

## **SCIENCE SHOULD DRIVE THE BUS OF CLINICAL DESCRIPTION; BUT HOW DOES “SCIENCE TAKE THE WHEEL”?: A COMMENTARY ON MARKON**

Allan R. Harkness, PhD, and Scott O. Lilienfeld, PhD

In our view, Kristian Markon is right: Science should drive the bus. The central argument of Markon’s scholarly, closely reasoned, timely, and much needed article is that clinical description should be built bottom-up from sound science, not constructed top-down by authority. We absolutely agree with him that science should drive the bus, not just be a slogan on the side of the bus. We also agree with him on *why* science should drive the bus: Real science offers “competitive epistemological pluralism” (Markon, 2013).

### **SCIENCE AS IT SHOULD BE VERSUS SCIENCE-CONCERNED SOCIAL ACTIVITY**

There is, of course, a critical distinction between science and science-concerned social activity, a distinction best captured by what we call the Popper–Feyerabend arc. Popper, at one end of the arc, described what science ought to be (Popper, 1967/1985a). Feyerabend (1975), at the other end of the arc, along with Kuhn and others, pointed to the fact that science is practiced by humans, and thus can become indistinguishable from other human activities, such as in high school, where the popular kids win.

We start with the Popper end of the arc. Popper described what science *ought* to be:

And the schema, originally valid for the animal world as well as for primitive man,

$$P_1 \rightarrow TT \rightarrow EE \rightarrow P_2$$

becomes the schema of the growth of knowledge through error elimination by way of systematic *rational criticism*. It becomes the schema of the search for truth and content by means of rational discussion (Popper, 1967/1985a, pp. 72–73, emphasis in original).

Popper contended that *individual* scientific activity boils down to  $P_1 \rightarrow TT \rightarrow EE \rightarrow P_2$ .  $P_1$  is an initial problem. The classic Popper example is that of a deer on top of a hill seeing a pond over a wood, and beyond, a smaller hill.  $P_1$  is the problem of finding a route to the pond. TT is a tentative theory

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From the University of Tulsa (A. R. H.) and Emory University (S. O. L.).

Apologies to Carrie Underwood for the title.

Address correspondence to Allan R. Harkness, Department of Psychology, 800 S. Tucker Ave., University of Tulsa, Tulsa, OK 74104. E-mail: allan-harkness@utulsa.edu

of how to get to the pond. The deer sets off on that route, and Popper had it encounter an obstacle, such as fallen trees. This calls for EE, or error elimination (O'Donohue, Fowler, & Lilienfeld, 2007). The fallen trees have falsified the original theory, TT. This leads to  $P_2$ : How does one get to the pond with fallen trees in the way? Through continued cycles of tentative theory polished by error elimination, the deer finds an answer.

But the answer is at first all in the animal's head: It is subjective knowledge. Popper (1967/1985a) distinguished three "worlds." World 1 is the world of physical objects, such as the pond, woods, and hills. World 2 is the subjective world, the cognitive maps or action tendencies within the nervous system of the deer. World 2 also includes constructs and more loosely formed ideas in the minds of scientists. World 3 is the published world: the objective contents of thought. In Popper's World 3, "objective" means simply that knowledge has become a tangible object, a journal article, a computer program, a text or phone statement, a published poem, or a work of art. A critical distinction is that World 2 exists only within individual minds, versus World 3, in which ideas have become tangible objects, accessible to other minds. Popper pointed out that the deer could "publish" its result by taking the trail every day and leaving a path. Others may then try the path or may even find a better one. Played out from World 3 materials, in journal articles, blogs, and phone conversations, there can be an intense effort by *rational criticism and discussion* to falsify bad ideas. This approach leads to better TTs, better solutions to the problems. This view is also consistent with Bartley (1984), who regarded the essence of science as the maximization of criticism.

We believe that Popper's description of what science ought to be captures what Markon means by competitive epistemological pluralism. Popper was no naïve falsificationist, as some have caricatured him. Like Lakatos, Quine, and others, he recognized that no single experiment of  $P_1 \rightarrow TT \rightarrow EE \rightarrow P_2$  is adequate to rid a field of poor theory (see also Meehl, 1978). It is only after extensive rational criticism in World 3, on the pages of articles, at the speaker stands of conventions, in blogs and texts, that minds will be convinced.

