

COMMENTARY

Mechanism Through Methodology: No Madness to the Method

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Grau and Joynes (2005) assess the current state of the field of animal learning and behavior, with particular emphasis on pedagogical and curricular issues. They suggest that the conventional framework which organizes lecture material around methodology is flawed and that an organization around mechanism should be used instead. They also advocate a shift from a purely behavioral approach to research on learning and behavior to a neural-functionalist approach more akin to contemporary behavioral neuroscience. While I support many of the suggestions for improving instruction, I disagree with their proposed shift away from purely behavioral investigations of animal behavior. Behavioral research continues to be a thriving and productive source of empirical and theoretical discoveries. The diverse array of specialized methodologies that have been developed to pursue this work are still paying dividends by illuminating the nature of behavioral mechanisms. Banishing purely behavioral approaches to learning and behavior, such as those used to study associative learning, animal cognition, and comparative psychology, would severely hamper our knowledge of behavioral mechanism.

“A complete theory of learning must speak to all of the ways in which experience can alter behavior...” (Grau & Joynes, 2005, p. 15). This statement resonates the views espoused by most contemporary researchers of learning and behavior. Grau and his students have published excellent work on the neural basis of learning in spinal rats. This research is exemplary in its use of rigorous methodology and the nuanced appreciation of the theoretical issues it raises. Thus, they are in an excellent position to comment on the state of the field of learning.

Grau and Joynes (2005) address a number of important issues that are germane to the study of learning and behavior. Most importantly, they provide a timely critique of the current state of pedagogy and curriculum. They delineate inherent problems with the manner in which theory and research are portrayed in standard textbooks for academic courses on learning. The conventional view, as Grau and Joynes see it, adopted by textbooks organizes information around the methodologies used to understand learning rather than around the mechanisms of learning. One problem with a methodological approach is that it fails to provide a coherent theme or framework in which to connect all of the disparate findings and theories. Thus, issues relevant to both Pavlovian and instrumental learning, for example, are presented in separate chapters with little integration into a larger, coherent framework. This disconnection between facts and framework probably contributes significantly to the tendency for students to perceive courses on learning as difficult or uninteresting. The conventional framework produces another negative consequence: neuroscientists who wish to study the neural mechanisms of learning often receive an antiquated (at best) or misguided (at worst) understanding of

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learning. Imagine the deficiency in knowledge of a medical student who is taught gross anatomy and physiology, but not molecular biology or genetics. As a result, with a few notable exceptions, many neuroscientists are unaware of the progress that has been made over the past few decades in our understanding of the processes and mechanisms of learning.

Another issue that Grau and Joynes (2005) attack is the problem created by defining learning too narrowly as associative. Such a definition relegates nonassociative processes as unimportant to the study of learning. As a result, attempts to control for many alternative (to associative) causes for behavioral effects have led to the ignorance or even vilification of important processes and mechanisms of learning. For example, nonassociative processes such as habituation, sensitization, and alpha responding are typically deemed unimportant or are perceived to interfere with an investigation of mechanisms of “true” (i.e., associative) learning. However, as Grau and Joynes elegantly demonstrate in spinal rats, these nonassociative processes may play an important functional role in the plasticity of behavior. They, along with many contemporary psychologists, view each learning phenomenon studied in the laboratory as part of a behavioral system. A behavioral system consists of a set of dynamically interrelated behaviors that have been adapted to solve important biological problems. Thus rather than trying to rule out nonassociative factors as annoying experimental artifacts, they assert that nonassociative processes “can be seen as an example of biological ingenuity rather than an experimental anomaly” (p. 15). The behavioral systems approach is a continuation, or rediscovery, of comparative psychology (Papini, 2002). Others in the field of experimental animal psychology have adopted similar views. For example, my own research has emphasized the learning-performance distinction, and has been motivated by the view that performance variables are themselves important mechanisms that warrant study if we are to fully understand learning phenomena (Blaisdell, 2003; Denniston, Savastano, & Miller, 2001).

Although I concur with many of the points raised by Grau and Joynes (2005), I am concerned that they go too far in disparaging the methodological approach to learning and behavior. Methodologies have been developed to study behavioral processes with the goal of discovering mechanisms at both the psychological and biological levels of analysis. While it is true that methodologies are tools and not end products, many methodologies are needed to properly analyze and dissect the behavioral phenomena to extract converging evidence for mechanism. For example, fear and appetitive conditioning procedures each hold advantages and disadvantages for studying various aspects of Pavlovian processes, such as interval timing. On the one hand, footshock is a very precisely controllable US, allowing virtually complete control over how the subject experiences the CS-US interval. On the other hand, procedures using food reward have allowed for precise estimations of temporal expectancies using psychophysical techniques. The combination of both procedures has enhanced our understanding of processes of interval timing beyond the limits of each individual procedure. Similar arguments can be made for the use of multiple methodologies to understand many other behavioral phenomena, such as stimulus competition (also known as retrospective revaluation), mechanisms of short-term and long-term retention, the spatial and contextual control of behavior, and configural learning.

