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## COMMENTARY

### Experimental Methods and Conceptual Confusion

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According to Grau and Joynes (2005), (1) the current classification of types of learning is based on methodology and assumes a correspondence between types of learning and distinct neural-functional mechanisms; (2) this assumption is wrong because experiments show that different mechanisms may underlie the same type of learning; consequently, (3) we should change the teaching of the psychology of learning. I argue that because Grau and Joynes misunderstood the nature of the classification of learning phenomena and cloaked their research findings with a garb of conceptual errors and infelicities, their recommendations concerning the teaching of learning should be rejected.

Grau and Joynes' (2005) overall argument in "A neural-functionalist approach to learning" may be divided into four parts. First, the current classification of types or kinds of learning (single stimulus learning, Pavlovian conditioning, and instrumental conditioning) is based on methodology. Second, this classification assumes a correspondence between each kind of learning defined methodologically and a distinct kind of neural-functional mechanism. Third, experiments show that the assumption of correspondence is incorrect because a variety of different mechanisms may underlie the same kind of learning when the latter is defined methodologically. Finally, given the empirical inadequacy of a classification based on methodology, as well as some of its additional negative consequences (e.g., a too restrictive view of learning), it should be replaced by a classification based on mechanisms. In what follows, I will argue that Grau and Joynes (1) misunderstood the nature of the classification of learning phenomena; (2) misconstrued the global significance of their own empirical findings; (3) mistook psychologists of learning for (some) neuroscientists when attributing the correspondence assumption; and, more generally, (4) cloaked their *important* empirical contributions with a garb of conceptual errors and infelicities that their recommendations concerning the teaching of learning should simply be rejected.

#### On the Classification of Learning Phenomena

According to Grau and Joynes, "researchers within the field of learning have traditionally divided their empirical world according to methodology, with phenomena classified as single stimulus learning, Pavlovian conditioning, or instrumental learning" (p. 1, Abstract). Thus, the "phenomena were classified together, not because they share a common underlying mechanism (the machinery that underlies learning), but rather because a similar methodology was used to infer

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their presence” (p. 2). Readers may search in Grau and Joynes’ paper for a clear statement of what “methodology” means in this particular context, for the sense in which habituation/sensitization, Pavlovian and operant conditioning are *methodological* categories; they may search, but they will not find it.

Given this glaring omission, I can only conjecture: Perhaps the authors mean a classification based on the procedures implemented in the laboratory, on the kinds of events manipulated explicitly by investigators and often described in the Procedure section of experimental articles. But if this is the case, then Grau and Joynes have misunderstood the nature of the classification. For whether an instance of learning examined in the laboratory is classified as, for example, habituation does not depend exclusively on what the experimenter does (call it S), but also on how the animal reacts to it (call it R) and on the nature of the dynamic relations holding between the two (call it S-R). A rat startles when hearing a brief, loud noise. When the same noise is repeated, say, every 15 s, the startle response typically wanes and may even cease (R). If the response decrement is due to the repetition of the stimulus (S-R) then psychologists will refer to this instance of learning as habituation. All three elements—S, R, and S-R—are important to classify the phenomena and two of them, R and S-R, are not at the experimenter’s disposal; they tell us something about the animal.

Consider a second example. For a couple of hours, a male juvenile cowbird sings hundreds of songs in front a female but is consistently ignored by her. Then during one of his songs the female moves her right wing, a display called wing stroking. Subsequently, the male sings that song more often than before and that song becomes more effective at eliciting copulation postures from females than other songs (e.g., West & King, 1988). A psychologist of learning would interpret the male cowbird’s behavior as follows: Its response of singing a particular song (R) was followed by a particular female reaction (S), and for that reason it occurred more often in the future. If this interpretation is correct, then the change in the male’s singing would be classified as an instance of operant conditioning. Although the interpretation could be wrong—perhaps the male cowbird sang the song more often because the female’s wing stroking simply elicited it—the point of the example is to show that more than methodology is involved in the classification. The change in the male’s singing behavior and the causative factors responsible for it are critical to the classification.

In the same vein, one could say that operant conditioning is not about giving M&Ms to kids, presumably the methodological criterion in Grau and Joynes’ terminology, but about giving them as a consequence of some behavior (the procedure) and observing changes in that behavior (the process). Operant conditioning is a form of behavioral change defined by both procedure and process. More generally, then, operant and Pavlovian conditioning as well as habituation and sensitization describe broad kinds of interactions between an animal and its circumstances. As such they are necessarily defined by the two endpoints of the interaction (abstractly represented by S and R) and by the properties of their relation (S-R). As Keller and Schoenfeld (1950/1995) remarked, “we need a dynamic, rather than a static picture of the behavior of organisms. To describe process, not to inventory elements, is [and has been, I add] our major concern” (p. 7).

