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PSYCHOLOGY IS A BEHAVIORAL SCIENCE, NOT A BIOLOGICAL SCIENCE. A DISCUSSION OF THE ISSUE AND A REVIEW OF *NEURAL THEORIES OF MIND: WHY THE MIND-BRAIN PROBLEM MAY NEVER BE SOLVED,* BY WILLIAM UTTAL

GARY GREENBERG and CHARLES LAMBDIN
Wichita State University

Since the adoption of the scientific model around 1879, psychology has been identified as a dualistic enterprise, that is, the study of behavior and of the mind. While the discipline has had little difficulty in defining what is meant by *behavior* (i.e., what organisms *do*), we have yet to achieve anything near consensus about what is meant by *mind*, or even if there exists such a thing or process. Ideas about the mind can be traced back to the beginnings of philosophy, but there is little dispute that the modern concept of the mind (i.e., mind-body dualism) can be attributed to Descartes (Leahey, 2000).

The book under review here is the latest of a series in which Uttal offers contemporary theorizing in behavioristic thinking about the role of the brain in psychology in general and the mind in particular. Two earlier books in the series set the stage, first by presenting Uttal's somewhat Watsonian conception of behaviorism (1998) and then by arguing against mental events being localized in particular brain areas (2001). Uttal's ideas of behaviorism are Watsonian because, while his arguments against strict neural reductionism are lucid and well reasoned, he nevertheless clings to the idea of minds that exist in brains. In a review of Uttal's 2001 book, Greenberg (2002, p. 113) said, "His insistence, however, that the brain-mind connection is real is reminiscent of Kuo's assessment of Watson's behaviorism—in the end, Kuo said, Watson was really a mentalist at heart (Greenberg & Partridge, 2001)." Uttal concludes his latest book (p. 262, the last page) by again insisting he is a behaviorist, although clinging to the *belief* (his term, p. 259) that the brain produces the mind. Understanding how is, for Uttal, science's greatest challenge: "[T]here is no more important science or difficult intellectual challenge than the mind-brain problem. Cosmology and basic particle physics, evolution and genetic coding, however important they may be, do not come close to the grand task of cognitive neuroscience—to understand (to the extent that it is possible) the nature of our minds and their origins in neural activity" (p. xv).

Gary Greenberg is a professor emeritus at Wichita State University. His present address is 4447 N. Malden St, Chicago IL 60640. (E-mail: gary.greenberg@wichita.edu).

What Is, or Is There Really, Mind?

Since Uttal's book is about the mind, our views about this elusive phenomenon are pertinent. Mentalism can be seen as a crutch that gets in the way of our undertaking "the more arduous task of understanding the integration of biological and contextual influences in terms of the developmental system of which they are a dynamic part" (Lerner, 2004, p. 20). No psychologist has made this point more succinctly than T. C. Schneirla (1949), who wrote, "'Mind,' ostensibly a term for a generalized functional entity, a very impressive term, actually is only an introductory expression for all of man's intellectual capacities and attainments considered as a system" (p. 225).

It can be argued that the mind is not some phenomenon discovered through patient and arduous empirical work but rather an invention, first by the Greeks and, for modern psychology, by Descartes to avoid the Catholic Church's power of thumbscrew, and later by others such as Freud as a way of legitimizing some extremely creative, imaginative, and most likely wrong, ideas (e.g., Bailey, 1965; Thornton, 1984). Indeed, in pointing out that we still have no satisfactory definition of the mind, Uttal agrees that today's mind was yesterday's *soul*:

> In previous times the word *soul* served the role that *mind* does now. . . . I use *soul* here with the understanding that its theological overtones are to be ignored and that *soul* is, for all practical purposes, synonymous with what modern science now calls *mind*. (p. 50)

Skinner's (1977) understanding is pertinent here:

> The Greeks invented the mind to explain how the real world could be known. For them, to know meant to be acquainted with, to be intimate with. The term *cognition* itself is related to *coitus*, as in the biblical sense in which a man is said to know a woman. Having no adequate physics of light and sound nor any chemistry of taste and odor, the Greeks could not understand how a world outside the body, possibly some distance away, could be known. There must be internal copies. Hence cognitive surrogates of the real world. (p. 5) . . . The mental apparatus studied by cognitive psychology is simply a rather crude version of contingencies of reinforcement and their effects. (p. 9)

One of us has argued against psychology's reliance on the brain as *the* organ of behavior (e.g., Greenberg, 1983). Indeed, while we do not deny the real necessity of the brain for behavior (or any life process. for that matter), we cannot dismiss the ignored and neglected writings of John Lorber, who in the 1970s accidentally came upon several young adults, normal in all respects, with virtually no brains at all, a result of early childhood hydrocephalus (Lewin, 1980)! The most that critics of these reports can say is that normal functioning without a brain is simply not possible, but no empirical evidence is ever offered to refute Lorber's reports.

