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Open Forum

SPECIAL SERIES: COGNITIVE VERSUS BEHAVIORAL PERSPECTIVES OF BEHAVIOR THERAPY: PART II

Functionalism in Cognitive Neuroscience and Radical Behaviorism: Narrowing the Focus of Debate

David B. Feldman, *University of Kansas*

At the 2000 AABT convention, a stimulating debate took place with the purpose of exploring differences and similarities in what were called "rival theoretical frameworks." On one side of the debate, John Forsyth and Joseph Plaud donned the mantle of radical behaviorism; on the other side, Steven Ilardi and Warren Tryon defended the interests of cognitive neuroscience. I was privileged to serve as moderator.

Despite its short, 2-hour convention time slot, this debate actually began nearly 2 years earlier, when two of the players—Ilardi and Plaud—began a spirited public e-mail conversation. What sparked this lengthy discussion was a single and not uncontroversial idea: The field of clinical psychology could better live up to its scientific aspirations. Plaud and Ilardi concurred that what many psychotherapists do bears only an indirect resemblance to the scientific theories and hypotheses formulated and tested in laboratories. What clinical psychology needed, they agreed, was a theoretical framework in which scientific findings could be organized and understood in relation to one another and diverse clinical phenomena. In the language of philosopher

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Thomas Kuhn, psychology needed a *paradigm* (Kuhn, 1962). Only within such a paradigm could anything like the Boulder model truly be realized. As it turns out, these two men agreed on this single notion and very little else.

For Plaud, radical behaviorism was the paradigm of choice. It had already proven its usefulness in many ways, both in the lab and the clinic. For Ilardi, however, a multidisciplinary approach—cognitive neuroscience—seemed to offer a more promising theoretical framework. A series of articles featured in the *Journal of Clinical Psychology* well represent the arguments offered from both sides (Ilardi & Feldman, 2001a, 2001b, 2001c, 2001d; Plaud, 2001a, 2001b, 2001c, 2001d).

Nonetheless, in reading these articles and reflecting on the content of the live debate, I cannot help but notice that, although individuals on both sides of the issue, including myself, are clearly speaking the same language (namely, English), they appear to use words in divergent manners. Terms like *cognition* and *mental state*, for instance, are never precisely defined. As such, during the question-and-answer session following the AABT convention debate, one audience member commented that participants seemed to be "talking past one another." If such confusion indeed is occurring, it undoubtedly leads to intellectual impasses in which opposing theoretical frameworks *both* appear logically sound and perhaps correct—a clearly untenable conclusion if the frameworks are truly mutually exclusive.

In this brief article, I will continue my role as moderator by addressing a central, definitional issue on which such impasses may easily occur. Namely, I will discuss possible meanings of the term *cognition*. This approach, at first blush, may appear overly pedantic and not directly applicable in answering the important question in response to which the debate began (i.e., What is the best paradigm for the science and practice of clinical psychology?). However, I propose that in order to proffer a logically coherent response to this question, one must first understand how each paradigm treats this important term. As Skinner wrote, "Behaviorism is not the science of human behavior; it is the philosophy of that science" (Skinner, 1974, p. 3). As such, we must engage in some philosophizing.

Defining Cognition

Of central importance in both cognitive neuroscience and behaviorism is the word *cognition*. Obviously, it is important in cognitive neuroscience because it represents a major theoretical construct within that paradigm. It is equally important in radical behaviorism, however, because it designates a concept that must be adequately supplanted by a purely behaviorist equivalent. But what exactly do behaviorists and cognitivists mean when they speak of cognition?

In several of his writings, Skinner begins to explore the topic of cognition with a brief review of what historically has been the most influential philosophy of mental activity: René Descarte's mind-body dualism. According to this theory, the mental and the physical exist as two ontologically distinct substances. The body is, of course, physical, and thus subject to scientific law. The mind, however, is made of a different "stuff," immune to the laws of nature, but nonetheless in causal contact with physical reality. A difficult conundrum, if one accepts this view, is how exactly mental and physical substances interact. How does the mind, which has no physical properties whatsoever, exert force upon the body? Although there is no clear answer to this largely abandoned question, it has certainly sparked some entertaining attempts (Leibnitz, 1992). Fortunately, cognitive science has never embraced this philosophical dead end.

Behaviorists, on the other hand, appear to have adopted mind-body dualism—not as an embraced philosophy of science, but as a kind of enemy with which to contend. Accordingly, thoughts (in the dualist sense) cannot possibly cause behavior because they are not physical events. Instead, thoughts have been reclassified as covert behavior—a venerable theoretical move which allows thoughts to exist within a monistic worldview. Unfortunately, this intellectual maneuver also eliminates the causal status of such mental events, rendering them impotent epiphenomena traceable to the *real* causes of all behavior: environmental contingencies. Thus, changes in the environment are said to cause thoughts (covert behavior), but thoughts are then rendered powerless to affect overt behavior. Viewed from this perspective, the following claim makes sense:

> Mentalistic explanations allay curiosity and bring inquiry to a stop. It is so easy to observe feel-

> ings and states of mind at a time and a place which make them seem like causes that we are not inclined to inquire further. . . . "What are you doing?" is frequently a request for further information. The question might be asked of someone rummaging through a box of small objects, and a characteristic response might be "I am looking for my old pocketknife." . . . A more direct question about causes is "Why are you doing that?" and the answer is usually a [mentalistic] description of feelings: "Because I feel like doing it." . . . [But] if a simple "I feel like it" suffices, nothing else will appear (Skinner, 1974, pp. 14-30).

Skinner's assertion is that mentalism distracts attention from what should be the variables of interest in psychological science: environment and behavior. As such, Skinner contends that all scientific explanation of behavior must be traced to environmental contingencies. Consider the following example: Persons A and B are preparing to leave their respective houses when they see that it is raining outside. Person A responds by using his umbrella. Person B, however, does not use his umbrella because he "believes" that lightning may strike it and injure him. How are we to explain the differing responses of these two individuals? A cognitive account would consider such belief to be an admissible variable for use in explaining these individuals' behavior (while not necessarily terminating the explanation upon discovering the belief, as Skinner implied above). A radical behaviorist, however, would appeal to these two individuals' distinct histories of reinforcement and punishment. Perhaps Person B had been struck by lightning previously, or maybe he had been reinforced for cautious behavior regarding lightning. In any case, the environment, not his belief, caused Person B's failure to use an umbrella. More specifically, his history with contingencies of reinforcement and punishment caused a change in the organism as a whole (Skinner, 1974)—that is, in Person B as a whole—which led him to behave differently in the presence of a discriminative stimulus (i.e., rain) than did Person A, who does not share the same history.

But what is this change in the organism as a whole? According to Skinner, who embraced neuroscience as a legitimate discipline, it is likely to be a neural change. In other words, it is a change in the structure of the organism's central nervous system. Presumably, then, Person A's central nervous system contained a neural pattern predisposing him to emit umbrella-using behavior, while Person B's central nervous system contained a corresponding pattern predisposing him not to use an umbrella in the same environmental context. Theoretically, one should be able to trace the internal paths of neural activation from input (Person B sees rain) to output (he leaves without an umbrella), specifying exactly how activity at each synapse contributes to his behavior. We can say, then, that the central nervous system, changed by past environmental contingencies, transforms input into output—that is, comes into contact with a stimulus and gives out a response—in a way corresponding to the individual's unique history with the environment.

This brings us to a theoretical viewpoint philosophers call functionalism. While there are numerous types of psychological functionalism (Block, 1980), all varieties reduce mental events to functional events. In more precise language, there is an identity between mental and functional "kinds" (Kim, 1998). A mental event is viewed as an internal organismic state with a specific function that serves a particular causal role. In other words, mental states are not defined by what they *are*, but by what they *do*.

Consider most philosophers' favorite example: pain. For the functionalist, pain is an internal state that fills a causal role relative to specified inputs and outputs (stimuli and responses). Pain receives tissue damage as input and yields wincing, groaning, and other pain behaviors as output. This is what pain *does*. It is a *real* internal state that serves this function, but it is *not* necessarily a conscious state with a phenomenological character. A mental state may include a phenomenological experience, but this is not what individuates it as "mental." It is considered mental because of the *functional* role it plays in mediating between relevant inputs, outputs, and other functional states.

In our above example of people carrying umbrellas, a functionalist may reasonably distinguish two mental/functional states. First, there is the desire to carry an umbrella. This desire may be functionally defined as taking an observation of rain as input and yielding umbrella-carrying behavior as output. However, it may also receive input from other mental/functional states, including belief. In this case, it receives input from a belief (which is, of course, only another functional state) that rain signals lightning and possibly danger (perhaps a third belief about the nature of the danger is involved here also). This belief also receives input from the environment (i.e., observed rain), but yields output that is fed as input directly into the aforementioned desire. As such, its causal function is as a sort of lightning warning device that changes the organism's behavior through influencing another causal function: the desire to carry an umbrella. Because one's history of experience with the environment is a valid cause of the formation of a belief, it is reasonable to think that Person A (who uses the umbrella) has had a different history with the environment than Person B (who does not use the umbrella). Thus, functional states are well grounded in environmental and behavioral data. In fact, they are equated with their causal roles relative to these data.

Skinner might respond to this characterization of functional states by commenting that it is overly mentalistic. He might assert that the functional/mental states of which I write are reducible to a history with environmental contingencies and thus are superfluous in scientific explanation. To avoid "mnemic" causation (causation that jumps a temporal gap), however, he would still rightly appeal to a change in the organism as a whole that predisposes certain actions as a result of such contingencies. This change, I believe, still fits well within a functionalist framework: it is an internal (presumably neural) state that serves a causal role with reference to specified inputs and outputs. In fact, Day and Moore (1995) contend that "Skinner has long-term squatter's rights when compared with philosophical functionalists, since he has been expounding the functional position at least since *Science and Human Behavior* (Skinner, 1953)," and Palmer and Kimchi comment that "prominent examples of psychological theories couched exclusively or primarily within the functional domain include Skinner's theory of organismic behavior" (p. 42).

Even given this affinity for functionalism, Skinner might ask, "Where's the mental in all of this? All you've said is that experience with the environment changes *physical*, neural structures within the brain that function to cause behavioral responses given exposure to certain stimuli." It is true that functionalism, a type of materialism, posits that functional states cannot exist independent of physical substrata. The functionalist would add, however, that functional states are *not* dependent on any *specific* class of physical material for their causal powers. Rather, functional states can be instantiated in many materials without losing their causal significance. Philosopher Jaegwon Kim (1998), addressing this issue with reference to the mental/functional state of pain, writes, "But aren't there pain-capable organisms, like reptiles and mollusks, with brains very different from the human brain? Perhaps in these species the neurons that work as nociceptive neurons—pain-sensitive neurons—aren't like human C-fibers at all" (pp. 69-70).

