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## Book review

### **Proteus Caught in A (Neural) Net. Animal Learning and Cognition: A Neural Network Approach**

Nestor A. Schmajuk, Cambridge Univ. Press,  
Cambridge, 1997, v + 340 pp. \$140.00 (hardback) ISBN  
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In *Animal learning and cognition: A neural network approach*, Schmajuk bravely attempts to construct neural-network models that explain the collective knowledge of the past century of research on learning and cognition in animals. The major goal of this book is to provide a unified theory of the biological basis for associative and cognitive processes in animals. At its core, the model relies on a single mechanism, referred to as the “delta rule,” by which brains build associations between cues and the events they predict. The delta rule has frequently been invoked by learning theorists for the past half century (e.g., [Bush & Mosteller, 1955](#); [Mackintosh, 1975](#); [Pearce, 1987](#); [Rescorla & Wagner, 1972](#)) and has appeared as the central mechanism of many other connectionist and neural-network models (e.g., [Pearce, 1994](#); [Rosenblatt, 1962](#); [Sutton & Barto, 1981](#); [Widrow & Hoff, 1960](#)). In the framework of neural-network models, associations manifest themselves as neural connections and the representation of information about the world is distributed across the network of connections. As the book unfolds, Schmajuk reveals how the instantiation of a modified Rescorla–Wagner delta rule as the basis for establishing neural connections can be used to build larger and more complex systems that can handle a variety of complex cognitive behaviors such as higher-order conditioning and occasion setting, pattern learning, choice behavior, and animal navigation.

There is much to commend about this book. Foremost, it provides a new and powerful type of modeling that has received relatively little attention in the field of animal learning and cognition. Neural-network and connectionist approaches to modeling have been a boon to human cognitive psychologists and behavioral neuroscientists. These models have guided the investigation of brain mechanisms of behavior and continue to guide the search for new discoveries.

The broad scope of this book brings a refreshing change to the field of animal learning and cognition. The field is currently fragmented into numerous independent “camps,” each with uncompromising adherents to individual theories or perspectives, and there is relatively

little open communication between camps. Even differences in ideology can act as barriers to the exchange of scientific information, such as Behaviorism vs. Cognitivism vs. Reductionism. As a result of the cliquish nature of experimental psychologists (and scientists in general), there are a large number of theories or theoretical frameworks that have been independently put forth to explain limited sets of data. However, the diverse behaviors investigated by these separate avenues of research share many fundamental underlying processes. For example, the rules governing associative acquisition and cognitive mapping are strikingly similar, indicating that these processes may result from the same behavioral, and perhaps even the same neural, mechanisms. Thus, Schmajuk’s attempt to cast his theoretical net across a diversity of research pools is particularly noteworthy and commendable. And he could not have picked a more powerful and widely used mechanism than the delta rule upon which to build a unified theory of animal cognition. The delta rule is a simple, error-correcting learning mechanism that builds associations between a cue and an outcome as a function of how well the cue predicts the outcome. By invoking the delta rule as the basic mechanism in his neural-network models, Schmajuk is able to construct elaborate networks to account for the diversity of behavioral phenomena from the entire field of animal learning and cognition. For example, the book contains networks designed to explain simple associative learning, including competition between predictors; complex associative processes, such as occasion setting and configural patterning; operant behavior; and cognitive processes, such as spatial navigation and the use of mental maps.

Another noteworthy feature of Schmajuk’s models is that they compute real-time predictions of animal behavior. Most contemporary associative models make predictions only on limited, and often contrived samples of time, such as by each trial or upon the occurrence of an external event. Some of the more eminent trialwise models include those of [Rescorla and Wagner \(1972\)](#), [Mackintosh \(1975\)](#), [Pearce and Hall \(1980\)](#), and [Pearce \(1987\)](#). But brain processes do not cease to operate during the time between trials or events. Thus, these models do not accurately capture the true nature of the ongoing, dynamic neural activity that results from learning experiences. Currently, [Wagner’s \(1981\)](#) Sometimes Opponent Processes (SOP) model is one of the few learning models that provides real-time predictions

regarding the learning process. However, Wagner's model, as currently stated, is limited to the set of simple associative processes. Therefore, Schmajuk makes a major advance in offering a real-time model to account for the entire range of animal cognition.

Finally, I applaud Schmajuk's praiseworthy attempt to wed learning and cognitive processes with the presumed underlying neural processes. Most contemporary theories of animal learning and cognition are silent about how they map onto the nervous system, or at best make speculative guesses as to which brain areas may be responsible for certain processes and computations. However, behavior is a direct product of physiological processes, so eventually psychologists will need to have biologically plausible models of animal behavior. For instance, the field of human cognitive science has undoubtedly benefited from our current understanding of the human brain, with progress from research in perception, cognition, psychopharmacology, behavioral and cognitive neuroscience, and clinical psychology weaving together a coherent picture of the human brain and how it functions.