Grau and Joynes (2005) advocate a neural-functionalist approach in which more traditional studies of learning are replaced with systems-level behavioral neuroscience. However, the removal of purely behavioral studies from psychological research on learning, motivation, and cognition would severely restrict what we could understand about behavioral mechanisms. A cursory flip through any current issue of the *Journal of Experimental Psychology: Animal Behavior Processes*, *Learning & Behavior*, *Learning and Motivation*, *Quarterly Journal of Experimental Psychology, B*, *Journal for the Experimental Analysis of Behavior*, *Behavioral Processes*, *International Journal of Comparative Psychology*, or *Animal Cognition*, reveals a rich and active study of psychology at the behavioral level. For example, we are constantly advancing our understanding of Pavlovian processes through the investigation of such diverse topics as renewal of fear conditioning (Bouton & Bolles, 1979), conditioned flavor and odor aversions and preferences (Batson & Batsell, 2000; Lubow & De la Casa, 2002), appetitive conditioning (Rescorla, 2002), autoshaping (Killeen, 2003), human contingency and causal judgments (Dickinson & Burke, 1996; Matute & Miller, 1998; Van Hamme & Wasserman, 1994), and spatial and temporal relations (Blaisdell & Cook, 2005; Savastano & Miller, 1998). Although grouped by common or similar methodologies, all of these investigations tell us something about the nature of learning and performance mechanisms involved in learning about stimulus-stimulus relations. This may lead some, such as Grau and Joynes to adopt the view that “Pavlovian conditioning refers to a class of methods designed to investigate how organisms encode stimulus relationships within the environment” (p. 9). Perhaps it is more accurate to say that Pavlovian conditioning refers to a class of behavioral phenomena and the investigation of the mechanisms that support it. A set of methodologies have been developed for this purpose, and new methodologies are continually being developed. However, the focus is on the behavioral phenomena and the underlying mechanisms, at various levels of analysis. There is already a rich and thriving field of behavioral neuroscience that is very close to the neural-functionalist framework proposed by Grau and Joynes. To do away with purely behavioral approaches to learning and behavior and related disciplines, such as animal cognition and comparative psychology, endorses an exclusive rather than inclusive philosophy.

What ramifications does an inclusive approach to the study of learning mechanisms have for Grau and Joynes’ (2005) work on spinal rats? In their preparation, the connection between the spinal cord and the brain in the rat is cut. To study Pavlovian conditioning, the spinal rat is given pairings between a mild electrical stimulus (CS+) applied to one leg followed by a strong shock to the tail, interspersed with a mild electrical CS- to the other leg unpaired with strong tail shock (Grau, Salinas, Illich, & Meagher, 1990). This training establishes stronger responding to the CS+ than to the CS-. However, a novel stimulus (CS neutral) that has not undergone any conditioning elicits the same magnitude response as does the CS+ (Joynes & Grau, 1996). This suggests that spinal cord neurons do not support Pavlovian conditioning, but rather, that the response to the CS+ is due to protection from habituation. That is, the presence of the US during training prevents the *unconditioned* response to the CS+ from habituating, rather than establishing a *conditioned* response to the CS+. The empirical effect is well documented and noteworthy. Learning (habituation) occurs in the control (unpaired) group, but not in the experimental group. Grau and Joynes conclude that “Such comparisons

highlight the importance of appropriate controls (e.g., the inclusion of a novel CS) and how a mechanistically-based view of learning must remain wedded to rigorous methodology” (p. 11-12). This is a valid point. But perhaps they are a bit premature in their final assessment of spinal mechanisms of learning. They only test the CS+ relative to a novel CS within an hour after the training episode has ended. Therefore, we cannot determine from these data whether there are any long-term effects of the CS-US experience. More controls and tests are needed. For example, what would be the outcome of tests of the CS+ and the novel CS given 24 h (or longer) after the end of training? Perhaps the CS+ would exhibit a stronger response than the novel CS. That is, the rate of habituation to the CS+ might be slower than the rate of habituation to a novel CS. If so, then this would be evidence for the acquisition of a CS-US association (i.e., Pavlovian conditioning) that is stored in the spinal cord. Whatever the eventual resolution, it is important to realize that support of *either* theoretical interpretation should be of interest to students of learning. Instead of denigrating a protection-from-habituation mechanism for masquerading as Pavlovian conditioning, we should embrace it as an interesting neural solution to an important behavioral problem. As such, learning in the spinal cord, does not provide “a good example of the breakdown between methodology and mechanism” (p. 7). It serves as a good example of how methodology (along with carefully designed control manipulations) can illuminate mechanism.

A final point of contention I wish to address is with the portrayal of the Learning-and-Behavior traditionalist as a straw man. While the prevailing view used to be that associative learning was relegated as the only *true* type of learning, it is very difficult to find anyone today who holds tenaciously to this opinion. According to Grau and Joynes (2005), the traditionalist would discount the work of Kandel and his associates (Kandel & Schwartz, 1982) because learning in *Aplysia* is arguably a form of alpha conditioning. I agree that it is interesting to study the neural mechanisms of nonassociative plasticity in *Aplysia*. There are, however, invertebrate systems in which *true* Pavlovian conditioning is demonstrable. For example, repeated pairings between a light CS, which normally elicits approach in the mollusk *Hermisenda*, and a rotation US, which normally reduces the rate of locomotion, results in a conditioned response of reduced locomotion to the light CS (Rogers, Talk, & Matzel, 1994). The CR elicited by the CS is opposite to the UR prior to training, and therefore, cannot be attributed to alpha responding. Perhaps the gill-withdrawal reflex in *Aplysia* was a poor choice of behavior system in which to investigate the neurobiology of associative learning (though this does not devalue its utility for the study of other forms of behavioral plasticity). But this does not mean that this type of learning cannot be studied in simple systems. Both types of learning are important and are worthy of study.

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