Moreover, similar or even identical functional states can exist in two distinct human organisms, yet utilize two completely different sets of neurons. In this same vein, because of brain plasticity, neural substrata utilized for specific functions can even shift over time, especially if the organism suffers some sort of brain lesion. This line of reasoning, known as the "multiple realization" argument, can be extended still further. Isn't it conceivable that a computer could also respond to environmental control by altering its behavior over time, thus instantiating functional states in silicon? This possibility vividly demonstrates the drawbacks of reducing mental/functional states to their material concomitants: one loses the ability to delineate functional similarities between organisms with differing physiologies. For this reason, we must say that while *some* physical substrate is necessary for the existence of a functional state, any one *specific* physical substrate appears expendable.

What are we to call these functional states of the "organism as a whole" that can be instantiated in multiple substrates without losing their causal status? It seems reasonable to refer to them loosely as cognitive states. In fact, cognitive scientists have long endorsed functionality as a criterion for what is to be considered "mental." Palmer and Kimchi (1986), in their lucid explication of the basic assumptions of information-processing (IP) theory (one approach to cognition), write:

> Another aspect of the first assumption [of IP theory] that needs to be discussed is the notion of a "functional" description. The intent here is to single out a domain of discourse for IP theories of mind that appropriately reflects the kind of accounts they offer. In this context, *functional* descriptions are to be distinguished from both *physical* and *phenomenological* descriptions. . . . The presumption is that this functional level is considerably more abstract and general than the physical level (in the sense that many physically quite different objects can have the same function). (p. 42)

As such, the functional description of an organism (or computer, for that matter) need not *explicitly* refer to underlying physical states of its nervous system (or equivalent) to posit causal relationships mediated by internal functional states. Rather, it must refer to abstractions of those physical states and their causal or functional roles in mediating between environmental input and behavioral output.

This functional description, in tandem with information characterizing an organism's *current* environmental context, is in principle sufficient for the prediction and explanation of an organism's behavior. It is, however, only a small portion of a complete understanding of the organism's psychology. This is true because functions may be instantiated in multiple classes and configurations of matter. Once a functional state has been discovered, it can be asked how this state is realized in the neural tissue of the brain. Or, conversely, once a certain neural circuit is discovered, it can be asked what functional state or states it instantiates (that is, what causal role it serves). Thus, functional and physiological analyses inform one another—i.e., if a function exists, it must have some physical basis, and if a neural circuit exists, it likely has some functional significance. Thus, the words *cognitive* (if we consider the functional state to be an adequate definition of what is usually called cognitive) and *neuroscience* appear to fit together nicely, as do the words *behavioral* and *neuroscience* (if we consider radical behaviorism to represent a type of functionalism).

Cognitive and Behavioral Neuroscience Contrasted

Even though cognitivism and radical behaviorism are two examples of philosophical functionalism, there is still much room for disagreement. Being an exemplar of functionalism does not guarantee a theory's compatibility with all other such exemplars. In order to understand how this is possible, it may be useful to consider exactly what functionalism means for the information processing theorist. According to Palmer and Kimchi (1986),

> What distinguishes IP theories from most other functional theories in psychology is its further claim that the appropriate type of functional description is *informational*; that is, mental events are to be characterized in terms of information and operations that relate information. . . . As we are using the terms, information is an abstract construct in theoretical descriptions of mental events. We have used it in this way to reflect the pervasive belief among IP psychologists that IP theories are *abstract*, *functional* entities that do not depend on at least certain physical characteristics of the events being described. (pp. 42-43)

In other words, cognitive scientists (at least of the information-processing variety) posit that information serves the *function* of transforming input into output. After all, information, like function, is an abstract entity that can be instantiated in multiple media (e.g., on paper, in cathode-ray diode television screens, in computer circuits, etc.). But, what do cognitive psychologists mean when they refer to the brain as an information processor? Of what exactly are these "processes" and the "information" that they process made? These are complex questions with no clear answers. I will propose, however, one possible set of responses.

In order for a system to be considered functional, *something* inside of it must bring about certain outputs given specific inputs. This "something" need not be decomposable into linear steps, nor must it operate on symbolic representations of input; perhaps the output results from simple association, reflexive response, or some other equally unitary function. In order for a system to be called an information processor, however, it must symbolize its input and perform functions on this symbolization that are describable as a set of steps. That is, it must be specifiable in terms of an algorithm (i.e., a set of "processes"), each step of which operates on a representation (i.e., "information").

A symbol or representation, in this context, may consist of merely a characteristic pattern of neural firing that occurs upon exposure to a particular stimulus, or a particular semipermanent organization of synaptic connections that results from a history with such a stimulus. Representations need

not be consciously experienced nor must consciousness or phenomenological experience even exist for representation to occur. An algorithm, then, is simply a systematic and sequential set of changes that this representation undergoes prior to output being returned. Each of these algorithmic steps is, of course, a functional event in that it fills a causal role in relation to preceding/subsequent steps and the entire functional system's input and output. As such, the *physical* pattern of neural activity that comprises representation A (that is, a representation of stimulus A) in one organism need not be the same physical pattern of neural activity that constitutes representation A in another organism; nor must the set of *physical* changes in neural activity comprising algorithm X in one organism's brain be identical to those constituting algorithm X in another organism's brain. Rather, the different possible physical instantiations of representations and algorithms must serve the same causal function in relation to one another *and* in relation to the inputs and outputs of the organism itself. Representations and algorithms, then, are abstractable in that they are dependent upon *some* physical substrate, but not upon any specific type or patterning of this substrate, as long as the causal connections between the instantiations of each of these abstract entities are consistent across all such substrates. They could as easily be instantiated in a digital computer as in a system of levers, gears, and pulleys (although certainly less efficiently).

This is clearly not the functionalism of the radical behaviorist, who would likely take issue with the notions of both representation and algorithmic processing. For the radical behaviorist, functional changes in the human brain (or any organism's brain, for that matter) are subsymbolic—that is, they do not rely on symbolic representation. Rather, such changes are brute-force alternations of neural connections that cause the organism to behave differently as a result of experience with the environment. While these alterations may be (loosely) characterizable as an algorithm, this algorithm certainly would not resemble the step-by-step processes responsible for transforming information in a symbol-processing computer.

The fact that both radical behaviorism and cognitive information-processing theory are examples of functionalism, however, goes a long way toward narrowing the focus of debate and discussion between these two rivals. The debate no longer hinges on well-worn issues such as whether there are changes in the internal organization of the organism as a result of experience with the environment, whether environment and behavior are legitimate and important variables in psychological research and practice, or whether positing functional states (which, for the cognitivist, are functional-informational states) halts further investigation into the causes of behavior. Rather, the debate hinges on whether the neural machinations of the brain allow for the vast array of behaviors displayed by the human organism (and undoubtedly traceable to the environment) *without* some form of representation and algorithmic processing.

The necessity of symbol processing in a complete account of human behavior is currently under debate in the cognitive science and neuroscience literatures. Whether "connectoplasm" (the generic "stuff" of neural networks) without any explicit algorithmic organization and symbol processing can account for such phenomena as language and formal reasoning is not a question I can legitimately address within the confines of this article. It should suffice to note, however, that many theorists claim that such phenomena *can* be explained without the use of information-processing concepts (McClelland & Rumelhart, 1986; Plaut, McClelland, Seidenberg, & Patterson, 1996), while others vehemently disagree (Fodor & Pylyshyn, 1988; Hadley, 1994; Marcus, 1998; Pinker, 1997; Pinker & Price, 1988). A brief perusal of this literature, however, should demonstrate to the reader the focused and precise nature of the questions under consideration. It is my belief that from this new debate, productive answers will shortly emerge.

Conclusion

In the preceding pages, I have attempted to use philosophical functionalism to draw parallels between radical behaviorism and cognitivism (including cognitive neuroscience). It has not been my intention to argue either field into a corner in favor of the other. Rather, I wished to propose a framework for focusing the debate between these theoretical rivals. If both radical behaviorism and cognitivism can be legitimately classified as instances of functionalism, then classical arguments between the two sides can be reconstrued as red herrings. Although this article has not directly addressed the question that spawned the debate at the AABT convention in 2000 (i.e., Which theoretical framework offers the best paradigm for the science and practice of clinical psychology?), perhaps by narrowing the focus of inquiry, an answer based on sound reasoning and theoretical principles can be achieved.

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Brains, Computer Games, and Behavior: What Do They Have to Do With Progress in Behavior Therapy?

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The behavior therapy movement has enjoyed a long history of success, much of it owing to a very simple formula: the integration of basic behavioral science (e.g., operant and respondent principles of learning) with the practical application of that science so as to understand and alleviate a wide range of human suffering. When put into action, this formula manifested as a unique pragmatic vision of basic and applied scientific activity, wherein behavior therapists sought scientific understanding with at least one eye on practical utility. Joseph Wolpe's pioneering basic and applied research on systematic desensitization is one of several examples of the integration of basic behavioral science with the practical application of that science in clinical contexts. The result was a carefully conducted program of research, yielding knowledge that systematic desensitization works, but also knowledge about why it works (cf. K. G. Wilson, 1997). By focusing on knowledge not only for its own sake but also for what it can accomplish, Wolpe and others were able to revolutionize behaviorism, behavior therapy, and clinical psychology more generally.

Scientific understanding without practical utility *or* practical utility in the absence of understanding (e.g., finding that a treatment works, but having no idea why it works) represents two extreme poles of basic and applied activity that are somewhat alien to the behavior therapy movement (cf. Stricker, 1992). This should not be taken to mean that scientific understanding as a goal is not worthwhile, or that practical utility without understanding is necessarily a bad thing. Indeed, often we may learn something new about phenomena within our purview and only later discover how to put such knowledge to practical use. Likewise, we may stumble upon a promising new intervention technology without clearly understanding why it works (e.g, eye-movement desensitization and reprocessing), including the active treatment components contained therein. Both cases, of course, require a next step. The practitioner needs to subject the promising intervention technologies to scientific scrutiny so as to identify whether the treatment does in fact work, including elucidating mechanisms of therapeutic action and the relevant principles underlying why the intervention works. The scientist likewise needs to outline the practical utility of the purported facts, or at least to hope that someone else will. In either case, failing to take the next step, or seeing that there is a next step to take, can be problematic for the continued development of behavior therapy as a truly integrated basic and applied science.