Though we, like Kuo commenting on Watson, maintain that in the end Uttal is not really a behaviorist, behaviorists should not casually dismiss or ignore his contributions to the current discussion of these important issues. One may wonder why, given our orientation, we say this about a book that adheres to the mind-brain connection. There are three reasons: first, Uttal presents one of the best discussions of the concept of theory in science that we have ever read; second, he presents a thorough vetting of the historical and contemporary treatments of the mind-brain connection; and finally, his orientation is pointedly not a strict reductionistic one, although he still believes the brain plays a more crucial role in behavior than most behaviorists would agree to. In fact, he even seems to imply that psychology is a biological science (e.g., "It is not inappropriate to mention the two great theories that guide modern biology *including all aspects of psychology*—evolution and genetic coding" [p. xiii, emphasis added]). However, like Kantor (1959) and Schneirla (1949), we believe that psychology is mature enough to be a uniquely psychological science with its own principles that are distinct from biological ones (Greenberg, Partridge, Mosack, & Lambdin, in press; Pronko, 1980).

What Is Theory?

Uttal's first chapter addresses at length just what is and should be meant by the term *theory* and whether there is such in psychology. As we have stated above, it is among the best discussions of this important topic we have ever read, a result of its thoroughness. All graduate students in psychology would do well to read this in their first year of study; indeed, professionals will also gain much from it. After reading this book, one could easily conclude that there are no true theories in psychology, at least not those with the explanatory power of one such as Einstein's theory of special or general relativity or even Darwin's theory of evolution by natural selection. These are theoretical propositions of great explanatory and predictive power. Psychologists have frequently referred to their propositions as theories, but such can be true only in the loose sense of the term as preliminary statements or hypotheses or as sets of ideas with minimal explanatory power.

It is surprising that there is nothing near consensus about just what a theory is, as this chapter makes clear. Uttal says, "There is little question that the nature of a theory is an active field of discourse in philosophy of science circles" (p. 38). When one of us (G.G.) first read this book in manuscript form, he commented that this chapter, while excellent, was too long. He is here recanting, primarily because of the important central issue confronted in this book, that of cognitive-neuroscientific theories of mind. It now seems essential to fully understand just what is meant by theory if we are to conclude that such theories of mind are impossible.

"The word *theory* is still used willy-nilly in an enormous number of contexts . . ." (p. 17). Thus, it does not surprise us that Freudian theory fails as a truly scientific one. Uttal even discounts Hull's once important neobehavioristic learning theory, despite its reliance on mathematical formulations. As Uttal points out,

> Some psychologists have defined theory purely in terms of the deductive power of the involved mathematics. . . . Hull's emphasis on mathematical deduction, however, is complicated by the fact that . . . he (and all other psychologists) have had to infer the properties of internal mechanisms [i.e., neurophysiological intervening variables]. . . . The mathematics of a theory may continue to work perfectly (i.e., describe and predict) *even though the neurophysiological assumptions of the theory may be shown to be totally incorrect.* (pp. 11–12, emphasis added).

This point may be extended toward the increasing general trend in psychology to rely on mathematical modeling: the same caution exists here as in attempting to infer causation from correlation. A mathematical model may "work" and may even be predictively powerful yet *still* may not be an accurate description of the actual causal mechanisms. If the researcher's goal is to accurately describe reality, to achieve "correspondence" (presumably the goal of *all* who investigate the mind-brain problem), then mathematical models may not be the way to go.

Many of course will thus consider Uttal's take on the bulk of psychology pessimistic, although he is certainly not alone in his conclusion. Take for instance the conclusion of Faust and Ziskin (1988) that no clinician or psychiatrist should ever qualify legally as an expert, as study after study conclusively demonstrates that their reliability, predictive insight, and accuracy of judgment are no more accurate than those of laypersons and that most psychological theories of personality are nothing but "verbal summaries of loosely bound conjectures" (p. 33). Uttal contends that psychology's imprecise use of the word *theory* is too often laden with vague-to-unstated assumptions. Further, the claims made by many schools in psychology rest on such a loose interpretation of the concept that if the definition of *theory* were to be cleaned up, many "trivialities to which it is all-too-often attached by current researchers" would be exposed as sheer parascience (p. xi). Making what is both an important and timely point, Uttal states that "the word *theory* encompasses such an enormous variety of ideas that it may have lost much of its meaning for serious scientific purposes" (p. xi). This point is essential, as much of the wrongheaded thinking about science in the media (and even among scientists) stems directly from a seeming near-ubiquitous misunderstanding of what the word *theory* even means.

A sampling of definitions from several philosophers of science reveals a number of important differences among those definitions, differences that render each of them not incorrect but, rather, incomplete. While there is near universal agreement in the science community that good science depends on theory construction, Uttal points to some important limitations of theories, including that they can channel research and bias observations. It is difficult to look outside today's current theories, and woe to the new, young scientist who attempts to do so. The history of cosmology, for example, is littered with examples of the results of the neophyte bucking tradition (Singh, 2004).

Nevertheless, "theory" says Uttal, "still represents the pinnacle of scientific thinking" (p. 15). Good theories are characterized by breadth or comprehensiveness and by testability. This latter point calls to mind Platt's (1964) "The Question" for conducting good science: "But sir, what experiment could *dis*prove your hypothesis?" (p. 352). Of course, science never ends, since theories are never complete, with new knowledge always resulting in new theories or, at least, in the tweaking of existing ones. Thus, Uttal states, "[T]heories come and go as new ideas, paradigms, and observations, become available" (p. 17).

