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# A Tale of Three Blind Men on the Proper Subject Matter of Clinical Science and Practice: Commentary on Plaud's Behaviorism vs. Ilardi and Feldman's Cognitive Neuroscience



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Plaud (J Clin Psychol 57, 1089–1102, 1109–1111, 1119–1120) and Ilardi and Feldman (J Clin Psychol 57, 1067–1088, 1103–1107, 1113–1117, 1121–1124) argue for two very different approaches to clinical science and practice (i.e., behavior analysis and cognitive neuroscience, respectively). We comment on the assets and liabilities of both perspectives as presented and attempt to achieve some semblance of balance between the three protagonists embroiled in this current debate. The vision of clinical science we articulate is more ecumenical and evolutionary, rather than paradigmatic and revolutionary. As we see it, the problem clinical psychology faces is much larger than the authors let on; namely, how best to make clinical science meaningful and relevant to practitioners, consumers, the general public, and the behavioral health-care community. Clinical psychology's immediate internal problem is not pluralism with regard to subject matter, worldview, methodology, or school of thought, but pluralism in clinical psychologists' adherence to a scientific epistemology as the only legitimate form of clinical psychology. On this latter point, we still have a very long way to go. © 2001 John Wiley & Sons, Inc. J Clin Psychol 57: 1133–1148, 2001.

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It was six men of Indostan  
To learning much inclined,  
Who went to see the Elephant  
Though all of them were blind,  
That each by observation  
Might satisfy his mind.

—John Godfrey Saxe (1963)

*The Blind Men and the Elephant*

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American poet John Godfrey Saxe's (1963) classic poem, *The Blind Men and the Elephant*,<sup>1</sup> was based on a fable that first was told long ago in India. The poem describes six scholarly blind men who each approach an elephant from different perspectives and attempt to describe what it is. One of the men happens on the elephant's side and claims it to be a wall; another approaches the trunk and concludes that the elephant is actually a spear, and the others conclude that the elephant is either a snake, a tree, a fan, or a rope. When the six men of Indostan came together to discuss their observations, we are told that they argued loud and long, each in his own opinion, exceedingly stiff and strong, though each was partly in the right, and all were in the wrong. They were wrong in the sense of holding to the view of having provided a complete description of the elephant as a whole. They were wrong in that they failed to appreciate the validity of different and potentially valuable scientific points of view; and wrong in not appreciating how their own observations are products of their respective analytic assumptions, values, goals, choice of methods, training, and history. Had each of the blind men moved about the elephant, they would have likely realized the incompleteness of their descriptions. Yet, each of the blind men believed that Truth is just one thing, instead of truth comprising many things to different people and for different purposes. Not only could they not see the elephant, but they also could not see the truth value of any other point of view. Indeed, the poem has little to do with physical blindness, but does have everything to do with the consequences of *analytic blindness* regarding disagreements at the level of scientific frameworks or worldviews. Saxe (1963) laments on the predictable outcome of such analytic blindness in the conclusion of the poem—where we also learn the moral and a valuable lesson—when he writes:

So oft in theologic wars,  
The disputants, I ween,  
Rail on in utter ignorance  
Of what each other mean,  
And prate about the Elephant  
Not one of them has seen!

Such an outcome is not an inevitable consequence of scientific activity or philosophic dialogue and debate. However, it is a predictable consequence of the kind of talk involving discussants each claiming to hold *the* view of reality, not *a* view of reality, and subsequently criticizing those who hold different (not better or worse) philosophic, analytic, and epistemic positions for not being more like them (see Ilardi & Feldman, 2001; Plaud, 2001). Those who get caught in this trap, however well intentioned, often devote considerable time and effort disputing the claims of others, defending their own claims, and, by trying to convert others to the validity of their own views by fiat, appeal to data and authority, use logical argumentation, and so on. Saxe's blind men nicely illustrate the dangers of such scientific tunnel vision and an associated preoccupation with one's own way of seeing things to such an extent that one cannot even imagine that there could be any value in other ways of looking at the world, or, metaphorically speaking, the human elephant in the case of clinical psychology. Within psychology, examples of this sort are easy to spot and usually involve claimants arguing for a one world psychology (i.e., one subject matter, one metatheory or theory, one school, one set of terms or principles, one worldview) while discrediting other existing forms of psychology for not conforming to

<sup>1</sup>The parable is thought to have originated in China sometime during the Han Dynasty (202 BC–220 AD) as *Three Blind Men and the Elephant* (Kou & Kou, 1976). Saxe expanded the parable to include six blind men. There is also an African version of the tale.

*the mold* (e.g., Staats, 1994). The unworkability of this approach should be self-evident, but often it is not. As with Saxe's blind men, it is difficult to see that others see things differently, and even that there is some value to pluralism in science and practice. After reading Plaud (2001) and Ilardi and Feldman's (2001) somewhat pointed exchange about how best to define and advance psychology, and clinical psychology in particular, we were immediately reminded of the blind men and message of Saxe's (1963) poem, although in the present case we are dealing with only three men, none of them fictional.

