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A Neural-Functionalist Approach to Learning

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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. This trichotomy, a vestige of our behaviorist past, continues to influence the field, both in the classroom and in the laboratory. Relying on data collected using a simple model system (learning within the mammalian spinal cord), evidence is presented that organisms can learn about an environmental relationship in multiple ways, an observation that argues against a simple isomorphism between methodology and mechanism. It is suggested that a new classification system is needed that focuses on mechanism rather than methodology, subdividing our empirical world along lines that make sense given commonalties in the neural-functional mechanisms involved.

For over a century, learning has remained a central force within psychology. Introductory students hear of Thorndike (1898), Pavlov (1927), and Skinner (1938), and advanced students routinely take a course on learning to fulfill their core requirements. It was learning theorists (e.g., Hull, 1943; Watson, 1913; also see Boakes, 1984) who sold psychologists on the value of studying animal behavior, providing evidence that we and our evolutionary brethren often adjust to new situations in a similar fashion. This proof of principle laid the foundation for the study of the neurobiology of learning and memory, an enterprise that relies primarily on infrahuman creatures, from simple invertebrates (Kandel & Schwartz, 1982) to primates (Mishkin, 1982).

The field of learning has remained a central focus of investigation primarily because many endorse its core assumptions. The first core assumption is that learning is essential. Natural selection cannot prepare a response for every situation that an organism might encounter. The range of environmental situations is simply too great and biologically relevant stimuli often occur in an unpredictable

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fashion. To accommodate changes in food supply, predator strategies, and stimulus relations, an organism's innate predispositions must be supplemented with ways of learning about the world.

The second core assumption is one of generality. Once nature discovered an efficient system for conveying signals within, and between cells, the mechanisms were maintained through evolution. For example, the electrophysiological recording of an action potential from a giant squid axon is practically identical to the neural response observed in a vertebrate neuron. The field of neurobiology has assumed generality across species and, armed with this assumption, we have learned a great deal about the human nervous system. In a like fashion, learning theorists have assumed that once nature devised some efficient strategies for storing information and adjusting to changes within the environment, these solutions would be well conserved across species.

A Problematic Path

Although we agree with these core assumptions, we will argue that the field of learning headed down a problematic path. The problem stems from the way in which earlier theorists interpreted the issue of generality. Seeking scientific legitimacy based on observable events, learning theorists focused on stimuli and responses, the venerable S and R. With just two events, there were few permutations. If presenting a stimulus alone altered its behavioral/psychological impact, the change in response magnitude was characterized as a form of single stimulus learning (habituation or sensitization). If the behavioral consequence of a stimulus was modified by its spatial-temporal relationship with another cue, the phenomenon was classified as a form of Pavlovian (or classical) conditioning. Finally, cases in which a response was modified by its consequence (the outcome) were given the label instrumental (or operant) conditioning.

Why has the focus on the S and R become problematic? Of course, a science does need to be based on objectively observable events. But somehow, the events themselves grew in stature, and came to dominate our thinking about learning phenomena. 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 their presence. Many within the field of learning now recognize that this course was misguided. Yet, the field of learning remains housed in an architecture that was framed nearly a century ago. The new residents often think about its rooms in novel, mechanistically-based ways, and have performed many renovations and additions. Yet, to researchers and students outside of the field, it is the traditional house that is seen. When we teach a course on learning, we organize the material on the basis of methodology. When neurobiologists explore the biological mechanisms that underlie learning, they often set up shop in a single room. Implicitly, there is an acceptance that the house is laid out in a sensible fashion, that it describes functional distinctions that have a biological reality.

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 (Figure 1). The lure of this assumption lies in its potential power. If the methods of Pavlov characterize a

distinct form of learning, a kind of biological universal, we could use the methods to identify a model system and uncover the underlying neurobiological mechanisms. The hope is that these discoveries would enjoy the same type of generality achieved by the description of an action potential based on studies of the giant squid (e.g., Hodgkin & Huxley, 1952). If so, the description of the biological bases of Pavlovian conditioning in another invertebrate (*Aplysia*) could reveal the biological universals used to encode Pavlovian relations in humans. Over the last 40 years, research groups have built upon this foundation and coalesced around particular model systems (Alkon, 1987; Kandel & Schwartz, 1982; Thompson, 1986) hoping to uncover the neurobiological substrates of learning.

Just as neurobiologists began to make substantial gains, fights emerged within the field of learning that led some to question its basic architecture. Examples of learning were discovered that seemed to violate well-established principles and/or that were not readily classified within the traditional structure (reviewed in Garcia, Brett, Rusiniak, 1989; also see Domjan, 1983; LoLordo & Droungas, 1989; Rozin & Kalat, 1971; Seligman, 1970; Timberlake & Lucas, 1989). The response to these rebels varied. Some suggested that the behavioral modifications they had identified did not represent examples of "true" learning (Bitterman, 1975; Gormezano & Kehoe, 1975; Mitchell, 1978; Riley, 1978). Others assumed that the violations arose, not because the underlying learning mechanisms varied, but rather because natural selection had tuned the system to solve a specific environmental puzzle. For example, a selective modification within particular perceptual, motor, or motivational systems, could bias the system to certain types of stimuli and behavioral solutions. Such alterations could dramatically affect performance, even though the learning mechanism itself was unchanged. Both of these solutions share an essential feature, for both allow us to preserve the notion of a biological universal. In this way, the solutions help preserve the isomorphism illustrated in Figure 1. While not denying the importance of tuning, we question restrictive views of what constitutes learning and, in doing so, cast doubt on the viability of such simplistic isomorphisms.

Why question a framework that has endured for over a half century? The coupling of operationalism with the mechanism of associative learning brings together two of the most powerful and influential concepts within psychology. The simplicity, power, and well-established success of the approach would seem to outweigh the tiresome challenges posed by cases that are not readily accommodated. The irony is that few modern learning theorists would likely endorse the traditional view. Many (perhaps most) recognize that non-associative mechanisms play an important role and that there is not a simple isomorphism between methodology and mechanism. We suggest that it is time to rebuild the house because many outside of the field seem unaware of how the architecture has changed over the last 30-40 years. This lack of recognition affects how other areas characterize the field, how we teach it, and how we explore/describe the underlying neurobiological mechanisms. Outsiders often see learning as it appeared in the mid-1960s. In the classroom and our summaries, we organize content by methodology, not mechanism. Seeking a safe course, seemingly endorsed by the field, neurobiologists favor paradigms blessed as true 30 years ago and view other approaches with suspicion.

