

The Interface between Learning and Cognition: The 2010 Winter Conference on Animal Learning and Behavior Focus Session

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The study of human and animal behavior in psychology is almost always framed at either the associative or the cognitive level of explanation. Despite continued debate between proponents of each approach, we appear to be no closer to a consensus view than we were when the debate began in earnest in the 1960s. Could it be that the two levels of explanation are irreconcilable? Or is it possible that both frameworks are useful, though incompatible? Perhaps these frameworks merely account for the same behaviors but at different levels of explanation, as characterized by hardware-software or genotype-phenotype analogies. This special issue provides a venue for contemporary scientists involved in this debate to express their views, and follows from a Focus Session of the same title held at the 2010 meeting of the Winter Conference in Animal Learning & Behavior.

“Nevertheless the difference in mind between man and the higher animals, great as it is, certainly is one of degree and not of kind.” (Darwin, 1871, pp. 101)

And with this resounding — some would say portentous — statement, so Darwin launched a debate that continues to reverberate in the halls of academia to the present day. What was Darwin really claiming with this statement, and does evidence against continuity really stand as evidence against Darwinian principles of evolution? I doubt any scholar of animal psychology and behavior, no matter where they stand on the issue of mental continuity between humans and the rest of the animal kingdom, envisions anything other than the Darwinian mechanism of evolution by natural selection as the driver of the origins and evolution of all attributes of mind, no matter how uniquely specialized to the human animal they may be (e.g., Penn & Povinelli, 2007). We can trace the phylogenetic and paleontological evidence for numerous examples of shared homology. The common skeletal template of forelimbs shared among all vertebrates is a textbook example. Such clear examples of shared homology do not, however, indicate that uniquely derived structures, found only in a small group of taxa or even in only a single extant species are the product of anything but a Darwinian mechanism of evolution. Only the elephant has a long trunk. Only the giraffe has an extremely long neck. Only humans among the primates are habitually bipedal. It is uncontroversial that these traits are the result of a process of heritable phenotypic variation with selection based on fitness consequences. This same analysis can be applied to behavioral phenotypes, and their underlying psychological and neurological processes. On the one hand, learning and memory systems are shared far and wide among vertebrates and invertebrates alike. Attentional and perceptual processes uncannily similar to those demonstrated by humans can also be found in our vertebrate cousins, such as rats and pigeons (Blough & Blough, 1997; Cook,

2001). On the other hand, only humans exhibit certain behavioral traits, such as language and higher-order relational processes. These behaviors seem to go well beyond any similar nascent abilities in our closest living relative — the chimpanzee (Penn & Povinelli, 2007). Yet, even proponents of the mental discontinuity between humans and our closest living relatives (e.g., Penn, Holyoak, & Povinelli, 2008) would agree that the blind process of Darwinian evolution designed these specialized mental processes. These behaviors serve as specialized adaptations that are uniquely human. They are a collection of autapomorphies of the mind. Presumably, if we could trace the lineage of these uniquely human abilities backwards in time from their current form to that found in our common ancestor with chimpanzees, we would observe a reversal of the evolutionary sequences that took place in the hominid lineages that brought these psychological traits to their current state. We would see the sometimes gradual, perhaps often sudden changes that connect the current psychological and neural processes to nascent processes in the human-chimp common ancestor some six or seven million years ago. The set of mental abilities found in this hypothetical creature served as pre-adaptations that enabled the road to modern human cognition to be traversed.

With this background and common ground in mind, we can now look at the history of comparative psychology to find the origin and lineage of the debate surrounding the role of cognitive models in psychology. We'll start with Romanes, (1882) who took the mantle from Darwin and pushed it beyond the limits of credulity. His book *Animal Intelligence*, replete with passionate stories standing in testament to the unfettered humanness of all animals in the animal kingdom, prompted a severe and swift response from his more grounded (critically-minded) contemporaries. C. Lloyd Morgan (1894) unleashed his famous canon to be levied at any and all cases of animal intelligence to seek the most phylogenetically ancient and therefore likely explanation. It is only by empirically ruling out (aye, there's the rub in the current debate) these more ancient (plesiomorphic) behavioral processes that we should then tentatively advance a more derived, evolutionarily-younger (apomorphic) psychological explanation.

