ASSOCIATIVE CONCEPT LEARNING IN ANIMALS: ISSUES AND OPPORTUNITIES

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> Though you stand on the top of the ladder of life, you must not kick out that ladder from under your feet. You must not deny your relatives, the other animals. Their history is your history, and if you kick them to the bottom of the abyss, to the bottom of the abyss you go yourself. By them you stand or fall. What you repudiate in them you repudiate in yourself — a pretty spectacle, truly, of an exalted animal striving to disown the stuff of life out of which it is made, striving by use of the very reason that was developed by evolution to deny the possession of evolution that developed it. This may be good egotism, but it is not good science.
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> (London, 1910, p. 508)

Famed author Jack London's pointed remarks from over a century ago help us frame and respond to the comments on our review of research into associative classes in nonhuman animals. The essence of our review is that associative learning principles are far broader and more relevant to human cognition than many in the field of behavior analysis may believe. Absolutely arbitrary stimuli become interchangeable with one another simply by virtue of having a common association with another stimulus, outcome, or response. That interchangeability can be seen to participate in superordinate categories, functional and stimulus equivalence classes, and, yes, even in human language. In our review, we detailed a range of phenomena in the nonhuman (mostly pigeon) literature demonstrating this interchangeability, and we described how a representational viewpoint has guided subsequent research examining the possible mechanisms of many of these phenomena.

Do we contend that all of the richness and complexity of human language and cognition can be conditioned in animals such as pigeons? Of course not, and we never made so bold a contention. But, the building blocks of those remarkable adaptive accomplishments can be observed in nonhuman animals if sufficiently inventive and incisive experiments are devised. Quoting from the Hughes and Barnes-Holmes (2014) commentary:

> Are there certain environmental and evolutionary conditions necessary in order to observe the emergence of complex forms of AARR (arbitrary applicable relational responding)…? Chasing these (and related) issues will not only lead to a better

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understanding of human and animal cognition but also identify important lines of fracture between and among species…. We look forward to increased collaboration and communication between…researchers as we continue to explore the commonalities that bind, and the differences that separate, humans from other species in the animal kingdom.

(p. 159)

Ironically, it is apparent from the commentaries that our advocating the involvement of associative learning processes (e.g., Colwill, 1994; Hall, 1991; 1996; Mackintosh, 1983; Overmier & Lawry, 1979; Schmajuk & Holland, 1998; Trapold & Overmier, 1972; Urcuioli, 2005) in the categorization of arbitrary stimuli has elicited varying degrees of pushback and skepticism. So, in the interest of increased communication, we address the salient concerns expressed in these commentaries.

Derived Relations and Behavior in Nonhuman Animals: Ontological Status

It is difficult to escape the impression that results that we have cited as evidence for associative concept learning in nonhuman animals are not regarded as such by others. For instance, in their commentary, Hughes and Barnes-Holmes (2013) appear to question whether these results truly represent derived relations. For example, on p. 157, they remark that "…nonhumans *purportedly* relate stimuli in untrained…ways." (Italics added) And, later "… see also Zentall and Urcuoli (sp), 1993 for evidence that *suggests* that derived behavior *may* occur in nonhumans." (Italics added). If "derived relations" are defined as new or untrained relations arising from other, explicitly trained relations, then we would argue that the variety of data that we cite in our review (and are reported elsewhere in the animal learning and cognition literature – e.g., Bonardi & Hall, 1994; Bovet & Vauclair, 1998; Honey & Watt, 1998) do definitively demonstrate that nonhuman animals can behave in a manner above and beyond the operant and/or Pavlovian relations on which they have been explicitly trained.