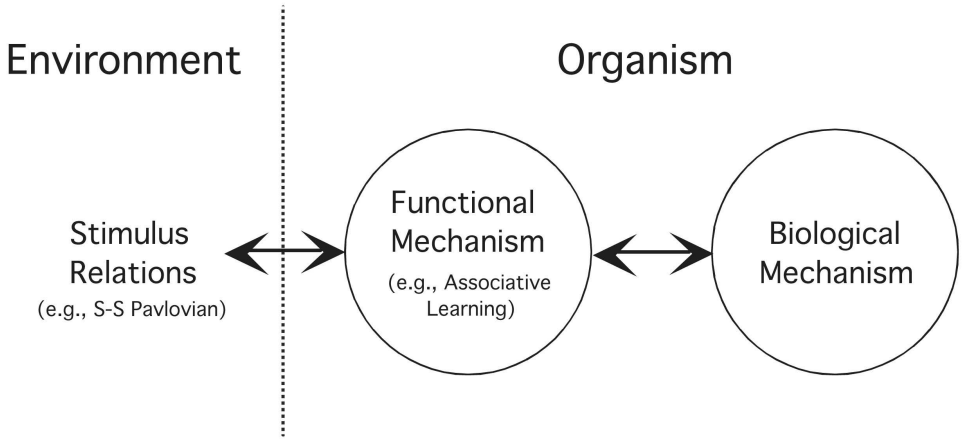

Figure 1. The relationship traditionally assumed between methodology (the imposed environmental relations) and the functional/biological mechanisms within the organism.

We will argue 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. Just as organisms have evolved different ways to move about within the environment, there are multiple (and qualitatively distinct) ways to encode stimulus relations and there is no reason, a priori, to believe that one is superior to the rest (Domjan, 2000, Joynes & Grau, 1996). Bipedal stepping works well for us on land but is a poor solution in water. Formally similar environmental puzzles can be solved (at a neural level) in multiple ways. Conversely, a single mechanism may be called upon to solve a variety of environmental challenges. For example, a mechanism that helps the organism encode Pavlovian stimulus relations can also contribute to learning that is classified (on the basis of methodological criteria) as single stimulus or instrumental learning. We will suggest that sorting out this complexity will require a more eclectic view of what constitutes learning and greater attention to mechanism.

By emphasizing a path that promotes mechanism over methodology, we are not suggesting that we give up on generality. Organisms have evolved a limited number of ways to move about their environment and what we learn about swimming from studying one species of fish can help us understand swimming in many other species. Rather, what we will advocate is a view that assumes each type of environmental relation can be encoded by a number of different mechanisms, but this set is finite (and hopefully limited to a manageable number). Our push is against the simple trichotomy that has dominated the field of learning and that continues to provide the organization for our undergraduate and graduate courses. We will argue for a more mechanistically based psychology of learning that provides a better framework for linking behavior to neuroscience and cognition. At the same time, we recognize the impotance of sound methology and the need for a careful detailed description of the input-output relations that control behavior.

Neural Functionalism

To see how we came to this position, we will describe some of our recent work on learning within the spinal cord (see Grau, Salinas, Illich, & Meagher, 1990; Grau, Barstow, & Joynes, 1998; Illich, Salinas, & Grau, 1994; Joynes & Grau, 1996). At the heart of this work is a form of neural comparison: How do different regions of the nervous system encode and store information? Using physiological manipulations to isolate distinct portions of the nervous system, we (Grau & Joynes, 2001) and others (for examples, see Packard & McGaugh, 1996 [place versus response learning] and Phillips & LeDoux, 1992 [alternative paths for CS encoding]) have shown that an organism can solve the same environmental puzzle in different ways. Lacking a name, we will refer to this approach as neural-functionalism (or neurofunctionalism). Functionalism because the approach focuses on the identification and comparison of operational modules designed to accomplish a particular goal, be it the abstraction of environmental relations, recognition of a food source, or spatial navigation. Neural because an integral component of the approach involves the specification of the underlying neural mechanisms. The approach is designed to focus our attention on the identification of processing systems, how they interact, and their underlying neurobiology. The approach is a close relative of cognitive neuroscience, but broader. As its name implies, cognitive neuroscience focuses on the mechanisms that underlie higher mental processes, such as memory, categorization, reasoning, problem solving and language. Neural modifications within an invertebrate ganglion, or the vertebrate spinal cord, are not normally included within this domain. Neural-functionalism is not limited in this fashion and is concerned with learning however, and wherever, it might occur within a nervous system.

Similarly, neurofunctionalism is consistent with the multiple memory systems view (Hirsh, 1974; Mishkin & Petri, 1984; Sherry & Schacter, 1987; White & McDonald, 2002; Zola-Morgan, Squire, & Mishkin, 1982), but broader. Central to both views is the assumption that environmental puzzles can be solved in alternative ways by different regions of the nervous system. From our perspective, researchers working within the multiple memory paradigm have adopted a neurofunctionalist approach, one that focuses on the brain mechanisms that underlie information storage. The concept of neurofunctionalism is meant to apply more broadly, to include brain-mediated processes that do not necessarily involve learning and memory (e.g., sensation and perception) as well as neural events that occur outside of the brain (e.g, in the periphery, the spinal cord, or within an invertebrate).

In earlier frameworks, a neural modification that occurred within the periphery was often dismissed as an annoying complication that had to be controlled for within our experimental designs. It mattered little whether this peripheral modification occurred in all species and might account for a tremendous portion of the variance. Neural-functionalism has no such prejudice and embraces all forms of plasticity, whatever their source (this includes neural-immune interactions [e.g., Jankowsky, Patterson, 1999], chemical interactions with glia [e.g., Pugh, Johnson, Martin, Rudy, Maier, Watkins, 2000], and modifications within the peripheral component of an afferent pathway [e.g., Carlton, 2001]). The only constraint that

we will impose is that the phenomenon affects how a neuron, or group of neurons, operate. Neurofunctionalism is, in this sense, a form of neurobiology. It assumes that neural components form functional systems designed to meet a particular end. The aim is to detail the properties of the functional system, the underlying biological mechanisms, and how they are linked. Although our focus is on one particular kind of neurofunctionalism, the component that describes how the system can adjust and learn, this should not be viewed as a limit. Neurofunctionalism is just as concerned with the innate properties that constrain the system. Indeed, we sometimes forget that the mechanisms that underlie learning are themselves innately given. In attempting to detail how these systems operate, the field of learning has, to a large extent, focused on detailing the properties of an innate system.