Thorndike (1898) also levied an attack on Romanes' anecdotal approach, finding many flaws in the method. He instead advocated the experimental approach in which the actual psychological processes can be studied and dissected in well-controlled settings where they can be repeatedly poked and prodded through the empirical methods of science. Thus, was born the science of animal cognition and its best practices.

Although the stark objectivist movement of North American behavioral psychology banished questions about animal mind and intelligence to the trash heap, a young behaviorist upstart named Tolman (1932) salvaged cognition from the trash heap and polished its tarnished surface to a shiny new patina. The debate between behaviorist and associative psychologists on one side, and the new, young cognitive psychologists on the other got going in earnest. But armed with empirical methods of experimental science, these young guns held their own quite well.

From the almost-cliché Cognitive Revolution of the 1960s and 1970s emerged a strong, vibrant science of comparative cognition that continues to grow in strength and scope. Witness the birth of scientific societies such as the Comparative Cognition Society, the Psychonomic Society, and the Society for

Neuroscience, where cognitive accounts of animal behavior are bandied about along with associative and other behaviorist accounts. In fact, associative theory has become infused with cognitive flavored theoretical concepts — especially Tolmanian concepts of expectancy and Pavlovian (1927) and Konorskian (1967) concepts of stimulus representation. The paper in the current issue by Andrew Delamater is testament to the power and nuance that representational accounts hold in building a rich description of animal behavior in associative learning paradigms.

Minds are designed by natural selection to map the world (what Gallistel, 1990, calls “functioning isomorphisms”) and build action plans that utilize these maps to achieve goals. Regularities in the world are perhaps the most critical features of the world to be mapped. Encoding regularities allows prediction and action to be effective. The literature on perceptual mapping systems (from the tonotopic layout of the basilar membrane of the auditory system, to place cells and grid cells in the hippocampus and surrounding cortex) speak to the importance the brain places (speaking metaphorically, of course!) on building accurate or at least useful (heuristic) representations of the world. So, too, do perceptual constancies, which reveal the important role of top-down interpretative processes in generating a unified, coherent perception of the world — the *umwelt* of the individual (von Uexküll, 1934/1957). Regularities can be found at more than just the level of direct perception, however. Many regularities, such as in patterns of events, must be abstracted over an individual’s many experiences. To be perceived, they must be repeatedly experienced until their presence can be detected and encoded. If there is a way for a brain to detect and encode the patterns generated by the hierarchical rules in the tasks described by Fountain and Doyle in this special issue, then that brain would be able to track these patterns and respond appropriately for reward. Moreover, abstracting the rules, patterns, or stimulus relations would allow the organism to respond to interpolations and extrapolations of the pattern to new elements, as well as to respond effectively and systematically to transformations of the physical elements of the pattern that preserve pattern integrity, such as reversing the ordering of steps through a sequence, or rotating locations in space around a common axis. Cognitive accounts of such abstract, cognitive maps that go beyond direct, immediate perception can provide a much more flexible, adaptive response to the world than models based on associative processes can afford (but see Urquioli (2008) for a beautiful example of the power of an associative model to predict sophisticated inferences with a cognitive flavor).

Does this analysis imply that any behavior that has been viewed from a cognitive perspective would not more readily and appropriately be explained from a less cognitive perspective — such as associative or non associative learning? In a word, no. In addition to the example provided by Urquioli, (2008), two additional examples illustrate the importance of debating levels of explanation, no matter what levels are involved. Our first example concerns Theory of Mind. The literature on Theory of Mind in nonhuman animals is rampant with over-interpretation (Povinelli & Vonk, 2003). It has become almost impossible to differentiate the predictions of a cognitive account for Theory of Mind (the attribution of mental states to others) from an associative or behaviorist account. All of the data from experiments on Theory of Mind in animals could be reinterpreted as the animal subjects being astute behavioral psychologists,

