Bouchard, "yes."

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References

APA Presidential Task Force on Evidence-Based Practice (2006). Evidence-based practice in psychology. *American Psychologist, 61*, 271–285.

Arkes, H. R., Wortmann, R. L., Saville, P. D., & Harkness, A. R. (1981). Hindsight bias among physicians weighting the likelihood of diagnoses. *Journal of Applied Psychology, 66*, 252–254.

Baker, T. B., McFall, R. M., & Shoham, V. (2009). Current status and future prospects of clinical psychology: Toward a scientifically principled approach to mental and behavioral health care. *Perspectives on Psychological Science, 9*(2), 67–103.

Bauer, H. H. (1992). *Scientific literacy and the myth of the scientific method*. Urbana, Ill: University of Illinois Press.

Belsky, J. (1986). Infant day care: A cause for concern? *Zero to Three, 6*, 1–19.

Bouchard, T. J., Jr., Lykken, D. T., McGue, M., Segal, N., & Tellegen, A. (1990). Sources of human psychological differences: The Minnesota study of twins reared apart. *Science, 250*, 223–228.

Bremner, J. D., Vermetten, E., Southwick, S. M., Krystal, J. H., & Charney, D. S. (1998). Trauma, memory, and dissociation: An integrative formulation. In J. D. Bremner & C. R. Marmar (Eds.), *Trauma, memory, and dissociation* (pp. 365–402). Washington: American Psychiatric Press.

Brown, L. T. (1983). Some more misconceptions about psychology among introductory psychology students. *Teaching of Psychology, 10*, 207–210.

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

Bunge, M. (1994). Counter-enlightenment in contemporary social studies. In P. Kurtz & T. J. Madigan (Eds.), *Challenges to the Enlightenment: In defense of reason and science* (pp. 25–42). Amherst, NY: Prometheus Books.

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

Champaigne, F. (2009). Beyond nature vs. nurture: Philosophical insights from molecular biology. *APS Observer, 22*(4), 1–3.

Cromer, A. (1993). Uncommon sense: The heretical nature of science. *Science, 265*, 688.

Dawes, R. M. (1994). *House of cards: Psychology and psychotherapy built on myth*. New York: Free Press.

Dawes, R. M., Faust, D., & Meehl, P. E. (1989). Clinical versus actuarial judgment. *Science, 243*, 1668–1674.

Dehue, T. (2000). From deception trials to control reagents: The introduction of the control group about a century ago. *American Psychologist, 55*, 264–269.

Della Sala, S. (Ed.). (2007). *Tall tales about the mind and brain: Separating fact from fiction*. London: Oxford University Press.

DiLalla, L. F., & Gottesman, I. I. (1991). Biological and genetic contributions to violence – Widom's untold tale. *Psychological Bulletin, 109*, 125–129.

Faulkner, W. (1951). *Requiem for a nun*. London: Vintage.

Ferris, C. G. (1996). The rage of innocents. *The Sciences, 36*(2), 22–26.

Feynman, R. P. (1985). *Surely you're joking, Mr. Feynman: Adventures of a curious character*. New York: Norton.

Fowler, R. D. (1999). *APA letter to the Honorable Rep. DeLay (R-Tx)*.

Fox, R. E. (1996). Charlatanism, scientism, and psychology's social contract. *American Psychologist, 51*, 777–784.

Freiheit, S. R., Vye, D., Swan, R., & Cady, M. (2004). Cognitive-behavioral therapy for anxiety: Is dissemination working? *The Behavior Therapist, 27*, 25–32.

Garb, H. N. (1998). *Studying the clinician: Judgment research and psychological assessment*. Washington, DC: American Psychological Association.

Garmezy, N., Masten, A. S., & Tellegen, A. (1984). The study of stress and competence in children: A building block for developmental psychopathology. *Child Development, 55*, 97–111.

Giesbrescht, T., Lynn, S. J., Lilienfeld, S. O., & Merckelbach, H. (2010). Cognitive processes, trauma, and dissociation: Misconceptions and misrepresentations (Reply to Bremner, 2009). *Psychological Bulletin, 136*, 7–11.

Gigerenzer, G. (2007). *Gut feelings: The intelligence of the unconscious*. New York: Viking.

Gigerenzer, G. (2008). Sound reasoning requires statistical understanding. *Science News*, 32.

Gladwell, M. (2005). *Blink: The power of thinking without thinking*. New York: Little, Brown, and Company.