One implication of the present meta-analytic argument is that knowledge claims and paradigmatic proposals that fall short of yielding scientific understanding with practical utility and practical utility with understanding should be viewed cautiously by behavior therapists. Another implication is that such claims can and probably should be evaluated in light of these mutually entailed meta-analytic dimensions (i.e., scientific understanding and practical utility), dimensions that are noncommittal with respect to content. Here, we attempt such a modest evaluation of three knowledge claims that have been proposed as alternative frameworks for the continued advancement of behavior therapy (for more detail see Forsyth & Kelly, 2001): (a) cognitive neuroscience (e.g., Feldman, 2002; Ilardi, 2002; Ilardi & Feldman, 2001), (b) neural network learning theory (Tryon, 2002), and (c) behavior analysis (Plaud, 2001, 2002). As we regard the reciprocal relation between knowledge for knowledge's sake (i.e., understanding) and knowledge for what it can accomplish (i.e., practical utility) as one, if not the, main reason for behavior therapy's historic track record of success, we will hold all three proposals to this arguably value-laden set of standards (really one standard, not two). We begin our evaluation by considering Ilardi and Feldman's proposal for the cognitive neuroscience framework as an overarching foundation for behavior therapy.

AUTHOR NOTE. Portions of this paper were presented at a panel discussion, "Radical Behaviorism and Cognitive Neuroscience: A Friendly Debate of Rival Theoretical Frameworks for the Science and Practice of Clinical Psychology," moderated by D. Feldman, at the 2000 annual meeting of the Association for Advancement of Behavior Therapy, New Orleans, LA.

Ilardi and Feldman's Cognitive Neuroscience as a Framework for Behavior Therapy

Overview

As we understand it, the interdisciplinary field known as cognitive neuroscience takes as its starting point brain function, and attempts to provide proximal causal explanations of how brain function, and to a lesser extent brain structure, subserves psychological functions (e.g., cognition, emotion, overt behavioral actions). Cognitive neuroscientists also assume that (a) psychologically important events occur as a result of neural information processing and (b) there is a one-to-one correspondence between mental events (e.g., thinking, emoting, perceiving), overt behavioral actions, and brain events (cf. Feldman, 2002; Ilardi, 2002; Ilardi & Feldman, 2001, for a brief summary). Much of this work, in turn, draws heavily on sophisticated brain imaging technology (e.g., fMRI, SPECT) and computer simulations of neuronal function (e.g., connectionist models and neural networks) to support inferences about brain-behavior relations.

With regard to the promise of cognitive neuroscience for advancing the science and practice of behavior therapy, the jury is arguably still out. As we see it, there are at least two obstacles facing cognitive neuroscience in the context of the behavior therapy movement: one scientific and the other pragmatic.

Scientific understanding with medical, but not psychological, practical utility. With regard to the science, it seems clear that the past 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 pharmacologic interventions for a range of medical and medicalized psychological problems. It has also paved the way for attempts to link neural brain function with psychological function and to use that link to explain behavioral activity. Yet, cognitive neuroscientists seem not to have escaped from the use of computer as metaphor to explain the psychological functions of the brain and a growing reliance on computers (as research subjects) to study those functions. Consistent with this view, one increasingly recognized as untenable even by cognitive psychologists (see McNally, 1998), Ilardi and Feldman (in press) speak of the brain as evolved to *process, transform*, and *represent* salient information; one that is comprised of *bits, symbol systems, algorithms,* and computational *networks*. Though neuroscientists would likely not quibble with Ilardi and Feldman's claim that each and 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. Moreover, despite the increasing technological sophistication of brain imaging technology, it has not been possible 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 also 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 are also accompanied, to no real surprise, by changes in the brain. Virtually all of this work in living, breathing organisms is correlational, albeit highly sophisticated and of potential relevance to behavior therapists. Yet, the computerese and highly inferential and metaphorical talk used to describe psychological functions of the brain seem far less precise compared with descriptions of what occurs at the neuronal level in biological terms.

Behavior analysis and therapy's integrated emphasis on "knowledge for knowledge's sake" (i.e., scientific understanding) and "knowledge for what it can accomplish" (i.e., practical utility) 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, have likewise integrated understanding (i.e., of brain structure and function) with practical utility in the form of developing pharmacologic remedies for a range of medical and psychological problems. This approach is entirely consistent with the medical model, but not a psychological treatment model. Behavior therapists, 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 postpsychotherapy. 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 psychosocial interventions. Though psychotropic interventions have their place, they are increasingly regarded as palliatives, not cures, for most psychological problems. Moreover, we are learning via randomized controlled clinical trials that, while pharmacologic interventions seem to do well in the short term, it is often cognitive-behavior therapies that produce more lasting behavior change.

For example, in a recent placebo-controlled, randomized clinical trial, Barlow, Gorman, Shear, and Woods (2000) found that cognitive-behavioral therapy for panic disorder is a more durable treatment by itself at 6-month follow-up compared to imipramine alone, or combinations of imipramine and placebo with CBT; though imipramine produced a more immediate symptom relief 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 treatment outcome (see also Clark et al., 1994; Marks et al., 1993). Rather, one needs to teach clients in therapy to also think differently, to feel differently, and otherwise behave differently. This, we believe, is, and has always been, the charge of the behavior therapy movement, and we have little doubt that a changed person and a changed brain occurs after meaningful changes in cognitive-behavior therapy. If cognitive neuroscience is to become a unifying metatheoretical framework for behavior therapy, 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, cognitive neuroscientists have not done so, and the field of cognitive neuroscience remains just a promise for behavior therapy.

Tryon's Neural Network Learning Theory and Behavior Therapy

Overview

Neural network learning theory (NNLT) is a learning theory based on connectionistic models of synaptic change and the configuration of neurons and their interconnections resulting from experience (Tryon, 1995). Neural networks involve sums of inputs, weights, and complex mathematical algorithms that make up sophisticated computer simulations of learning-based models of synaptic change. Though NNLT is still in its infancy, it has made some potentially important inroads regarding understanding how neural functioning and neural organization is modified by experience.

Brains and computer games are interesting and fun, but what do they have to do with the practice of behavior therapy? In terms of knowledge for knowledge's sake, NNLT proposes interesting and legitimate questions about the structure of neural connections in the brain and their relation to brain function and behavior. The parallel processing approach of NNLT is a step above the old computer metaphor based on serial processing. NNLT has also provided interesting computer analogues of the types of processing that may occur in real neural systems (Bates & Elman, 1993). For example, this growing body of research has attempted to elucidate the relations between neural networks and excitatory and inhibitory processes, summation of activation, and the distribution of patterns across neural connections. Most importantly, neural network computer simulations can learn and change as a result of experience and the resulting changes in the neural network can be examined directly.

As a learning theory that has the potential to bridge the fields of cognitive theory, behaviorism, and neuroscience, NNLT has much promise, but still has a very long way to go. For instance, though one can analyze the complex algorithms that control the output or "behavior" of a computer, it is a metaphorical leap to suggest that biological neural connections work the same way. We know how computers and computer simulations of neural activity learn because we have access to the complex algorithms controlling the *simulated* neurons and the resulting changes in their connections (via analyses of changes in synaptic weights after the fact). The same level of precision is not available when it comes to examining neural relations in a human brain. That is, we do not have technology that allows a window on the complex mathematical algorithms and changes in neural weights (if there are such things) when it comes to real neural activity, in real brains, of real living, breathing, organisms.

Correspondence between the behavior of the computer simulations and human behavior is, in our view, an unsatisfactory basis for claims about the relevance of NNLT to behavior therapy. For instance, one might be tempted to say that the neural network model could account for activity in a real brain, provided that the antecedent and consequent events were the same for both the computer and the human, including identical behavioral output. This argument, however, is akin to saying that two people with the identical *DSM* diagnoses and associated symptoms have identical learning histories and hence the same neural connections in the brain, or that identical learning histories among two people will result in similar changes in the brain and hence resulting behavior. In both instances, we know these claims to be false.

Unfortunately, there is no way to know whether neural network models bear any explanatory force when it comes to human behavior. Moreover, real neurons, real neural connections, and real brain activity are regarded as considerably more complex than what any computer is capable of simulating. Indeed, the most powerful supercomputer performs approximately 8 orders of magnitude of operations per second, which is 100 million times less than a human brain (cf. Tryon, 1995). Even if a computer could someday perform at the same level of complexity as the human brain, this does not therefore mean that the human brain is analogous to the simulated neural operations of the computer. In our view, the important contribution of NNLT as a model of brain activity is just that, a model whose application to explain real neural biological events based on correspondence alone is tenuous at best.

If actual neural processing were in all ways similar to computer simulations, then we should not only be able to predict and influence the output of computers using such models, but human behavior as well. Neural network research is not yet on the level of predicting, let alone specifying, how one might use such models to influence human behavior. For instance, the Deep Blue II program that defeated the human chess champion Gary Kasparov is an example of an outstanding artificial-intelligence program with complex mathematical algorithms and the ability to learn. An analysis of how and why Deep Blue II performs its chess moves is within our technical grasp at a microscopic level. Yet, neural network learning theorists are unable to achieve the same level of precision when it comes to explaining the chess-playing behavior of Gary Kasparov. With the human subject, the neural network learning theorist has access to environmental antecedents, perhaps a window on electrical and glucose changes in the brain that occur as a function of those antecedents (e.g., via sophisticated brain imagining technology), and observations of what the human subject does as a result. Neural network scientists do not have access to algorithms and synaptic weights that are presumed to underlie, and hence control, the chess-playing behavior of Kasparov. Brain imaging technology will not help fill this gap, as it does not provide a window on algorithms, weights, and the like. Pragmatically, the behavior therapist will want to know how to produce good chess-playing behavior. Moreover, behavior therapists will want to know whether knowing that one has produced specific changes in the brains of clients via behavior therapy really matters, how one would know whether such neural changes occurred, and, more importantly, how neural network theories provide behavior therapists with more effective psychological interventions and ultimately how NNLT would make one a more effective behavior therapist. NNLT advocates have thus far provided no direct answers to such questions, nor have they provided information that can directly help clinicians in delivering efficacious psychosocial treatments.

Though one can not fault NNLT devotees for not having developed practical applications of neural network basic research (i.e., basic science often lags behind practical need), one should raise an eyebrow or two when NNLT is being *sold* as a paradigm that behavior therapists should adopt. The reason is that most behavior therapists are attempting to use science to achieve practical ends in alleviating human suffering in and outside therapy contexts. Tryon (2002) notes that NNLT has the potential to yield new and more powerful tools for the creation of effective and efficacious therapies, which, if true, would be of great value to behavior therapists. Perhaps as the field of neural networks matures, evidence from research may yield information leading to new and more powerful psychosocial treatments. We would certainly welcome such a development, whether from NNLT, cognitive neuroscience, or any other branch of psychological science. To date, however, NNLT has not yielded such practical information, and like its cognitive neuroscience cousin, remains just a promise for behavior therapy.