Although there is much to commend about the book, there are also many problems with it. I will address three major criticisms I have of the book: (a) the neural-network models presented in the book are not biologically plausible; (b) the book is very difficult to read, and therefore restricts itself to a narrow audience, and the model itself is difficult to use; and (c) the model fails to account for many important animal learning data.

*The neural-network model is not biologically plausible:* Is the field of animal learning and cognition ready to be accurately modeled in a biologically plausible way or are we currently better off with theories of behavior that do not tie themselves so specifically to brain processes? Although great strides have been made in our understanding of how the nervous system works and the brain locations of general behavioral processes, we are still far from understanding how simple associative processes are truly represented in the brain (e.g., Shors & Matzel, 1997, 2000). This calls into question one of the core assumptions of the book, that "associative learning reflects changes in the efficacy of synapses" (p. 9). Though changes in synaptic efficacy may be one result of conditioning treatments, it is by no means clear how such changes are directly responsible for the molar behavior observed in the laboratory or in the field. There is growing evidence for the important role intracellular and molecular mechanisms play in many aspects of behavior, including associative learning and the timing of conditioned responses (e.g. Fisher, Fischer, & Carew, 1997; Gibbon, Malapani, Dale, & Gallistel, 1997; Meck, 1996). It is clear that the search for Lashley's engram is still closer to a pipe dream than to a reality, and may even incorrectly conceptualize how brain processes contribute to behavior. We know that

there are many ways the brain probably *does not* produce behavior. For example, there are many convincing studies showing that extinction (i.e., non-reinforcement of a conditioned stimulus) does not result in the unlearning of a conditioned stimulus–unconditioned stimulus (CS–US) association. Rather, models of extinction as an inhibitory process or as a type of memory-interference process fit the empirical data far better. The delta rule, which lies at the heart of Schmajuk's neural-network approach, treats extinction as unlearning. Therefore, a core feature of Schmajuk's model is biologically *implausible*.<sup>1</sup>

*The model is difficult to understand and use:* *Animal learning and cognition* presents a unified theory and therefore covers a wide range of disciplines. Any book that traverses separate disciplines should provide a clear and readable map of all terrains covered. Having expertise in the field of experimental psychology but not in behavioral neuroscience, I approached this book with the hope of learning something about neural-network modeling and its application to the type of molar animal behavior I study. The book, however, assumes expertise in both fields. I struggled with the many details of the model and how it handles behavioral phenomena. In my opinion, this link between the mathematics of the model and animal behavior receives too little attention. What is worse, the model is very complex and requires the use of a lot of mathematics. The model uses at least 11 parameters ( $\lambda_{US}$ ,  $V_{i,US}$ ,  $B_{US}$ ,  $\tau_i$ ,  $K_i$ ,  $R$ ,  $\theta$ ,  $X_i$ ,  $Z_i$ , Novelty, and  $H_{i,j}$ ), many of which are enumerated in the simulations presented in the text. For example, the simulation on p. 73 contains 19 separate values of  $K$  alone! Although the density of parameters allows the model to account for a large spread of behavioral phenomena, it interferes with comprehension and ease of use. The mathematics should serve as a tool to better understand psychology, not interfere with it or serve as a replacement. The fact that a simulatable version of the model on computer disc is included with the book only attests to the complexity of the model's computations. This complexity compromises the model's heuristic value and its ability to make crisp, clean predictions, both hallmarks of the Rescorla and Wagner (1972) model. In fact, by changing parameters or connections, the model is able to be reworked to explain almost any experimental outcome. Protean models are difficult to grasp and lack utility. Science advances not through clever curve fitting

<sup>1</sup>Schmajuk's model is biologically implausible in another way, as well. There is much evidence that each distinct area within the brain is multifunctional and contributes to many processes. However, Schmajuk proposes that specific areas of the brain only perform specific computations. For example, he proposes that the entorhinal cortex of the hippocampus only performs one computation whereas the hippocampal CA3/CA1 cells only perform another computation. There is little support for such an extreme modular view.

but by promoting falsifiable models that inspire the search for new phenomena and knowledge.