Plaud (2001) argues that the proper subject matter of psychology and clinical science is behavior, not mind, and is dismissive of the position Ilardi and Feldman (2001) are advocating, namely the cognitive neuroscience perspective, and, by implication, most other branches of psychological science that are not behavior analytic in their approach. There is no room in Plaud's clinical science for cognitive psychology (as traditionally conceived), and little (if any) room for biological levels of analysis. Ilardi and Feldman (2001) take a similar stance from a different perspective and, unlike Plaud, start (and end) inside the skin and argue that cognitive neuroscience, with the brain as subject matter, is the best means of explaining and modifying human behavior. Not surprisingly, behaviorism, behavioral and non-behavioral therapies, and surprisingly some well-established brands of cognitive theory and therapy (e.g., Beck's cognitive therapy for depression) are given short shrift when pitted against what the authors see as our primary task of mapping facets of human-brain functioning. Moreover, they go on to suggest that clinical scientists and practitioners, in essence, should stop what they are doing and adopt the cognitive neuroscience paradigm as *the* working metatheoretical framework for the science and practice of clinical psychology. Both sides of this debate see a fundamental problem with the current state of clinical psychology, and they see the solution as involving one of two alternatives: behaviorism or cognitive neuroscience.

Our purpose here is to comment on the behavioral and cognitive neuroscience perspectives as presented by the authors in this miniseries in terms of some general themes. Specifically, our intent is to achieve some sort of balance between the two "competing" camps, while correcting what we see as some fundamental errors in the manner in which the positions were presented. We then attempt to offer a modest conciliatory view of clinical science and practice as situated within the context of the psychological sciences, not *a* particular brand of psychological science—that is, a pluralistic clinical science where clinical psychologists uphold a shared scientific approach in their efforts to understand, prevent, and alleviate a wider range of human suffering. The problem, as we see it, has little to do with clinical psychology being comprised of several different (not better or worse) analytic frameworks and schools of thought. The real problem is that clinical psychology is still struggling with how best to incorporate science as a means to find answers to fundamental questions about human suffering and its alleviation. That is, clinical psychology's problem is that there are too many clinical psychologists who behave inconsistently with a scientific epistemology, whether in laboratory or clinical settings (see McFall, 2000), not that there are several psychologies.

### I See Behavior, You See the Brain, We See a Problem

The context of the exchange between the authors in this series is most clearly one of debate; a term that derives from the Old French *debatre* that, when taken literally, means to beat or to batter. In modern usage, the term most often denotes a verbal contention by words or arguments between parties (i.e., a verbal beating). Debates, therefore, entail opposition, divisiveness, and division, and quite often polarized points of view. In such a context, there is a tendency for the parties to want to be right, and particularly to do a

better job of convincing others that one is, in fact, right. There is little room for open recognition by the protagonists of where they might be wrong, for doing so would seemingly weaken the position one is advocating. Debates are, at the core, about winners and losers, and ultimately about swaying those lurking or riding the fence to align themselves with the victor. Ironically, debates rarely, if ever, achieve such outcomes in politics or science (e.g., mind–body debate, cognitive–behavioral debate, scientist–practitioner debate, prescription privileges debate, efficacy vs. effectiveness debate, and so on). Some might even suggest that debate, particularly at the level of values and schools of thought, should be left to religion and politics, not science. A debate (verbal beating) simply is not an effective way to promote the assets of an analytic framework in science, particularly when the sides pitted against one another differ fundamentally with respect to scientific values and assumptions. Unless one is willing to hold tentatively to such assumptions (as one probably should), verbal argument alone (no matter how convincing) will not work. Saxe's blind men did not learn this lesson, and the three protagonists embroiled in the current exchange also seem caught in a similar trap.

As the target articles in this miniseries illustrate, it often is notoriously difficult to escape from the more insidious function of debate where the point of contention revolves around fundamental ideological differences that cannot be resolved simply (if at all), particularly by use of logical argument, appeal to other like-minded ideologues, or even by marshaling data and experimental testimony (see Plaud, 2001; Ilardi and Feldman, 2001). The history of science and psychological science suggests that knowledge claims are not value free and do not lead themselves to resolution by brute force argument or appeal to data and authority (e.g., Staats, 1991, 1993, 1994). In fact, this tactic can result in exactly the opposite effect—namely, further alienation of the audience one is trying to reach. John B. Watson's now classic 1913 article, "Psychology as a Behaviorist Views It," is a case in point. In that article, Watson attacked the mainstream definition of psychology as the science of mind or consciousness and argued forcibly that psychology should be viewed as a purely objective experimental branch of natural science whose theoretical goal is not the understanding of mind, but the prediction and control of behavior (Watson, 1913). The zeitgeist at the time of Watson's behaviorist manifesto was one of controversy within psychology, particularly regarding the nature of consciousness and the methods to study it. In a radical sweep, Watson seemingly resolved the issue by throwing out the bread and butter of psychology's scientific subject matter and methods (i.e., consciousness and introspection, respectively) and replaced them with behavior as subject matter and observation as the method. Yet, as Wozniak (1997) noted,

... it was more than ten years before Watson's ideas had any real impact within American psychology and, when it did, it did so *not by converting the old guard* to Watson's vision but by attracting the young to a set of intellectual commitments that had already become broader, more varied, and philosophically more sophisticated than those of Watson (pp. 198–199; emphasis added).

The simple lesson we learn from Watson is that one is likely to have the greatest impact on those that are not wedded already to one particular school of thought. In fact, few of Watson's contemporaries converted to behaviorism. We also learn that science is more evolutionary than revolutionary with respect to innovation and advances.