Plaud's Behavior Analysis and Behavior Therapy

Overview

The basic and applied branches of behavior analysis have a long history of affiliation with behaviorism and the behavior therapy movement more generally. Behavior analysis from its beginnings considered behavior situated in and within its context as the fundamental unit of analysis (i.e., behavior-environment relations, not just behavior, and certainly not just environment alone), with prediction and influence of behavior serving as mutually entailed analytic goals (one goal, not two). Behavior analysis was conceived in the context of a radically different philosophy of science, a philosophy that was radical precisely because of the manner in which psychological science and behavior were conceptualized. The products of this perspective speak for themselves, and research studies that followed provided a wealth of knowledge about controlling variables over adaptive and maladaptive behavior and how such variables can be influenced so as to better understand and treat a wide range of problems in living.

Though the historic affiliation of behavior analysis with behavior therapy requires no elaboration, the reason why Plaud (2001, 2002) feels the need to remind behavior therapists of this close affiliation deserves some comment. As we see it, Plaud is concerned that modern behavior therapy has lost its behavioral moorings, particularly contact with post-Skinnerian behavior analysis. There is probably some truth to this claim. Applied behavior analysis has been over-

AABT People

Mary Jane Eimer, CAE, Executive Director of AABT, has been awarded the New York Society of Association Executives' 2002 Outstanding Association Executive Award. It is the highest professional recognition one can receive from the NYSAE, whose members are association executives and staff specialists.

Effective April 1, 2002, **Dr. Alan E. Kazdin**, Yale's John M. Musser Professor of Psychology and a member of the Child Study Center faculty, will become Director of Yale's Child Study Center.

looked by behavior therapists as a source of clinical inspiration in the routine treatment of adult problems since the mid-1960s. This may be due, in part, to perpetuation of the view that behavior analysis is limited to controlled environments (e.g., in schools, hospitals, etc.), certain kinds of problematic behaviors and populations (e.g., autism, developmental disabilities, children), and can only account for a restricted range of psychological phenomena (e.g., overt behavioral actions, but not clinically rich phenomena such as thinking, feeling, and emotion). These and other misconceptions have likely prevented the more widespread application of behavior analysis in less controlled outpatient therapy contexts (Kohlenberg & Tsai, 1991). *Yes, we have both knowledge for knowledge's sake and knowledge for what it can accomplish.* The principles of behavior, developed in the research laboratory, have been extended by behavior therapists to prevent and ameliorate a wide range of human suffering in individuals, couples, groups, schools, and organizations. The behavior therapy track record speaks for itself: The majority of treatment approaches considered as empirically supported by the APA's Division 12 Task Force on Promotion and Dissemination of Psychological Procedures (Chambless et al., 1998; Task Force, 1995) are based on principles of behavior, such as extinction via exposure, counterconditioning, shaping, modeling, and positive reinforcement. Research evidence also exists to support the notion that the components of therapy that are based on behavioral principles are just as effective, if not better, than other components of therapy, including the cognitive components of some cognitive-behavioral therapies (for examples, see Craske, Rowe, Lewin, & Noriega-Dimitri, 1997; Ito, Noslivvani, Basoglu, & Marks, 1996; Jacobsen et al., 1996).

Behavior analysts have always been pragmatic and integrative (in terms of science and practice) and have never claimed that all behavior therapists should become behavior analysts. However, numerous (and quite complex) reasons exist to explain why behavior analysis has not been universally embraced by behavior therapists, and these include: (a) behavior analysis is overall a difficult path to follow because the terminology used and the way behavior analysts talk about behavior is quite different from the way we have been taught to think and talk; (b) behavior analytic science of private events has been slow to progress; (c) behavior analysis until very recently had not contributed to mainstream behavior therapy in terms of viable psychosocial interventions for problems common in highly verbal outpatient clients; and (d) continued misunderstanding and misrepresentation of behavior analysis in the literature (e.g., as a version of S-R psychology, as only concerned with observable behavior, and even as mechanistic, reductionistic, and otherwise sterile). Unfortunately, many behavior therapists continue to place Skinner in an equivalence class with behavior analysis, and consider operant learning as of limited value in explaining and influencing complex human behavior. Though Skinner made important contributions to the development of behavior analysis, his account of behavior analysis was one account, and by no means *the* behavior analytic account. Indeed, post-Skinnerian behavior analysts, and particularly clinical behavior analysts, have moved beyond the original three-term contingency in addressing other clinically rich phenomena, such as thinking, feeling, emotion, attributions, resistance, transference, suicide, depression, anxiety, psychological acceptance and avoidance, including several new promising psychotherapies anchored in the basic behavioral science. In so doing, however, clinical behavior analysts have not given up on a science of behavior, but rather have extended that science in meaningful, coherent, and creative ways (Hayes & Toarmino, 1999).

Behavior analysts of the basic, applied, and clinical stripe have always kept true to behavioral science's early mission, which is to have an integrated basic and applied science. Behavior therapy, to some extent, has lost this priceless integration, with the science of behaviorism no longer closely linked to its applied branch. Since the mid-1960s behavior therapy has increasingly drifted from its grounding in basic behavioral science, favoring instead a more general empirical approach to the treatment of clinical problems. There are relatively few behavior therapists with one foot planted in basic research laboratory and the other foot in the clinic. Few of those calling themselves behavior therapists are thoroughly familiar with behaviorism and contemporary principles of learning from behavioral science, and even fewer look to behavior theory and behavioral principles for clinical inspiration. Ironically, it is precisely this approach that paved the way for behavior therapy's early successes; one that contemporary behavior therapy seems to have lost sight of, particularly judging the need for the thematic title—"Bridging the Gap from Science to Clinical Practice"—of AABT's 1994 annual meeting. Most behavior therapists identify themselves loosely as cognitive-behavior therapists and do not see use of this hyphenated term as a conceptual redundancy. Novel treatment innovations are few and far between, and the trend is to repackage tried and true treatment interventions (e.g., relaxation training) and test for their efficacy with diagnostically dissimilar clinical conditions in the context of large-scale randomized clinical trials. Interest in the conceptual foundations of behavior therapy, and the practice of conceptually driven behavior therapy, is, for the most part, now seen as irrelevant for the successful implementation of behavioral intervention technologies. What it means to be a behavior therapist is now, more than ever, anyone's guess.

There is no muddled definition of what it means to behave as a behavior analyst. In fact, the opposite is true, with the definition of a behavior analyst being the same whether one works with rats, pigeons, or human beings either in or outside of therapy. Behavior analysis is unambiguously concerned with the prediction and control (influence) of behavior via identification and manipulation of consequences that follow behavior. The basic and applied divisions of behavior analysis share the same language and concepts, like other fields of science that have basic and applied branches. For instance, engineering represents the applied branch of physics that is concerned with the practical application of the known laws of physics. In so doing, engineers build bridges, buildings, roads based on a science of physics, including the relevant terminology. The engineer does not, however, create new terms and concepts not tied to the basic science of physics or, worse yet, invent or ignore known facts and laws of physics when building a bridge. At one time behavior therapists shared a similar basic and applied integrated focus, a technology, a terminology, grounded in a behaviorism and a psychology of learning. This feature of behavior therapy seems to have been lost.

The close link of behavior analysis to basic science has contributed additionally to the understanding of how behavioral interventions work, and not just that they work. Some very promising therapy approaches have recently been born out of behavior analysis' close link between scientific knowledge and practical utility (e.g., Acceptance and Commitment Therapy; Hayes, Strosahl, & Wilson, 1999; and Functional Analytic Psychotherapy; Kohlenberg & Tsai, 1991). Though data on the efficacy and effectiveness of these treatment procedures are still limited, the more important point here is that such novel treatment procedures emerged from advances in behavioral science and entail a foundation in technical concepts from the basic branch of the science.

Conclusion

Behavior therapy's long track record of success owes much to the contributions of behavior therapists to scientific knowledge with an eye on practical utility as it does to pluralism with regard to multiple theories and viewpoints, without which progress in behavior therapy, and more generally in the field of clinical psychology, would not be possible. We recognize that behaving as a behavior therapist means many things to different people, and that no perspective should be taken as *the* way to conduct behavior therapy. In fact, there is value in having a number of different perspectives in one field, where a broad array of theories and analytic approaches may speed up the process of understanding the variables and processes that contribute, either in whole or in part, to human suffering and its successful alleviation (Weems, 1999). Cognitive neuroscience and NNLT have contributed much to our understanding and thinking about the structure of the brain, brain function, and brain-behavior relations more generally. In terms of behavior therapy's recipe for success, both approaches have also contributed much in terms of scientific understanding. Yet, both suffer from a lack of knowledge contribution with clear practical utility. In our view, this issue is the main impediment for serious consideration of NNLT and cognitive neuroscience as overarching frameworks for the science and practice of behavior therapy. Behavior analysis, by contrast, has never had a problem with practical utility, and continues to maintain a close link between the lab and the clinic. In so doing, contemporary behavior analysis resembles the original components of behavior therapy's very successful formula. Yet, behavior analysts continue to suffer from an enormous public relations problem, some of which has to do with the very behavior of behavior analysts in trying to have their voices heard. Our hope is that those affiliated with the behavior therapy movement will, at the very least, pause to ask whether they are advancing scientific understanding *with* practical utility and/or practical utility *with* scientific understanding. Gaps along such dimensions are problematic not just for cognitive neuroscientists, neural network learning theorists, but for the continued advancement of behavior therapy in the years to come.

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Comments on Kelly, Forsyth, and Karekla's "Brains, Computer Games, and Behavior: What Do They Have to Do With Progress in Behavior Therapy?"

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Kelly, Forsyth, and Karekla (2002), hereafter referred to as KFK, critically comment on articles by Ilardi (2002), Feldman (2002), and Tryon (2002). The first approach taken by this reply is to react to the general issues involved. The second approach specifically addresses particular points made to each of the three presentations.

General Comments

The underlying theme of this debate is about the *sufficiency* of *functional analysis* as a theoretical basis of behavior therapy. All debate participants agree that functional analysis is *necessary* for good behavior therapy and we further agree that functional analysis has been responsible for the emergence of behavior therapy as a major therapeutic approach. We disagree about whether functional analysis provides a *sufficient* theoretical base for behavior therapy. KFK propose that the functional analytic perspective is entirely sufficient and that no further extension of our explanatory base is needed. Their pejorative characterization of serious scientific computer simulations of learning in multilayered adaptive networks constrained by neuroscience as "computer games" precludes any significant consideration for extending the explanatory base of behavior therapy beyond functional analysis.