Likewise, non-experts of the field of animal learning and cognition will only be confused by the terse coverage of the behavioral phenomena the model purports to explain. For example, the contents of at least one-half of a learning textbook are squeezed into 12 pages (pp. 20–31) of text. And many important experimental results in learning theory from the last 20 years receive no attention (probably because they are problematic for Schmajuk's model). Furthermore, the brusque exposition of the model's successes do not sufficiently connect the model with the behavior of real living animals. Curve after curve of simulated data is presented to show that the model can produce all the basic phenomena of animal learning and cognition. Nevertheless, one wonders if all that is being done is curve fitting to limited sets of data. Much of the actual data the simulations are compared to come from experiments on rabbit eye-blink conditioning. But will the model accurately predict these effects in other species, such as rats, pigeons, or humans; and using different paradigms, such as the conditioned emotional response, taste-aversion learning, or appetitive conditioning? There are many discrepant findings in the literature that result from the use of different species or tasks. It does not seem likely that Schmajuk's model will be able to handle such contradictions without resorting to a fair bit of parameter manipulation.

Because the book is so impenetrable, it can only fail to reach a broad audience—a major shortcoming. Instead, the book is accessible primarily to scientists with a strong background in both classical conditioning and neural-network modeling. It is written for those who can look at mathematical equations and immediately say to themselves “Ah! I see what is going on.” Perhaps the problem with the book is that it largely reflects a thorough archeology of Schmajuk's publishing history. It is as if each chapter was independently adapted from a few narrowly focused articles published in technical books or journals. These articles present specially designed neural networks tailored to specific behavioral phenomena, such as the acquisition and extinction of associations (Schmajuk & DiCarlo, 1991b), latent inhibition (Schmajuk, Lam, & Gray, 1996), escape and avoidance learning (Schmajuk, 1994; Schmajuk, Urry, & Zanutto, 1998), stimulus configuration and place learning (Schmajuk & Blair, 1993), occasion setting (Schmajuk, Lamoureux, & Holland, 1997), and cognitive maps (Schmajuk & Thieme, 1992; Schmajuk, Thieme, & Blair, 1993). Other articles provide accounts of the presumed physiological underpinnings of such behaviors, such as how the hippocampus modulates classical conditioning (Schmajuk, 1989; Schmajuk & DiCarlo, 1991a; Schmajuk & Moore, 1985, 1988). The book reads more like a collection of articles filled with technical jargon and

complicated equations, rather than as a textbook for the average psychologist or science-minded layperson. There does not seem to be any attempt to bring the material down to “our” level.

It may seem to someone without a strong background in animal learning and cognition that these neural networks provide accurate accounts of animal behavior. The model (or suite of models, this is never made clear) is presented as a statement of fact. The perception that the theory is iron-clad is strengthened by the inclusion throughout the text of many simulated curves that match the empirical data almost perfectly. However, a quick look beyond the cited data shows just how limited the book's successes really are. For example, there are many findings in the classical conditioning literature that contradict not only individual neural-network models presented in the book, but the very foundational assumptions that they are built on. Schmajuk largely ignores empirical data that are inconsistent with the model—data that reveal a number of theoretical flaws.

*The model fails to account for many important animal learning data:* I will address four representative flaws in turn. Many of these flaws result from using the Rescorla and Wagner (1972) learning algorithm to compute changes in connection weights (i.e., associations). While the Rescorla–Wagner model has enjoyed many successes in the past 30 years, there are clearly many failings with it (see Miller, Barnet, & Grahame, 1995). For example, the Rescorla–Wagner model (and therefore, Schmajuk's neural-network model) treats extinction as the unlearning of the original CS–US association. Evidence contradictory to this “extinction as unlearning” view comes from demonstrations that extinction is reversible in the absence of additional CS–US pairings. Some examples include spontaneous recovery of responding to the extinguished CS (Pavlov, 1927), external disinhibition (Pavlov, 1927), and “renewal” of excitatory responding to the extinguished CS when tested in a different context than that in which extinction took place (e.g. Bouton & Bolles, 1979). An alternative explanation for the effects of extinction is that it produces new learning that interferes with retrieval and expression of the original learning (e.g. Bouton, 1993). Thus, a realistic neural model of behavioral extinction should not treat extinction as unlearning but in some other way, such as response inhibition or the contextual control of responding.

Schmajuk's model (as does that of Rescorla & Wagner, 1972) also incorrectly predicts that a conditioned inhibitor can be extinguished through non-reinforced presentations of the inhibitor alone. Schmajuk erroneously claims that his model predicts no extinction of inhibition (p. 44). He claims that the aggregate prediction of the US ( $B_{US}$ ) cannot become negative and therefore a conditioned inhibitor cannot be extinguished via conventional extinction treatment.