However, the primary focus of this book is not on theory in psychology in general but on the possibility of a neural theory of mind, by which is meant

> an expression of an idea in which a particular kind of objectively measurable neural activity is assumed to be related to some kind of mental or cognitive activity. . . . As one digs deeper for a meaningful answer to the question of what is a neural theory of mind, however, it becomes clear that the answer is not as simple as we would like. (pp. 46–47)

All of the theories discussed in Uttal's book, however, have one unifying feature that separates them from other types of theory: they are all patently reductionistic attempts to explain the mind by reducing it to its neuronal correlates. Of course, Uttal concludes that such an explanation is not possible, which is what will make this book especially appealing to behaviorists.

Historical Survey

In chapter 2 Uttal provides a valuable history of philosophy and science concerning the mind and brain. Given the brevity of this account, it is remarkably complete and informative. Even readers familiar with this history will glean much from this discussion and may even be surprised by two things: (a) Many of the current views of the mind-brain connection really have not changed since the time of the ancient Greeks (e.g., quoting Hippocrates, "Men ought to know that from the brain and from the brain only arise our pleasures, joys, laughter, and jests, as well as our sorrows, pains, grieves, and tears" [p. 64]); and (b) much of what we know about the brain we have actually known since the 1800s. Many seem to think that neuroimaging technology was necessary to make such discoveries and that neurological science is a new discipline. Neither of these beliefs is accurate, as Uttal has shown in his earlier book (2001).

Uttal begins the discussion with Thales of Miletus, who is credited with being the first to suggest that we should focus on the material world. Uttal opts out of discussing Socrates and Plato, for good reason—their philosophies actually hindered scientific development, being inherently rationalist as they were, focusing more on ethics and metaphysics than the explanation of the world around us. Not discussed by Uttal is that the rationalist-empiricist debate, which is usually characterized by contrasting Lockean and Kantian philosophy, was already raging in ancient Greece. As

Russell tells us, Plato was a supreme rationalist who thought empiricism was base and unsavory, being concerned with explaining worldly facts as it is. Plato thus was terrified of Democritus and actually had much of the philosopher's writing burned (Russell, 1960/1974)!

The Heart of the Book: Uttal's Critique

Uttal devotes the bulk of this book, four chapters, to a thorough review of the various theories, past and present, about the functioning of the nervous system and its possible responsibility for the mind. Included are discussions of technological achievements that made the study of the nervous system possible. This valuable survey would not be out of place as assigned reading for graduate students in psychobiology. The review is noteworthy not only for its breadth of coverage but also for its critical assessment of the shortcomings of mind-brain theorizing.

In the first of these chapters, Uttal argues that all current theories about the mind-brain problem are either vague analogous simulations (such as computer models) or explanations of lower levels of complexity that are ultimately irrelevant, given the higher level of complexity of the phenomena under investigation. Though it is certainly tempting to explain mind and the functioning of the brain with vague references to computer processes, we must not forget that historically, theories of mind and brain were usually based on some technological innovation new to the time. It was once the received view that the mind is a fluid of sorts. This analogy, based on hydraulic theory and fluid mechanics, has been supplanted by conceptions of mind based on the steam engine, the telephone, optical holography, the computer, and programming languages.

Further, research that shows a correlation between some sensory input and some neuronal response "simply does not speak to the fundamental question of how the brain makes the mind" (p. 107). To stress that some specific part of the brain "lights up" when some specific quale is experienced or when some cognitive task is executed simply does not explain *how* that sensation was generated. Claiming that such a correlate is an *explanation* of consciousness or that part of the brain "reads" the code produced by some *other* part of the brain, thereby producing a mental experience, commits the homunculus fallacy. This claim is akin to saying that a person experiences what she or he does because a littler person in the head experienced it. In other words, it simply adds another step to what we are trying to explain without actually explaining the *process* at hand.

It is in this chapter that Uttal returns to the topic of scientific theory and a discussion of approaches to theory testing. Here it seems that Uttal is building to the stance that he is a pragmatist, which he then denies by stating,

> Although there is a certain practical sense to this viewpoint (pragmatism), it must not be allowed to distract us from the ultimate purpose of science—to describe reality in as truthful and complete manner as possible, whatever the ultimate limits of science may

> be. To substitute a "useful" interpretation for a more "valid" one for some practical purpose can only lead to a diminishment of the eventual progress of any science toward the ideal of explaining ourselves and our world as fully as possible. (p. 112)

Uttal closes this chapter by reminding us that although such may be the goal of science, it may not be an entirely realizable one. He refers to Gödel's work, which demonstrates that all theories are (a) undecidable in that certain axioms contained within will always be immune to proof or disproof and (b) incomplete in that certain statements contained within can always be shown to be contradictory. Therefore, though an internally consistent and totally confirmable theory may be a goal for all scientists, it will always, in certain respects, be an unattainable goal. It should be added that this assured failure does not mean the end of science but only reminds us that confirmed theories, or "truth," for that matter, are never fully achieved, only asymptotically approached.