Traditionally, three methods have been used to compare the operation of adaptive systems. The strategies involve examining how functional and neural capacities vary across age, species, or level of the neural axis. Developmentalists focus on age-related changes in how a particular system operate. Those that adopt a comparative approach examine differences across species. The last approach has its roots in the work of Sherrington (1906) and Jackson (1931-1932). Here both developmental level and species are held constant. The key comparison is across different regions of the nervous system.

While each approach can provide insights into how neural-behavioral systems are designed and operate, our own work is comparative in the Jacksonian sense. Our core assumption is that distinct regions of the nervous system may solve a common puzzle in different ways. For example, both the hippocampus and cerebellum can encode a temporal relationship between an auditory CS and a US that elicits an eyeblink response. But the cerebellum appears to use a simpler algorithm, one that has difficulty spanning spatial-temporal gaps (Moyer, Deyo, & Disterhoft, 1990; Solomon, Vander Schaaf, Thompson, & Weisz, 1986). Just as the ability to solve more complex environmental puzzles increases with development, higher regions of the nervous system (the forebrain) have functional capabilities that lower systems lack. It appears that there is both a quantitative and qualitative shift in how environmental relations are processed across different regions of the nervous system.

In vertebrates, it has often been assumed that the most interesting forms of learning are mediated by the most advanced neural structures. Pavlov (1927) assumed his learning occurred in the cortical lobes of the upper brain (forebrain). Lower-portions of the central nervous system (brainstem and spinal cord) were thought to organize some simple reflexes and, at most, support primitive forms of learning (e.g., habituation). Following Pavlov's lead, many subsequent authors have seemed predisposed to attributing the phenomena they studied to the most advanced structures, a kind of neural egotism. For Pavlovian conditioned responses (CRs), and many other phenomena, subsequent work has revealed that the neural requirements are not nearly as complex as the authors had presumed. The lead author knows this story first hand, for he too was tempted down this path in developing a theory of pain modulation (Grau, 1987). Following others (Watkins & Mayer, 1982), it was suggested that forebrain mechanisms mediate conditioned changes in pain reactivity. As we will see below, subsequent work revealed exceptions to this claim.

We will develop our argument based on studies that examined the functional limits of learning within the spinal cord. We believe that this provides a particularly interesting example, for the results obtained run counter to long-held views. More important for present purposes, learning within this system provides a good example of the breakdown between methodology and mechanism. Spinal cord systems are sensitive to Pavlovian relations, but the rules that govern learning within this system differ from those employed by the brain to regulate the same response system (Grau & Joynes, 2001). In the sections that follow we outline the empirical basis for this claim.

Spinal Cord Neurons Support Pavlovian Conditioning

Working with Mary Meagher, we (Meagher, Chen, Salinas, & Grau, 1993) and others (e.g., Watkins & Mayer, 1986), showed that pain (*nociceptive*) signals are regulated at multiple levels of the nervous system. In many of these studies, we inferred changes in nociceptive reactivity using a spinal reflex, tail-withdrawal from noxious heat (the tail-flick test). The nociceptive pathway that organizes this motor response is modulated by both intraspinal and descending pathways. Under normal circumstances, higher neural systems in the brainstem and forebrain regulate the tail-flick response. For example, when an organism encounters a mildly aversive stimulus, brain systems often inhibit nociceptive reflexes within the spinal cord, producing a decrease in nociceptive reactivity known as *antinociception*. The direct, intraspinal, pathway appears to be engaged under more limited circumstances, when the organism encounters an event that is relatively severe. Casually speaking, it is as if the low-level mechanism functions as a safety-switch that is only engaged under the most dire of circumstances.

Prior studies had revealed that learning can influence the activation of the antinociceptive systems (see Figure 2A). For example, a conditioned antinociception can be produced by pairing a cue (the CS+) with an aversive shock (Fanselow, 1986). The impact of this cue is then compared to another CS (the CS-) which was presented an equal number of times but never paired with shock. The usual outcome is that the CS+ generates antinociception (longer tail-flick latencies) relative to the CS-. Following others, we suggested that this learning depended upon neural systems within the forebrain (Grau, 1987). The antinociception generated by the intraspinal circuit was characterized as a simple unconditioned response to noxious stimulation that was mediated by a mechanism that was insensitive to Pavlovian relations.

A past student (J. Salinas) questioned this assumption, noting that others (Durkovic, 1975; Fitzgerald & Thompson, 1967; Patterson, Cegavske, & Thompson, 1977) had found evidence of Pavlovian conditioning within the spinal cord. Given this, we designed an experimental paradigm that would allow us to study how intraspinal mechanisms work after communication with the brain has been severed (Grau et al., 1990). This was accomplished by transecting the spinal cord at a relatively high level (the second thoracic vertebra, T2). Transected (spinalized) rats retain the use of their forelimbs but are paralyzed below the forelimbs (paraplegia). Using this preparation, we can examine how the lower spinal cord processes information using stimuli applied to the lower extremities (hindlimbs and tail). To study how spinal mechanisms change with experience, we use stimuli