In the present case, we very much doubt that Plaud's (2001) version of behaviorism will result in Ilardi and Feldman (2001)—including other readers who are not aligned already with behavior analysis—joining the behavior analytic cause. The same can probably be said regarding the impact of Ilardi and Feldman's (2001) version of the cognitive neuroscience perspective. Here, our choice of the word "version" in both cases is not

accidental. Plaud's rendition of behaviorism is *one* behavior analytic account, and most certainly not *the* behavioral account. Behaviorism is not a singular approach and should not be equated solely with Skinner or behavior analysis. There is a great deal more to contemporary behaviorism and behavior analysis than what Skinner said (e.g., Dougher, 2000; Hayes, Hayes, Reese, & Sarbin, 1993; Hayes, Jacobson, Follette, & Dougher, 1994; Hayes, Strosahl, & Wilson, 1999; Kohlenberg & Tsai, 1991; Kohlenberg, Tsai, & Dougher, 1993), a point not entirely obvious by Plaud's (2001) limited textbook rendition of the philosophy and science of behavior analysis. Likewise, the interdisciplinary system described by Ilardi and Feldman (2001) as cognitive neuroscience should not be taken as *the* cognitive neuroscience perspective. Indeed, cognitive neuroscientists routinely disagree at the level of theory, terms, and models, and the same is true among members of other so-called natural sciences (e.g., physics, chemistry), including scientists operating within unique subareas such as behavior analysis. The authors in this miniseries would want us to believe that clinical psychology (and psychological science more generally) would be served best by having all psychologists conform to either a behavior analytic or cognitive neuroscience worldview. In our view, clinical psychology, and psychological science more generally, is served best by advancing multiple perspectives that share a commitment to advancing science as an iconoclastic way of knowing, a point that we will return to shortly. Before doing so, however, we would like to comment briefly on some specific points and general themes of each of the target articles. We preface our comments by acknowledging our own analytic leanings as clinical scientists, leanings that are grounded in the philosophy and science of clinical behavior analysis and behavior therapy, but that also routinely incorporate analytic approaches from cognitive science and psychophysiology in our basic and applied clinical research.

#### Clinical Psychology as a Behaviorist Views It

In his behavioral manifesto for clinical psychology, Plaud (this issue) expresses discontent, and one might say even frustration, with the current state of basic and applied clinical science and argues that (a) psychology of any stripe is viewed best as a branch of the natural sciences, and that (b) behavior is the only legitimate and proper subject matter of psychology as a science. Both claims echo arguments originally put forth by Watson (1913) and later developed by Skinner and other behavior analysts who follow the philosophy of science known as radical behaviorism. Both claims also generally are consistent with the behavior analytic worldview, but are not synonymous with it. As indicated, there is a great deal more to behavior analysis than what Skinner said, a point not entirely obvious by reading what Plaud (2001) has said.

Our intent here is not to promote the goings on within the field of behavior analysis post-Skinner, nor is our intent to convince the reader who is not affiliated already with behavior analysis that he/she is necessarily on the wrong track and should therefore contact the behavior analytic literature. We certainly hope that the intellectually curious reader might see for him/herself what clinical behavior analysts have done post-Skinner, particularly regarding the understanding of important clinically rich phenomena such as thinking, feeling, emotion, and, more generally, their relation to human suffering and its alleviation (e.g., see Dougher, 2000). Our immediate concern, however, is that the reader will not bother to look, in part, because Plaud has advanced behavior analysis by using it as the gold standard by which to criticize other differing perspectives within psychology (e.g., cognitive psychology, biopsychology, cognitive neuroscience, including some therapies such as cognitive therapy and Rational Emotive Behavior Therapy). For example, Plaud (2001) states that the problem with modern cognitive psychology is that it does not

explain behavior; it defines terms that are, in reality, more behavior that is in need of explanation at the behavioral level. Here, Plaud faults cognitive psychologists for not talking like behavior analysts about their subject matter. He also goes on to equate (albeit wrongly in our view) cognitive psychology with biological psychology, and argues that both should be cut out of psychological science. We see this rhetorical strategy as fundamentally misguided in principal, for one legitimately cannot advance one's position by criticizing others for not holding to it, a cautionary note also relevant to how Ilardi and Feldman (2001) advocate for the cognitive neuroscience view. Cognitive psychologists' research and verbal interpretations about cognition and cognitive processes are perfectly consistent with a mechanistic worldview and related assumptions and scientific values, whereas a behavioral analysis of language and cognition is consistent with a contextual worldview and related epistemic values. No amount of "behaviorese" will convince cognitive psychologists otherwise. Although one might be tempted to stand on data and experimental testimony to convince others to abandon their preferred scientific frameworks and modes of explanation, there is ample evidence that such a strategy is unworkable. Indeed, the cognitive-behavioral debate is a classic modern (though some might say antiquated) example where a data-driven mode of argument should have rendered the debate moot long ago, particularly as both sides have plenty of data to back up their respective claims. At best, all that one can do is articulate clearly one's own assumptions and values and how both frame answers to scientific and practical questions. This latter strategy promises to offer the best chance of promoting understanding and contact with basic and applied behavior analytic work.

