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A Reply to Uttal (2004)

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In his article, Uttal (2004) lays forth several, rightly justified, caveats in the pursuit of elucidating the neural basis of higher cognitive functions using functional magnetic resonance imaging (fMRI). Adding to the onslaught of criticism from cellular physiologists, Uttal's central tenet is that the increased utilization of this new technology may be "ill advised," and should be replaced with more effort and time being directed towards a "revitalized behaviorism." Although we agree with many of Uttal's views, we contend that it is not the methodology of fMRI itself but the *application* of fMRI to unravel more intangible cognitive phenomena that is ill advised. Specifically, we believe that Uttal has mistakenly disregarded the potential role that fMRI research could make in the advancement of behavioral science.

Long before the advent of fMRI, Skinner (1974) clearly appreciated the role that neuroscience could play in completing the behavioral account:

> The physiologist of the future will tell us all that can be known about what is happening inside the behaving organism. His account will be an important advance over a behavior analysis, because the latter is necessarily "historical"—that is to say, it is confined to functional relations showing temporal gaps. Something is done today which affects the behavior of the organism tomorrow. No matter how clearly that fact can be established, a step is missing, and we must wait for the physiologist to supply it. He will be able to show how an organism is changed when exposed to contingencies of reinforcement and why the changed organism then behaves in a different way, possibly at a much later date. What he discovers cannot invalidate the laws of a science of behavior, but it will make the picture of human action more nearly complete. (pp. 236–237)

To take an example, consider the application of fMRI to the analysis of performance on the Stroop color–word task (Stroop, 1935). In this task, individuals are shown a series of color words (e.g., *red, green, brown*) printed in colors incongruent to the word and are asked to name the color of each word as quickly as they can. A behaviorist would argue that people experience difficulty on this task due to competing stimulus-control topographies. In the presence of a red stimulus, when asked "what color is this?" in the past, saying "red" has usually resulted in a reinforcing consequence. Similarly, in the presence of the written word *blue*, when asked to read the word in the past, saying "blue" also usually resulted in a reinforcing consequence. The colors and words therefore control two different response classes. If the word *blue* is subsequently written in the color red, the two properties of the stimulus compete for the respective responses, thus resulting in longer response latencies. The behavioral account is therefore an historical one. Cognitive psychologists would argue that the effect is due to cognitive interference. Several fMRI studies have shown that the anterior cingulate (in addition to other brain areas) is involved when individuals perform the Stroop task (Adleman et al., 2002; Kerns et al., 2004). Does the activation of the anterior cingulate therefore tell us why the "Stroop effect" occurs? Does it help to explain the cognitive interference? Surely, the Stroop effect can be explained by the individual's reinforcement history. Nevertheless, identifying the neural systems that underlie specific behaviors may be the first step in completing the account.

It must be noted that Uttal appreciates the advances that have occurred in

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biomedical imaging over the last decade, pointing to the potential involvement of fMRI in diagnosis and treatment. However, and rightly so, Uttal points to the many "underdiscussed" conceptual and technical problems that plague fMRI. For example, there is considerable debate concerning whether the use of blood flow (or blood-oxygenation-level-dependent signal) is the appropriate measure of neuronal activity (see Raichle, 1998). Thus, looking at activation areas in the brain is analogous to, as Nichols and Newsome (1999) elegantly put it,

> looking out the window of an airplane at night. We see patches of light from cities and towns scattered across the landscape, we know that roads, railways and telephone wires connect those cities, but we gain little sense of the social, political and economic interactions within and between cities that define a functioning society. (p. 35)