The large majority of behavior therapists characterize themselves as cognitive-behaviorists because they disagree that functional analysis provides a sufficient basis for practice and research. They have found it necessary to expand the explanatory base of behavior therapy to include psychological processes of cognition and memory. Most of the recent empirically supported behavior therapies have been developed by cognitively oriented clinicians with psychodynamic training. I refer specifically to Beck, Ellis, and Linehan as examples. These treatments would not have developed had behavior therapy restricted its explanatory base exclusively to functional analysis.

Tryon (2002), Ilardi (2002), and Feldman (2002) support a further extension of the explanatory base of behavior therapy regarding learning. Behavior therapy has been defined (Eysenck, 1964; Wolpe 1969; Wolpe & Lazarus, 1966) and recently has been redefined as applied learning theory (Tryon, 2000a). Learning theory has generally been narrowly interpreted to mean principles of operant and respondent conditioning. The large majority of behavior therapists have a cognitive-behavioral orientation because they believe that conditioning principles provide an inadequate theoretical (scientific, explanatory) and clinical basis for contemporary cognitive-behavioral therapy. Comments made by KFK regarding Plaud (2002) concur with this concern. If one accepts that successful behavior therapy entails new learning, then it follows that knowing more about the learning process is better for both the science and practice of behavior therapy. Neural Network Learning Theory (NNLT), my term for Parallel Distributed Processing (PDP) Connectionist Neural Network (CNN) models, is a modern learning theory that is consistent with explanation by selection; the primary explanatory basis preferred by behavior analysts. The work of John Donahoe (Donahoe, 1991, 1997; Donahoe, Burgos, & Palmer, 1993; Donahoe & Dorsel, 1997; Donahoe & Palmer, 1989; Donahoe, Palmer, & Burgos, 1997a, 1997b) clearly demonstrates that NNLT is highly consistent with behavior analysis. NNLT now explains virtually all conditioning phenomena. Donahoe (1997) persuasively argues for the necessity of broadening behavior analysis to include neural networks. NNLT is a superset of behaviorism that includes all conditioning phenomena. NNLT is also a cognitive theory because it was developed, in part, by cognitive psychologists to understand the microstructure of cognitive processes. NNLT is informed by neuroscience and constitutes part of cognitive neuroscience. This is just the sort of integrative modern learning theory that cognitive-behaviorists need and deserve.

A major reason why functional analysis does not provide a sufficient theoretical basis for behavior therapy is that it *presumes* rather than *explains* the *mechanisms* that enable the functional relationships included in a functional analysis. For example, reinforcement is fundamental to functional analysis and behavior analytic explanation but questions about what makes a reinforcer reinforcing and how reinforcement modifies behavior are not asked because it is presumed that behavior therapists do not need to know the answers to such questions. True, behavioral technicians do not need to know this information in order to modify behavior but behavioral scientists should have a more complete understanding of the relevant causal processes involved. Functional analysis presumes psychological processes of perception, cognition, learning, and memory. They are not formally included into a causal analysis despite utilization of an S-O-R model. The main rationale appears to be that such inquiry has in the past distracted clinicians and scientists from completing a functional analysis of behavior. Contemporary efforts to promote functional analysis by excluding inquiry into underlying causal processes have been rejected by cognitive-behaviorists who currently constitute the large majority of behavior therapists. I agree that many cognitive explanations are circular and redescribe rather than explain behavior, but NNLT explanations entail selection rather than design (cf. Donahoe, 1991, 1997; Donahoe, Burgos, & Palmer, 1993; Donahoe & Dorsel, 1997; Donahoe & Palmer, 1989; Donahoe, Palmer, & Burgos, 1997a, 1997b) just as behavior analytic explanations do. But NNLT explanations of behavior include perception, cognition, learning, and memory and are therefore fully psychological versus narrowly behavioral.

Behavior therapists want and need to better understand the learning process so that more effective treatments can be prepared. The principle of parsimony prefers a single learning theory that explains conditioning and cognition in humans and animals with one set of principles in a way that is consistent with neuroscience. NNLT provides such an expanded explanatory base. Choosing this theoretical approach will not necessarily change the way behavior therapy is currently practiced, and that is not a deficiency of NNLT. Achieving a more complete understanding of why current cognitive-behavioral treatments work is a valuable contribution in and of itself. Tryon's (1998, 1999) Bidirectional Memory Model of PTSD is the first theory to satisfy all explanatory requirements specified by Jones and Barlow (1990) and by Brewin, Dalgleish, and Joseph (1996). This is an important scientific contribution. Tryon (1999) also makes novel predictions that may lead to more effective treatments. Other contributions of cognitive neuroscience may be more distant, but precluding inquiry into this area can only deprive all of us from productive future developments in this field. Some behavior therapists consider this uncertain price worth paying if it preserves an emphasis on functional analysis. Donahoe has successfully maintained a primary focus on functional analysis while expanding his explanatory base into the cognitive arena, thereby leading the way for other behaviorists to do so (cf. Donahoe & Dorsel, 1997). Tryon (in press) has argued elsewhere that the parallel distributed processing version of connectionism that constitutes NNLT is a superset of behaviorism that promotes acceptance of selectionist arguments among psychologists generally and may accordingly advance a more widespread acceptance of behavior analysis. It is ironic and counterproductive for behavior analysts to oppose an effective means of promoting explanation by selection upon which functional analysis is based.

Ilardi (2002) and Feldman (2002)

This section responds to specific criticisms made regarding Ilardi (2002) and Feldman (2002). I agree with KFK that science without application and application without science are both problematic. My comments go to the former problem. Behavior therapy currently does not seek a scientific explanation of why its procedures work. Only outcome questions are being addressed. Process considerations should of course wait until outcome has been established to be sure there is something to explain, but a substantial and growing list of empirically supported treatments provide much to be attributed. My aim is to expand the explanatory base of behavior therapy while preserving its applied focus (cf. Tryon, 2002).

Cognitive neuroscience was criticized for contributing more to medical than psychological understanding, where medical understanding largely meant drug development. A major insight provided by NNLT is that psychological treatments and drug treatments probably affect the same synapses and neurotransmitter systems in similar ways; the primary difference being that learning-based treatments produce more long-lasting changes. Contrasting drug vs. psychological effects seems to be becoming a false dichotomy and therefore not a valid criticism.

Tryon (2002)

NNLT and cognitive neuroscience intend to broaden our conceptual understanding of why empirically supported treatments work. Their explanatory success does not require any change in the clinical practice of behavior therapy. Hence, pointing to the absence of such change is irrelevant. Technicians do not need to understand why methods work, but behavioral scientists do.

KFK question both the validity and explanatory force of studying the functional properties of multilayered networks via computer simulation as a means of understanding the functional properties of multilayered organic networks. John Donahoe is a behavior analyst who opposes this view. He has argued that the study of neural networks is essential to the future of behavior analysis (cf. Donahoe, 1997).

KFK's concern with processing speed misses at least two central points. One, NNLT shares the criticism that the traditional serial processing artificial intelligence approach as fundamentally incorrect because its approach is orders of magnitude too slow to explain real time behavior. Two, PDP connectionism is concerned with network principles that enable operant and respondent conditioning to occur at all. KFK cannot explain how behavioral variation is instantiated nor how reinforcers alter response variants. They do not even see these questions as in need of explanation.

Computer simulations can and do predict behavior in that they successfully predict the behavioral outcome of many experiments, including conditioning studies. Many psychological and behavioral phenomena are now well understood in NNLT terms.

Plaud (2002)

KFK correctly perceive that Plaud (2002) is concerned that behavior therapists have steadily separated themselves from behavior analysis since the mid-1960s and that behavior analysis now supports only parts of some treatments like Dialectical Behavior Therapy. This observation directly supports the conclusion that functional analysis provides an important but limited basis for modern behavior therapy. Clinicians, frequently with psychodynamic backgrounds, have developed the cognitive components of most current empirically supported treatments.

KFK's comments regarding "Plaud's Behavior Analysis and Behavior Therapy" presents Acceptance and Commitment Therapy (Hayes, Strosahl, & Wilson, 1999) as a new behavioral analytic therapy. The concepts of acceptance and commitment are not the original product of behaviorism or of basic behavioral science constrained by the recommended explanatory prohibitions against mediating variables and hypothetical constructs. Some behavior analysts continue to reject the concept of learning as a circularly defined hypothetical construct (cf. Tryon, 2000a). Acceptance and commitment are not better defined. These hypothetical constructs presume the psychological processes of learning, memory, and cognition that functional analysis rejects. Hayes et al. may have used the hypothetical constructs of acceptance and commitment, among others, to produce behavioral change but they have not done so within the explanatory restrictions imposed by KFK (2002) or by Plaud (2002).

KFK's comments regarding Plaud (2002) state that Kohlenberg and Tsai's (1991) "functional analytic" therapy is based on the development of a therapeutic relationship. Meta-analysis reveals that the working alliance is therapeutic (Constantino, Castonguay, & Schut, 2002; Martin, Garske, & Davis, 2000) by itself, but the first proponents of this perspective were psychoanalytic, not behavioral. Kohlenberg and Tsai recognize the need to expand their explanatory base beyond the important core but strict confines of functional analysis by including relationship factors and the psychological processes they presume.

Plaud (2002) argues that it is not too late to return to principles of learning. Tryon (2000a) has recently defined behavior therapy as applied learning theory. We both emphasize functional analysis and explanation by selection. Our primary difference is that I recommend addressing the underlying causal mechanisms that enable learning and behavior change to occur; Plaud does not.

Conclusions

Functional analysis is fundamental to effective behavior therapy and will continue to constitute a core component of the most effective empirically supported behavior therapies. This said, the behavior of most behavior therapists has made it very clear that the explanatory basis provided by functional analysis is insufficient for both the science and practice of contemporary behavior therapy. Successful therapy very likely entails new learning. It is therefore better to know more than less about the learning process. Behavior therapy entails the application of modern learning theory (Tryon, 2000a). NNLT is a modern comprehensive learning theory that (1) is a superset of behaviorism, functional analysis, and selection, (2) is consistent with cognitive theory, and (3) is part of cognitive neuroscience. The work of John Donahoe, a senior behavior analyst, exemplifies this cognitive-behavioral theoretical integration. I share his conviction that neural networks are essential to the future of behavior analysis. NNLT provides both a necessary and sufficient explanatory base upon which behavioral and cognitive therapists can build their scientific and clinical future.

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The Cognitive Neuroscience Framework and Its Implications for Behavior Therapy: Clarifying Some Important Misconceptions

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Radical behaviorists often lament the fact that their conceptual framework is widely misunderstood and underappreciated by nonbehaviorists of all stripes. I believe the complaint is warranted. There is a touch of irony, therefore, in the fact that my radical behaviorist/contextualist colleagues in this friendly debate (Kelly, Forsyth, & Karekla, 2002; Plaud, 2002) appear to have misconstrued some important facets of the cognitive neuroscience perspective, which Tryon (2002a) and I (Ilardi, 2002) have outlined in *the Behavior Therapist* (and elsewhere; e.g., Ilardi & Feldman, 2001). Although Tryon (2002b) does an admirable job of addressing many of these issues, a few additional points of clarification may prove helpful.