Regarding understanding, behavior analysts have long felt poorly understood by much of mainstream psychology, including psychologists operating within closely affiliated branches of psychology, such as behavior therapy. Although some of this misunderstanding can be traced, in part, to misrepresentation and mischaracterization of behavior analysis in the literature (see Todd & Morris, 1992, for a discussion), much of it has to do with the nature of behavior analysis itself and the behavior of behavior analysts.

Behavior analysis is, in many respects, a difficult path to follow, in part because the behavior analytic worldview (i.e., contextualism)—including related scientific terms and principles—run counter to common nonscientific forms of talking, both with respect to our own actions and explanations offered for the actions of others (see Hawkins & Forsyth, 1997; Jacobson, 1997; O'Donohue, Callaghan, & Ruckstuhl, 1998). For instance, every day we hear ourselves and others speak of feeling tired, stressed, angry, remorseful, hurt, anxious, afraid, joyful, and such terms quite often are offered as explanations for what we and others do. Clients likewise frequently speak of private events as causes of their suffering, often metaphorically (e.g., "I am depressed," "I feel empty or hurt," "The pain is like a knife going through my chest," or "I can not fly in a plane because I might have a panic attack"). In clinical science and practice, one routinely can find similar emotional terms that are invoked as objects of study (e.g., the DSM), as explanations for behavior, and as targets for change in therapy (e.g., unwanted thoughts or emotions). Yet, most emotion theorists will be quick to point out that emotional states denoted by such words (e.g., fear) are not technical "scientific" terms within psychology or "things" in any ontological sense, but rather convenient short-hand descriptions of contextually situated biological, psychological, and overt behavioral events. The same can be said for hundreds of other terms referring to emotional states and modes of thinking that have made their way into psychological science from everyday discourse. Behavior analysts generally are dissatisfied with using such terms to explain behavior in psychological science, preferring instead to address their explanations at the level of the phenomena to which such terms refer in a manner consistent with basic principles and laws of behavior. What behavior analysts do not do, however, is reject the very real phenomena denoted by

such terms as beyond the purview of behavioral science (e.g., Anderson, Hawkins, & Scotti, 1997). A science of private life is what makes radical behaviorism radical and is what differentiates behavior analysis from Watsonian and other forms of neo-behaviorism. Our point here is that such scientific talk about behavior is quite foreign and, in many respects, represents one of several barriers to a more widespread acceptance of behavior analysis (O'Donohue et al., 1998).

Behavior analysts value parsimony in explaining behavior by invoking principles of behavior that are both precise (technically speaking) and broad in scope (practically speaking; e.g., positive reinforcement). Behavior analysts also place a premium of explanations that allow for prediction and influence and openly acknowledge that our only direct means of influence over what we think, feel, and why we otherwise behave as we do is at the level of the environment. Thus, the subject matter of behavior analysis is not behavior per se, but behavior–environment relations, with behavior including, as Plaud (2001) points out, what traditionally are referred to as thoughts, feelings, emotions, and overt actions. No new terms are invented to explain behavior occurring within the skin. Instead, behavior analysts interpret and explain all behavior in context using a common set of technical terms and principles, as have been developed from the basic science. What this means for the science is that both the basic and applied branches speak the same language and truly are integrated, such that as the basic science branch advances, so too does the applied branch and *visa versa*. The immediate clinical and pragmatic implications of this strategic position is that it forces an analysis of the variables that lead to problematic behavior and the variables that maintain the problematic thinking, feeling, and overt action. Perhaps most importantly, however, this view addresses itself to the vexing clinical question as to how the talk that goes on between therapist and client in therapy can lead ultimately to behavior change and the alleviation of human suffering outside of therapy (e.g., see Kohlenberg et al., 1993), an issue that should be of immediate concern for clinicians of any theoretical stripe or school of thought that share an interest in applying and developing better and more effective strategies for influencing human suffering in therapy.

However, by this account one should not be misled into thinking that behavior analysts are concerned only about behavior–environment relations and disregard the importance of biological factors in explanations of behavior. Although Plaud (2001) argues that biology and physiology are unimportant in a behavior analytic or psychological account and therefore should remain outside the purview of psychology, we disagree (see also Baer, 1996; Donahoe, 1996; Poling & Byrne, 1996). According to Moore (2000), a behavior analyst, physiology is not only important for an adequate behavior analytic account, but he suggests further that behavior analysis and theoretical behavioral neuroscience are complementary sciences. According to Moore (2000),

Physiological phenomena concern two unavoidable temporal gaps in a behavioral account. The first gap is between behavior and the variables of which it is a function, as the behavior takes place. The second gap is between the experiences of an organism in its surrounding circumstances and any resulting changes in its behavior, as that behavior is observed in the future. Information about the events that take place during these gaps will be provided by physiologists, rather than psychologists, although psychologists might inform physiologists of the factors for which they should look (e.g., Catania & Harnad, 1988, p. 470). In doing so it [physiology, neuroscience] provides additional information that will guide efforts to predict and control behavior. . . . (p. 180)

