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IN RESPONSE: *PSYCHOLOGY IS A BEHAVIORAL SCIENCE, NOT A BIOLOGICAL SCIENCE,* BY GARY GREENBERG AND CHARLES LAMBDIN—CORRECT CONCLUSION, UNSOUND ARGUMENTS

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Greenberg and Lambdin's review (in the summer 2007 issue) does an excellent job of summarizing the contents of Uttal's book *Neural Theories of Mind: Why the Mind-Brain Problem May Never Be Solved* (hereafter NTM). Furthermore, these authors make several insightful comments about the issues raised in NTM. I disagree, however, with two aspects of Greenberg and Lambdin's review: one a matter of opinion, the other of fact. First, I am surprised at the authors' generally positive assessment of NTM, because they (and I) disagree with Uttal's fundamental notion that minds exist and that the brain is the source of behavior. Second, I take issue with some of Greenberg and Lambdin's interpretations of neuroscience research, which they use, rightly, to argue that psychology is not a biological science.

Greenberg and Lambdin offer three reasons why they think NTM is worthwhile reading for behaviorists: it is a thorough treatment of the concept of theory in science, it contains an analysis of the historical and contemporary treatments of the mind-brain connection, and finally, Uttal's orientation is not reductionistic. The first two points refer to the pedagogical value of NTM. I do not agree completely that NTM is a good teaching aid—for example, the chapter on theory does not cite Skinner (1950)—but the literature review is the strongest aspect of NTM. The third reason is most important because it relates to the core thesis of NTM. According to Greenberg and Lambdin, NTM is "especially appealing to behaviorists" (p. 461) because Uttal concludes that reductionistic attempts to explain the mind must inevitably fail. Some of this appeal is probably lost, however, when Greenberg and Lambdin correctly recognize that Uttal calls himself a behaviorist although he retains the notion of minds that exist in brains. Indeed, the authors are to be commended for highlighting Uttal's eclectic approach to behavioral psychology; I wish all reviews of Uttal's work would make this point. In essence, Greenberg and Lambdin argue that Uttal's fundamental assumptions are wrong but that NTM has some pedagogical value. Thus, to describe NTM as an "otherwise fine book" (p. 473) is akin to a surgeon describing an operation as a success

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The author thanks Alan Power for creating Figure 1.

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even though the patient died. It is precisely *because* Greenberg and Lambdin have correctly identified Uttal's muddled approach that I find it hard to understand their praise for NTM.

My second point of departure with Greenberg and Lambdin's review concerns the interpretation of neuroscience research. The New Phrenology, by Uttal (2001), contained basic factual errors in discussions of brain structure and electrophysiological measures (Hubbard, 2003), and therefore reviewers of NTM should be particularly vigilant for such inaccuracies. For example, Uttal (2005) questions the utility and reliability of electroencephalograms (EEGs) on page 150 of NTM. Greenberg and Lambdin quote Uttal's comment that EEG and event-related potential (ERP) recordings "are neither accurate nor consistent from trial to trial, from subject to subject, or from experiment to experiment" (Uttal, 2005, p. 250). This is simply not the case. Although it is true that an ERP may not be obvious on every trial, due to noise, ERPs are generally similar across subjects and experiments. Figure 1 displays the averaged auditory evoked potential (AEP) of the current author: it is certainly consistent with the typical AEP.