Hughes and Barnes-Holmes (2014, p. 157) state that "…these types of (derived) behavior are normally defined as largely respondent because they do not emerge from an appropriate history of arbitrarily applicable relational responding…" First, we are puzzled by the assertion that they are "normally defined as largely respondent." By whom? Certainly not by us nor by others who study animal learning and cognition. Second, we get the distinct sense that if untrained relations are seen to emerge in the absence of "an appropriate history" (apparently a history specifically defined by relational frame theory [RFT]), then they do not qualify as derived relations. Should our impression be correct, such a claim would anoint only a particular subset of untrained relations as "derived" or "emergent." We do not view such a theoretical maneuver to be suitably justified.

McIlvane et al. (2014, p. 161) notes that recent demonstrations of associative symmetry in pigeons (Frank & Wasserman, 2005; Urcuioli, 2008, Experiment 3) have "…not much resembled the behavior of humans (or even sea lions) when they exhibit symmetry." Moreover, accepting these findings as demonstrations of symmetry requires "…acceptance of relative response ratios rather than high accuracy as indicators of symmetry." Obviously,

we accept differential response rates (or discrimination ratios) as an analogous and valid dependent variable, as do others including McIlvane himself (e.g., Campos, Debert, Barros, & McIlvane, 2011; Debert et al., 2009). These differential rates are needed to measure conditional discrimination performances in the standard successive (go/no-go) matching paradigm which was adopted to help establish the very coherence McIlvane regards as indispensable for constructing valid emergent relations tests. If this measure is not regarded as commensurate with accuracy in n-alternative matching tasks, then how are our symmetry test results to be explained? To us, those results clearly demonstrate interchangeability among the baseline matching stimuli, the very hallmark of derived relations.

Dymond (2014, p. 152) takes issue with results of Zentall, Clement, and Weaver (2003), specifically with the fact that "…all test trials in Zentall et al. (2003) were presumably still reinforced, which makes interpretation of the emergent basis of the performance difficult." In fact, interpretation is not difficult at all.

In that study, pigeons were trained on a variation of symmetrically reinforced baseline relations in which A1-B1, A2-B2, B1-A1, and B2-A2 sequences ended with food. The question was whether such training would produce an acquired equivalence between A1 and B1 as well as between A2 and B2. To test this prediction, pigeons were then trained on two-alternative matching with B1 and B2 as samples and new stimuli (C1 and C2) as the reinforced comparisons, respectively; this training was followed by an A1-C1 and A2-C2 emergent-relations test (i.e., a test for A-B interchangeability). To avoid the extinction of responding that would most certainly occur with nonreinforced testing, reinforcement was provided for choosing comparison alternatives corresponding to the hypothesized emergent A1-C1 and A2-C2 relations for half of the pigeons (the "consistent" group). For the other half of the pigeons (the "inconsistent" group), reinforcement was provided for choosing comparison alternatives that were the opposite of those hypothesized relations. This latter group was a control for the effects of reinforcement per se during testing. Thus, the observed between-group difference in test performance—using the same measure of "accuracy" (i.e., choosing C1 after A1 and choosing C2 after A2)—cannot be explained simply in terms of new learning. Indeed, as predicted by acquired equivalence, choice of C1 after A1 and C2 after A2 was *below* chance (50%) in the inconsistent group.

Dymond (2014) also appears to have missed the point of this study when he says that "other studies on functional equivalence have yielded better outcomes using MTS based procedures without the need for symmetry training or combined successive and simultaneous discrimination tasks (e.g., Kastak, Schusterman, & Kastak, 2001). The purpose of the Zentall et al. (2003) study was to determine whether or not symmetry training was *sufficient* to result in functional equivalence. As a side note, the "better outcomes" in Kastak et al. (2001) occurred only when a differential outcomes procedure was used in training and testing (unlike Zentall et al.'s nondifferential outcomes procedure). Consequently, their "better outcomes" can easily be explained by outcome-expectancy mediation of transfer performances (Urcuioli, 2005) i.e., using the principles of two-process, associative learning theory (Overmier & Lawry, 1979).