On the Nature of Neurally Instantiated Information Processing

Kelly and colleagues (2002) contend that the cognitive neuroscience perspective is overly reliant on the *computer metaphor* in describing brain activity, and they question the degree to which such a metaphor could "add anything in explaining the activity of real neurons in real brains." *Computer metaphor*, of course, is a loaded term—one not infrequently employed in straw-man arguments to "prove" the wrongheadedness of all things cognitive. The rhetorical force of such arguments (echoes of which may be heard in the Kelly et al. paper) tends to hinge on the accusation that cognitivists believe the brain functions more or less like a commercially available computer. Such a misconception calls for a statement of the obvious: There is no physical respect in which the brain operates as does one's computer. The former, for example, does not contain a serial central processing unit, nor does it come preloaded with a crash-prone operating system courtesy of Bill Gates! Indeed, for the purposes of this discussion, the only important commonality between such devices and human brains is the fact that both are physical systems designed—the former by engineers, the latter by natural selection—with a functional capability of instantiating and processing certain forms of information.

But isn't *information processing* just an equally unhelpful—and unscientific—metaphor? Fortunately, it is not. As outlined by mathematician Claude Shannon (1938) in his seminal work on signal detection theory, information may be technically defined in a functional, objective manner as *that which functions to reduce uncertainty about the occurrence (or nonoccurrence) of a given event* (Best, 1989). Information, defined in this fashion (i.e., the reduction of event-based uncertainty), is a term commonly employed in natural science disciplines ranging from physics to biology. As I've explained elsewhere,

> In the biological realm, such uncertainty is ubiquitous, and each motile organism stands to increase its adaptive fitness to the extent that it is capable of reducing uncertainty (i.e., gaining information) about salient features of its environment: Is there a food source to be encountered in the vicinity? In what quantity? At what distance? And what about the presence of potential predators? Potential mates? And so on. From the vantage point of information theory, sensory perceptual data subserve the function of reducing uncertainty in such domains, and thereby provide the sensing organism with valuable information about its environment upon which to base behavior. (Ilardi & Feldman, 2001, p. 1070)

The neuron, in fact, has been exquisitely designed by natural selection to serve the function of information signal propagation (Bownds, 1999). The brain's interconnected arrays of neurons, likewise, function to integrate thousands of individual neural information signals in performing higher-order transformations upon them (Marr, 1982). Such a transformational process (i.e., information processing) is readily observed, for example, in the task of visual object recognition, in which hundreds of thousands of raw photochemical informational signals from the retina are transmitted to neural arrays in the brain's occipital cortex specifically adapted to the function of pattern detection (Pinker, 1997). The adaptive significance of such cerebral information processing functions is beyond dispute. As observed by evolutionary scientists Tooby and Cosmides (1995), "The evolutionary function of the brain . . . is obviously the adaptive regulation of behavior and physiology on the basis of information derived from the body and from the environment. The brain performs no significant mechanical, metabolic, or chemical service for the organism; its function is purely informational, computational, and regulatory in nature" (p. 1190).

It follows, therefore, that the scientific task of "explaining the activity of real neurons in real brains" (to use the phrasing of Kelly et al., 2002) will of necessity be informed by consideration of the adaptive function by virtue of which such brains have been selected over successive eons—that is, the function of processing information for the adaptive regulation of behavior. The cognitive neuroscience claim that the brain performs such information processing operations—in the precise technical sense articulated by information theorists (Shannon, 1938; Turing, 1950; see also Marr, 1982)—is not an example of so-called "metaphorical talk," as Kelly and colleagues allege. It is, rather, an objectively verifiable claim, one long regarded as a given by esteemed colleagues in the natural sciences (Wilson, 1998), including Nobel laureates such as Francis Crick (1994) and Murray Gell-Mann (1994).

The Correspondence Between Brain Events and Psychological Events

In a related line of attack on the cognitive neuroscience framework, Kelly and colleagues (2002) have called into question the degree to which there exists a direct correspondence between neural events and psychological/behavioral events. Despite the existence of an accumulating wealth of supportive empirical evidence for such a correspondence from the neurosciences and related disciplines, Kelly et al. claim, "virtually all of this work in living, breathing organisms is correlational" (p. 80), and concede only that behavioral and psychological events may lead to changes in the brain, but *not* vice versa. They are mistaken on this point. There exist numerous published investigations that have utilized experimental manipulation to demonstrate that specific alterations to the function or structure of the brain lead to very specific alterations in psychological and behavioral experience. Such manipulations range from the induction of self-deprecatory thoughts during an acute neuronal catecholamine depletion challenge (Berman et al., 1999) to the altered recall of affectively toned autobiographical memories during anaesthetic inactivation of the right cerebral hemisphere (Ross, Homan, & Buck, 1994). There exists, moreover, a robust clinical literature documenting the direct alteration of specific neurocognitive functions—e.g., impulse regulation, psychomotor sequence initiation, memory consolidation, perception of prosodic cues, etc.—on the basis of focal lesions to highly specific cerebral areas (Mesulam, 2000). On the basis of such evidence, in tandem with a burgeoning functional neuroimaging literature, the high degree of skepticism of Kelly and colleagues regarding the correspondence of brain events and psychological events appears unwarranted.

Why is it worth belaboring this point? And for that matter, why might radical behaviorists resist acknowledging the increasingly obvious correspondence of brain events and psychological events? I believe the answer is to be found, at least in part, in the logical implications of such a correspondence regarding (a) the objective scientific status of psychological events, and (b) behavioral scientists' ability to investigate the causal role of such events in the proximal mediation of behavior. Historically, of course, behaviorists have evidenced a very strong aversion to the claim that psychological events—thoughts and feelings, for example—are capable of mediating environment-behavior relations, and this objection has been based in part on the contention that such "private events" are in principle objectively unobservable (Plaud, 2002). But in recent years influential radical behaviorists have also begun to acknowledge explicitly that the environmental selection of behavior is proximally mediated by neural network events in the brain (e.g., Donahoe, 1998), observable events that are amenable to objective investigation. Therefore, once it is conceded that such neural network events (or at least a subset thereof) correspond directly with psychological events, the radical behaviorist is left in the awkward position of having to further concede (a) that psychological events—as the subjectively experienced manifestations of neural network events—are directly amenable to *objective* empirical investigation, and (b) that psychological events (again, as the subjectively perceived correspondents of neural network events) are capable of proximally mediating behavior.

The Pragmatic Utility of the Cognitive Neuroscience Framework

Perhaps the most compelling concern of Kelly and colleagues (2002) regarding the cognitive neuroscience framework is a pragmatic one: "If cognitive neuroscience is to become a unifying metatheoretical framework for behavior therapy, 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" (p. 80). And, despite the fact that there already exist many efficacious somatic interventions for psychological disorders, developed by investigators informed by the theory and methods of cognitive neuroscience, it seems indisputable that many behavior therapists will not find the cognitive neuroscience perspective fully persuasive until they are convinced of its pragmatic utility in the form of enhanced methods of *psychosocial* assessment and intervention.

As we now briefly consider the available evidence on this point, however, it will be important to bear in mind that the history of science is replete with examples of elegant theoretical advances that required many years—often decades—to

From the President

Richard G. Heimberg, Ph.D.

These are economically challenging times for many individuals and organizations. AABT is no exception. We are projecting large financial deficits in the next few years based on a variety of factors.

In a perfect world, we would get everything we want, exactly when we want it. In the next best world, we'd at least be able to keep what we already had. In the world that most of us inhabit, finding the most appropriate compromises is the business imperative.

AABT's Board of Directors, confronting some financial realities, asked each of the Association's major areas (membership, publications, and meetings) to cut costs. This process has begun. The Central Office has been reduced by two positions: the Membership/Marketing Manager and the Assistant to the Executive Director.

The Board of Directors has instituted monthly conference calls to transact the business of the Association and will not hold a Spring meeting this year. An Ad Hoc Committee on Fund Raising has just been constituted, and we thank Robert Leahy for agreeing to chair this important effort on the part of the Association.

In publications, cost savings will be attained by publishing 8, rather than the usual 10, issues of *the Behavior Therapist* this year, forgoing the May and Summer issues. Depending on our future finances, we may be able to reverse this cut soon, which is our hope.

We know you like *the Behavior Therapist*. In each survey we've done over the last decade or so, you've identified it as the most important and most visible member benefit. Our advertisers like it, and find it a useful vehicle for their products, services, and, especially, their staffing needs. And we see it as an essential communication tool among the Association's many constituencies: elected and appointed officers, staff, advertisers, researchers, clinicians, educators, and students; we also believe that it is essential to keep this line of communication open.

The Board and the staff are working harder than ever to make sure that membership in AABT remains important to every professional and student. Within the next month we will be launching the AABT listserve, thanks to member Lynn Marcinko and her staff at the Harbor UCLA Medical Center. We will also be posting more information on our Web site. Staff continues to explore ways to expand our technology and services with an eye toward cost efficiency.

Our convention will continue to offer the high-quality diversified program our members have come to expect. However, preregistration for CE activities will be looked at very carefully, and those offerings that are undersubscribed will be cancelled. This is a step we have not had to face in better times.

We hope that the financial pressures that necessitated these compromises are short-lived. If they're not, we will continue to develop a well-considered plan to address them.

We appreciate your continued support of AABT. Here are ways you can help: Encourage colleagues to join (our Membership Committee will soon be launching its member-get-a-member campaign). Be sure your institution subscribes to our journals and purchases our useful new series of clinical demonstration videotapes. Suggest to publishers you might work with that they advertise and exhibit with AABT. Offer your services—like the two members listed above, and the dozens who are active in our governing structure. You can make a difference.

Please share your thoughts with us. Write me care of the AABT central office or via e-mail to Mary Jane Eimer, AABT's Executive Director, at mjeimer@aabt.org ✍

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yield dramatic payoffs in the form of widespread pragmatic applications. For example, although Watson and Clark articulated the double-helix molecular structure of DNA in 1952, thereby catalyzing the development of the field of molecular genetics, decades passed before efficient gene-splicing techniques began to make widely available an array of clinical extensions of their work, such as the synthesis of human hormones (e.g., insulin) in genetically altered infrahuman species. Thus, in light of the fact that cognitive neuroscience is still a "young" scientific discipline (Wilson, 1999), and the related fact that there exists at present only a small number of behavior therapy researchers whose work is directly informed by the theory and methods of cognitive neuroscience, it would be surprising to find at this point a large pragmatic payoff for cognitive neuroscience vis-à-vis behavior therapy applications.