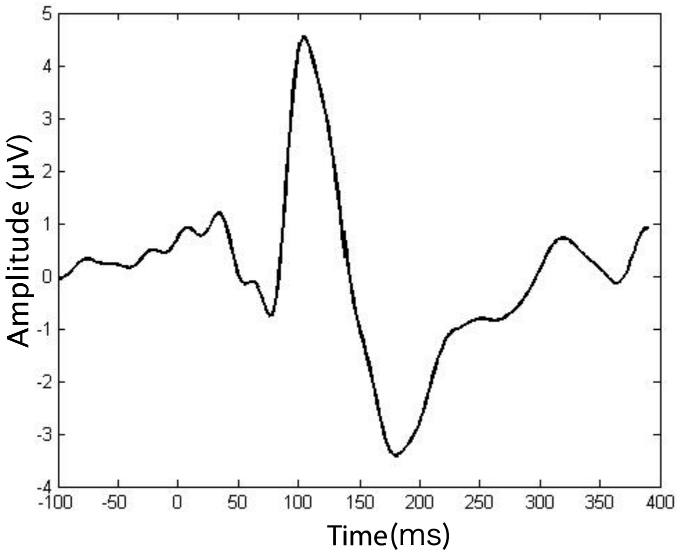

Figure 1. The author's auditory evoked potential (AEP). Data are shown for a single electrode located over the left parietal lobe, referenced to a frontal midline electrode (P7 and FPz, respectively, according to the International 10–20 System).

Rather than reprimanding Uttal for his inaccuracy, Greenberg and Lambdin augment the unwarranted skepticism by selectively quoting Uttal: "'[t]he details of how . . . EEGs, ERPs . . . arise from the action of individual cells has not been definitively established' ([Uttal, 2005] p. 114)" (Greenberg & Lambdin, p. 464). Even Uttal qualified this statement in his next sentence (which the authors do not quote): "However, it is thought that they are most likely the accumulation of . . . potentials from ordered pyramidal cells" (p. 114). It should be acknowledged that the origin of the EEG is quite well understood and that several *thousand* published studies on ERPs and EEGs show reliable and replicated effects.

It is likely that most psychologists would agree with the position that there is more to behavior than the brain, but I found Greenberg and Lambdin's arguments unconvincing. Lorber's study of individuals with apparently almost no brain is contentious. Lorber, a pediatrician, never published these data in a peer-reviewed journal, and most of the discussion about this study has been by Lorber himself in the mass media (primarily in a British TV documentary). It is not true that "[t]he 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" (Greenberg & Lambdin, p. 458). Lewin (1980) cites some nonhuman models of hydrocephaly, in which brain functioning was preserved after deliberate brain insult. It is now thought likely that the normally functioning individuals studied by Lorber had an approximately normal number of brain cells, although confined in a smaller space. These patients had late-onset hydrocephalus, and thus the cranium could not expand because it had already calcified. Also, the hydrocephalus developed at a rate slower than usual, giving the brain time to adapt (Beyerstein, 1999). The interested reader is encouraged to examine Lewin and Beyerstein for alternative views of Lorber's reports.

Kennedy (1959) is an interesting but thoroughly discredited piece of research, which Greenberg and Lambdin cite unflinchingly in support of their argument. Kennedy argued that the EEG alpha rhythm is not a result of cellular activity but rather is an artifact due to the gross physical properties of materials from which the recordings are made (i.e., brain and skull). His experiment consisted of sending mechanical pulses through gelatin, which he assumed was an accurate replica of the brain. This work has been cited only five times in almost half a century: three of these were cogent refutations of Kennedy's study (Miller, 1968; Oswald, 1961; Rosner, 1961). The overly skeptical attitude in some parts of Greenberg and Lambdin's review is perhaps best reflected in the following statement: "[d]espite 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" (pp. 470–471). How many of today's neuroscientists would agree that *no* progress has been made since the midsixties in terms of understanding the brain-behavior relationship?

In conclusion, Greenberg and Lambdin have produced a thorough review of NTM, admirably capturing the spirit and content of the book. They, almost uniquely among reviewers, have noted that Uttal's behaviorism is at odds with mainstream behavioral psychology. I disagree that NTM is worthwhile reading for behaviorists (or psychologists in general), because Uttal's basic assumption—that the mind exists and that psychology can be reduced to neurophysiology—is flawed. This is, however, merely a matter of opinion. I disagree more strongly with Greenberg and Lambdin's uncritical citations of obscure and dated neuroscience research. This flaw is unfortunate, because Greenberg and Lambdin are correct on the substantive issue: psychology is a behavioral science, not a biological one.

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