Gladwell, M. (2009). *Outliers: The story of success*. New York: Little, Brown, and Company.

Gleaves, D. H. (1996). The sociocognitive model of dissociative identity disorder: A reexamination of the evidence. *Psychological Bulletin, 120*, 42–59.

Gottfredson, L. S. (2009). Logical fallacies used to dismiss the evidence on intelligence testing. In R. Phelps (Ed.), *Correcting fallacies about educational and psychological testing* (pp. 11–65). Washington, DC: American Psychological Association.

Gross, P. R., & Levitt, N. (1994). *Higher superstition: The academic left and its quarrels with science*. Baltimore: Johns Hopkins University Press.

Hart, W., Albarracin, D., Eagly, A. H., Lindberg, M. J., Merrill, L., & Brechan, I. (2009). Feeling validated versus being correct: A meta-analysis of selective exposure to information. *Psychological Bulletin, 135*, 555–588.

Hays, K. A., Rardin, D. K., Jarvis, P. A., Taylor, N. M., Moorman, A. S., & Armstead, C. D. (2002). An exploratory survey on empirically supported treatments: Implications for internship training. *Professional Psychology: Research and Practice, 33*, 207–211.

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

Herbert, J. D., Sharp, I. R., & Gaudiano, B. A. (2002). Separating fact from fiction in the etiology and treatment of autism: A scientific review of the evidence. *Scientific Review of Mental Health Practice, 1*, 25–45.

Herculano-Houzel, S. (2002). Do you know your brain? A survey on public neuroscience literacy at the closing of the decade of the brain. *The Neuroscientist, 8*(2), 98–110.

Horgan, J. (2005). In defense of common sense. *The New York Times*, A34.

Hunsberger, P. H. (2007). Reestablishing clinical psychology's subjective core. *American Psychologist, 62*, 614–615.

Hunsley, J., & DiGuilio, G. (2002). Dodo bird, phoenix, or urban legend? The question of psychotherapy equivalence. *Scientific Review of Mental Health Practice, 1*, 11–22.

Hunt, M. (1999). *The new know-nothings: The political foes of the scientific study of human nature*. New Brunswick, NJ: Transaction.

Huxley, T. H. (1902). *Science and education*. New York: D. Appleton and Company.

Jacobson, J. W., Foxx, R. M., & Mulick, J. A. (Eds.). (2005). *Controversial therapies for developmental disabilities: Fad, fashion, and science in professional practice*. Hillsdale, NJ: Lawrence Erlbaum.

Kelley, L. P., & Blashfield, R. K. (2009). An example of psychological science's failure to self-correct. *Review of General Psychology, 13*, 122–129.

Kessler, R. C., Soukup, J., Davis, R. B., Foster, D. F., Wilkey, S. A., Van Rompay, M. I., et al. (2001). The use of complementary and alternative therapies to treat anxiety and depression in the United States. *American Journal of Psychiatry, 158*, 289–294.

Kluger, A. N., & Tikochinsky, J. (2001). The error of accepting the theoretical null hypothesis: The rise, fall, and resurrection of common sense hypotheses in psychology. *Psychological Bulletin, 127*, 408–423.

Koocher, G. (2006). Psychological science is not politically correct. *APA Monitor on Psychology, 37*(9), 5–6.

Lakatos, I. (1978). In J. Worral & G. Currie (Eds.), *Philosophical papers: Vol. 1. The methodology of scientific research programmes*. New York: Cambridge University Press.

LeDoux, J. E. (1998). Nature vs. nurture: The pendulum still swings with plenty of momentum. *Chronicle of Higher Education*, B7.

Lehrer, J. (2009). *How we decide*. Boston: Houghton-Mifflin.

Levy, S. E., & Hyman, S. L. (2003). Use of complementary and alternative treatments for children with autism spectrum disorders is increasing. *Pediatric Annals, 32*, 685–691.

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

Lilienfeld, S. O. (2005a). Challenging mind myths in introductory psychology courses. *Psychology Teacher Network, 15*(3), 1, 4, 6.

Lilienfeld, S. O. (2005b). Scientifically supported and unsupported treatments for childhood psychopathology. *Pediatrics, 115*, 761–764.

Lilienfeld, S. O. (2002a). When worlds collide: Social science, politics, and the Rind et al. child sexual abuse meta-analysis. *American Psychologist, 57*, 176–188.