Fortunately, however, the psychotherapeutic promise of the cognitive neuroscience perspective is already becoming apparent, even at this very early stage of the applied clinical research process. For example, cognitive neuroscience researchers at UCLA have recently demonstrated the applied utility of neuroimaging assessment for patients with obsessive-compulsive disorder (OCD). Specifically, these investigators have identified subsets of OCD patients who respond preferentially to acute treatment with exposure-based behavior therapy versus SSRI medication, and vice versa, on the basis of cerebral glucose metabolism patterns evident on PET scan (Brody et al., 1998)—a finding with obvious pragmatic clinical value. In another exciting line of investigation, Tim Strauman has reported preliminary neuroimaging data that suggest not only that depressed patients experience decreased activation in a region of the right posterior cerebral cortex, which mediates the comprehension of nonverbal communication cues (e.g., those contained in others' tone of voice, body language, and facial expressions), but also that depressed patients who are treated acutely with cognitive-behavior therapy (CBT) experience reactivation in these cortical areas, while those treated with antidepressant medications do not (Strauman, 2000). Such a finding may help explain in part the superior prophylactic benefit of CBT regarding the risk of depression relapse, and may make possible the identification of specific components of the CBT protocol that are essential to the reactivation of right posterior cortical areas involved in nonverbal comprehension tasks. My own recent collaborative cognitive neuroscience work in the investigation of depressive cognition has led to the detection of an apparently stable depressive cognitive bias for negatively valent verbal information, even among recovered depressed individuals. Interestingly, this rather robust bias exists only for words presented via divided visual field exclusively to the right cerebral hemisphere (Atchley, Ilardi, & Enloe, in press; Enloe, Ilardi, Atchley, Cromwell, & Sewell, 2001; Ilardi & Atchley, 2001), which has a specialized role in emotion comprehension and the processing of episodic (autobiographical) memory. Because depression investigators have heretofore been unable to detect robust depressotypic cognitive biases among previously depressed individuals—at least in the absence of dysphoric mood priming—our findings may hold promise for enhancing the assessment of depressive cognitive biases. By extension, we are early in the process of employing this divided visual field/cerebral hemispheric lateralization assessment approach to determine (a) the degree to which individual differences in enduring right hemispheric negativistic biases render some individuals especially vulnerable to depression relapse, and (b) the specific set of therapeutic techniques that are the most effective in ameliorating such biases.

The promising findings reviewed above, of course, represent merely the tip of the proverbial iceberg. It is certain, for example, that with each passing year, techniques of neuroimaging will become increasingly accurate in the spatial and temporal resolution of brain events (i.e., the recording of real-time brain events in increasingly fine-grained levels of detail). There will doubtless also occur a commensurate increase in discovery regarding the detailed manner in which precisely measured cerebral neural network events reflect the adaptive processing of salient information (e.g., regarding the organism's external and interoceptive environments), inasmuch as scientific discovery is invariably catalyzed by major advances in the precision of measurement. Moreover, it is clear that in the years and decades immediately ahead, neuroimaging procedures will continue to become more widely disseminated, more heavily utilized in clinical settings, and much more readily accessible to psychological researchers and practitioners. Accordingly, it appears reasonable to predict an exponential growth in clinical psychologists' utilization of the theory and methods of cognitive neuroscience to enhance both their conceptual understanding of psychological disorders and their ability to treat them effectively. Behavior therapists who steadfastly choose to ignore such developments, or who persist in relegating them to a distant disciplinary realm beyond the scope of behavioral psychology (e.g., Plaud, 2002), risk missing out on important advances in the field's ability to "predict and control behavior."

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Letter to the Editor

The Efficacy of Neurofeedback

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As a joint committee representing the Society for Neuronal Regulation and the Association for Applied Psychophysiology and Biofeedback, we are writing to express our concerns about a recent article in *the Behavior Therapist*. Lohr, Meunier, Parker, and Kline (2001) published a review of neurofeedback, concluding that it does not qualify as an empirically supported treatment. Unfortunately, the review was not comprehensive and demonstrated systematic bias in failing to include the most rigorous research on neurofeedback.

Sterman (2000) comprehensively reviewed the literature on the neurofeedback treatment of uncontrolled epilepsy. Overall, this literature documented that 82% of the most severe, uncontrolled epileptics demonstrated a significant reduction in seizure frequency. Although 3 studies found significant seizure reductions regardless of the EEG feedback contingencies that were rewarded, "the vast majority of patients responded only when feedback contingencies provided reward for SMR activity and normalization of the sensorimotor cortex EEG" (p. 49). A total of 12 studies examined whether the EEG improved, and 66% of the patients showed significant EEG improvement (e.g., Lubar et al., 1981). Two studies even measured sleep EEG pre- and post-training and documented significant normalization following conditioning. Sterman concluded,

> The consensus arising from these findings is that most epileptic patients who show clinical improvement with EEG biofeedback also show contingency-related EEG changes and a shift toward EEG normalization. However, not all patients who respond to this treatment show expected EEG changes, and a few patients who show EEG changes experience little clinical improvement. One is reminded of the fact that a similar percentage of patients undergoing anterior temporal lobectomy for the surgical treatment of complex-partial seizures failed to show the expected hippocampal sclerosis or other lesions in microscopic studies of the tissue removed. Further, 27% of those patients with documented lesions showed little clinical improvement. Both EEG neurofeedback and anterior temporal lobectomy treatments are confounded by our relatively primitive comprehension of neural regulation and seizure pathology, and by the limitations of current analytic methodology. (pp. 52-53)

We would argue that the absolute requirement for EEG normalization in the Lohr et al. (2001) paper is not justified. The findings summarized above indicate that normalization often occurs. However, when it does not, the possibility of non-specific effects must be considered. But this is not the only potential explanation. For instance, it may be that neurofeedback sometimes promotes greater cognitive flexibility in response to cognitive demands.

Nine studies reviewed by Sterman (2000) had control conditions that variously included noncontingent reinforcement, EMG, an ABA crossover design, random feedback, and relaxation training with EEG electrodes. One study included yoked noncontingent and wait-list control groups. Another new controlled study (Kotchoubey et al., 2001) validated its effectiveness compared to medication and placebo. None of these studies were reviewed by Lohr et al. (2001). We thus conclude that neurofeedback meets the criteria for being both an efficacious and specific treatment for uncontrolled epilepsy by the Chambless et al. (1998) and Chambless and Hollon (1998) criteria.

The Lohr et al. (2001) review also neglected three studies (Garrett & Silver, 1975) of test anxiety. These studies included random assignment, alternative treatment control groups, and a wait-list control group. In one experiment, the group receiving alpha EEG enhancement training produced 33% more alpha post-treatment, and all three feedback groups demonstrated significant reductions in test anxiety, while the untreated control group and the relaxation training group experienced no significant reduction. In another experiment, subjects received phases of alpha enhancement training and EMG biofeedback training. The alpha training increased alpha production from 64% to 78% of the time, and anxiety scores dropped significantly ($p < .001$) for the combined treatment group compared to a nontreatment group. Thus, we also believe that the neurofeedback treatment for phobic anxiety qualifies for the category of possibly efficacious. Space limitations prevent us from providing further evidence for the efficacy of neurofeedback for treating ADD/ADHD and brain injuries.

We wholeheartedly agree with Lohr et al. (2001) that many areas of clinical application of neurofeedback require improved research validation and that more and better-quality research is needed in several areas. However, we disagree with their stringent and excessive standards, which are in contrast to the Chambless and Hollon (1998) and APA Division 12 Task Force (Chambless et al., 1998) criteria in that they apparently require a placebo control condition. That raises important methodological and ethical issues that Lohr et al. are either unaware of or chose not to address. Requiring a placebo control condition where a known effective treatment is already available has been deemed unethical by both medical ethicists (Lurie & Wolfe, 1997; Rothman, 2000) and by the Declaration of Helsinki of the World Medical Association. This issue is discussed in greater depth in La Vaque and Rossiter (2001) and Kotchoubey et al. (2001).

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WATCH, LEARN, UNDERSTAND WORLD ROUNDS VIDEOS

OBSERVE INTERNATIONALLY RENOWNED MASTER CLINICIANS CONDUCTING LIVE CLINICAL INTERVIEWS WITH MOCK CLIENTS.

☐ Cognitive Behavior Therapy With a Couple

Frank Dattilio, *University of Pennsylvania School of Medicine*

Dattilio blends humor, metaphor, and clinical acumen to engage a troubled couple.

Karen and Kevin, married 6 years, are struggling to reestablish trust after Kevin's affair with a coworker. Dattilio demonstrates how to help the couple broaden their perspective while adjusting their unrealistic expectations. The therapist models the basic steps of setting ground rules for treatment, conceptualizing the couple, and putting together a concrete plan. Techniques demonstrated include cognitive restructuring, perspective taking, assessment of relationship schemata, and homework.

This tape is appropriate for beginning or seasoned clinicians; it is both a thorough introduction and a valuable refresher course.

☐ Redirecting Anger Toward Self-Change

Raymond DiGiuseppe, *St. John's University and the Albert Ellis Institute for Rational Emotive Therapy*

Angry clients often wish to focus on others' behavior rather than their own.

DiGiuseppe demonstrates methods to help angry clients stop setting themselves up for continued frustration, to stop relying on mind-reading, and to replace ruminative anger with strategies that facilitate resolution. Together, the client and therapist use mental imagery to revisit anger-provoking situations. New responses are formulated and implemented via role-plays, and independent practice is planned.

Appropriate for any clinician wanting to develop better ways to help clients decrease anger and its lingering aftereffects. Demonstrations of imaginal exposure to anger-provoking events are particularly helpful.

☐ Cognitive Hypnotherapy in Anxiety Management

E. Thomas Dowd, *Kent State University*

"Don't worry, I'm not going to make you quack like a duck."

This demonstration will dispel misconceptions or doubts about the viability of hypnosis as a therapeutic technique when used to augment the process of therapy. Based on Meichenbaum and Gilmore's model of content, process, and structure, cognitive hypnotherapy is used as a nonverbal cognitive restructuring tool. After helping the client achieve a deeply relaxed state, Dowd guides her through three imaginal scenarios that serve as metaphors for her problem. In cognitive hypnotherapy, the clinician is an active guide who presents the client with numerous ways to frame and solve problems.

This is an informative tape for clinicians of all levels.

☐ Acceptance and Commitment Therapy

Steven C. Hayes, *University of Nevada, Reno*

ACT works exclusively through process rather than content to diffuse patterns of the mind and, instead, emphasize experience. The ultimate goal: the realization that there is no ultimate goal.