Although Moore, like Plaud, sees such efforts as suited best for physiologists, not psychologists, we see no reason why psychologists, of any stripe, cannot do both kinds of work. In fact, we think they should, particularly as psychologists are positioned best to

identify important gaps in our understanding of human action that biology and physiology might help account for (see also Skinner, 1987). Yet, we are somewhat skeptical that psychological levels of analysis some day will be reduced to biological levels of analysis, or that such a reduction would be beneficial for psychology as a basic and applied science. Our skepticism rests on our own pragmatic predilections, namely those favoring contextually situated explanations that allow for direct influence, including direct influence over biological events. To date, the only means of direct influence over psychological phenomena (i.e., cognition, emotion, overt behavior, physiology) is at the level of the environment, whether this be administering a drug, performing a surgical procedure, talking in therapy, learning by watching others, speaking and listening, or simply changing a deficit or inappropriate environment and related contingencies to promote more adaptive functioning. Behavior analysts and therapists have capitalized on this simple observation, leading to powerful psychosocial intervention technologies, technologies that are linked closely to the basic science. The net result is that we not only know that such interventions work, but also quite a bit about why they work.

Our final comments here concern a broader theme of Plaud's (2001) target article, one that Skinner (1987) explicitly addressed in the question posed by the title of his *American Psychologist* paper, "Whatever Happened to Psychology as a Science of Behavior?" In that article, Skinner noted the following regarding behavior as subject matter and the place of behavior analysis in the field of psychology:

For more than half a century the experimental analysis of behavior as a function of environmental variables and the use of that analysis in the interpretation and modification of behavior in the world at large have reached into every field of traditional psychology. Yet they have not become psychology, and the question is, Why not? (p. 782; emphasis original)

Plaud (2001) answers this question by pointing the blame at mentalism, cognitive science, and cognitive theory and therapy, whereas Skinner (1987) blamed humanistic psychology, cognitive psychology, and psychotherapy for impeding progress within psychology as a science of behavior. However, we place the blame squarely with the behavior of behavior analysts. Behavior analysis has not become psychology, in part, because behavior analysts have not done a very good job of showing that behavior analysis is relevant to the concerns of psychologists. Arguing how the other 99% of the psychological community is wrong headed, as Skinner did in his 1987 article and elsewhere, is not, in our view, the most constructive means of promoting a science of behavior. As a tactical matter, behavior analysts will need to address squarely the problems faced by clinical psychologists in their work with outpatient clients. In terms of science, behavior analysts will need to do the empirical research to back up the extensive interpretive analysis of cognition, emotion, and their relation to human suffering, research that is accelerating at a rapid pace. Behavior analysts also will need to make more concerted efforts to show how basic and applied research is relevant to understanding human behavior, which may include efforts to provide non-technical translations of basic and applied data geared for non-behavior analytic scientists and practitioners. On the educative front, behavior analysts will need to do a better job of promoting the assets and liabilities of a behavior analytic approach to students, colleagues, and the general public. What behavior analysts cannot afford to do is to remain isolated from other areas of psychology (including other sciences), particularly from the phenomena of interest to clinical scientists and practitioners. Indeed, it only has been within the last decade and a half or so that clinical behavior analysts have begun to make serious inroads in these and other areas of scientific and clinical interest (e.g., see Hayes et al., 1999; Jacobson & Gortner, 2000). In our view, much of this has occurred as a function of behavior analysts making interdisciplinary contact with scientists and practitioners from non-behavioral approaches such as

cognitive psychology, psychoanalysis, and neuroscience, including work by clinical behavior analysts in traditional outpatient settings with highly verbal clients presenting with a range of complex problems. In so doing, behavior analysis has not become psychology, but it has demonstrated a remarkable track record of success in dealing with subject matter of interest to psychologists. Whether the same can be said of cognitive neuroscience is far less clear. Whether the cognitive neuroscience perspective offers a viable metatheoretical framework for unifying clinical psychology (see Ilardi & Feldman, 2001) raises a host of other issues to which we now turn.

### The Cognitive Neuroscience Promise for Clinical Psychology

Psychology is a relatively young science when compared to other branches of the natural sciences (e.g., biology, physics, chemistry, astronomy), and the science of clinical psychology (including its applied form) is a more recent newcomer within psychology. The problem with the current state of clinical psychology, according to Ilardi and Feldman (2001), is that it lacks a unifying metatheoretical framework that is consistent with the natural sciences. That is, Ilardi and Feldman (2001) appear uncomfortable with the fact that clinical psychology is comprised of many players who operate from within different analytic frameworks. They, like Arthur Staats (1991, 1994)—one of the more vociferous proponents of unification with psychology—view clinical psychology as a field comprised of feuding mini-paradigms, conceptual discord, confusion, theoretical balkinization, or put simply, a chaotic mess. Accordingly, we are to believe that psychology should emulate the natural sciences, where scientists presumably live united under one Utopian metatheoretical framework. Whether the natural sciences fit this unified vision is debatable. Whether the natural sciences arrived at any semblance of unity via scientists suddenly realigning themselves under one Kuhnian revolutionary umbrella does not seem to fit the facts (see O'Donohue, 1993, for a critical analysis of the Kuhnian metaphor as applied to psychological science; see also Greenwood, 1999, on Kuhn and the cognitive revolution). The field of immunology provides one of several examples of progress typical in science.