### On Grau and Joynes' Neural-Functional Mechanisms

According to Grau and Joynes, the current classification of learning phenomena is associated with the “simplistic” (p. 3) assumption of an isomorphism between methodology and mechanism. “One-to-one correspondence” would be a more accurate term than isomorphism, for the assumption is that distinct neural mechanisms underlie distinct methodologically-defined kinds of learning. Be that as it may be, according to Grau and Joynes the isomorphism assumption is contrary to fact: Animals can learn about conditional and unconditional stimulus relations or about responses and their outcomes in multiple ways that engage different neural-functional mechanisms. As a consequence, the authors propose a classification of types of learning based on the variety of underlying mechanisms. This new arrangement of the field of learning provides “a better framework for linking behavior to neuroscience and cognition” (p. 4), and it also promotes a richer, less restrictive conception of learning than the old one.

Although Grau and Joynes speak repeatedly about neural mechanisms, the research they summarize does not deal with them directly. That is, the authors do not identify the specific neural structures involved during learning nor describe their interactions. Instead, they define the putative mechanisms of learning functionally; hence the name they give to their approach, neurofunctionalism. To illustrate, having established conditioned antinociception in a rat's spinal cord preparation, the authors tried “to uncover how this learning was accomplished—the *mechanism* that underlies this particular example of Pavlovian conditioning” (p. 9, italics added). They advanced three possibilities: the formation of a CS-US association, paired specific enhanced sensitization, and protection from habituation. The results of some manipulations (e.g., increase the number of CS-US pairings or the intertrial interval) led them to favor the last possibility. In summary, according to the authors, the rat's spinal cord was able to abstract the CS-US relation through the mechanism of protection from habituation. They conclude: “The methods of Pavlov provide great tools, but that is all they are. Having established the usefulness of the tools, we need to understand how systems sensitive to Pavlovian relations operate, realizing that other tools may be used to uncover the underlying machinery. Our central concern is with how the learning engine works [e.g., protection from habituation], not the tools used to take it apart [e.g., Pavlovian conditioning]” (p. 16).

I will argue that Grau and Joynes' argument suffers from a fundamental conceptual error, a sort of categorical mistake. Ironically, because of that error, the authors misconstrued their own research findings. Consider the three possibilities advanced to illustrate the mechanisms underlying Pavlovian conditioning: protection from habituation, paired specific enhanced sensitization, and the formation of an association. The authors believe these are functional descriptions and that they are somewhat better than the methodological description Pavlovian conditioning. More to the point, their key argument is that the broad (methodologically-based) category Pavlovian conditioning should be replaced by functional or mechanistically-based categories such as protection from habituation, for example. But the authors failed to notice that protection from habituation, paired specific enhanced sensitization, and the formation of an association are at best subcategories of Pavlovian conditioning; they identify in greater detail how the CS and US arrange-

ments in a particular case affected behavior; they do not show the inconsistency, irrelevance, or the misleading nature of the superordinate category Pavlovian conditioning. On the contrary, they depend on it for their own intelligibility. To remove the superordinate category Pavlovian conditioning is like removing the category *Homo* and then trying to understand the relations among *sapiens*, *erectus*, *neanderthalensis*, etc.

The conceptual error is further evinced when Grau and Joynes refer to the superordinate Pavlovian conditioning as a methodologically-defined category but refer to protection from habituation, a subordinate category, as a functional, mechanistically-defined category. I showed above that the former reference is incorrect. I will show next that the latter reference also is incorrect. If we define Pavlovian conditioning as a pairing of two stimuli (e.g., CS = tone and US = food) such that the response (e.g., a dog's salivation) elicited by the US is "transferred" to the CS, then we see that this definition (or any other similar one for that matter) involves two things: a particular arrangement of stimuli and a particular change in the animal's behavior. Consider now protection from habituation: A stimulus elicits a response that decreases when the stimulus is repeated. However, if the stimulus is paired with another stimulus (the US), then the response will not decrease when the former stimulus (call it CS) is repeated. We may say, picturesquely, that the US protected the CS from habituation. But what is the difference in the *logical* status of the two definitions? Both include an environmental manipulation (procedure) and a behavioral effect (process). In this regard, they are both defined methodologically or functionally or mechanistically (in the authors' phraseology).

Furthermore, among the three possibilities advanced to account for conditioned antinociception, the authors came to favor protection from habituation. They were influenced by a series of tests that elucidated the dynamic properties of the behavior under scrutiny (e.g., the effects of increasing the number of trials). But note that initially they had to ascertain that the conditioned antinociception itself was an instance of Pavlovian conditioning. How did they do it? Precisely in the same manner they did to ascertain that their preparation was an instance of protection from habituation: They used a variety of tests that revealed the dynamic properties of the behavior in question, properties typically associated with Pavlovian conditioning (e.g., overshadowing, blocking). The authors concluded: "Together, we believe that these observations suggest that the phenomenon is reasonably classified as an instance of Pavlovian conditioning" (p. 9). Strangely enough, following the *same* logic, in one case the authors reached a mechanistically-defined category (protection from habituation) whereas in the other case they reached a methodologically-defined category (Pavlovian conditioning). And they suggest that only the former has "biological reality" (p. 2).