In the second of these critical chapters (chapter 4) Uttal gets to the point of the book. Everything that precedes it can be understood to be a primer for the discussion to come. Here he discusses several field theories, all of which focus on global patterns of brain activity. Included are the formulations of the Gestaltists, Pribram's holographic theory, John's statistical theory, Freeman's mass action theory, McFadden's CEMI (conscious electromagnetic information) field theory, Lehar's harmonic resonance theory, quantum field theories of mind, and Fourier theories. While diverse, such approaches have in common the idea that while global patterns of brain activity (be they electroneural, electrochemical, or quantum in nature) are caused by the activity and interconnection of billions of discrete neurons, it is the global pattern that is the mind itself.

This global pattern assumption is maintained for eight reasons: (a) We simply cannot scientifically study the interconnections of the neurons in the brain. Mathematics suggests that because of the numerous and nonlinear nature of the interactions at hand, this problem is ultimately an intractable one that even future computers will be unable to cope with. Thus, Uttal wryly says, field theorists do what they can rather than what they should (p. 115). (b) The mind, our own experience seems to tell us, is essentially unitary. (c) Because of b, our gut then tells us that a holistic physical substrate should exist to explain the holistic nature of phenomenal experience. (d) Such global fields are thought to provide a solution to the "binding" problem: How is the unitary nature of mind produced by multitudinous, discrete processes? (e) In psychology there is a trend toward holism in general. (f) Since Descartes it has been thought that to understand a complex system one must take it apart and analyze its component parts while holding the other parts constant. (g) Field theories superficially seem to answer the problematical question of how spatially removed areas of the brain "communicate" with each other, which would be difficult to explain on an individual neuron-to-neuron level, given the distance and finite speed at which neurons can conduct. (h) Due to the sheer numerousness of neurons it is more convenient to focus on global fields, which may be associated with cognitive activity.

Many such theories arose as a consequence of new technologies that allow us to measure the electric field of the brain, which is then assumed by the theorist, either with or without any equivocating, to be the mind itself. A classic objection to the assumption that technologies like electroencephalography (EEG) give us a "window into the mind" is to simply point out or to remind that what they in fact do is allow us to measure a particular aspect of brain *behavior*. This physical behavior (the emission of an electric field) may in fact be highly correlated with the execution of certain cognitive tasks or processes, but this correlation itself does not imply that the measured brain behavior in question is the cause of "mind" or is in any way the psychoneural equivalent of the cognitive feat. In other words, this phenomenon is yet another example of the old statistics axiom that "correlation does not imply causation."

Uttal calls this the "sign-code distinction" and tells us that

> these correlates need not necessarily be the "psychoneural equivalents" of the cognitive processes or exert any causal force on the brain or, for that matter, the mind. Rather, they could be neural epiphenomena (signs) generated by some physiological processes other than those that are the true equivalents (codes) of mind. (p. 118)

We would add that this is a cogent argument that few seem to take seriously.

Despite the fact that "[t]he details of how these global, relatively low frequency electrical fields (i.e., EEGs, ERPs [event related potential]) arise from the action of individual cells has not been definitively established" (p.114), Uttal suggests "why not look to them as the psychoneural equivalents of cognitive processes" (p. 115). In light of Kennedy's (1959) demonstration that it is possible to generate alpha waves indistinguishable from those recorded on human brains from ordinary bowls of gelatin, Uttal's suggestion is without basis. While Uttal acknowledges Kennedy's demonstration, he simply dismisses it, asserting instead that neural electrical activity is real and not an artifact. However, he continues, "The possibility of a gross misunderstanding of the meaning and significance of the EEG and other global electrical measures of brain activity, however, cannot be discounted" (p. 118). One is left with the impression that Uttal is himself not entirely convinced of much of what he presents as neurological facts.

Uttal also informs us of experiments that make it seem unlikely that the overall electrical field generated during brain activity is the cause of psychological functioning. In two such experiments, by Lashley, Chow, and Semmes (1951) and Sperry, Miner, and Myers (1955), metal foil and pins inserted into the brains of animals had no effect on their behavioral measures or perceptual responses, even though the placement of the foil and pins disrupted the electrical fields generated by the animals' brains. Pribram (1971) reported that applying aluminum hydroxide to the visual area of an animal's brain greatly disrupted the animal's EEG reading, though it had no effect on the animal's performance on a pattern discrimination task.

About the time Pribram executed this important experiment, he also advanced his own holographic field theory. Pribram's account argued that all of memory and perception are simultaneously coded in all parts of the brain, a proposition which is of course analogous to that curious feature of holographic images: the entire holographic image is represented everywhere in the hologram. Any small piece, no matter how selected, has the entire original image. It was thought that this reflected the fact that those with brain injuries often retain memory even though parts of their brain are now destroyed. Uttal again reminds us that Pribram's holographic theory, like all field theories, implicitly assumes that the measured bioelectric brain activity (or behavior) *is not just an indicator but actually is the mind itself!* Whether the field theorist in question ultimately equivocates, only alluding to the field's "influence" on the mind, or, as in McFadden's CEMI field theory, comes out and explicitly claims that the field is consciousness itself, Uttal rightly states that in either case this is a huge leap in logic.