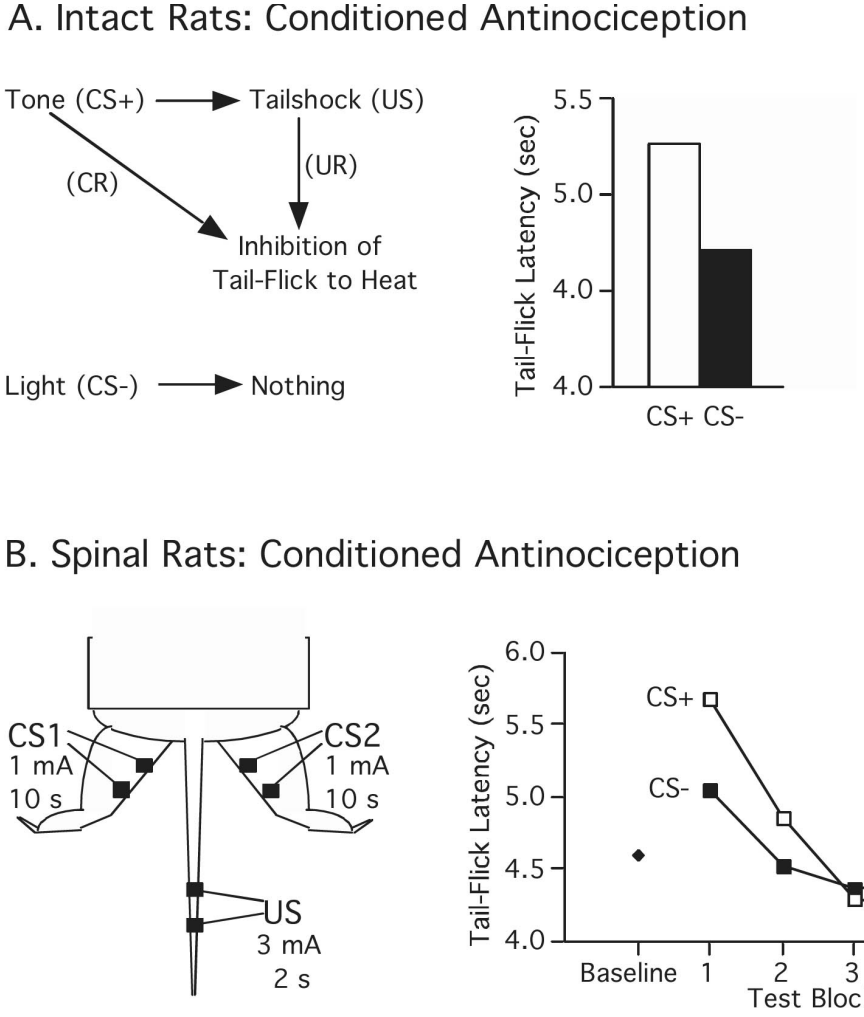

Figure 2. (A). Conditioned antinociception can be demonstrated in intact rats by pairing a cue (e.g., a tone) with an aversive stimulus (tailshock). Another cue (e.g., a light) is presented an equal number of times but never paired with tailshock. The paired cue serves as the CS+ and the unpaired cue is the CS-. The impact of each cue on pain reactivity is then assessed by measuring the latency of tail-withdrawal from radiant heat. Subjects typically exhibit longer latencies during the CS+. (B). Conditioned antinociception can be demonstrated in spinally transected rats using cutaneous stimuli (mild shocks) applied to the left or right hind leg. An intense tailshock serves as the US. One leg stimulus (the CS+) is paired with tailshock while the other (the CS-) is presented alone (which leg is paired with the US is counter-balanced across subjects). After 30 presentations of each stimulus, withdrawal latency is assessed using the radiant heat device. Subjects exhibit longer latencies during the CS+ and this effect wanes (extinguishes) over the course of testing. (Adapted from Grau & Joynes, 2001).

that engage spinal reflexes that produce a measurable behavior (e.g., tail or leg withdrawal).

We first examined whether spinal cord mechanisms could support a form of Pavlovian conditioning (Grau et al., 1990). For our CSs, we used mild shocks that were applied to the left or right hind leg (Figure 2B). One CS (the CS+) was repeatedly paired with a noxious tailshock (the US) that we knew from past studies would generate a powerful antinociception. Stimulation of the opposite hindleg

served as the CS-. The CSs were 10 s in duration and the US was presented during the last 2 s of the CS+. Spinalized rats received thirty CS-US pairings, and 30 presentations of the CS-, spaced over an hour interval. An hour later, tail-flick latencies were assessed during each CS. We found that rats exhibited longer tail-flick latencies during the CS+ and that the magnitude of the CS+/CS- difference extinguished over the course of testing (Figure 2B). It seems that spinal neurons are sensitive to the CS-US relation, the defining attribute of Pavlovian conditioning at a methodological level.

Because studies of spinal learning cannot use typical stimuli (lights and tones), questions sometimes arise as to whether the results obtained depend on the unusual nature of the stimuli. On this issue, it should be recognized that we and others use electrical stimulation simply as a convenient method for engaging afferent neural activity and that others (including Pavlov, 1927) have used electrical stimulation as a CS without invoking specialized learning mechanisms. We also know that intense (noxious) vibrotactile stimulation can support Pavlovian conditioning (Joynes, Illich, & Grau, 1997). Finally, we have shown that spinal mechanisms can support some basic Pavlovian phenomena, including extinction, latent inhibition, blocking, and overshadowing (Grau et al., 1990; Illich, Salinas, & Grau, 1994). Together, we believe that these observations suggest that the phenomenon is reasonably classified as an instance of Pavlovian conditioning.

Alternative Mechanisms for Encoding a S-S Relation

Pavlovian conditioning refers to a class of methods designed to investigate how organisms encode stimulus relationships within the environment. Spinal circuits appear sensitive to a simple CS-US relation and thereby support a form of Pavlovian conditioning. Our next task was to uncover how this learning was accomplished—the mechanism that underlies this particular example of Pavlovian conditioning. Figure 3 represents three possibilities. First, the CS input might be linked to the US pathway through the development of a new association. This *associative* learning would allow a neutral CS to generate a new response, the CR. Another possibility is illustrated by the type of mechanism thought to underlie Pavlovian conditioning in the invertebrate *Aplysia*. In this preparation, exposure to a noxious stimulus (e.g., a severe tailshock) sensitizes the organism to tactile stimulation (e.g., touching the siphon or mantle shelf). Walters, Hawkins, and their colleagues have shown that the magnitude of this sensitization is modulated by the temporal relationship between the tactile stimulation and tailshock (Hawkins, Abrams, Carew, & Kandel, 1983; Walters & Byrne, 1983). Greater sensitization is observed when the tactile stimulus (the CS) is paired with the noxious tailshock (the US), producing a form of Pavlovian conditioning known as pairing-specific enhanced sensitization. A third possibility is suggested by studies of habituation. With repeated presentation, the capacity of a stimulus (the CS) to generate a response often wanes. Researchers have previously established that the presentation of a biologically significant event (the US) can reestablish (dishabituate) the response. If the magnitude of this effect depends on the temporal relationship between the two events, a Pavlovian relation would yield differential effects. Specifically, a CS that is paired with the US should habituate at a much slower rate and, as a result, retain its capacity to produce a behavioral response. This phe-

nomenon is referred to as *protection from habituation* (Humphrey, 1933; Mitchell, Scott, & Mitchell, 1977; Pfautz, Donegan, & Wagner, 1978; Rescorla, 1984; Riley, 1978).