On the other side of the arc, the Feyerabend school emphasizes that scientific activity, practiced by humans, is subject to all the social forces and foibles of power, popularity, will, personal animus, insularity, provincialism, and arrogance that can characterize any human activity. The product literally *is* a social construction. As Markon aptly observes, the essence of Feyerabend's philosophy of science is "anything goes." But when human activity falls away from Popper's ideal, it is not genuine science, it is what we call *science-concerned social activity*. Anything goes? Really? We hope all of us would reject faking data, ignoring or replacing the opposition's good argument, or spreading gossip about colleagues to undermine their science. These rejections of "anything goes," along with commitment to the Popperian ideals of trying to falsify flawed ideas by rational argument in World 3, are the heart of real science, the only science worthy of driving the bus. Powerful committees asserting their own interests, and membership votes, even when the individuals are well-respected scientists, are not real science (see also

Blashfield & Reynolds, 2012), unless there is honest *competitive epistemological pluralism*, as in Markon's phrase.

### **WHY MARKON'S ARGUMENT IS SO IMPORTANT: AUTHORITY POISONS SCIENCE**

Authoritative diagnostic schemes become frozen in time because they are not subjected to the improvement of Error Elimination and rational criticism. They become stale. In Harkness, Reynolds, & Lilienfeld (in press), we presented this quote from Allen Frances, and we cannot find a better one:

We must accept the fact that our diagnostic classification is the result of historical accretion and accident without any real underlying system or scientific necessity. The rules for entry have varied over time and have rarely been very rigorous. Our mental disorders are no more than fallible social constructs. (quoted in Phillips et al., 2012, p. 25)

To be certain, some *DSM* diagnoses, for example, Panic Disorder, have been well informed by replicated, high-quality science, and ride, as we will explain later, on top of an understanding of an evolved adaptive system. But many *DSM* disorders seem to reflect dubious categories that have been imposed from on high and are thereby insulated from potentially falsifying research.

As an example, consider research on ADHD. Federally funded research examines it, and pharma strongly supports its study. But the research almost always begins with the assumption that the authoritarian scheme is correct: that there is a categorical mental disorder called ADHD. But consider the important question: Who will fund basic research that attempts to falsify the idea that ADHD is a distinct or categorical condition? Who will fund research to examine the hypothesis that there is a naturally occurring dimension of "fluidity of shifting attention" spanning all the way from individuals whose attention tends to stick and be unmoveable, to those whose attention flits easily about? Perhaps this dimension is related to two great occupations in recent evolutionary human history, the agrarian and the hunter-gatherer. Maybe it is just a normal continuum of human variation in an evolved adaptive system of attention. Perhaps the range from stickiness to fluidity of attention is modulated, in part, by dopaminergic systems, and perhaps responses to medication happen merely because the medications have pharmacodynamic effects on sites of action within the evolved and normally adaptive attention system. Suppose some people at each end of the distribution want to change, and we can help them, but others have been diagnosed by authority (*DSMs* and school systems) and medicated against their will?

The ceding of scientific decisions to *DSM* committee authority has, in our view, had baleful consequences. In many respects, it has been a Faustian bargain. We offer this apocryphal story that might have happened to psychology in the late 1970s, and even earlier to psychiatry:

Psychology and the devil happen to meet walking along the road one day.

"How nice to see you," says the devil, "I was meaning to call."

Psychology, grateful for the attention responds, “Yes, of course, delightful to see you, how can I help?”

“No, no, I want to help you!” says the devil, “You have been talking to patients for years, but you are still not getting the respect you so richly deserve.”

“Absolutely right! We talk to patients even more than psychiatrists, but we get paid less!”

The devil put a hand to his beard, considered thoughtfully, and said, “I can fix this terrible situation. I will empower you to name the problems afflicting patients.”

Psychology looked disappointed. “I want to be respected and well paid! What good is naming problems?”

The devil looked amused. “You are Psychology? Humans are the verbose mammals! Brandishing a name conveys the highest of powers and garners great respect. Remember when you had that skin rash?”

“Yes, I went to the dermatologist and I found out I had *idiopathic dermatitis*.”

The devil broke into laughter, “You went in knowing you had a skin rash. When the dermatologist pronounced the words *idiopathic dermatitis*, he was telling you, in dead languages, that you had a skin rash and that he had no idea what had caused it. But you were in awe of his skill and paid him a handsome fee.”