The discussion turns to other field theories, such as John's statistical theory, Freeman's mass action theory, and Lehar's harmonic resonance theory, all of which still assume that the integrated action of many discrete neurons generates an overall spatial or temporal pattern of the field that is actually the mind. Uttal contends that each of these theories share

> a lack of any direct empirical validation with all other theories of the mind. What is even worse is that each and every such theory, based on whatever driving metaphor or analogy motivates it, will always be bedeviled by the likelihood that such validation can never be forthcoming. (p. 126)

In discussing quantum field theories of the mind, Uttal rightly points out that attempts to base speculative claims regarding psychology or the universe on the vague implications of quantum mechanics *never* produce solid scientific theory. Einstein argued that quantum mechanics only seems to imply what it does because it is incomplete, and Feynman stated that quantum mechanics doesn't imply anything about reality. Nevertheless, many gravitate toward the speculative and tantalizing musings of some quantum physicists, especially when such physicists deviate from their own domain of expertise to offer romantic speculations about the mind based on controversial interpretations of quantum mechanics.

It is easy to explain why so many find such musings tantalizing: many believe that science is cold and boring, that it comes in and takes the wonder out of things and leaves uninteresting equations in its wake. Quantum mechanics, however, seems to add even more mystery, confusion, and wonder, implying that man *is* the measure of all things. No one can tell us what to believe? Everything I believe is true for me? Mind is all that exists? All the better. Many pseudoscientists and self-help authors, such as Deepak Chopra (2004), feed on such sentiments. They are of course nothing but attitudes and value judgments that have little to do with science itself. For others, nature is complex and fascinating;

science is awe-inducing and exciting; a mystery is not an explanation, and a sense of magic has nothing to do with confusion and supernatural humbug. As Uttal points out, with quantum field theories of mind we come dangerously close to crossing the line: we are no longer simply talking about incorrect theories; we are now talking about the supernatural, about pseudoscience. As he states, "All of this comes indistinguishably close to a kind of spiritualistic antiscience" (p. 143).

> [A]lmost any farfetched hypothesis, however unverifiable or however it may run counter to normal science, garners an audience. To the degree that it provides some semblance of hope for the residual dualism that permeates human society, even the most abstract proposal becomes grist for parascientific and spiritual fantasies. (p. 148)

Chapter 5 is a discussion of single-neuron theories of the mind. Such theories were stimulated by the development of microelectrodes and, rather than focusing on global patterns or networks, imply that the action of single neurons produces mental events. The history of research in this field is rich and yields much about the functioning of the neuron. However, in the end, it has resulted in the same situation that followed EEG studies: researchers began to assume that the single-neuron responses in perception studies were *the* psychoneural equivalents of the cognitive perceptual experience (p. 162), an assumption that was extrapolated to include all cognitive functions. However, single-neuron studies suffer from an inherent methodological flaw:

> [T]he method [of stimulating single neurons] . . . is deeply flawed. . . . Such an experiment is fundamentally an uncontrolled experiment! Even with several, a hundred, or a thousand microelectrodes . . . it is not possible to account for the codes and activities of all of the other neurons that might be involved or activated, but which are not examined. (p. 193)

It is also in discussing such single-neuron research that Uttal offers a pertinent caution: so much is going on in the brain, even with the most simple of cognitive functions, that not only will a correlated response be found if sought but also so much is likely occurring in the brain unobserved that almost any hypothesis, however implausible, may likely be supported if a researcher only looks to support it. In short, what the insertion of microelectrodes into neurons can tell us is the sensitivity of that neuron to certain stimuli and conditions. Nothing from such information follows about how consciousness is produced, even though the comparison is a tempting one, since a neuron's sensitivity seems consistent with certain spatiotemporal stimulus patterns.

Thus Uttal hints at an important point concerning scientific research in general: that a certain theory seems to be supported by the evidence at hand never means that it is exclusively supported by the evidence or that its particulars can ever totally be confirmed by the evidence. For example,

what is among the most widely believed claims of single-neuron theories simply isn't true: that certain special neurons, or groups of neurons, respond to nothing but faces. Uttal informs us, however, that evidence for this claim is likely an artifact of the complexity of the stimuli chosen in such experiments. More current research has shown that these "face-specific" neurons also respond to other shapes that share some of the simpler aspects of the shapes found in faces (e.g., Jiang, Rosen, Zeffiro, VanMeter, Blanz & Riesenhuber, 2006).

Uttal directs his attention to a critique of neural-network theories of mind in chapter 6. Such theories are the most widely adhered to by contemporary cognitive psychologists. They are, however oversimplified and overly artificial. Most are based on computer simulations, even though there is no reason to assume that computer programs are analogous to neural functioning. Most also "introduce a kind of unrealistic, nonbiological, pseudo-crystalline regularity to make it possible to apply mathematical techniques" (p. 198), mathematics, as pointed out in chapter 2, being assumed (falsely) to be among the most important characteristics of good theory. Such assertions are inappropriate, as the assumed regularity fails to resemble the actual nonregularity of the brain.