In this refreshingly different video, Hayes works with Candace, a young woman with social phobia who views her anxiety as a problem. He encourages the client to deconstruct anxiety into a set of harmless individual symptoms and meaningless words. Through the use of metaphor and sensory exercises, Hayes guides Candace to a state of acceptance of her anxiety in social situations. He strives to help her disentangle from language and, instead, promote her true intentions by "watching the chatter" of her mind without doing anything about it.

This video is sure to be thought-provoking to all clinicians, and demonstrates a viable alternative to standard CBT approaches.

☐ Imaginal Exposure

Edna B. Foa, *University of Pennsylvania School of Medicine*

When conducted properly, exposure therapy provides an opportunity for clients to reencounter, reevaluate, and bring new meaning to traumatic events.

Dr. Foa, a founder of prolonged exposure therapy for PTSD, demonstrates treatment with Janie, a young woman who was raped by an ex-boyfriend in a college dormitory.

The therapist offers the client the rationale for exposure therapy and explains the concept of emotional processing; leads a session of imaginal exposure, in which the client recounts the trauma in detail with the support and guidance of the therapist; and encourages the client to include more details each time the story is told. Between narratives, the therapist reframes the event, highlighting the adaptive value of the client's response, and reinforces the fact that memories can't hurt us. The end of the session includes a demonstration of diaphragmatic breathing and a homework assignment.

This tape is a must for any clinician working with traumatized populations, regardless of level of experience.

☐ Dialectical Behavior Therapy for Suicidal Clients Meeting Criteria for Borderline Personality Disorder

Marsha M. Linehan, *University of Washington, Seattle*

"Suicide is always in the back of my mind." These are not words a therapist hopes to hear from a client. What happens next in the therapy session could influence your client's decision to live or die. Are you as prepared as you should be to deal with chronic suicidality?
Marsha Linehan, master clinician and founder of Dialectical Behavior Therapy, demonstrates techniques used to persuade clients to refrain

TREATMENT CONCEPTUALIZATION AND IMPLEMENTATION ARE HIGHLIGHTED.

Open Forum

Exclamation-Point Usage in the Cognitive-Behavioral Literature

Neah Piste, *Private Practice, Brooklyn, New York*

The cognitive-behavioral literature is virtually devoid of the exclamation point. The APA *Publication Manual* fails to even mention the exclamation point in its section on punctuation. Is psychological writing doomed to the period? And if so, what does this say about the field? Here are some examples of exclamation-point usage from the recent literature. As these sentences reveal, the exclamation point is a valid means of disseminating your ideas. Don't be afraid! Behavior therapy is filled with passionate utterances, if you make them.

"But, deciding to ritualize can be equally painful!" (Grayson, 2000, p. 418)

"That's research, not psychotherapy!" (Raw, 1993, p. 76)

"Mainly, but not dogmatically or absolutely!" (Ellis, 1996, p. 255)

"Were it only so easy for eating disorders researchers (and it is not even that easy for depression researchers)!" (Joiner, 2000, p. 191)

"Behavior therapy can help you and your patients!" (Van Horn, 2000, p. 200)

"Quite a hefty order of business, particularly when working with an anxious child!" (Brodbeck, 2001, p. 278)

"Even psychoanalysis could have been proposed as a behavior therapy, because, after all, its procedures are unquestionably behavioral!" (Wolpe, 1997, p. 634)

"You bet your sweet bippi!" (Robb, 1998)

". . . I gave some of the lectures in the evening and at our social functions I would often cook spaghetti and make salad!" (Cautela, 1990, p. 211)

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Special Interest Groups

News of the SIGs

Andrea Seidner Burling, *SIG Committee Chair*

We are pleased to report that a number of AABT's SIGs supported Marsha Linehan's presidential goal at the recent SIG Poster Expo and Cocktail Reception at the 2001 Convention. Dr. Linehan's goal for her presidential year was "to increase the number of clients receiving effective treatment by increasing the number of providers and provider systems applying effective treatment programs." Each SIG was asked to select a poster at the Expo that best supported or furthered this goal and the posters listed below were chosen by their SIGs. We would like to take this opportunity to congratulate the authors of these posters for their efforts to support AABT's 2001 presidential goal.

Addictive Behaviors SIG: "Factors Associated With Cannabis Self-Resolution and Barriers to Treatment Seeking"—T. Ellingstad, *University of California at San Diego* and *Nova Southeastern University*, K. Venner, *University of New Mexico*, and L. Sobell, M. Sobell, and L. Goldsmith, *Nova Southeastern University.*

Computers & High Technology in Behavioral Practice SIG: "The Drinker's Checkup and Follow-up Drinker's Checkup: A Computer-Based Brief Motivational Intervention With a Follow-Up Component for Evaluating Outcomes"—R. Hester and D. Squires, *Behavior Therapy Associates* and *University of New Mexico.*

Couples Research & Treatment SIG: "Behavioral Couples Therapy With Alcoholic Men and Their Intimate Partners: The Comparative Effectiveness of Bachelor's- and Master's-Level Counselors"—W. Fals-Stewart, *Research Institute on Addictions*, and G. R. Birchler, *University of California San Diego School of Medicine.*

Disaster & Trauma SIG: "Experiential Avoidance and Interpersonal Victimization: Beyond Self-Report Inventories"—M. L. Rasmussen Hall, M. Z. Rosenthal, V. M. Follette, and K. M. Palm, *University of Nevada, Reno.*

Insomnia and Other Sleep Disorders SIG: "Adolescent Sleep Smart Program: A Pilot Evaluation"—A. Wolfson, C. Marco, A. Campbell, T. Charron, and C. Rossi, *College of the Holy Cross.*

Study of Lesbian, Gay, Bisexual, and Transgendered Issues SIG: "Cognitive Processing Therapy for Acute Stress Disorder Following Homophobic Assault: A Case Study"—D. Kaysen and P. Nisbith, *University of Missouri-St. Louis.*

Call for Candidates for Editors of *Cognitive and Behavioral Practice* and *the Behavior Therapist*

Candidates are sought for Editor-Elect of *Cognitive and Behavioral Practice*, volumes 12 - 15. The official term for the Editor is January 1, 2005, to December 31, 2008, but the Editor-Elect should be prepared to begin handling manuscripts approximately 1 year prior.
and . . .
Candidates are sought for Editor-Elect of *the Behavior Therapist*, volumes 27 - 29. The official term for the Editor is January 1, 2005, to December 31, 2007, but the Editor-Elect should be prepared to begin handling manuscripts approximately 1 year prior.

Candidates should send a letter of intent and a copy of their CV to Arthur Freeman, Ed.D., Publications Coordinator, AABT, 305 Seventh Avenue, 16th Floor, New York, NY 10001-6008 or via email to teisler@aabt.org

After an initial screening by the Publications Committee, successful candidates will be asked to prepare a vision letter in support of their candidacy. David Teisler, AABT's Director of Publications, will provide you with more details at the appropriate time. Letters of support or recommendation are discouraged. However, candidates should have secured the support of their institution.

Questions about the responsibilities and duties of the Editor or about the selection process can be directed to David Teisler at the above email address or, by phone, at (212) 647-1890.

Letters of intent MUST BE RECEIVED BY July 1, 2002. Vision letters will be required by September 1, 2002. The Publications Committee will forward their recommendation to the Board of Directors at the Annual Meeting in Reno in November.

Classifieds

For classified rates and closing dates, contact Stephanie Schwartz, AABT Advertising Manager, (212) 647-1890, or via e-mail: sschwartz@aabt.org.

<u>Positions Available</u>

SUMMER FELLOWSHIPS IN COGNITIVE BEHAVIOR THERAPY AND REBT FOR FULL-TIME UNIVERSITY FACULTY. A limited number of 3-week fellowships for university faculty in psychology, psychiatry, counseling, or social work are being offered at the Albert Ellis Institute in July 2002. Featuring intensive practica in REBT, direct supervision of therapy sessions, special seminars, and co-leading a group with Albert Ellis. Send reasons for wishing to participate and vita to Kristene Doyle, Albert Ellis Institute, 45 East 65th Street, New York, NY 10021; or fax at 212-249-3582; or e-mail at krisdoyle@msn.com.

PROGRAM DIRECTOR, NATIONAL DRUG ABUSE CLINICAL TRIALS NETWORK. Faculty Position—Johns Hopkins University Psychiatry Department is soliciting candidates for an Instructor or Assistant Professor level position to direct resources and participate in research opportunities at the Mid Atlantic Node of the National Drug Abuse Clinical Trials Network, under the direction of the node Principal Investigator. Applicants must possess a doctorate in Psychology or other health related field. In addition, it is highly desirable that candidates have leadership skills in initiating and managing research projects and excellent communications skills working with a wide variety of professionals and para professionals.

Johns Hopkins University is the coordinating site of the Mid Atlantic Node and part of a unique and exciting new clinical trials initiative funded by the National Institute on Drug Abuse. The Mid-Atlantic Node is one of 14 groups nationwide comprised of treatment providers and researchers who collaborate to conduct multi-site clinical trials of substance abuse treatments in community treatment settings. Applicants should have a strong interest in the mission of the CTN, which is to conduct studies of therapeutic interventions in rigorous, multisite clinical trials that will ultimately improve the quality of drug abuse treatment throughout the country using science as the vehicle.

Working with the Node PI, the successful candidate will direct central core research support, services and resources, including supervision of core staff and oversight of multiple clinical trials conducted within the Node. This position also coordinates the work of offsite treatment providers, researchers and academic collaborators, in the service of integrating clinical care and high-quality research.

As a faculty member, the person in this position would also be expected to develop and lead research projects within the CTN network, collobarate with Johns Hopkins researchers and/or develop independent grant-supported research. Excellent clinical and research resources are available within the CTN and surrounding Johns Hopkins research institutions.

Salary depends on rank and experience. JHU offers excellent benefits. Johns Hopkins University is an Equal Opportunity Employer. Minorities and women are encouraged to apply. Send letter of interest, Curriculum Vitae, and three names of references to: Maxine L. Stitzer, Ph.D., Behavioral Pharmacology Research Unit, Johns Hopkins Bayview Medical Center, 5510 Nathan Shock Drive, Baltimore, MD 21224; Phone- 410 550-0042; Fax- 410 550-0030; Email: mstitzer@jhmi.edu

MASTER'S LEVEL CLINICIAN. Degree in Psychology, Social Work, Counseling, or other mental health related fields, or near completion. Two years experience working with developmentally disabled and/or psychiatric populations. Knowledge of the mental health system. To provide clinical consultation and training to mental health disabled/mentally ill population. Included working with outpatient departments, screening centers and psychiatric inpatient units. Fax resume to 908-931-0304 or e-mail to mariaisdt@hotmail.com.

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