Consider the foregoing issue from yet another perspective. Suppose Grau and Joynes were familiar with habituation in a specific context—when stimulus S is repeated, response R typically wanes. One day these researchers encounter a new situation—when a distinct stimulus is also present, the response to S does not habituate. They name the new phenomenon protection from habituation. However, because they are not satisfied with methodological classifications, they search for the underlying functional mechanism. Two possibilities come to their minds, one in which protection from habituation is due to the animal's state of arousal induced by the new stimulus, and another in which it is due to the pairing of S with the new

stimulus. A series of experiments with appropriate control conditions (e.g., in one the arousal state was induced by presenting the new stimulus alone; in another the interval between the two stimuli was varied considerably) convinces the two authors that their case of protection from habituation is due to stimulus pairing, not to arousal state. Grau and Joynes conclude that underlying the methodologically-defined protection from habituation is the functionally-defined Pavlovian conditioning, the opposite of their conclusion in the antinociception case! The message is clear: One should be suspicious of a way of reasoning that reaches opposite classifications from the same set of facts.

### **On the Assumption of Isomorphism**

Concerning this assumption, I question whether any learning psychologist embraces it nowadays. (Moreover, did any learning psychologist ever believe that in protozoans and humans habituation is implemented by one and the same mechanism? Although the answer may seem obvious, before giving it one still needs a clear, working description of what “mechanism” means in this context. Grau and Joynes provide none.) Instead, I believe most remain silent on this issue, as they should, because no neural mechanism is likely to be identifiable on the basis of behavioral data exclusively (Uttal, 1998). In fact, when Grau and Joynes criticize the assumption of isomorphism they always seem to have in mind a neuroscientist, not a learning psychologist: “For *neurobiologists*, the traditional view encourages an elegantly simple linking hypothesis that couples learning about distinct environmental relations (defined by methodology) to particular biological mechanisms” (p. 2; italics added); “*neurobiologists* favor paradigms blessed as true 30 years ago” (p. 3; italics added); “*neurobiologists* assume that any preparation that demonstrates a sensitivity to a response-outcome relation can be used to elucidate the mechanisms that underlie operant learning” (p. 15; italics added). Perhaps Grau and Joynes have simply mistaken the identity of their opponent.

### **Conceptual Infelicities and the Teaching of Learning**

I found the remaining key arguments in Grau and Joynes’ paper unconvincing because the authors were either vague or inaccurate when clarity and precision were called for. For example, it is impossible to agree or disagree with vague statements such as the following: “Seeking scientific legitimacy based on observable events, learning psychologists focused on stimuli and responses, the ‘venerable S and R’... [but] somehow the events themselves grew in stature and came to dominate our thinking about learning phenomena” (p. 2). Unfortunately, they do not tell the reader what it means for stimuli and responses to grow in stature. Similarly, it is impossible to agree or disagree with a conclusion when one does not know how key inferences were drawn: “At conferences, we and our colleagues will make light of the difficulty of maintaining student attention through a month of Pavlovian conditioning. The sad fact is that this organization, a historical artifact from our behaviorist past, leads students to a mistaken view—that the field of learning has become constricted, perhaps stagnant, and is out of tune with modern developments.” (p. 17) But how or why do students conclude that the field of learning is stagnant based on the organization of the material? How or why would

an alternative classification based on mechanism eliminate “the sad fact”? And how or why should we believe that student enthusiasm and learning would be enhanced if protection from habituation, paired specific enhanced sensitization, and similar categories replaced Pavlovian conditioning and operant conditioning? And when Grau and Joynes were more specific they often misrepresented the field. Thus when they state that “the vestiges of the traditional view have led researchers to ignore behavioral/biological mechanisms that play a pervasive role in helping an organism adjust to new environmental relations” (in reference to non-associative forms of learning) they ignore the behavioral research on habituation/sensitization and the attempts to “use” habituation to explain apparently more complex forms of learning and behavior (e.g., contrast effects in multiple schedules, McSweeney, Hinson, & Cannon, 1996; interval timing, Staddon, 2001).

As for the current teaching of the psychology of learning, the crux of the problem for Grau and Joynes seems to be the emphasis on behavior: “Researchers have learned a tremendous amount about the neurobiological mechanisms that underlie learning, but only recently has this material begun to creep into our texts. Relative to behavioral studies on learning, there are far more reports, both at conferences and in our journals, on the neurobiological mechanisms of learning. Yet, behavior has remained the central focus within the classroom” (p. 17). But in their claim for a privileged place in the classroom and textbooks for the neuroscience of learning at the cost of the psychology of learning, the authors express no scientific or pedagogical wisdom, only their peculiar biases.

In conclusion, there is much that is good and much that is novel in Grau and Joynes’ paper. But what is good is not novel and, alas, what is novel is not good. The authors seem exhausted with the teaching of learning and mistakenly conclude that the psychology of learning itself is exhausted. It is not.

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