While the first five chapters of this book are excellent examples of the application of informed skepticism to popular claims, in chapter 6 much of this skepticism is seemingly abandoned. Uttal calls this chapter "Network Theories—Truth Denied," because, he tells us, he considers the neural network of the brain to be the physical equivalent of mind: "The aspect of brain function that is the most plausible psychoneural equivalent of all aspects of mind . . . is the adaptive interaction within the huge lattice of neurons operating collectively" (pp. 251–252). However, these networks are so nonlinearly complex, says Uttal, that we will likely never scientifically understand them. Perhaps the major stumbling block for neural-network theories is this scaling problem. "For reasons that are both arcane and obvious, no one has ever run a computer simulation of a neural network that even begins to approximate the number of neurons involved in even the simplest cognitive processes" (p. 235).

It is in this chapter that Uttal pays appropriate homage to one the giants of 20th-century neuropsychology, D. O. Hebb, in a positive discussion of his idea of the cell assembly. Decades of research have shown his ideas to stand the test of empirical validation, and it has been, "without question, profound for all subsequent neural network theories" (p. 207). Nevertheless, Uttal argues that the brain is simply too complex to put Hebb's ideas to an adequate test. "There are just too many neurons, too many unobservable situations, and too many synaptic connections" (p. 208) to permit adequate testing of the cell assembly idea in real brains in the laboratory.

A final word about this chapter concerns the vacuousness of the idea of localized functions in the brain or, in Uttal's lighthearted terminology, "chunkology" or "bumpology." This idea was discussed at length and similarly dismissed in Uttal's earlier book (2001). The best

way to summarize the argument against localization is to quote from another neuroscientist, Eliot Valenstein: ''The impression exists that if electrodes are placed in a specific part of the brain, a particular behavior can inevitably be evoked. Those who have participated in this research know that this is definitely not the case'' (1973, p. 87). It is distressing that this outworn idea has become one of the cornerstone tenets of the highly popular, though seriously misguided discipline of evolutionary psychology (Lickliter & Honeycutt, 2003); that is, the mind, it is proposed, is composed of mental modules inherited from our Pleistocene ancestors (Pinker, 2002).

The final chapter of the book should be required reading for all graduate students in cognitive psychology. Professionals will profit from it as well. With clarity and concision it makes many important general points concerning research in all of experimental psychology. Uttal refers to his earlier discussion of theory and uses the review to address whether the many theories discussed in the book meet the criteria of good scientific theories. The short answer is, "No!" Field theories fail, as they are narrow oversimplifications. They are speculations made possible by technologies such as EEG and ERP recordings, which "are neither accurate nor consistent from trial to trial, from subject to subject, or from experiment to experiment" (p. 250).

Field theories are seductive in that they offer a spurious solution to the "binding problem" (i.e., how do various areas of the brain join together to produce mind?). We are in complete agreement with Uttal's assessment that the binding problem is a "hypothetical conundrum that is generated by the need to put back together that that had been incorrectly separated into parts" (p. 250). Single-neuron theories fail in that they are all uncontrolled experiments. It is significant that single-neuron theories, as well as all field theories and neural-network theories, are essentially untestable, the sine qua non requirement of any good scientific theory. Such theories will always be untestable as long as the black box problem persists. As Uttal reminds us, "The brain may be anatomically exposed but is still closed to examination and analysis by complexity, entropic, and chaotic considerations" (p. 261). "Entropic and chaotic considerations" here refers to two problems: systems with high amounts of information (variability) have low entropy, or predictability, and random processes, even if deterministic in nature, cannot be deterministically predicted since early, small events have later, very large effects. Given innumerably small perturbations early on and their unpredictable, though large effects later on, it becomes impossible to backtrack from effects to causes (you can't uncook an omelet).

Uttal reminds us that each of the theories discussed in this book is really nothing more than a conceptualization of the mind determined by new technologies. Historically, new technologies have supplied new analogies and metaphors that were assumed to be corollaries of mind. This book reviewed such technologies (microelectrodes, EEG, interconnecting lattices, etc.) that have provided new ways of studying the

brain and which, in turn, have been taken as demonstrating the nature of the mind itself. In each of these cases the technology used to explore the brain provided us with the theory. Uttal is correct in his warning that (and this is one of the most important statements in his book) "*concern should be raised when the methodologies rather than the evidence determine the nature of the theory"* (p. 255, emphasis added).