McIlvane et al. (2014, p. 161) is unconvinced by current evidence "…that nonhumans (pigeons and rats in particular) must ultimately and inevitably demonstrate first- and second-order stimulus equivalence relations." At this point, neither are we ready to support such a strong statement. But, we are still very early in the game. To us, McIlvane's next statement that "Demonstrations of such capabilities, of course, are required to support arguments that a given species exhibits behavioral processes relevant to the analysis of basic, subordinate, and superordinate categories" does not follow. If a given species shows clear evidence of derived stimulus control like those examples we detail, then how could this evidence *not* be relevant to a comprehensive analysis of categorization?

Derived Relations and Behavior in Nonhuman Animals: Origins

Much was said in the commentaries about the origin(s) of associative symmetry and other types of derived relations, including statements and implications that there are fundamental differences between humans and other animals in this regard. For example, Hughes and Barnes-Holmes (2014, p. 158) assert that it is "important to acknowledge that humans are not shackled to associative concept learning…but can instead behave as if stimuli are related to one another in many different ways." Ignoring the pejorative characterization of associative learning as if it were confinement in a dismal dungeon, doesn't the ability to relate stimuli hierarchically, temporally, comparatively, and as opposites rely on these very processes at some point(s) in our lives? We suspect that these complex forms of behavior and understanding, too, have their foundations in our extensive operant, Pavlovian, and observational learning experiences. Where else might they arise?

Hughes and Barnes-Holmes (2014, p. 156) note that "…even after extensive training nonhumans find it difficult to demonstrate the simplest form of AARR (i.e., symmetry or mutual entailment)." Putting aside the symmetry results obtained in successive matching after minimal training, the elephant in this comparative room is the term "extensive training." We do not believe that the "extensive" training and other task-relevant experiences provided to nonhuman animals is anywhere close to those that a young human child or adolescent or adult has over the course of their lifetimes. We are also unwilling to accept that a human's lifetime of experiences, interactions, and explicit teaching regimens adds little to exhibiting derived behavior vis-à-vis the experiences of nonhuman animals kept under laboratory conditions.

On a similar note, if rule-governed or instructional control (Hughes & Barnes-Holmes, 2014, p. 156) depends on human language capabilities (e.g., in order to comprehend the rules or instructions), then we are not surprised, nor do we think it is significant, that creatures without language do not exhibit such control. Our position is that any such human-nonhuman differences provide insufficient justification to reject the possibility that other vital behavioral processes and mechanisms may be similar across species. In short, language proficiency may be entirely irrelevant to the possible commonality of associative conceptualization in humans and nonhuman animals.

The origin of pigeons' associative symmetry (Frank & Wasserman, 2005; Urcuioli, 2008, Experiment 3) attracted particular attention in the commentaries. We should be clear at the

outset that the term "associative symmetry" is *not* "procedure-bound with successive MTS" (Dymond, 2014, p. 154) as evidenced by its use in the paired-associates (e.g., Murdock, 1965; Winters, Daggett, & Kologinsky, 1978), serial recall (Sommer, Rose, & Büchel, 2007), and choice (Brosgole & Lepak, 1976; Tomonaga & Fushimi, 2002; Velasco, Huziwara, Machado & Tomanari, 2010) literatures. In addition, pigeons' associative symmetry does *not* require concurrent identity training with the arbitrary matching baseline (Campos, Urcuioli, & Swisher, 2014) nor does it require that the sample and comparison stimuli be presented in the same location (Swisher & Urcuioli, 2013).

Hughes and Barnes-Holmes (2014, p. 157) cite Barnes and Roche (1996) for "…some speculative RFT-based analyses concerning the relationship between identity matching (or reflexivity) and derived symmetry responding…." But Barnes and Roche (1996) did not, in fact, try to explain derived (emergent) symmetry in terms of identity matching/reflexivity training. Instead, they offered an explanation of derived (emergent) reflexivity (which they called "indirect reflexivity") in terms of explicit training on symmetrical relations between stimuli:

> In effect, explicitly reinforcing A-B and B-A matching may be functionally similar to explicitly reinforcing A-A and B-B matching, and thus during a subsequent test reflexivity might emerge without an explicit history of reinforcement for nonarbitrarily applicable sameness responding.