demonstrating an expert ability to predict another individual's behavior from their prior actions and surrounding conditions. Positing an intervening Theory of Mind to the subject serves as a compelling but explanatory vacuous intervening variable. It adds nothing to our understanding of the "mind reading" behavior. Our second example concerns a much less sexy topic, and one that may surprise some readers. It turns out that clever experiments revealed that the spinal cord is capable of Pavlovian conditioning. This research uses Pavlovian conditioning procedures in spinal rats — rats that have had the connection between the spinal cord and the brain severed—in which a tactile stimulus applied to the leg serves as a CS to a painful shock to the leg as the US. Such pairings result in increased tolerance to pain on a subsequent tail-flick test conducted on a hot plate (Joyes & Grau, 1996). As appealing as this account is, however, the actual learning mechanism involved has come under scrutiny. Rather than serving as evidence for a Pavlovian learning mechanism at the neural level (e.g., long term potentiation), Jim Grau has argued that it is more likely that the underlying neural mechanism instead involves prevention of habituation. That is, presentation of the US following the CS may prevent the habituation process to the CS from occurring. Thus, such a seemingly uncontroversial account of behavior at the associative level may actually be better accounted for by a non-associative processes which stands lower on the psychological (and phylogenetic) scale. This example should serve as a sobering reminder that the associative level of explanation is not uniquely privileged. Instead, even the associative account is to be questioned and tested against simpler, more phylogenetically ancient accounts. Levels of explanation are all relative. A science remains healthy by tolerating struggles and arguments both within and between levels of explanation. Scientific progress is most rapid where competing ideas are tested and discarded, or failed to be discredited, following the Popperian method.

In February, 2010, the Winter Conference in Animal Learning & Behavior arranged a Focus Session that provided a venue for the contributors to this special issue, all contemporary scientists involved in animal psychology research, to express their views in the debate regarding associative versus cognitive accounts of behavior. In addition, to facilitate an exchange among this special group that could further define the question and generate additional insights, there was a Research Seminar held at the end of the last day of the conference. A video recording of the Research Seminar, where we debated and discussed these issues in an open format, is available for viewing on Vimeo: Part 1 (<http://vimeo.com/19943758>) and Part 2 (<http://vimeo.com/20261769>). We encourage readers to view it.

On that video, those participating in the Seminar Session, members of the Focus Session plus other conference participants, going from left to right around the discussion circle are: Scott Barrett, Kevin Myers, Kenneth Leising, Stan Weiss, Melinda Beane, Andy Delamater, Andy Baker, Dominic Dwyer, Cameron Buckner, Jonathon Crystal, Aaron Blaisdell, and Steve Fountain. The 2010 Conference participants, Program and links to the *Interaction between Learning and Cognition* Research Seminar can be found on the WCALB website, <http://www.american.edu/academic.depts/cas/psych/wcalb.htm>.

This special issue provides an opportunity for each participant in the Focus Session to provide an extended response to the debate between the theoretical

paradigms of Association and Cognition. We kick off the special issue with a paper by **Buckner** placing the debate more deeply in its historical context. Buckner takes us on an illuminating journey through the history of comparative psychology, highlighting the pathways taken in conceptual and philosophical thought, and reflections on terminology (*cognitive, behaviorist, associative*). He places the contemporary associative versus cognitive debate in this historical context, addressing layers of relationships in admirable and thoughtful attempts at solutions to this dilemma.

The next three papers pursue an agenda that lean on associative interpretations of animal behavior and psychology research. In the first of these, **Dwyer** and **Burgess** examine three examples of complex “cognitive” behavior in rats — causal reasoning, sensitivity to stimulus absence, and relationship between effort and reward — and propose accounts in terms of simpler mental processes that stand “lower” on the psychological scale, to use Morgan’s phrasing, as sufficient explanations of these behaviors. They hammer on the importance of invoking Morgan’s Canon as a constant refrain whenever we are interpreting experimental results of animal behavior, especially of a cognitive flavor. They also raise the issue that the terms “simple” and “complex” are often ill defined, and thus can be misleading when used to evaluate explanatory accounts. We would add that the same caution applies to evaluating functional versus mechanistic accounts of behavior. For example, the expression “sensitive to ambiguity” may be interpreted as being about “mechanism” by some authors (e.g., Dwyer & Burgess), or about “function”— with no mechanism implied — by others (e.g., Fast & Blaisdell, in press). We must strive for clarity in defining our terminology.