Lilienfeld, S. O. (2002b). A funny thing happened on the way to my American Psychologist publication. *American Psychologist, 57*, 225–227.

Lilienfeld, S. O. (2007). Psychological treatments that cause harm. *Perspectives on Psychological Science, 2*, 53–70.

Lilienfeld, S. O., Ammirati, R., & Landfield, K. (2009). Giving debiasing away: Can psychological research on correcting cognitive errors promote human welfare? *Perspectives on Psychological Science, 4*, 390–398.

Lilienfeld, S. O., Lohr, J. M., & Olatunji, B. O. (2008). Encouraging students to think critically about psychotherapy: Overcoming na realism. In D. S. Dunn, J. S. Halonen, & R. A. Smith (Eds.), *Teaching critical thinking in psychology: A handbook of best practices* (pp. 267–271). Malden, MA: Wiley-Blackwell.

Lilienfeld, S. O., Lynn, S. J., Kirsch, I., Chaves, J., Sarbin, T., Ganaway, G., et al. (1999). Dissociative identity disorder and the sociocognitive model: Recalling the lessons of the past. *Psychological Bulletin, 125*, 507–523.

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

Lilienfeld, S. O., Lynn, S. J., Namy, L., & Woolf, N. (2009). *Psychology: From inquiry to understanding*. Boston: Allyn and Bacon.

Lilienfeld, S. O., Lynn, S. J., Ruscio, J., & Beyerstein, B. L. (2010). *50 great myths of popular psychology: Shattering widespread misconceptions about human behavior*. New York: Wiley-Blackwell.

Loftus, E. F. (1993). The reality of repressed memories. *American Psychologist, 48*, 518–537.

Lubinski, D., Benbow, C. P., Webb, R. M., & Bleske-Rechek, A. (2006). Tracking exceptional human capital over two decades. *Psychological Science, 17*, 194–199.

Lykken, D. T. (1968). Statistical significance in psychological research. *Psychological Bulletin, 70*, 151–159.

Lykken, D. T. (1991). What's wrong with psychology anyway? In D. Cicchetti & W. M. Grove (Eds.), *Thinking clearly about psychology Volume 1: Matters of public interest* (pp. 2–39). Minneapolis, MN: University of Minnesota Press.

Mahoney, M. J. (1977). Publication prejudices: An experimental study of confirmatory bias in the peer review system. *Cognitive Therapy and Research, 1*, 161–175.

Maslow, A. (1943). A theory of human motivation. *Psychological Review, 50*, 370–396.

McCauley, R. N. (2000). The naturalness of religion and the unnaturalness of science. In F. Keil & R. Wilson (Eds.), *Explanations and cognitions* (pp. 68–85). Cambridge, Mass: MIT Press.

McCutcheon, L. E. (1991). A new test of misconceptions about psychology. *Psychological Reports, 68*, 647–653.

McFall, R. M. (1996). Making psychology incorruptible. *Applied and Preventive Psychology, 5*, 9–16.

Meehl, P. E. (1971). Law and the fireside inductions: Some reflections of a clinical psychologist. *Journal of Social Issues, 27*, 65–100.

Meehl, P. E. (1978). Theoretical risks and tabular asterisks: Sir Karl, Sir Ronald, and the slow progress of soft psychology. *Journal of Consulting and Clinical Psychology, 46*, 806–834.

Mischel, W. (2005). Alternative futures for our science. *APS Observer, 18*(3), 2–3.

Mooney, C., & Kirshenbaum, S. (2009). *Unscientific America: How scientific illiteracy threatens our future*. New York: Basic Books.

Mussell, M. P., Crosby, R. D., Crow, S. J., et al. (2000). Utilization of empirically supported psychotherapy treatments for individuals with eating disorders: A survey of psychologists. *International Journal of Eating Disorders, 27*, 230–237.

Nickerson, R. S. (1998). Confirmation bias: A ubiquitous phenomenon in many guises. *Review of General Psychology, 2*, 175–220.

Nordal, K. (2009). Taking issue with Newsweek. *Your Mind, Your Body: American Psychological Association,* 1.

O'Connor, D. J. (1975). *The correspondence theory of truth*. London, UK: Hutchinson.

Paris, J. (2000). *Myths of childhood*. New York: Brunner/Mazel.

Peters, D. P., & Ceci, S. J. (1982). Peer-review practices of psychological journals: The fate of published articles, submitted again. *Behavioral and Brain Sciences, 5*, 187–255.