> (p. 502)

Independent of its merits, then, this speculative account does not apply to the associative (emergent) symmetry phenomenon we demonstrated.

McIlvane et al. (2014, p. 163) says that we have not proven "beyond a reasonable doubt" that our associative symmetry findings show across-species generality in the behavioral processes underlying emergent equivalence relations. To reach that level, he requires better explanations for inter- and intra-subject variability and a need to "secure performances that meet the highest human standards" (recognizing that "…the level of behavioral technology needed to secure (those) performances…does not yet exist.").

We are less bothered than he about inter-subject variability. After all, 33% of the children studied in Sidman et al. (1982, Experiment 3) did not show symmetry, and they most certainly had far more task-relevant, extra-experimental experiences than did our pigeons. At the individual subject level, trying to tease apart and identify all of the factors affecting the performance of individual organisms would be a huge undertaking and one complicated by unknown effects of prior testing. It would also have questionable merit especially so early in an investigation whose goals are to answer questions like "Can we find *any* evidence that animals are capable of demonstrating a particular emergent effect?" And, if so, then "What are the important aspects of baseline and/or test condition(s) that are responsible for the effect?"

If meeting the "highest human standards" requires a history as extensive as those experienced over many years by humans, then those standards are practically unattainable. In other words, trying to reproduce the extent and variety of those experiences would

definitely be a "logistical nightmare," to which we would add "from which there would be no awakening." We also question if the "highest human standards" are reasonable ones to expect.

An analogy might be helpful to illustrate our misgivings. Is it reasonable to expect students with a recent degree in accounting to be held to the "highest accounting standards" as professionals with decades of experience? We think not. Nevertheless, we believe that the processes underlying the different levels of performance in these two groups of individuals are similar.

Concluding Remarks

In closing, we want to clearly state what we have *not* claimed and what we are *not* asking. We have not concluded that nonhumans have shown stimulus equivalence (Dymond, 2014, p. 152) as defined by Sidman and Tailby (1982). We are not asking if nonhuman animals "are… capable of routinely learning stimulus equivalence relations?" (McIlvane et al., 2014, p. 161). These are certainly important and significant issues; but, at this juncture, we are not yet prepared to address them.

To do so, however, it is absolutely crucial to fully understand the nature of the functional matching stimuli for pigeons and other nonhuman animals because only then will we be in a position to design properly controlled and incisive experiments. In other words, we must know precisely what features of the stimuli that *we* use in *our* experiments control *their* performances (McIlvane et al., 2000). Are only the nominal aspects of the stimuli important? Or does spatial location and/or temporal location also matter (Lionello & Urcuioli, 1998; Urcuioli, 2008)? Without this knowledge, "Chasing these (and related) issues" will *not* "lead to a better understanding of human and animal cognition," but will, instead, simply end up as the proverbial dog chasing its own tail. The "restricted set of conditions under which nonhuman equivalence responding is seen to emerge" (Hughes & Barnes-Holmes, 2014, p. 156) is actually a reflection of the significant progress we have made in this area, rather than an indication of other animal's limitations. Apropos to Jack London's introductory remarks, the limitations are really our limitations. Good science demands that we relinquish the quest to prove humankind's uniqueness and to systematically and dispassionately pursue our place in the community of "the other animals."

In our review, we tried to underscore how productive programs of research on associative concept learning have been realized by focusing on questions like: "Can nonhuman animals (viz., creatures without language) demonstrate derived stimulus relations?" "What varieties of emergent relations effects can be observed in nonhumans?" "What mechanism(s) is (are) responsible for these diverse emergent relations?" "How do the responsible mechanisms resemble or differ from one another?" We plan to explore those commonalities and differences without precondition or preconception. The results so far provide ample reason for optimism in answering these challenging questions.

Acknowledgments

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