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COMMENTARY

Pavlovian Conditioning Requires Ruling Out Nonassociative Factors to Claim Conditioning Occurred

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In a thought provoking article based on their studies concerning the behavioral capacities of spinally-transected rats, Grau and Joynes (2005) claim that protection-from-habituation and pairing-specific enhanced sensitization are learned phenomena that occur during pairings between a conditioned stimulus and an unconditioned stimulus. Our commentary questions whether such effects: (1) should be put in the same category as associative learning; (2) necessitate or warrant a new neurofunctionalism; (3) suggest that the field should have less emphasis on the methods of Pavlov and Skinner and more focus on function and neuroscience; (4) suggest that our textbooks be revised.

Grau and Joynes (2005) propose a new approach to the study of learning, labeled "neural-functionalism," stemming from their critique of perceived problems involving the practice (i.e., methodological issues) and image management (for our students and for colleagues in other disciplines) of the field of animal learning and conditioning. We discuss these critiques and this new approach by focusing on these two problems in turn. Although some important, if primarily familiar, issues are raised, we are unconvinced by Grau and Joynes's arguments.

Methodological Issues

The views articulated by Grau and Joynes developed out of their research into the learning capacities of a reduced animal preparation, spinally-transected rats. This work, involving the analysis of behavior produced by pairing a conditioned stimulus (CS) with an unconditioned stimulus (US), appears to be amenable to explanation in terms of one of three factors: Pavlovian conditioning as it is conventionally viewed, protection from habituation (PH), or pairing-specific enhanced sensitization (PSES). Most researchers would ask whether the change in behavior observed is due to associative learning or an artifact (PH or PSES). Grau and Joynes concluded that the behavioral effects obtained in their spinal rats were not due to the former factor and, rather than accepting an artifactual interpretation, the authors took a much bolder step by declaring that PH and PSES should be considered

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subcategories of Pavlovian conditioning. If we assume for the moment that sensitization and habituation are nonassociative effects, then Grau and Joynes are arguing that these effects are subcategories of Pavlovian conditioning because their occurrence requires the pairing of a CS with a US. This is an intriguing idea. One of the present authors contemplated this same issue when he was exploring *US reinstatement* or *reminder* effects (Schachtman et al., 1983; see Miller & Springer, 1973). A conditioning trial (say, a light-shock pairing) would be given to the subjects and poor expression of learning would result for one reason or another; then, the experimenter would administer a *reminder treatment* (e.g., a shock alone exposure) after conditioning and prior to testing for the conditioned response (CR), and this would cause learning to be expressed (i.e., a CR). If control groups (rats receiving the CS alone, US alone, or the CS and US unpaired, instead of a CS-US pairing) did not show the CR when the reminder was administered to them, the behavioral effect was assumed to depend on associative processes and was, therefore, not artifactual (i.e., not due to nonassociative factors). But one might ask, *What if, in order for the reminder treatment to produce a nonassociative effect, the subject must receive the CS and US together?* Such an effect seems unlikely at first blush, but could happen since, afterall, the presentation of a light produces a certain neural response as does a shock and maybe the experience of both close together in time causes the animal to be subject to the nonassociative effects of the reminder treatment. Grau and Joynes have not only produced such effects in their reduced preparation but even have plausible nonassociative mechanisms for them (PS and PSES, assuming they are nonassociative effects). But, should these behavioral effects be explained by broadening the domain of Pavlovian learning or, alternatively, should we simply acknowledge that we do not currently have ample control conditions to rule out all possible nonassociative (i.e., artifactual) effects? Grau and Joynes argue for the former, we prefer the latter. Furthermore, their choice provides the departure point for their new conceptualization of conditioning called neural functionalism. We do not see any compelling reasons to follow in that direction.

Perception of the Field and Image Management

As we all know, when an instructor teaches an undergraduate introductory psychology class, he/she is forced to greatly simplify the theories and findings. The students are not ready to accommodate more than that. In science, there are many instances where we must draw crisp lines on a canvas when, in reality, the image is fuzzy and complicated. For instance, in the physical sciences we have courses in (and departments of) biology, physics, and chemistry when we know that these areas are conceptually overlapping and inseparable. Even in our own psychology departments with separate training areas, we have a difficult time trying to figure out where to put areas or researchers studying emotion, motivation, judgment, or even personality since such things bridge many areas within psychology. Grau and Joynes are correct that many texts/courses in conditioning are separated into Pavlovian and operant conditioning. Students may not be ready to have the text or course structured in terms of mechanism (and, by mechanism, we refer to psychological mechanism as discussed below, not biological mechanism as apparently suggested by Grau and Joynes since that would be a text or course in neurosci-

