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ANIMAL COGNITION AS PART OF COGNITIVE SCIENCE: A FRINGE ACTIVITY?

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This paper on "The mind of organisms" by Prato Previde, Colombetti, Poli and Spada provides an excellent account of the aims and assumptions underlying contemporary research in animal cognition. It puts forward a point of view that most researchers in the area today would probably share, but rarely make explicit. Among a behaviorist minority, who do not subscribe to such views and are unsympathetic to the whole idea of "animal cognition," there is still a strong suspicion that "cognitive" denotes both a loose anthropomorphism and a highly regrettable relapse into dualism. Consequently, what the present paper identifies as the two fundamental assumptions of the cognitive approach they endorse need to be emphasized again and again: first, that cognitive processes "are fully realized in the nervous system of the organism"; and, second, that they can be described at an abstract level that makes no claims about subjective experience nor the underlying neural events.

The paper ends with the call for animal cognition "to be considered a component of cognitive science in its own right." In making this claim the authors may well be pushing against an open door. I doubt that many cognitive scientists object in principle to the inclusion of natural, but nonhuman, cognition within the science. The question is whether it is likely to be, or should be, an important element. What grounds might one have for including animal cognition when, for example, planning a new course on cognitive science for a degree course? What does animal research have to export to its fellow members of the community of cognitive science?

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MENTAL HYGIENE

The paper suggests two related answers to this question. One may be termed the mental hygiene argument. The strong tradition of experimental analysis in animal psychology and the need to operationalize theoretical assumptions and implications, so that some measure of the behavior of a nonverbal organism can be used to test a theory, provide very strong encouragement of precision and conceptual clarity. This tradition and this need provide protection against the illusory belief that by invoking familiar concepts from folk psychology an explanatory account of some process is achieved. However, a similar argument has been employed by those impressed by computer analogies to emphasize the need to express psychological theories in terms of computer programs or, more recently, of neural network models (Rumelhart & McClelland, 1986). For many areas in cognitive science, notably those in which human language is involved, computers rather than nonhuman animals will continue to be seen as the preferred means for ensuring mental hygiene. We need to be clear about what distinctive contribution animal cognition can make in this respect.

INTENTIONAL ACTION

One very promising example is included in the paper. This is the research by Dickinson and his colleagues on the processes underlying instrumental conditioning. Lever-pressing by a rat in a Skinner Box has been the archetypal behaviorist preparation. Now it appears that an explanation of why this occurs may need to include both a belief (that this action is followed by a certain outcome) and a desire (currently wanting this outcome). However, this alone is unlikely to enrich theories of human behavior. A remarkable finding that may well have important implications for theories of human action is what Dickinson refers to as "incentive learning." As noted in the present paper, a change in motivational state from hunger to thirst, for example, may not be sufficient to produce a switch in behavior towards actions that in the past produced sucrose solution and away from those that produced dry food pellets. Before this occurs, the animal has to learn that sucrose solution is something to be desired when thirsty. Balleine and Dickinson (1991) have recently found that incentive learning is also necessary for an appropriate change in instrumental behavior to occur following the devaluing of a reinforcer resulting from the use of a conditioned flavor aversion procedure. Thus, a rat that has learned to press a lever for sucrose, which has then been followed by a lithium injection, will subsequently press the lever in extinction as frequently as a control animal injected with saline, unless it has an opportunity to experience the sucrose again and learn that it is now undesirable. If such incentive learning takes place, then a reduction in the instrumental action occurs.

This finding is particularly interesting in that comparable post-conditioning exposure to the reinforcer does not appear to be necessary for producing a change in classically conditioned behavior. Thus, in the test phase of Balleine and Dickinson's (1991) experiments rats that have not been given an opportunity for incentive learning nevertheless show decreased approach to the area of the dipper—a measure of classically conditioned behavior—as if at one level they "know" that sucrose has become aversive, even though their instrumental performance remains unaffected by the sucrose-lithium pairing.

At the very least, the incentive learning effect in instrumental performance shows that the same event, e.g., presentation of some sucrose solution in a dipper, is represented separately for instrumentally—as opposed to classically-based behavior. It is important to find out whether an analogous dissociation can be detected in human behavior. It may, for example, provide a new way of approaching the old problem of understanding the differences between rational action based on conscious belief and irrational attitudes, values, habitual responses and emotional reactions, of whose origins we are rarely conscious.

Whatever the outcome of future research on this topic these experiments provide a clear example of a domain of cognitive science in which research using animals is of great potential importance. It is difficult to imagine how such a distinction might have arisen from research using humans.

UNCLUTTERED FUNDAMENTALS

Which brings me to the second answer the paper makes to the question of why cognitive science in general should take note of animal cognition. This is the simplicity, or "uncluttered fundamentals," argument that has been used to justify inclusion of the study of nonhuman animals within psychology since the early days of this century (Boakes, 1984). Watson (1914) argued on these grounds for including comparative studies within psychology, as did, from a completely different perspective, both Koehler (1925) and Tolman (1932). For psychology as a whole there are plenty of concrete examples to support this argument, but for cognitive science compelling examples are thin on the ground. The flow of ideas had been very much in the direction of human to animal research. Despite a decade or more of substantial research on animal memory, for example, the impact on theories of human memory is hard to perceive.

Perhaps it is just a matter of time, so that in a few years hence the kind of research Dickinson's group are engaged upon, or current developments in the study of categorization (Pearce, 1988; Shanks, 1991) or of perceptual learning and latent inhibition (Hall & Honey, 1989) in animals will be seen to be as important for understanding human cognition as in an earlier era were Koehler's ideas on problem-solving and Tolman's on spatial learning.

In contrast, a very strong case for the current importance of animal cognition can be made on other grounds. It is one which Prato Previde and her colleagues omit to discuss. This is to provide a crucial link to neuroscience by means of animal models which allow exploration of the neural basis of cognitive processes. Current theories of memory processes may owe little to animal research, but our knowledge of what areas of the brain are important for various aspects of memory are largely based on animal studies (Aggleton, Hunt, & Rawlins, 1986). Understanding the attentional deficits shown in schizophrenia in terms of neurotransmitter balance is likely to be based both on the use of human cognitive tests and psychophysiological measures (Michie et al., 1990) and on the use of animal models such as the latent inhibition paradigm (Gray et al., 1991).

A "REAL" LEVEL OF EXPLANATION?

Rather than add further examples to what could be an impressively long list, I want to make it clear that the point of such examples is not to suggest that animal cognition is of value only to the extent that it contributes to research on the neural basis of cognitive processes. Prato Previde and her colleagues make some important points about different levels of explanation when, for example, contrasting pictorial with propositional representations and suggesting that both may be subsumed by some form of neural network theory. However, the further suggestion that explanations at a neural level are the only real ones and all others merely descriptions smacks of the kind of reductionism that makes particle physics the only true science. Just as economists and sociologists may develop entirely valid explanations for the phenomena that they study, without basing these on a psychological theory of the behavior of individual human beings, those studying animal cognition should strive to develop explanatory theories at an appropriate conceptual level, whether or not these can be related to events at a neuronal level.

The limitations of the behaviorist approach are well summarized at the beginning of the present paper. It is important to note, however, that the explanations offered by such theorists proved wrong in the face of behavioral evidence, not because of conceptual flaws or a failure to make contact with events at a neural level. One of the major achievements of behaviorist theorists was to show that one can develop explanations of behavior at a distinctive conceptual level, distinct both from the concepts of everyday folk psychology and from the level used in neuroscience. This is an important theme in Skinner's *The behavior of organisms.* It is not something to be thrown away in the process of substituting mind for behavior.

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