Psychology began to grin sheepishly, “Okay, what do I need to do to gain this power of diagnosis?”

“You must give up any little dribbles of skepticism you might have about the scientific provenance of the names, even if it is decided on by a vote of the membership of the American Psychiatric Association. To gain the power of naming, you must give up science and submit to authority.”

“Done!” cried Psychology, donning a white coat.

### **THE PUBLIC BELIEVES THE DIAGNOSTIC SCHEME IS THE PRODUCT OF REAL SCIENCE**

We believe that the Allen Frances quote presented earlier would come as a great shock to much of the general public. Frances’s frank language might surprise lawyers and legislators who apply the scheme to Americans with Disabilities Act cases and personal injury suits. It might surprise parents who medicate their children under this scheme, not to mention insurance and Medicare administrators. If people pay their psychologists and psychiatrists to practice based on science, we certainly have a duty to present to the public the scientific status of diagnostic pronouncements, just as surely as we are required to present the science behind treatment and assessment. We have a duty to inform the public about the limits of our scientific knowledge regarding current diagnoses. If caffeine withdrawal, now classified as a mental disorder in *DSM-5* (American Psychiatric Association, 2013), does not have the same ontological status as typhoid, should not the public be informed?

Although we regard telling a client that he or she has a borderline personality disorder as a potential violation of *primum non nocere*, anyone who elects to do so perhaps ought to begin like this: The term “borderline” came from a now rarely used technique called classic psychoanalysis, the practitioners of which found that some of their ostensibly worried patients became

psychotic—out of touch with reality and highly emotional “on the couch.” The diagnosticians should then explain any science bolstering the distinct disorder conceptualization. They certainly should make reference to scientist–practitioner Marsha Linehan’s alternative and better-supported conceptualization: some people experience very strong emotions and have trouble regulating them (Crowell, Beauchaine, & Linehan, 2009).

If Markon, Frances, and we are right that the current diagnostic scheme is largely a pronouncement of authority not based on sound science, then it is immoral, unethical, and poor practice to brandish a diagnosis without providing the same level of careful caveats we are required to provide for assessment and treatment. For example, responsible clinicians present the scientific basis of a treatment, its record of success or failure, its risks and potential benefits, and any potential alternatives. Patients deserve the same information concerning diagnostic categories. Caveats regarding the scientific provenance of and scientific support for psychiatric diagnoses should become routine in testimony before legislatures, before the courts, and in reports to clients and their insurance companies.

### **AREAS OF DISAGREEMENT WITH MARKON?**

In our attempt to provide a rational discussion of his important article, we find a few areas of potential disagreement, but it is possible that we misunderstood or misconstrued Markon’s arguments. We appear to understand constructs differently, and we believe we hold a somewhat different view of what “wins” in the competitive epistemological pluralism of science.

### **CONSTRUCTS VERSUS CRITERIA**

Markon seems to discuss constructs almost as if they are criteria. He writes, “I would note that a measure or construct is only valid or useful for a particular purpose, or within the context of a particular theory” (pp. xxx). He also writes that, “the answers to what constructs are most salient likely depend on the context and are perhaps impossible to definitively distinguish from closely related constructs more relevant in some other context” (p. xxx). We believe it is crucial to distinguish constructs from measures (Cronbach & Meehl, 1955; Loevinger, 1957). To be falsifiable, constructs must be connected to measurements and must have material implications (Harkness, 2005), but the construct is not equivalent with the measurement (see Skeem & Cooke, 2010, for a recent example in the field of psychopathy). For example, in the Personality Psychopathology–Five (PSY-5) program of research, a careful distinction has always been made between constructs (Harkness & McNulty, 1994) and measures (Harkness, McNulty, & Ben-Porath, 1995; see also Harkness & Hogan, 1995). Constructs are explanatory elements in theories. In Popper’s understanding of science, without Tentative Theory, there is no science. Facts cannot speak for themselves; they only speak through theory, that is, some explanation of what the facts mean. Popper (1960/1985b) pre-

sented highly persuasive arguments against the Baconian empiricism that lies behind “operational definitions”: They are empty of theory.