Also problematic is that when comparing measures of brain behavior to cognitive functions, we are relying on hypothetical constructs. "All too often in our science, we may be comparing ill defined phenomenological phantoms, accessed through fallible introspection or marginally effective stimulus control, with objective physical measurements" (p. 257). The problem encountered by the psychologist is that while most of her or his variables are hypothetical constructs, we often cannot seem to agree on how to define and measure them. "It is imperative that I once again reiterate one of the greatest impediments to understanding in this field. It is very difficult, if not impossible, to objectively define any of these terms [i.e., mind, self-awareness, consciousness, cognitive processing]" (p.246). Further, with respect to neural theories of mind, such comparisons are likely false analogies. What is being compared may seem similar, though for very different reasons. Because a computer program mimes some trivial aspect of cognitive functioning (e.g., it "learns" something) in no way implies that the computer program is "like" the brain in any structural way, shape, or form. As the underlying structure of cognition is not known, theories built on such analogies are ultimately untestable and in no way constitute causal explanations. "There is an unfortunate lack of appreciation throughout cognitive neuroscience that such analogies are based on superficial functional descriptions and not reductive explanations" (p. 258). Neural theories of mind are all correlative in nature, are based on "superficial similarities," and contain large conceptual gaps between their premises and conclusions. What is up to behavioral scientists is whether they fill in this gap with fanciful hypotheses, unwarranted bridging assumptions, and unconstrained speculations or instead insist on demonstrable evidence. Choosing the latter may lead us to reject many of the spurious claims made by cognitive psychologists that are fraught with logical errors and untestable assumptions.

Recalling the errors made by experimenters who claim to demonstrate that certain neurons recognize faces, Uttal makes an astute remark that applies to all experimental methodology in general: "[T]he posing of a question can lead to an empirical answer that is preordained by the selection of stimuli or the construction of the experimental protocol" (p. 258). Also pertinent are his remarks concerning the sheer amount of data available, in general; there is so much data out there, in fact, that findings can be marshaled to support almost any theoretical claim. Competing theories regarding the same issue commonly do not even refer to the same experiments, just as papers making rival claims frequently have little overlap in their references. This factionalism and selective sampling are not the types of synthesis that scientific theories are supposed to

embrace. The implication of this fact is that thorough and complete meta-analyses are among the essential studies in psychology, though they are relatively rare in practice. Even when executed, their findings frequently are ignored (see Grove & Meehl, 1996, for a notorious example).

Uttal concludes that "we do not yet have the barest glimmerings of how the brain makes the mind" (p. 259). Further, cognitive psychology, or mentalism, whose theories seldom meet the standards of science because they are often unfalsifiable and inadequately constrained by observable evidence, has proven to be a theoretical dead end. Uttal suggests that psychology replace such with a revitalized form of positivistic behaviorism. We wholly agree that the sentiment of positivism deserves a renaissance. Uttal ends his informative and wonderful book with a list of characteristics that he believes should frame this renewed behavioristic outlook, including the following: it must accept a compromise between both empiricists and nativists and empiricists and rationalists. Both ontogeny and phylogeny developmentally matter, and it should be recognized that behavior has stimulus causes as well as logical (inferential) causal sequences. It must include a much-needed return of operationalism, in which constructs are defined in terms of procedures rather than "unverifiable, ad hoc hypothetical mentalist constructs" (p. 262). It must be experimental, as all the best of psychology has been in the past; and, if psychology is going to maintain any claim at being a science, it must not embrace pragmatism. This point cannot be stressed too greatly. Psychology then cannot consist solely of a program enacted to meet the needs of society but must rather be an active quest for verifiable knowledge. If psychology is only the former and never the latter, then all claims that it is a science *must* be abandoned. We must never forget that theories are not to be tested for their usefulness; programs might be, but not theories of truth. A theory can, after all, be useful and, at the same time, be entirely false (especially in psychology!).

Why Psychology Is Not a Biological Science

"*Psychology . . . can be completely explained in the language and data of neurophysiology—in principle if not in fact*" (Uttal, p. 155, emphasis added).

Uttal sums up his main supposition succinctly: "The predominant conviction among most 21st-century cognitive scientists is that although it is not yet known how the brain produces mental activity, it most certainly does so as a consequence of natural physical, chemical, and informational processes" (p. 80). We will leave the question of whether mental processes, in fact, actually exist, for another forum. Suffice it to say that our opinion mirrors those of such prominent antimentalist psychologists as Kuo, Kantor, and Skinner. Rather, it is the prominent role attributed to the brain in the genesis of psychological processes that we want to address at this point.

Despite the recent successes of the highly publicized Decade of the

Brain, we are no closer to understanding the relationship of the brain to behavior as we were in 1965 when Bullock could say,

> The gulf between our present level of physiological understanding and the explanation of behavior as we see it in higher forms is wider than the gulf between atomic physics and astronomy and is indeed the widest gap between disciplines in science. (1965/1970, p. 451)

Of course, this is the major theme of *Neural Theories of Mind*. Our argument, however, is not about this lack of understanding but rather with the idea in general that the brain is *the* organ of behavior. Lest readers think we are overstating this difference, it is instructive to cite the following, from an introduction to physiological psychology:

> If your brain were removed and another put into your skull, who would you be? Your friends would recognize your face but you would not recognize your friends or know their names. You would not know where you lived or who your parents were. You would not joke like or think like or dream like the original you. You would be a different you since the original you was stored in the brain that was removed. With a new brain you would have a different mind and personality even though your original body remained. (Plotkin & Mollenauer, 1978, p. 10)

Pretty fanciful stuff, even for 1978! Since the days of Dewey (1896) some have appreciated the now often-overlooked fact that it is the whole organism and not any one of its parts that is responsible for the aspects of behavior referred to in that imaginative statement. Indeed, current understanding allows us even to stretch Bentley's (1954) idea of the skin as philosophy's last line of defense and argue that it is the organism and its context that makes a behavioral event (Lerner, 1991). It is even possible to argue that "an organism is not a thing, but a place where certain highly interesting processes are taking place" (Hardin, 1956, p. 112). We do not deny the importance of the brain and have, in fact, made a case that brain evolution plays a crucial role in behavioral complexity (Greenberg, Partridge, Weiss, and Haraway, 1999). But there is more, much more, to behavior than the brain.