Next, **Barberia**, **Baetu**, **Murphy**, and **Baker** contrast rational/propositional accounts of causal learning, on the one hand, with associative accounts, on the other. They build a compelling argument that rational context choices and causal structure can emerge from elemental associative processes. They back this argument up by presenting an auto-associative network that effectively provides a simple, bottom-up account of causal structure. In the third paper with an associative theoretical slant, **Delamater** reviews the growing literature on cognitive factors in Pavlovian and instrumental conditioning. He characterizes the distinction between representational concepts of expectancy and image of reward and highlights the fundamental differences between them. He also discusses how temporal, motivational, and sensory properties of reward involve different neural systems. After detailed exposition of nuances and intricacies of the associative structures that underlie the rich representational content of learning, he ends by contrasting associative from inferential reasoning (i.e., rational) approaches to instrumental action. He advocates a continued push to see just how far associative models will go to account for complex behavior. Ultimately his analysis mirrors the refrain of Dwyer and Burgess, and of Barberie et al., that bottom-up, associative processes may provide more plausible and preferred accounts of seemingly complex, rational behavior.

The final two papers in the special issue marshal evidence for a non-associative (or perhaps super-associative in the sense of existing on top of, and in addition to, associative mechanisms), cognitive processes that act independently of associative processes. In the first of these more cognitively-flavored papers,

Crystal probes the conceptual barrier between learning and cognition by raising questions about the necessity of new versus established methods, and of the types of hypotheses that can be tested to dissociate learning from cognition. He focuses on the conceptual, methodological, and hypothesis-testing perspectives to provide a scholarly context for framing the debate. Compelling examples drawn from rodent models of episodic memory, mostly from the author's own lab, are marshaled to validate an animal model of cognition that goes beyond mere associative-level accounts. The special issue concludes with a paper by **Fountain** and **Doyle**, who provide an excellent discourse on the separation of associative from rule-based processes in sequential learning tasks in rats. They present an interesting application of an associative model (that encodes paired-associates and generalization processes) to see how far it can account for sequential behavior. The model elegantly handles much of the data from early literature, but proves insufficient (in current instantiations) to account for data from the Fountain lab involving 8-alternative choice hierarchical structured sequences. What is clever about the sequential-choice paradigm is that it directly places reinforcement-driven associative processes and rule-based pattern abstraction in conflict. Thus, evidence for control by the set of hierarchical rules provides an existence proof for non-associative, cognitive processes at the computational level of analysis. Of course, this does not mean that such symbolic-like behavior is not subserved by some analog, associative neural network hardware, but simply that such hardware can generate computationally sophisticated behaviors that cannot be accounted for by contemporary associative algorithms (Blaisdell & Waldmann, in press; Penn, Holyoak, & Povinelli, 2008). This distinction is a game-changer when considering the set of all possible psychological processes that may account for any sophisticated behavior a rat or a pigeon (Garlick, Fountain, & Blaisdell, 2011) may produce. Of course, the simplest account is preferred, but given the full range of possible mechanisms, the simplest computational account may end up being the cognitive one; not an associative account which may approach the convoluted complexity of a Rube Goldberg machine by the time it is twisted into shape to "explain" the data.

The debate will undoubtedly continue, but hopefully the papers in this special issue prove constructive in framing the debate. A useful framework can provide guideposts for future research and thinking about these fundamental and fascinating issues involving the psychological control mechanisms of behavior. Ultimately, such mechanisms must interface with the underlying neurobiology. This is perhaps the biggest gap in our understanding of behavior, and the only way forward across this ominous divide is to bring all of our conceptual and methodological tools to bear. These are the exciting frontiers of our young science.

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