Markon quite rightly criticized the operational definitions of psychiatry’s diagnostic criteria. Operational definitions, as we have argued (Harkness et al., in press), are not falsifiable; they can only be improperly applied. We also strongly argued against the “C is for Criteria” in NIMH’s recent Research Domain Criteria (RDoC) initiative. Authority-derived criteria or operational definitions are not disputable because they are designed to be devoid of theoretical content (see MacCorquodale & Meehl, 1948). One meter is no longer defined as the length of the standard bar in Paris, it is now defined as  $(1/299,792,458) \times$  the distance light travels in one second, in a vacuum. The meter bar was not falsified; it was replaced with a more stable standard.

Constructs, in contrast, are the explanatory components of theory. Specific measurement criteria can be found to be either good or poor markers of the construct in a program of construct validity. Heat has come to be understood as a quantity of energy—the average kinetic energy of atoms and molecules whizzing around in a substance. Temperatures are proportional to this average kinetic energy, whether they are measured by water or mercury expansion in a thermometer, change of shape or conductivity of a bimetallic probe, or black body radiation emission frequencies. All temperature measures have to be understood relative to the construct heat, and its inner nature, the faster whizzing of atoms and molecules. Heat now explains how the instruments work. What makes heat successful as a construct is not its context dependence, but its context independence.

### **WHAT SHOULD WIN IN SCIENTIFIC COMPETITION IS THE EXPLANATORY REACH OF CONSTRUCTS, THEIR CONTEXT INDEPENDENCE**

Markon advocates, as do we, for fair competitive epistemological pluralism. We believe this competition takes the form of rational discussion using Popper’s World 3, where ideas are made into concrete objects—journals, blogs, and texts. We contend that concepts with explanatory reach across sciences and scales of phenomena should be the winners in competitive epistemological pluralism. Ideally, scientific consensus should *follow*, never precede, the winners in fair competitive epistemological pluralism, but we explain that even in this case consensus should be provisional.

Nevertheless, Feyerabend (1975) was right that scientific activity is inevitably a human activity. Scientific consensus can have many non-scientific causes. Scientists may reach consensus because of authority, commitment to paradigms learned in graduate school, the control of research funding, or a simple failure to think and reason critically. We note with delight the failure of the Nazi nuclear program, due largely to a hatred of “Jewish science.”

If competitive epistemological pluralism lies at the core of science, final consensus is not a good thing. Early on we understood Huntington’s to be an autosomal dominant disorder. Perhaps a victory for categorical thinking? However, researchers later found that at the gene locus, the number of

CAG repeats predicts age of onset, which runs in pedigrees. Is number of repeats a victory for quantitative variation over categorical diagnosis? Now we know the phenotype shows both Mendelian (single gene) inheritance *and* the quantitative effects of number of repeats. Such refinements only operate by questioning and revising consensus (e.g., Bartley, 1984). Because science operates by falsification, complete consensus is the enemy of progress; it can be stifling, freezing progress on a topic. Such consensus can represent a victory of Feyerabendian social authority over reason and critical thinking.

Instead of consensus, it is the explanatory breadth, reach, and predictive power of constructs that should be convincing, not a vote of scientists. What do we mean by breadth, reach, and predictive power of constructs? In the genetics example, varying numbers of CAG repeats happens at *a particular locus* in the genome, explaining both the Mendelian inheritance and familiarity of age of onset. Markon (p. xxx) mentions that, “someone studying a molecular neurobiological feature of mental illness might find an entirely different set of constructs useful or valid than someone studying socioeconomic influences on mental illness.” This is true. Our point is that a construct that can explain both should be the winner in competitive epistemological pluralism.

Constructs of reach and predictive power connect *across* sciences, what Wilson (1998) called consilience. Stanovich (2009) argued that “connectivity,” the capacity of a research program to connect with broader findings in other well-established disciplines, is a key route to success in science. At one time, alchemy and chemistry were largely inseparable. The ideas from this mixture that were able to connect optics, metallurgy, and pharmacy compounding became modern chemistry. The ideas we now call alchemy, which lacked predictive power and consilience/connectivity, died out. Only with the development of nuclear physics did this idea make a comeback: An atom of one “element” could become an atom of another element. However, the ideal of transforming fecal material into gold rightfully remains in the out basket. Had Einstein been successful in constructing a theory explaining both cosmic scales and subatomic scales, it would have quickly replaced both general relativity and quantum mechanics. This is because a comprehensive theory would have had to predict both in the context of large scales *and* in the context of small scales. Again, constructs that display context independence are especially likely to be long-term scientific victors.