Redding, R. E. (1998). How common-sense psychology can inform law and psycholegal research. *Roundtable, The University of Chicago Law School, 5*, 107–142.

Reichenbach, H. (1938). *Experience and prediction*. Chicago: University of Chicago Press.

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

Ross, C., & Pam, A. (2005). *Pseudoscience in biological psychiatry: Blaming the body*. New York: Wiley.

Ross, L., & Ward, A. (1996). Na realism: Implications for social conflict and misunderstanding. In T. Brown, E. Reed, & E. Turiel (Eds.), *Values and knowledge* (pp. 103–135). Hillsdale, New Jersey: Lawrence Erlbaum.

Rowe, D. C. (1987). Resolving the person-situation debate: Invitation to an interdisciplinary dialogue. *American Psychologist, 42*, 218–227.

Rubin, R., & Rubin, R. A. (2005). Response to "scientifically supported and unsupported interventions for childhood psychopathology: A summary". *Pediatrics, 116*, 289.

Ruscio, J. (2006). *Critical thinking in psychology: Separating sense from nonsense*. Pacific Grove, CA: Wadsworth.

Russell, G. W., & Dua, M. (1983). Lunar influences on human aggression. *Social Behavior and Personality, 11*, 41–44.

Sackett, P. R., Borneman, M., & Connelly, B. S. (2008). High stakes testing in education and employment: Evaluating common criticisms regarding validity and fairness. *American Psychologist, 63*, 215–227.

Sagan, C. (1995). *The demon-haunted world: Science as a candle in the dark*. New York: Random House.

Sarnoff, S. K. (2001). *Sanctified snake oil: The impact of junk science on public policy*. Hillsdale, NJ: Praeger.

Satel, S. (2000). *PC, M.D.: How political correctness is corrupting medicine*. New York: Basic Books.

Singer, M. T., & Lalich, J. (1996). *Crazy therapies: What are they? Do they work?* San Francisco: Jossey-Bass.

Skinner, B. F. (1953). *Science and human behavior*. New York: Macmillan.

Skinner, B. F. (1971). *Beyond freedom and dignity*. New York: Bantam.

Stanovich, K. E. (2009). *What intelligence tests miss: The psychology of rational thought*. New Haven, CT: Yale University Press.

Tavris, C., & Aronson, E. (2007). *Mistakes were made (not by me): Why we justify foolish beliefs, bad decisions, and hurtful actions*. Boston: Houghton-Mifflin.

Titchener, E. B. (1929). *Systematic psychology: Prolegomena*. Ithaca, NY: Cornell University Press.

Tsoi-Hoshmand, L., & Polkinghorne, D. (1992). Redefining the science-practice relationship and professional training. *American Psychologist, 47*, 55–66.

Vaughan, E. D. (1977). Misconceptions about psychology among introductory psychology students. *Teaching of Psychology, 4*, 139–141.

Waldman, I. D. (2007). Behavior genetic approaches are integral for understanding the etiology of psychopathology. In S. O. Lilienfeld & W. T. O'Donohue (Eds.), *The great ideas of clinical science: 17 principles that every mental professional should understand* (pp. 219–242). New York: Routledge.

Watson, J. B. (1930). *Behaviorism* (revised edition). Chicago: University of Chicago Press.

Wegner, D. M. (2002). *The illusion of conscious will*. Cambridge, MA: MIT Press/Bradford Books.

Weissman, M. M., Verdeli, H., Gameroff, M. J., et al. (2006). National survey of psychotherapy training in psychiatry, psychology, and social work. *Archives of General Psychiatry, 63*, 925–934.

Westen, D., Novotny, C. M., & Thompson-Brenner, H. (2004). The empirical status of empirically supported psychotherapies: Assumptions, findings, and reporting in controlled clinical trials. *Psychological Bulletin, 130*, 631–663.

Whitehead, A. N. (1916). *Presidential address, British Association for the Advancement of Science*. New Castleo-n-Tyne.

Wick, J., & Smith, T. (2006). *Controversial treatments for autism in the popular media*. Poster presented at the international meeting for autism research, Montreal, Quebec, Canada.

Wolpert, L. (1993). *The unnatural nature of science: Why science does not make (common) sense*. Cambridge, Mass: Harvard University Press.

Wood, J. M., & Nezworski, M. T. (2005). Science as a history of corrected mistakes. *American Psychologist, 60*, 657–658.