Historically, there were several different schools of thought regarding the mechanism of immunization. One approach, espoused by Louis Pasteur, was the biological model of immunity, where a living (but weakened) microbe injected into the host organism would exhaust trace nutrients in the host's system and leave no nutrients available for future invasions of more virulent microbes. The competing theory, the chemical conception of immunity, represented by Jean-Joseph Henri Touissant, supposed that if a microbe were "killed" and then injected, it would initiate the development of a soluble substance in the bloodstream toxic to similar microbes (Geison, 1995). The latter theory won out after a vigorous competition and empirical demonstrations of utility, and led the way to our modern knowledge and practice of clinical immunology. We may ask what would have happened if, at such an early point in the development of immunology, we had stuck only to Pasteur's approach. Having one or more competing theories and epistemic approaches did not hinder immunology's development in this case; it actually sped up the process. Numerous other examples like this one exist in the natural sciences, illustrating that the concept of revolution in science is more of a revisionist post-hoc metaphorical construction of the facts than an accurate description of the way science actually progressed.

The reader no doubt may be familiar with other non-Kuhnian metaphors of scientific progress. For instance, one may describe progress in science using a Darwinian metaphor, where science is described as a progressive and deliberate enterprise (i.e., as a slow evolving process). In this view, the products of science that stand the test of time (i.e., they have withstood replication and demonstrations of practical utility) also stand the

greatest likelihood of being retained (i.e., they become the facts of science), whereas those that do not withstand such tests (i.e., failed replication and lack of practical utility) are selected out. As with evolution, variation within science is requisite for progress in science, ultimately resulting in more precise and cogent scientific products. Such variation, in turn, is manifest in pluralistic scientific approaches to understanding human behavior. This evolutionary metaphor is consistent with Ilardi and Feldman's (2001) unification premise and the adaptive advantages of a mature science. The evolutionary view, however, is at odds with premature efforts to engineer unity when the science is not ready for it or in cases where the advantages of an alternative paradigm are not at all clear.

In our view, clinical science is nowhere near the point where we can or should unify at the level of facts, theories, or schools of thought (e.g., cognitive neuroscience), particularly when we cannot even agree as to our basic subject matter, including the terms used to describe it and the appropriate methods to study it. The epistemic obstacles to such integration require addressing barriers that transcend mere facts, such as values and core assumptions about science and practice, terminological differences in how we talk about what we study, including varied criteria for truth and explanation. To achieve consilience of the type described by Ilardi and Felman (2001)—the linking of facts and fact-based theory across disciplines—one first needs to be clear on what the facts are within disciplines. Next, one needs to agree on how those facts will be talked about across disciplines, and ultimately one needs to agree as to which fact-based theories serve as useful bridges across domains and which ones should be discarded. Of course, these and other issues assume that all members of a discipline are committed equally to the facts and, more generally, to a scientific epistemology. Toward this end, clinical psychology still has a very long way to go.

Regarding the promise of cognitive neuroscience for advancing the science and practice of clinical psychology, the jury is arguably still out. As we see it, there are at least two obstacles facing cognitive neuroscience as a viable "unifying" paradigm for clinical psychology: scientific and pragmatic. Regarding the scientific, it seems clear that the last several decades have yielded a remarkable increase in our understanding of the nature and functioning of the human brain. This work, in turn, has paved the way for increasingly more sophisticated and powerful pharmacological interventions for a range of medical and medicalized psychological problems. It also has paved the way for attempts to link neural brain function with psychological function and to use that link to explain behavioral activity. As we understand it, this latter effort is the charge of cognitive neuroscience.

Yet, cognitive neuroscience seems not to have escaped from the use of the computer as a metaphor to explain the psychological functions of the brain, and relies increasingly on computers as subjects to study those functions. Consistent with this view, one increasingly recognized as untenable by cognitive psychologists (see McNally, 1998), Ilardi and Feldman (2001) speak of the brain as evolved to *process, transform, and represent* salient information, and that is comprised of *bits, symbol systems, algorithms, and computational networks*. Though neuroscientists would not likely quibble with Ilardi and Feldman's claim that every thought, impulse, affect, perception, and motivation is associated with a commensurate pattern of brain activity, they will likely quibble with the notion that (a) the computer metaphor and related simulations add anything in explaining the activity of real neurons in real brains of real living, breathing organisms, and (b) that psychological phenomena are completely isomorphic with specific neural activity.

Regarding (b) above, we know that the brain is considerably more plastic than previously thought. What this means, among other things, is that the brain can compensate (and we use that term loosely) for damage to neurons that normally subsumed specific

adaptive functions. Despite the increasing technological sophistication of brain-imaging technology, it has been impossible to show that a thought, an emotion, or a perception is isomorphic with a specific pattern of neuronal activity within a given person, and particularly across persons who report the same private events. It would be quite compelling if one could show that when Thought *X* occurred, it was accompanied by Neural Pattern *X*, irrespective of the context in which Thought *X* is said to occur. This, as far as we know, has not been demonstrated, and we seriously doubt that it ever will. What the voluminous body of research in behavioral and cognitive neuroscience has shown is that psychological functions are correlated with brain functions; that certain psychological functions are dependent on intact and adequate functioning of certain regions of the brain (e.g., eating behavior, fearful behavior), but not always, and that changes in psychological and overt behavioral functioning also are accompanied, to no real surprise, by changes in the brain. Most of this work in living, breathing organisms is correlational, albeit highly sophisticated, and of potential relevance to clinical scientists and practitioners. Yet, the “computerese” used to describe psychological functions of the brain seems far less precise than sticking with descriptions of what occurs at the neuronal level.