### **IS ALL OF THE INTERNATIONAL CLASSIFICATION OF DISEASE-9TH EDITION-CLINICAL MODIFICATION (ICD-9-CM) DICTATED BY AUTHORITY?**

We have argued elsewhere (Harkness et al., in press) that most of the description of problems in medicine and biology, psychiatry excepted, is performed by a Review of Systems (ROS). Much of *ICD-9-CM* is organized around major evolved adaptive systems: circulatory, respiratory, digestive, sensory, and so on. The typically adaptive functions of such major systems are well understood and agreed upon (but the agreement here flows from science,

rather than being imposed from above), and the basic science underlying those systems—their anatomy, physiology, comparative vertebrate homologies, biology, and biochemistry—is well developed. Sound general psychology takes these systems into account as well. Psychologists have been major contributors in understanding sensory and perception systems. And let's not forget that Pavlov's Nobel was for digestive physiology—classical conditioning grew out of sound observation, measurement, and theory building in his research program.

In the ROS sections of *ICD-9-CM*, the understanding of problems is always layered on top of the basic science explaining a normal, evolved, adaptive system. Visual problems are not just called “reading disorder” or “seeing flashing points disorder,” for example. Instead, visual problems are understood only in terms of the anatomy, physiology, biology, and biochemistry of an adaptive system. Only constructs that integrate across the sciences are powerful enough to survive competitive epistemological pluralism. Clinical problems, under the ROS, are argued out primarily on the pages of *Lancet*, *Journal of the American Medical Association*, *New England Journal of Medicine*, *Science*, and *Nature*. Moreover, committee work, in ROS areas, involves racing to keep up with scientific progress rather than the issuing of authoritarian edicts. That is how things ought to be.

Harkness and colleagues (in press) suggested that clinical and psychological problems can best be understood by developing the science underlying the major normal, evolved systems that adapt to the *external* environment. We suggested a non-exhaustive list of five major adaptive systems: Reality Modeling for Action, Short-Term Danger Detection, Long-Term Cost/Benefit Projection, Resource Acquisition, and an Agenda Protection System. In the article, we discussed how such systems dynamically interact in the nervous system, and how they have created the great cross-cutting themes in the history of general psychology, individual differences, and psychopathology. We hazard a bold conjecture (Popper, 1965) that rational discussion and arguments organized by these major adaptive systems will ultimately provide winning concepts in a free competition in competitive epistemic pluralism.

What if Harkness and Lilienfeld had the power to impose this ROS on the field? Not likely, of course. But we hope we would have the commitment to science to avoid such a temptation of power. It would poison and forever weaken the constructs by protecting them from error elimination and improvement on the pages of journals, blogs, and texts. The constructs we propose comprise only tentative theory that must be exposed to criticism, error elimination, and refinement. Any success of these constructs should arise from the reach and breadth of their explanatory and predictive powers, not from dicta.

### **HOW DOES SCIENCE TAKE THE WHEEL?**

To be on the winning side in this rational battle of the future, psychology and psychiatry will have to change. The discipline of psychology has been overly influenced by Max Weber's concept of social science: That *Homo sapiens* is

so different from other species that we may call ourselves scientists despite remaining naïve and ignorant of all other sciences. Humans are quite special, but anyone with an open mind after Darwin knows that we are not a special creation, somehow above the rest of nature. Darwin showed how life itself is within the scope of scientific examination. Psychiatry adopted a philosophical atavism—it attempted to link Baconian empiricism, an antitheoretical stance since *DSM-III*, with empirical trial and error psychopharmacology: Make a diagnosis by theory-free diagnostic criteria, then try a sequence of drugs, A, B, C... and combinations, A & C, etc., until the complaint stops. It sounds harsh, but who doubts the truth of it? Instead, we need psychologists and psychiatrists who are real scientists, willing to invest the time and effort to understand the normal, evolved adaptive systems that allow mammals to exist in and capitalize on the external environment. If psychology and psychiatry refuse, eventually neuroscience will develop a clinical branch that supplants them. That would be a shame, as we psychologists and psychiatrists are unmatched in our understanding of how these adaptive systems manifest at the psychological level of analysis. Yet only when we become scientists able to conceptualize and debate major adaptive systems will we possess constructs of the reach and power needed to supplant authority.