However, his model clearly states that the predicted US is equal to the sum of the associative strengths of the CSs currently present. Because the inhibitor has a negative associative strength ( $V_{CS-,US} < 0$ ), when it is presented in the absence of an excitatory CS, the predicted US is negative (i.e.,  $B_{US} = -V_{CS-,US}$ ). Therefore, contrary to what Schmajuk claims, inhibitor-alone presentations result in an *underexpectation* of the US (i.e.,  $\lambda_{US} = 0$ ,  $B_{US} < 0$ , therefore,  $\lambda_{US} - B_{US} > 0$ ), thereby extinguishing the inhibitor's negative associative strength. This prediction is not supported by the data (e.g. DeVito & Fowler, 1986, 1987; Hallam, Grahame, Harris, & Miller, 1992; Williams, Travis, & Overmier, 1986; Zimmer-Hart & Rescorla, 1974) which show that extinction treatment given to an inhibitory CS, if anything, increases inhibition.

The model also fails (as does that of Rescorla & Wagner, 1972) to adequately account for cue-competition effects, such as overshadowing, blocking, overexpectation, and the effect of relative stimulus validity. Cue-competition effects result when the target CS is conditioned in compound with a more salient or more valid predictor of the US, resulting in attenuated responding to the target CS (relative to a control group lacking the competing predictor). In the framework of Schmajuk's model, the competing CS prevents the target CS from acquiring a strong association with the US. However, many recent demonstrations contradict this prediction, indicating instead that a strong, but latent, target CS–US association is acquired during cue-competition training (e.g. Balaz, Gutsin, Cacheiro, & Miller, 1982; Batsell, 1997; Blaisdell, Bristol, Gunther, & Miller, 1998; Blaisdell, Denniston, & Miller, 1999a, 2001; Blaisdell, Denniston, Savastano, & Miller, 2001; Blaisdell, Gunther, & Miller, 1999b; Cole, Barnet, & Miller, 1995b; Cole, Denniston, & Miller, 1996; Cole, Gunther, & Miller, 1997; Kasprow, Cacheiro, Balaz, & Miller, 1982; Kaufman & Bolles, 1981; Kraemer, Lariviere, & Spear, 1988; Matzel, Schachtman, & Miller, 1985). Such effects are better explained by performance-focused models of compound conditioning (e.g. Denniston, Savastano, & Miller, 2001; Miller & Matzel, 1988) where the competition between multiple predictors of an outcome occurs at a late stage of processing, such as in the retrieval or expression of learned associations.

Finally, according to Schmajuk's model, second-order conditioning (by which a cue comes to produce a conditioned response through pairings with an established CS) must always be inferior to first-order conditioning (p. 49). However, there are a number of published demonstrations showing that second-order conditioned responding can be *superior* to first-order conditioned responding (e.g. Barnet & Miller, 1996; Cole, Barnet, & Miller, 1995a). Related to this failure of the model is its inability to deal with simultaneous and backward excitatory conditioning. Although simulta-

neous and backward conditioning procedures often result in little or no excitatory conditioned responding which is consistent with the model, there have been many successful demonstrations of strong excitatory responding following backward conditioning (e.g. Barnet & Miller, 1996; Cole & Miller, 1999; Heth, 1976; for a review see Spetch, Wilkie, & Pinel, 1981).

I must commend Schmajuk for his diligence to construct a model that attempts to bridge the gap between behavioral processes and brain functioning, as well as to unify the broad scope of research in animal learning and cognition. The model's explanatory power capitalizes on the successes of the Rescorla and Wagner (1972) model and extends them to new behavioral phenomena and domains. The model also makes real-time predictions of behavior, which is better than most contemporary learning models can do. The field of animal behavior and learning theory can only move forward with such bold attempts. However, the model shares the many failures as well as the successes of the Rescorla–Wagner model. These failures weaken the plausibility that the model accurately represents the mechanisms by which behavioral processes are mapped onto the brain. Although the Rescorla–Wagner model lacks biological plausibility, its elegant simplicity has allowed it to be a driving force of empirical research and inspire new theories of animal learning for the past 30 years. Conversely, the neural-network models proposed in *Animal Learning and Cognition* are anything but simple and straightforward. Furthermore, the fact that the book is written at a highly technical level, with relatively little translation from mathematical speak to plain English, makes for difficult reading by lay scientists and experimental psychologists alike. Unfortunately, all of these factors diminish the potential impact this book may have on the field of animal learning and cognition. Nor would it be a good choice as a textbook for a graduate-level course on the subject. Nevertheless, this book should prove a valuable resource for mathematically sophisticated psychologists and neuroscientists who wish to approach animal learning and cognition from a neural-network perspective.

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