It has been easier to use the computer metaphor and related computer simulations (e.g., connectionist modeling, neural networks) to guide guesses about human information processing than it has been to find tests of which guesses are correct. Showing that a computer behaves in *X* fashion as a function of specified inputs and mediating connection weights between input and output does not mean that a person who behaves in *X* fashion is doing so based on the corresponding computer model. As McNally (1998) noted, “Emotions require a body; their essence cannot be distilled in a disembodied computer program” (p. 480). In our view, the danger is that such computer simulations will be extrapolated metaphorically to *explain* human behavior based on correspondence alone, not on demonstrating the validity of such models in the actual brains of human beings. Unfortunately, such metaphorical extrapolation already is happening to some extent, much like the machine metaphor once was extrapolated to explain the workings of human physiology. What is clear is that no one has demonstrated that the neurons in the human brain operate according to same complex programming and sophisticated mathematical algorithms of the neural computer simulations. At present, this task is beyond the reach of cognitive neuroscience, partly because neuroscientists do not have immediate access to the programming and the weights necessary to predict and control the behavior of a real brain system. In the future, it may turn out that the brain behaves very differently than what is described by computer neural models; unfortunately, we may never know.

Some work in artificial intelligence is based on representations and algorithms, with no apparent connection to biological intelligence. Although such work may be highly successful at achieving high levels of competence on cognitive tasks, it does not fall within the scope of cognitive science. For example, the Deep Blue II program that defeated the human chess-champion Gary Kasparov is an example of an outstanding artificial-intelligence program that has little or no apparent psychological relevance, and hence would not be considered part of cognitive science. Yet, Ilardi and Feldman (2001) use this example as supporting cognitive neuroscience and to challenge behavior analysis for its inability to predict the behavior of Deep Blue II in the same manner that a cognitive neuroscientist might, assuming, of course, that the cognitive neuroscientist has knowledge of the complex algorithms driving Deep Blue’s behavior. The authors also use the example to accuse behavior analysis of what they call “greedy reductionism” by claiming that the behavior analyst would tend to reduce Deep Blue II’s algorithms to underlying microcircuit states. The reductionism claim seems misplaced, particularly as Ilardi and Feldman (2001) could be accused of a similar reductionism in their claim that cognitive

neuroscience assumes a “one-to-one correspondence between mental events (e.g., thoughts, emotions, perceptions, etc.) and brain events,” and that each is “isomorphic with a commensurate pattern of brain activity.” In our view, this form of reductionism makes talk of brain functioning in terms of information processing (including representation, reduction of uncertainty) superfluous, as such talk adds little when one can specify, at the neuronal level, the relevant neurophysiology driving the psychology.

Putting the debate regarding reductionism aside for a moment, let’s turn around the chess-playing example to focus on a pragmatic question—namely predicting and controlling (with precision and scope) the chess playing behavior of Gary Kasparov. Here, the cognitive neuroscientist will be at a distinct disadvantage, for there is no way to get inside the head of Kasparov to know in any exact sense what so-called information-processing algorithms are operative. Providing the cognitive neuroscientist with a window on the functioning of Kasparov’s brain via access to sophisticated brain-imaging technologies (e.g., fMRI) will not help in this regard, for such technologies provide no window on the algorithms or synaptic weights required to predict the behavior of computer-simulated neural networks. With the human subject, all the cognitive neuroscientist has at his/her disposal are environmental antecedents, a window on electrical and glucose changes in the brain that occur as a function of those antecedents, and observations of what the human subject does as a result. The cognitive neuroscientist is interested in changes in the brain and their proximal link to behavior, whereas the behavior analyst would likely be more interested in those changes as a function of the relevant antecedents, and particularly consequences for behaving, provided that knowledge of brain changes aids the behavior analyst in predicting and influencing behavior. If the questions instead become: “How do we produce good human chess players?”; or “Can we predict and influence appropriate chess playing behavior in humans?”, then we are dealing with pragmatic issues that behavior analysts will have no trouble addressing. Similar pragmatic questions are also a main concern of practitioners in their routine work with clients in psychotherapy (e.g., “What can I do as a therapist to alleviate my client’s suffering and to improve and broaden their range of functioning and quality of life?”). Cognitive neuroscience, at the present stage of development, is relatively silent on such issues. We believe this is one of the main obstacles of cognitive neuroscience as a unifying paradigm for clinical psychology, but not necessarily psychiatry.

Clinical psychology—and behavior analysis and therapy in particular—always has had an eye on “knowledge for knowledge’s sake” (i.e., scientific understanding) and “knowledge for what it can accomplish” (i.e., practical utility). In turn, this integrated focus has helped pave the way for several efficacious psychosocial cognitive-behavioral interventions for a wide range of problems (e.g., Chambless & Hollon, 1998; Chambless et al., 1996). Cognitive neuroscientists, but particularly behavioral neuroscientists, likewise have integrated understanding (i.e., of brain structure and function) with practical utility in the form of developing pharmacological remedies for psychological problems. This view is entirely consistent with the medical model, but not a psychological treatment model. Clinical psychologists, and particularly front-line clinicians, will want to know more than “Brain functioning *X* is correlated with Condition *X*” or that “Brain Structure *Y* predicts Behavior *Y*,” and even that the brains of patients look different pre- to post-therapy. Rather, they will want to know how one produces a changed brain, and, more importantly, how cognitive neuroscience informs clinical practice in terms of pointing to more powerful psychological interventions. Though psychotropic interventions have their place, they increasingly are regarded as palliatives for most psychological problems, not cures. Moreover, we are learning via randomized controlled clinical trials that, while pharmacological interventions seem to do well in the short term, it is cognitive-behavior