Before proceeding, two points must be acknowledged. First, we presented these three mechanisms as if they represented the full range of logical possibilities and as if intermediate cases do not exist. This was done to simplify our exposition. We recognize that the list provided may not be complete and that variation in mechanism may lead to intermediate cases that are difficult to classify. Second, we recognize that we are not the first to push this type of approach and that it is often adopted by researchers who take a more comparative approach (e.g., Papini, 2002).

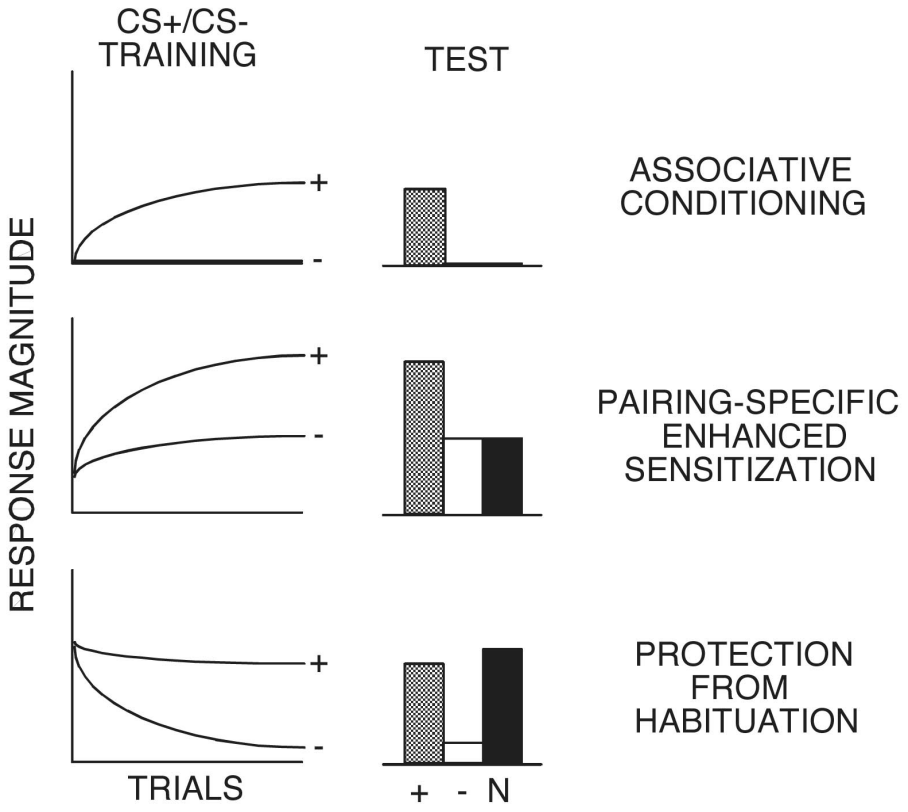

Figure 3. Three ways in which the CS-elicited response can be modified by a paired US. In protection from habituation, the CS initially elicits a behavioral response. If presented alone, this response weakens over trials (habituation). Pairing a cue (the CS+) with the US protects the CS from habituation. As a result, the paired cue (+) generates a stronger response than the unpaired cue (-) at the time of testing. However, the paired cue does not produce a response greater than that observed to a novel cue (N). In pairing-specific enhanced sensitization, presenting the US sensitizes the CS-elicited response. If greater sensitization develops when the CS and US are paired, the CS+ will acquire the capacity to generate a stronger response relative to both the unpaired (-) and novel (N) cues. In associative conditioning, the cues have no capacity to generate a response at the start of testing. Pairing one cue (the CS+) with the US endows it with the ability to generate a conditioned response. Under ideal conditions, only the CS+ would generate a conditioned response at the time of testing. (Adapted from Joynes & Grau, 1996, Behavioral Neuroscience, 110, 1375-1387. © 1996 American Psychological Association).

To evaluate the mechanism that underlies spinal conditioning, we first examined whether our CSs generated an antinociception prior to training (Joynes & Grau, 1996). We found that they did, suggesting that either pairing specific enhanced sensitization or protection from habituation might be involved. Next, we assessed whether repeated presentation of the CS would cause this antinociceptive response to habituate. We found that it did—after the CS was presented alone 30 times, it generated a weaker antinociception than a novel cue. Finally, we tested whether the development of habituation is affected by the paired presentation of the US. Subjects received the CS and US in a paired or unpaired fashion. The impact of this experience was then compared to a novel CS. We found that the unpaired CS produced less antinociception than the novel cue, an outcome indicative of habituation. The paired cue retained its antinociceptive quality, which suggests that the US had protected it from habituation.

Together, these findings imply that conditioned antinociception within the spinal cord may reflect a form of protection from habituation. It is important to realize, however, that these observations alone do not force the conclusion. The possibility remains that CS exposure produced habituation and that associative learning was superimposed upon this effect. Through associative learning, a new CS-US link could be formed that allows the CS to maintain its original capacity to generate antinociception. Such an outcome would produce a pattern of results that is similar to those expected given a protection from habituation mechanism. To disentangle these alternatives, we needed some manipulations that should affect protection from habituation and associative learning in opposite ways.

We reasoned that protection from habituation might slow, but would not completely prevent, the development of habituation (Joynes & Grau, 1996). If this is true, then the CS+/CS- difference would grow smaller with experience, as the CS+ slowly loses its ability to generate antinociception. In contrast, associative theories generally assume that increasing the number of CS-US pairings should, if anything, strengthen the CS-elicited CR, increasing the CS+/CS- differences. We evaluated these alternatives by increasing the number of training trials 5 fold. Under these conditions, the CS+ lost its ability to generate antinociception, effectively eliminating the CS+/CS-difference. This outcome implies that the learning was mediated by a form of protection from habituation. (Alternatively, one could argue that a post-asymptotic decline in the magnitude of the conditioned response is a common characteristic of associative learning [Overmier, Payne, Brackbill, Linder, & Lawry, 1979]. It is true that other preparations sometimes exhibit a weakening of the CR as training is continued. In some cases, this may occur because protection from habituation contributes. However, it is evident that CR magnitude can wane for more than one reason. For example, in a systematic examination of the phenomenon in intact animals, Overmier et al. [1979] demonstrated that the effect was due to a reduction in US effectiveness, possibly due to the development of an opponent process. Supporting this, it was shown that presenting the US alone can reduce CR magnitude. Our analysis of protection from habituation depends on a different mechanism, habituation of the CS, and yields a distinct pattern of results. For example, extended training enhances the response produced by a novel CS, relative to both the CS+ and CS-, in spinal conditioning, but has no effect in the paradigm examined by Overmier et al., where an overtrained CS and novel CS have similar behavioral consequences. 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.)