Uttal, however, identifies the mind-brain myth as one of his fundamental assumptions: "[E]verything involved in our mental lives is the result of the activity of our nervous systems" (p. 99), and

> There is no basis for any remaining questioning concerning the locus of the mind. It is only the brain to which we must turn for the answers to the "world knot." Any allusions to the idea that the mind is distributed to other parts of the body or to the external environment are vestiges of the supernatural that are no longer tenable. (pp. 96–97)

It was the Nobel laureate Sir Charles Sherrington, acknowledged

by Uttal in his history chapter for contributions to brain science, who defined the healthy person not by the status of his brain but as a "set of organs of interlocking action regulating each other, the whole making a self-regulating system" (Sherrington, 1951, p. 163). These organ systems are, of course, interdependent, the functioning of one depending on the functioning of all the others. To put the burden of behavior on only one of the ten or so organ systems that comprise a human being seems to us to be simply wrong. That this is not some wild-eyed idea is reflected in the conclusion of Rogoff (1992) that "the organs in an organism work together with an inherent interdependence . . . [and any feature] . . . would lose all meaning if it were actually separated from the whole" (p. 317). This idea is embodied in the important principle of behavioral gradients proposed by Kuo (1967): "In any given response of the animal to its environment—internal or external . . . the whole organism is involved" (p. 92). Kuo further stipulated that the degree of involvement of the animal's component parts varies with the particular behavior. Thus, in behavioral (and "mind") events, the brain functions interdependently with the rest of the organism's parts.

Psychology has amassed a great many facts in debates between cognitive nativists and behaviorist environmentalists since the days of Wundt. It became clear in the latter half of the 20th century that these debates were intractable because, in part, proponents of these various schools of thought were examining their own small parts of the same integrated biopsychosocial system. The central task of scientific psychology in the 21st century should be to articulate a set of principles that can fruitfully organize the set of factors ranging from genetic to neural, to intrapersonal, to micro- and macroecological. Our take on the evidence at hand is that development is that organizing principle, specifically the metatheoretical perspective of developmental systems. In sharp contrast to our colleagues, made weak by finding and fact, yet failing to yield and clinging to the search for an organizing "executive agent," be it the brain or the gene, the developmental systems perspective builds on the empirical properties of dynamic complex adaptive systems (Ford & Lerner, 1992; Oyama, Griffiths, & Gray, 2001). These systems have been shown across a wide array of mathematical abstractions as well as biological, behavioral, and sociological instantiations to display the patterns of coherence and variability, plasticity and canalization, and complex integration so pervasive in psychology. Further, the organization of these systems is not reliant on an organizing agent but rather is inherent in the dynamical transactions of the genetic and epigenetic aspects of the biopsychosocial ecology in which behavior is embedded. We entered the 21st century on the heels of two expensive and popular scientific efforts, The Decade of the Brain and the Human Genome Project. Both purported to put to rest the search for the origins of behavior. The former endeavor sought to put the entire burden of behavioral origins on the brain, the latter on the human genome. Both, of course, came down on the nature side of the nature-nurture equation; both failed to "take development seriously" (Robert, 2004). Psychology,

we believe, would benefit greatly by rejecting the biological, deterministic outlook that accompanies taking the nature side of the nature-nurture false dichotomy. Even clinicians would benefit by rejecting the medical model they have now fallaciously clung to for decades.

In his review of a book by Daniel Dennett on the natural history of religion, Wieseltier (2006) made this telling comment: "Like many biological reductionists, Dennett is sure that he is not a biological reductionist" (p. 12). While we admire much of the writing of Uttal, we are afraid that in the end, this comment applies to him as well, and it is especially poignant given his insistence that he is at bottom a behaviorist. To place an undue burden on the brain, as Uttal does in this otherwise fine book, is to be guilty of the same sort of reductionism Uttal has extensively written about and argued against.

Concluding Comment

We end our review in full agreement with Uttal on a fundamental point:

> Debates between behaviorists and mentalists, between reductionists and nonreductionists, between empiricists and rationalists, and between elementalists and holists (among others) rage on. None of these great controversies, however, reduce the validity of the vast amount of factual knowledge that cognitive neuroscience has gathered. (p. 170)

This sentiment is what contributes to the value of this fine book. We do not suggest that brain research should stop; nor do we profess to have the final word on the issues raised in this review. Part of the beauty of science is that it seems always to be at the beginning.

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