ence). Most if not all areas in psychology organize their texts and courses around procedures and behaviors rather than the mechanisms underlying them. And it is true that the *phenomena* of conditioning (e.g., a CR following Pavlovian conditioning) are operationally defined giving them the semblance of merely being a methodology. However, for most researchers investigating Pavlovian conditioning the CR reflects the formation of an association between the CS and US and the subject experiences, upon presentation of the CS, an expectancy of the US. The presentation of the CS can evoke a representation of the US in active memory. Similar processes have been explored for responses and outcomes in instrumental conditioning. As Rescorla rightfully put it in the title of his 1988 paper, "Pavlovian conditioning: It's not what you think it is." We claim that it is more than a methodology and subsumes associative learning. PH and PSES are not Pavlovian conditioning any more than pseudoconditioning is conditioning.

The functional mechanisms level of Figure 4, in the target article, mix different levels of discourse or levels of analysis. They describe two effects that are operationally defined (PH and PSES) just as Pavlovian conditioning is operationally defined. All three effects are produced by a similar environmental condition. All three phenomena should be placed on a similar level: Behavioral responses to the methodological/environmental level in Figure 4 of the paper by Grau and Joynes (i.e., pairing of a CS with a US) and then there should exist a level below that in which the underlying (psychological) mechanism of each phenomenon be determined. Associative learning is likely one mechanism of Pavlovian conditioning and this associative mechanism may very well be further explicated psychologically with respect to the expectancies, activation of event representations, retrieval, and other psychological processes potentially involved with such associations). Grau and Joynes argue for "a more mechanistically based psychology of learning" (p. 4) but we believe that the field of conditioning is already very mechanistically based. The underlying mechanisms of PH and PSES remain to be determined. In fact, conditioning theory requires that certain mechanisms that have been explored in one procedure (e.g., conditioned emotional response, conditioned eyeblink, conditioned taste aversion) be explored in other procedures (and Grau and Joynes assume, for instance, that associative basis of long-term habituation applies to spinal cases of habituation and, yet, this remains to be determined).

We struggled with some of the arguments presented in the Grau and Joynes article. In large part this was due to difficulty understanding some of the terminology and a lack of detail about some of their points. Our appreciation of this article would, for example, have benefited from defining what is meant by functional mechanism (since the authors assert that the field currently is not mechanistically based, when they seem to mean that it is not centered around neuroscience). They claim they "question restrictive views of what constitutes learning" (p. 3) and so it would have been helpful if they had defined learning itself so one can appreciate how this conceptualization restricts learning. They claim that "the architecture has changed" (p. 3) and yet do not specify what changes they are referring to. Secondly, since nonreinforced conditioning with a CS can produce conditioned inhibition, it is not clear how much the effects in Figure 2 are due to conditioned excitation to CS+ and inhibition to CS-. We also do not know if the effects obtained by Grau and Joynes are stimulus specific. We also do not understand why statements would be made that claim "Tradition…would have us ignore identified biological

mechanisms" (p. 13). Does tradition include learning theorists ignoring neuroscience? We think not. It is simply another level of discourse or analysis (in fact, a different course).

Grau and Joynes acknowledge that many of the features of neural functionalism are already fully appreciated by the researchers in the field. This is true. Nothing in the current field denies that organisms can solve an environmental puzzle in different ways (and potentially using more than one brain mechanism for each way and using different brain mechanisms for different ways). No new field of neural-functionalism is needed. The authors claim that "researchers working within the multiple memory paradigm have adopted a neurofunctionalist approach, one that focuses on the brain mechanisms that underlie information storage" (p. 5). Once gain, to us, this sounds like part of the domain of the field of neuroscience and no new label is needed.

The authors are correct that it is a challenge to make our conditioning courses palatable to students and to retain respect from our colleagues from other disciplines. Students can and do get excited when they learn about the different potential underlying psychological mechanisms of conditioning (expectancies, activation of event representations, etc.) and so we should expose these intriguing processes in our lectures. There are many things that will assist our image management with colleagues. For instance, the paper by Grau and Joynes refers to our field as the "field of learning" (p. 18) and we would guess that our human learning and cognition colleagues scoff when they see such expressions. These colleagues must think we still linger in the middle of the twentieth century when our field *was*, for the most part, the field of learning. We are the field of conditioning or the field of animal learning or, if you like, the field that explores a subset of learning processes. In the final analysis, progress in animal learning and conditioning is evolutionary in the sense that the merits of new ideas, theories, or viewpoints are determined, not by individual appeals to correctness, but by whether these new ideas have greater utility than those they seek to supplant. As it should be, time will be the judge of the merits of such a case.

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