Despite this problem, and in a defense of fMRI methodology, Donaldson (2004) suggests that new improvements in task design, a shift away from blocked and event-related designs toward mixed designs, will allow researchers "to 'parse,' rather than simply 'map' the brain" (p. 442). In blocked designs, stimuli are presented in alternating "on" and "off" blocks over the duration of the task, whereas in event-related designs, stimuli are presented randomly throughout the task. (Of course, behaviorists have been using these ABAB and multielement designs for decades to investigate the effect of environmental stimuli on subsequent behavior.) Blocked designs allow researchers to determine whether a brain region exhibits sustained activity, whereas an event-related design can allow transient activity to be investigated. A mixed design, a combination of the blocked and event-related designs, is advantageous, Donaldson says, because brain regions can be dissociated according to whether they exhibit either sustained or transient activity. Indeed, Donaldson suggests that these mixed designs will also allow neuroscientists to go beyond simply asking the "where" question (i.e., where in the brain the activity occurs) to allow them to ask questions about what the activity reflects. In rounding off his defense of fMRI, Donaldson (2004) says, "the interpretation of mixed design studies still depends on theoretical accounts of cognition. However, to be adequate, theoretical accounts of cognition must take into account the functional information provided by neuroimaging data" (p. 444).

It appears then that neuroscientists are waiting for advances in theoretical accounts of cognition, and cognitive psychologists are waiting for enhancements in fMRI to help them guide their theory construction. Do we smell a rat here? The point that Uttal raises about the localization of high-level cognitive processes is therefore still as epistemologically valid as it was when Skinner (1987) said,

> Do we know how anxiety changes intention, how memories alter decisions, how intelligence changes emotion, and so on? . . . Questions of this sort should never have been asked. Psychology has much to gain by confining itself to its accessible subject matter and leaving the rest of the story to physiology. (p. 7)

Uttal's remark concerning the conceptual issues that underpin subtraction methods in fMRI, along with the "arbitrariness of statistics and thresholds" (p. 4) are good points that present formidable challenges to the field. Indeed, subtraction is based on the principle of *pure insertion,* a principle that contends that neural activation observed during the task of interest can be directly isolated once the activation in the control condition is removed. Despite the clear problems with this assumption, Uttal should have acknowledged the many well-designed studies that overcome this problem by utilizing conjunction, parametric, and correlational approaches to elucidate the phenomena of interest (Frackowiak et al., 2003). Therefore, despite our general agreement with Uttal, it must also be argued that fMRI is far from an expansion of phrenology, attaining remarkable consistency with clinicopathological, homologous primate, and psychophysiological studies. The efficacy of fMRI as a tool to understand the dynamic brain should be taken seriously.

Uttal's assertion that neuroscientists lack any appreciation for the complexities of the human brain is clearly an overstatement. Few, if any, neuroscientists would disagree with the fact that the human brain is one of the most complex and enigmatic structures known to man. Uttal could have mentioned the work currently being undertaken to understand the distributed networks and multimodal regions in the brain, including, for example, the methodological breakthroughs in functional and effective connectivity (e.g., the application of correlational or structural equation modeling methods to elucidate distributed neural systems that underlie various neurobiological operations; see Friston, 2002, for further discussion).

Furthermore, Uttal's questioning of data reliability has come to the attention of the neuroscience community and prompted the instigation of large databases such as the fMRI data center (http://www.fmridc.org/f/fmridc) where researchers can freely reanalyze data. Uttal's use of Cabeza and Nyberg's (2000) review (which is not a meta-analysis) of the fMRI and positron emission tomography literature from 1995 to 1998 is out of date. Advances in task design (e.g., stochastic, mixed, and parametric designs; see Frackowiak et al., 2003), understanding of resting baseline activation (Raichle et al., 2001), deactivation, and hemodynamic time course have partially cleared up many of the issues regarding data analysis that Uttal raises.

So, with all these methodological advances, should fMRI be used to elucidate the neural basis of classic learning concepts such as stimulus control, reinforcement schedules, stimulus equivalence, extinction, punishment, self-control, impulsivity, and behavioral momentum? A handful of researchers have already begun to use such paradigms with success (e.g., Dickins et al., 2001). We hope that in the future, behaviorists will conduct these studies, using their talents in behavioral research and paradigm construction to complete the behavioral account. Thus, we believe that behaviorists should be embracing fMRI rather than running away from it.

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