Next, we assessed the impact of increasing the interval between training trials (the intertrial interval, or ITI). Prior studies have shown that the magnitude of the nonassociative form of habituation depends on the ITI; greater habituation is observed with short ITI (massed presentation) and increasing the ITI (spaced presentation) weakens the development of this form of habituation (Davis, 1970; Groves, Lee, & Thompson, 1969; Whitlow, 1975). In contrast, learning that is thought to rely on an associative mechanism generally exhibits the opposite relation; given an equal number of pairings, spaced presentation produces a stronger CR than massed presentation Gibbon & Balsam, 1981; Miller & Matzel, 1989; Spence & Norris, 1950). To evaluate these alternatives, we increased the ITI 5 fold. We found that this manipulation weakened habituation to the CS- and thereby eliminated the CS+/CS- difference (Joynes & Grau, 1996). Again, our results imply that the learning was mediated by a form of protection from habituation.

Towards an Integrative Model of Conditioning

As we noted above, Pavlovian conditioning is typically defined on methodological grounds. If differential conditioning produces a CS+/CS- difference that extends beyond the training period, the criterion for Pavlovian conditioning has been met. What is sometimes forgotten is that this temporal information can be encoded in a number of ways. Our studies of learning within the spinal cord highlight the importance of this consideration. We have shown that spinal mechanisms are sensitive to Pavlovian relations and support a range of Pavlovian phenomena, including latent inhibition and overshadowing (Grau et al., 1990; Illich et al., 1994). Yet, this learning does not appear to reflect associative learning, but instead depends on a simpler process based on protection from habituation. On the one hand, these findings suggest that some functional capacities (e.g., cue competition) may be inherent to any system capable of abstracting a CS-US relation. On the other, it is clear that the mechanism that governs spinal conditioning in our laboratory obeys rules that are qualitatively different from those that govern associative learning. Two variables (spaced practice and increased training) known to strengthen associative learning weakened the spinally-mediated CR. Protection from habituation likely differs from associative learning in other important ways. For example, only the latter may support trace conditioning, negative patterning, sensory preconditioning, and mediated acquisition (Holland, 1990; Sutherland & Rudy, 1989; Solomon et al., 1986). These phenomena require some capacity to integrate information across space, time, or sensory modality. Processing of this sort may depend on a form of associative learning and seems beyond the capabilities of the spinal cord.

A traditionalist of the old sort would argue that we have gravely erred. To the traditionalist, only associative learning represented *true* learning. Pairing specific enhanced sensitization and protection from habituation do not, from this perspective, represent valid forms of learning. The latter two were discounted as examples of alpha conditioning, for in both cases the CS has some capacity to generate a CR-like response prior to training. By their definition, learning was necessar-

ily associative in nature. Consequently, any behavioral phenomenon that failed on this criterion was not an example of learning. Most within the field of learning now recognize that this view is too limiting and unreasonable.

If we adopt the traditionalists position, we must discount the work of Kandel and associates (Kandel & Schwartz, 1982). The problem is that conditioning in *Aplysia* reflects a form of alpha conditioning, because the CSs used to study learning in this creature routinely elicit a CR-like response prior to training. Fear conditioning in rats would be similarly dismissed, despite its clinical implications, because the CSs used generally produce some freezing (the usual CR) before the US is introduced. A mechanism like protection from habituation would be discarded even though it may play a much more pervasive role in shaping behavior. Tradition in these cases would have us ignore identified biological mechanisms, known to contribute to behavioral plasticity, that seem to have a pervasive role in helping the organism adapt to its environment. To us, this does not represent a rational course.

Other Methodologies Engage Multiple Mechanisms

Evidence suggests that other methodologies can enlist an equally diverse mixture of mechanisms. For example, consider the prototypic example of single stimulus learning, habituation. At a behavioral level, habituation is said to occur when the repeated presentation of a single stimulus brings about a decrease in its behavioral and/or psychological effect. Again, the hope for a simple mapping between methodology and mechanism breaks down. Multiple mechanisms can bring about a decrease in response amplitude and distinct functional systems abide by qualitatively different rules. Short-term mechanisms can produce a rapid decrement in response amplitude, but yield little savings over time (Whitlow, 1975). A long-term form that relies on a kind of associative learning can preserve the behavioral effect over days or weeks. Whereas short-term habituation grows stronger with a decrease in the inter-stimulus interval, the long-term form gets weaker (Groves et al., 1970; Wagner, 1976; Whitlow, 1975). Habituation within the spinal cord seems governed by a short-term mechanism (Groves et al., 1970; Joynes & Grau, 1996). Long-term habituation may require more advanced neural structures (Leaton, Cassella, & Borszcz, 1985). If we move to a biological level of analysis, we discover that the diversity multiplies once again. Functional mechanisms that appear to differ quantitatively, but not qualitatively, seem mediated by distinct neurophysiological events (e.g., homosynaptic or heterosynaptic depression; a decrease in spike duration; inactivation of Ca^{++} channels; or a decrease in synaptic contacts; Sahley & Crow, 1998).

A similar situation arises in studies examining how organisms encode response-outcome relationships. Historically, the term "instrumental conditioning" has its roots in the reflexive tradition of Thorndike, Konorski, and Hull (Hilgard & Marquis, 1940). In drawing the distinction between Pavlovian (Type I) and instrumental (Type II) conditioning, Konorski relied on an example of learning in which an elicited leg movement was modified by a response-outcome relation and posited that the consequences of this learning could be described by the strengthening of an underlying reflex (Konorski & Miller, 1937). Below, we describe an example of spinal learning that is completely consistent with this view. We recog-

nize, though, that there are many examples of instrumental learning that have qualities that appear to go beyond the modification of a pre-existing reflex. As Skinner (1938) noted, the most sophisticated examples of behavior allow for a fair degree of flexibility in terms of both the behavioral solution and reinforcer. Such unconstrained learning/performance would allow an organism to respond to a novel situation in a variety of ways, executing either an increase or decrease in a target response and performing the behavior for a wide range of reinforcers (both appetitive and aversive). Moreover, as Skinner (1938) noted, it is often difficult (perhaps impossible) to confidently identify the functional stimulus in such complex situations, an observation that led him to distinguish between operant and respondent behavior.