therapies that promise to produce long-term behavior change. For example, in a recent placebo controlled randomized clinical trial, Barlow and colleagues (2000) found that cognitive-behavioral therapy for panic disorder is a more-durable treatment by itself at six-month follow-up than imipramine alone, or combinations of imipramine and placebo with CBT, although imipramine produced a more immediate benefit in the short term. This finding is consistent with the idea that one needs to do more than alter brain functioning at the neurotransmitter level to effect lasting meaningful outcome (see also Clark et al., 1994; Marks et al., 1993). Instead, one needs to teach clients in therapy to think differently, feel differently, and otherwise behave differently. We believe this is, and has always been, the charge of clinical science and practice, and we have little doubt that a changed person and a changed brain occur after meaningful changes in psychotherapy. If cognitive neuroscience is to become a unifying metatheoretical framework for clinical psychology, it will need to address the charge of clinical science and practice by showing that it can contribute to novel and efficacious psychosocial clinical interventions. To date, it has not done so, and cognitive neuroscience remains just a promise for the practice of clinical psychology.

#### Clinical Psychology's Real Problem: Sticking to Science

The notion that clinical psychology comprises both basic and applied branches of science is not new. Yet, there seems to be some resistance to the simple idea that scientific clinical psychology is the only legitimate and acceptable form of clinical psychology (cf. McFall, 1991, 2000). This view represents both a moral and ethical imperative and leaves no room for psychological approaches that fail to use a scientific epistemology in addressing basic and applied questions. Note that this cardinal principle is silent with respect to the framework one adopts in the conduct of clinical science. For instance, one need not be a cognitive-behavioral therapist to behave consistent with scientific clinical psychology, nor does one need to be a behavior analyst or a cognitive neuroscientist. Note further that its subject matter, theories, models, concepts, laws, or types of therapy practiced do not define a scientific clinical psychology. Judgements of scientific validity and utility are not all-or-none matters, but rather take time as evidence is mounted by degrees for or against the validity of specific claims (cf. McFall, 2000). Whether or not one agrees with this view is not critical. What is critical is that clinical psychologists approach their research and clinical work with a skeptical scientific worldview, for to do anything less is not science, and therefore not part of clinical psychology.

A commitment to science and its application in the form of delivering clinical services that promise to alleviate human suffering is the core, the nut, of clinical psychology. Clinical psychologists need to take seriously this scientific approach and consistently uphold it in their work, particularly as we struggle for scientific legitimacy within the larger scientific community (including the psychological sciences), in the eye of the general public, and within the rapidly changing behavioral health-care marketplace. At one level, this simply means increasing say-do correspondence (e.g., "I will keep abreast of the research literature as a guide to my practice," "I will evaluate empirically what I am doing with my clients so that I can provide the highest quality, most effective services to those that I am called on to serve," "I will not practice in the face of scientific knowledge," "As a researcher, I will work at meeting the needs of practitioners by rapidly disseminating clinically relevant science in a form accessible to those who might benefit from it"). At another level, it means that we need to recognize that there are many viable and valid ways to behave as a clinical scientist, and that no approach lays claim to the scientific marketplace. In fact, what actually may inhibit the evolution of clinical psy-

chology is the one-sided approach offered by the authors in this miniseries. We propose that a broad array of theories and analytic approaches would have the most impact on the development of our science and speed up the process of advancing our understanding of human suffering (Weems, 1999).

Quite obviously we see great value in the availability of multiple perspectives in psychological science and anticipate that the yield will be great in the long term. The proviso, of course, is that without a shared commitment to science, such pluralism is unworkable and perhaps even harmful. In our view, science is the foundation upon which we stand the greatest likelihood of making a difference in the lives of the clients we serve. In our view, putting science squarely in the practice of clinical psychology is not very difficult (see Hayes, Barlow, & Nelson-Gray, 1999), but it does require a commitment.

### Summary

Plaud (2001) and Ilardi and Feldman (2001) should be commended for calling attention to several valuable lessons. First, science is more than just the facts. Second, each of us may see differently the same reality, depending on one's own background and experiences. Third, disagreements in science often are value laden, not value free. Fourth, several hypotheses may exist simultaneously for the same reality, but observing several smaller pieces of reality may not always equal total reality when all are added together. Fifth, making consistent observations using a consistent scientific epistemology should lead to better understanding about human suffering and its successful alleviation. Sixth, science is fundamentally an iconoclastic, self-correcting, and, at times, uncertain human enterprise. Lastly, and perhaps most importantly, collaboration within and across sub-areas of basic and applied clinical science should yield a more reliable and useful knowledge base upon which to alleviate a wider range of human suffering. Of course, the reader could have learned as much by reading Saxe's tale of the *Blind Men and the Elephant*. We also hope that the reader will look at what behavior analysis and cognitive neuroscience has to offer them as well.

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