## REFERENCES

- American Psychiatric Association. (2013). *Diagnostic and statistical manual of mental disorders* (5th ed.). Washington, DC: Author.
- Bartley, W. W. (1984). *The retreat to commitment* (2nd ed.). New York: Knopf.
- Blashfield, R. K., & Reynolds, S. M. (2012). An invisible college view of the DSM-5 personality disorder classification. *Journal of Personality Disorders, 26*, 821–829.
- Cromer, A. (1993). Uncommon sense: The heretical nature of science. *Science, 265*, 688.
- Cronbach, L. J., & Meehl, P. E. (1955). Construct validity in psychological tests. *Psychological Bulletin, 52*, 281–302.
- Crowell, S. E., Beauchaine, T. P., & Linehan, M. M. (2009). A biosocial developmental model of borderline personality: An elaboration and extension of Linehan's biosocial theory. *Psychological Bulletin, 135*, 495–510.
- Feyerabend, P. K. (1975). *Against method*. London: Verso.
- Harkness, A. R. (2005). Essential Paul Meehl lessons for personality assessment psychology. *Journal of Clinical Psychology, 61*, 1277–1294.
- Harkness, A. R., & Hogan, R. T. (1995). The theory and measurement of traits: Two views. In J. N. Butcher (Ed.), *Clinical personality assessment: Practical approaches* (pp. 28–41). New York: Oxford University Press.
- Harkness, A. R., & McNulty, J. L. (1994). The personality psychopathology five (PSY-5): Issue from the pages of a diagnostic manual instead of a dictionary. In S. Strack & M. Lorr (Eds.), *Differentiating normal and abnormal personality* (pp. 291–315). New York: Springer.
- Harkness, A. R., McNulty, J. L., & Ben-Porath, Y. S. (1995). The personality psychopathology five (PSY-5): Constructs and MMPI-2 scales. *Psychological Assessment, 7*, 104–114.
- Harkness, A. R., Reynolds, S. M., & Lilienfeld, S. O. (in press). A review of systems for psychology and psychiatry: Adaptive systems, personality psychopathology-five (PSY-5), and the DSM-5. *Journal of Personality Assessment*.
- Loevinger, J. (1957). Objective tests as instruments of psychological theory. *Psychological Reports, 9*(Suppl.), 635–694.
- MacCorquodale, K., & Meehl, P. E. (1948). On a distinction between hypothetical constructs and intervening variables. *Psychological Review, 55*, 95–107.
- Markon, K. E. (2013). Epistemological pluralism and scientific development: An argument against authoritative nosologies. *Journal of Personality Disorders, 27*(5), 554–579.
- 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*(4), 806–834.
- O'Donohue, W. T., Fowler, K. A., & Lilienfeld, S. O. (2007). Science is an essential safeguard

- against human error. In W. T. O'Donohue & S. O. Lilienfeld (Eds.), *The great ideas of clinical science: 17 principles that every mental health professional should understand* (pp. 3–27). New York: Routledge.
- Phillips, J., Frances, A., Cerullo, M. A., Chardavoigne, J., Decker, H. S., First, M. B., ... Zachar, P. (2012). The six most essential questions in psychiatric diagnosis: A plura-logue, part 1: Conceptual and definitional issues in psychiatric diagnosis. *Philosophy, Ethics, and Humanities in Medicine*, 7, 14.
- Popper, K. R. (1965). *Conjectures and refutations: The growth of scientific knowledge* (2nd ed.). New York: Basic Books.
- Popper, K. R. (1985a). Knowledge: Subjective versus objective. In D. Miller (Ed.), *Popper selections*. Princeton, NJ: Princeton University Press (Original work published 1967).
- Popper, K. R. (1985b). Knowledge without authority. In D. Miller (Ed.), *Popper selections*. Princeton, NJ: Princeton University Press (Original work published 1960).
- Skeem, J. L., & Cooke, D. J. (2010). Is criminal behavior a central component of psychopathy? Conceptual directions for resolving the debate. *Psychological Assessment*, 22, 433–445.
- Stanovich, K. (2009). *How to think straight about psychology* (8th ed.). Boston: Pearson.
- Wilson, A. O. (1998). *Consilience: The unity of knowledge*. New York: Knopf.