Research suggests that neurons within the spinal cord can support a simple form of instrumental learning. In these studies, a spinally transected rat (the master subject) receives shock to one hind leg whenever that leg is extended (response contingent shock). Over time, master rats learn to maintain the trained leg in a flexed position that minimizes net shock exposure (Grau, Barstow, & Joynes, 1998). To demonstrate that this behavioral change depends on the respone-outcome (shock) relation, other rats are experimentally coupled (yoked) to the master rats and receive shock at the same time and for the same duration independent of limb position (noncontingent shock). Yoked rats do not exhibit an increase in flexion duration. Further, they fail to learn when later tested with response contingent shock applied to either the previously shocked (ipsilateral) or untreated (contralateral) leg (Crown, Joynes, Ferguson, & Grau, 2002), a learning deficit that is reminescent of learned helplessness (Maier & Seligman, 1976). As observed in intact subjects, prior exposure to response-contingent (controllable) shock can immunize rats against the adverse consequences of noncontingent (uncontrollable) shock, and a form of behavioral therapy can attenuate the deficit induced by uncontrollable shock (Crown & Grau, 2001). It has also been shown that disrupting response-outcome contiguity undermines learning and that the key reinforcing event is shock onset (Grau et al., 1998). On the basis of these findings, we concluded that spinal cord neurons are sensitive to response-outcome relations, the defining attribute of instrumental learning.

As suggested elsewhere, we use the term instrumental in the most general sense, to refer to learning situations in which the change in a neurally-mediated response depends on a response-outcome relation (Grau, 2001; Mackintosh, 1974). We assume that this type of learning can occur across a wide range of situations, including cases where the target behavior can be described in reflexive (elicited) terms. From this perspective, instrumental learning represents a broad behavioral category that includes operant behavior and instances of response-outcome learning that Skinner would classify as examples of respondent behavior. The corrolary to this is that operant behavior represents a more advanced form of instrumental behavior and meets additional criteria (e.g., neither the nature of the response nor the reinforcer are constrained; Grau et al., 1998). For present purposes, there are two important implications. The first is that multiple mechanisms can produce a system that is sensitive to a response-outcome relation—there are likely distinct forms of instrumental learning. The second is that the type of instrumental learning observed in simple systems may lack properties exhibited by more sophisticated examples of this category of learning. Indeed, we doubt that the simplest

forms instrumental conditioning would meet Skinner's criteria for operant learning.

Many today have forgotten the significance of these early distinctions and behave as if the terms instrumental and operant are synonyms. With the terms used freely, neurobiologists assume that any preparation that demonstrate a sensitivity to a response-outcome relation can be used to elucidate the mechanisms that underlie operant learning. Again, a simplistic isomorphism is being assumed, wherein a single methodology (the imposition of a response-outcome relationship) is thought to engage a unitary functional mechanism. Once more, research on spinal learning suggests that this view obscures important mechanistic distinctions (Grau et al., 1998).

Reports that the spinal cord is sensitive to response-outcome relations first emerged over 30 years ago and gave rise to considerable controversy (Chopin & Buerger, 1976; Church & Lerner, 1976). According to one camp, operant behavior is the province of the brain (with most assuming that the forebrain is essential) and any claims of operant learning in the absence of the brain (whether they be in the spinal cord or an invertebrate) are a ruse. Contemporary members of this camp are often surprised to learn that we agree. The issue is not whether the spinal cord can support operant learning, but rather, whether response-outcome relations can be encoded in multiple ways. If a simple isomorphism is assumed, and just one type of encoding deemed true, trouble arises. But if we assume instead that such an important environmental puzzle can be solved in multiple ways, an alternative solution can be seen as an example of biological ingenuity rather than an experimental anomaly.

These problems are compounded when nonlearning theorists read the standard texts on learning. Seeing that learning theorists often pay no attention to the distinction between instrumental and operant learning (for it violates our desired isomorphism), they assume that any demonstration of instrumental conditioning implies a form of operant learning. It does not, and it is likely that such a relaxed application of the terms has led to needless debates and many exaggerated claims. Learning about the neurobiological mechanisms that underlie the modification of head waving in *Aplysia* (Cook & Carew, 1986) may tell us a great deal about the mechanisms that underlie instrumental conditioning, but little about the mechanisms that underlie the behavioral felxibility inherent in operant behavior.

Implications of a Neural-Functionalist Approach

Theories of associative learning have yielded many important insights and this concept will likely remain a cornerstone of learning. Yet, a complete theory of learning must speak to all of the ways in which experience can alter behavior and this will require a more open attitude and a shift in focus. The first step requires that we clearly distinguish methodology from mechanism and recognize that most environmental puzzles can be solved in a variety of ways. Second, we must change our attitude, taking associative learning off the pedestal and giving other mechanisms equal weight. Finally, our approach to teaching should reflect the heart of the field. For many of us, these considerations lead to a focus on mechanism, at both a functional and a neurobiological level. We know that multiple biological mechanisms exist and that Pavlovian relations can be encoded by qualita-

tively distinct systems, a form of evolutionary *homoplasy* (convergence). Such complexity cannot be addressed within a system that attempts to maintain a simple isomorphism between methodology and mechanism. 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, not the tools used to take it apart.

Our view assumes a more complex system (see Figure 4), with a number of functional mechanisms capable of encoding an environmental relation. We further assume that each function may be accomplished by multiple biological mechanisms. For example, both the facilitation of neurotransmitter release (a presynaptic mechanism), and its physiological impact on the recipient cell (a postsynaptic mechanism), can bring about an increase in response magnitude within a conditioned pathway (Kandel, Swartz, & Jessell, 2000). Finally, we assume each biological mechanism contributes to multiple functional systems. Few biological mechanisms are likely wedded to a single functional system. Rather, nature can use the same biological bricks for multiple purposes, to build a variety of learning systems (Hawkins & Kandel, 1984).

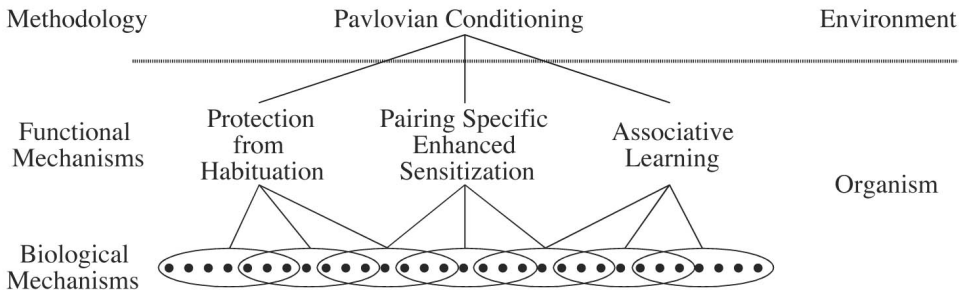

Figure 4. A neural-functionalist perspective on Pavlovian conditioning. It is assumed that a Pavlovian relation within the environment can be encoded by multiple functional mechanisms within the organism. Functional mechanisms that operate in a formally similar fashion can be constructed from different biological components (symbolized with black dots) and the same biological parts may contribute to multiple functional systems.

The framework we are suggesting will remind many of the behavior system approach suggested by Timberlake and his colleagues (Timberlake, 1999; Timberlake & Lucas, 1989). Indeed, the characterization of a behavior system can be viewed as an essential step towards the development of a neural-functionalist approach. What distinguishes our view is that it seeks links to the underlying neurobiological machinery. There is also a greater relative emphasis on the derivation of abstract formal systems that can be used to characterize how the organism adapts across distinct motivational systems. A behavior system tells us how the functional components are put together to build a motivational system. Our approach focuses more on how to describe the tokens of this behavioral language and their neurobiological underpinnings.

If we can identify the appropriate tokens, and their various biological instantiations, we could derive a theory with considerable explanatory power. For

example, specifying the type of functional system involved would constrain the range of biological mechanisms considered. If pairing-specific enhanced sensitization is routinely mediated by one of *N* biological mechanisms, identifying a new example of this learning would immediately tell us that it too is likely mediated by one of these mechanisms. Similarly, having characterized the functional properties of each mechanism, the identification of an example of learning as a case of protection from habituation would allow us to predict its capacities and limits across a range of situations. Further, knowing that two behaviors are mediated by distinct functional systems could provide information about their potential interactions. An interesting example of this was described by Domjan and his colleagues (reviewed in Domjan, 2000), who showed that stimulus competition (blocking) may not occur across distinct functional systems.

Taking Neural-Functionalism to the Classroom

The field of learning has had, and continues to have, a major impact on how we study and interpret the phenomena of learning, both in the laboratory and the classroom. The field is built upon a solid methodological foundation and there is no need to give this up. Our concern is with the future of the field and the framework we use to organize material. We believe that this framework must be structured by mechanism rather than methodology. In practice, many researchers seem to have already moved in this direction. In the laboratory, and their published work, researchers often focus on mechanism and use a host of methodological and biological tools to explore its properties. But in the classroom, most still present the field in the traditional fashion, organizing material by methodology rather than mechanism.

A typical course on learning begins with an introductory section that provides an overview of the historical/philosophical factors that led researchers down the current path. Material on single stimulus learning, Pavlovian conditioning, and instrumental/operant conditioning, is then discussed in turn. Close to 2/3 of the course may focus on the latter two issues. In recent years, a 2-3 week section on cognitive mechanisms is often tagged on to the end. The lead author knows this organization well, having used it for 15 years. 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. 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.

Our current paradigm for teaching learning seems even stranger when you compare the field to other areas of experimental psychology. Imagine a course on cognitive psychology that organized the material by methodology. The class might begin with a week-long discussion of the impact of presenting a visual or auditory mask followed by two weeks on distractor effects. Next, the course could proceed

with a few weeks on list learning and paired associates. Our guess is that student interest, understanding, and retention, would plummet. Material about distractor effects is of interest to students for the same reason it is of interest to cognitive psychologists—it tells us something about how our minds work. Further, cognitive psychologists recognize that multiple operations can be used to infer a mechanism and that a single mechanism may be brought into play across a variety of distinct methodologies. For these reasons, it makes far more sense to organize the material by mechanism, not by methodology. We suggest the same is true for the field of learning.

Updating a course is a time-consuming endeavor. Yet, we must recognize that this is our future. If the field of learning is to remain a central player within psychology and neuroscience, and attract new students, we need to convey the excitement that now exists within the field. Of course, there are new discoveries being made about the functional mechanisms, but we would argue that for every behavioral discovery, there are 10 new insights regarding the underlying biology. Sadly, many students see little of this excitement and walk away from a course on learning with an acquired aversion to the field.

Conclusion

Early in the development of any field, researchers must define the circumstances under which their phenomena occur—the essential causes (Killeen, 2001). Learning researchers have done this and in the process, developed some powerful methodologies for studying learning and memory. The difficulty is that these methodologies have acquired a life of their own, being used to classify new phenomena and frame debates. We have argued that the mapping from methodology to mechanism does not involve a simple isomorphism. Creatures have evolved multiple mechanisms to solve a common environmental puzzle and there is no reason to ordain just one as *true* (Tolman, 1949). A full understanding of the phenomena requires that we characterize all the mechanisms involved, how they are related, when they come into play, and the underlying biological mechanisms. Understanding the essential causes is not sufficient; we also need to characterize the formal, material, and final causes of behavior (Killeen, 2001). This requires a new approach to the study of learning, both in the laboratory and the classroom.

Some non-learning theorists will surely greet our assertions with a self-satisfied, "but of course." Indeed, for many studying the neurobiology of memory, neurofunctionalism simply provides a label for a perspective they have held for many years. Similarly, those taking a comparative perspective already teach their material organized by mechanism, not methodology. The trick will be getting our learning colleagues to follow suit. A mechanistically-oriented approach to learning has the potential to integrate the field with neuroscience, cognition, and ethology. If our focus and organizational framework remains centered on methodology, we will come to be viewed as a cabinet of behavioral methodology that can be consulted as needed. By shifting our focus to mechanism, we gain a forum for discussing a much wider range of new research findings and help assure that the field will remain a central player.

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