

ports one of the most sophisticated procedures yet developed by which we can study what seems by most normal intuitive standards to be knowledge about an animal's own knowledge.

It is also important to note, I believe, that the opinion I have just expressed has a subjective component. I have suggested (Shimp, in press) that a scientific-peer-review analog of truth in advertising might be facilitated if reviewers acknowledged how their methodological and theoretical priorities might affect how they see a paper they evaluate. In that spirit, I should note that it is relevant that I already believe the distinction between knowledge and metaknowledge is an important one to clarify, and I am inclined to believe that new empirical methods that produce systematic, quantitative functional relations contribute greatly to the development of explanatory theory.

For these reasons, I see the target article in an extremely favorable light. I do suspect, however, that the authors put the adaptive function of consciousness in a more favorable light than it might generally deserve. Perhaps this is because the authors wish to focus more on the existence of metacognition in nonhuman animals than on its general function. For whatever reason, the authors emphasize the fact that animals can be better off knowing what they are doing. Presumably, the behavior of scientists is another such case: We are presumably better off being aware of the theoretical assumptions we make because, again presumably, we can in that case better evaluate them. However, it also seems clear that in some cases, such as with a performing musician, or a baseball player swinging a bat at a fastball, we are better off in many ways not knowing what we are doing, because the processing of metaknowledge might interfere with performance. In short, the authors address the case where something akin to awareness might be functionally adaptive, but there is also the case where metacognition might be correlated with poorer performance.

It has been suggested that such a case can even be demonstrated in the pigeon. As a researcher who believes that the cognitive capabilities of the pigeon are generally grossly underestimated, it was a bitter pill to swallow to read that the authors are skeptical of the pigeons' capability for metaknowledge. But their acknowledgment that the end of the story for birds is not yet written was at least a mild restorative. Without having the space to go into details – and while acknowledging that in all cases of metacognition in nonhuman animals there remain the most basic issues yet to be determined, including in my own experiments – it is possible that some kind of conditional discrimination might conceivably some day explain the results (but see Anderson & Bower 1973). I would like to suggest that the diversity of demonstrations already available puts avian metacognition in a more favorable light than do the authors (Clayton et al. 2000; Shimp 1982; 1983; Zentall et al. 2001).

I strongly endorse the authors' position on the role of parsimony in comparative cognition: This role is, most ironically, extremely complicated and needs to be evaluated very carefully in each case where it is proposed as an evaluative criterion. I agree with the authors that, while parsimony has a long and fairly honorable history, it can also lead to much mischief (Shimp 1999; 2001). If, for example, parsimony were to be used to reject out of hand the kind of conceptual development the authors propose, I believe it would be sharply counterproductive.

The authors have been so successful that I am encouraged to wonder about future possible experiments using a monitoring response, particularly with avians. Can a monitoring response be successfully used in research on avian visual multidimensional categorization performances (Herbranson et al. 1999; 2002), using a method adapted from human research (Ashby & Maddox 1998)? That is, would a pigeon more frequently choose not to categorize more difficult stimuli? Finally, does the monitoring response depend on temporal parameters of a task in ways that at least intuitively correspond to the functionality of consciousness?

Implicit metacognition, explicit uncertainty, and the monitoring/control distinction in animal metacognition

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Abstract: Smith et al. demonstrate the viability of animal metacognition research. We commend their effort and suggest three avenues of research. The first concerns whether animals are explicitly aware of their metacognitive processes. The second asks whether animals have metaknowledge of their own uncertain responses. The third issue concerns the monitoring/control distinction. We suggest some ways in which these issues elucidate metacognitive processes in nonhuman animals.

In the target article by Smith et al., the authors demonstrate that nonhuman, nonverbal animals are able to inform us that they feel “uncertain,” one proposed measure of metacognitive ability. Prior to these data, it was not known whether an animal could report introspective assessments about retrieved memories and response choices. This new and exciting line of research has sparked debate, discussion, and further studies investigating the level of metacognitive abilities in nonhuman species, as well as shaking the definition of metacognition itself. We commend Smith and his colleagues for daring to go where no researchers had gone before. We discuss three potential avenues for research in this new field of animal metacognition: the implicit/explicit distinction, knowledge of uncertainty, and the monitoring/control distinction.

One exciting discussion is that human metacognitive processes involve both explicit and implicit influences (Reider & Schunn 1996). Traditionally, researchers have defined metacognition as the ability to explicitly assess the certainty of retrieved items from memory and consciously verbalize the judgment. However, some recent research suggests that some metacognitive processes may act without conscious awareness (Reider 1987; Reider & Schunn 1996). For example, Reider (1987) showed that people could decide that they knew something more quickly than they could actually retrieve the information – indicating a lack of explicit awareness in the decision. If uncertainty is an implicit “feeling,” animals may be able to respond similarly to how they might respond to a “feeling” elicited by a fear-invoking stimulus. Thus, some now propose that the definition of metacognition must encompass both explicit and implicit abilities to be uncertain, whether it is reported verbally or behaviorally (see Son & Kornell, in press).

A second and related issue concerns the animal's knowledge of its own uncertain responses – a genuine *metacognitive* process. Most of the research in the Smith et al. target article relies on evidence using an opt-out response, an option simultaneously presented with other response options. When uncertain, the animal can escape negative consequences of being incorrect on that trial. This escape response cannot address the issue of whether the animal has knowledge of being uncertain, because the option is presented concurrently with other options, as well as with the features of the question (i.e., Hampton 2001) – making it more similar to a mere cognitive response, rather than a *metacognitive* response. The escape response confounds the object-level task (e.g., which patch is denser), and the meta-level task (i.e., confident or uncertain) does not distinguish between an animal motivated to avoid penalties and one actually becoming aware of its uncertainty.

Can animals show us that they know how uncertain they are? This would entail that the animal makes a metacognitive response after a cognitive response, not simultaneously. A procedure developed by Son et al. (2002) investigated the issue of whether animals can make these higher-level metajudgments (also see Hampton 2001). Rhesus macaques were asked to choose the longest of nine lines and, once the lines disappeared from the

screen, they made a high- or low-confidence judgment. Results showed that the monkeys reliably chose high confidence after easy trials that were more likely to be correct and low confidence after difficult trials that were more likely to be incorrect. The animals acted metacognitively and, moreover, continued to do so when the risk task was transferred to other cognitive tasks that did not involve lines. This retrospective judgment suggests that not only is the animal motivated to avoid penalized responses, but that it can report knowledge of its state of uncertainty. Analogously, a human might feel one line is the longest, but not be sure, or be absolutely certain they were guessing. Son et al.'s task is easily modifiable to distinguish between states of low confidence and guessing.

Another issue in the human metacognitive literature is that there is an important difference between metacognitive monitoring and metacognitive control. Monitoring refers to peoples' ability to become aware of how well their cognitive processes are working. To do this, the monitoring process builds a model of how the basic process is working. Monitoring processes serve as the basis for metacognitive judgments. Control refers to a decision that the metacognitive system makes based on the output of the monitoring system. If the monitor informs the person that learning is going poorly, the control processes can redirect attention to the necessary to-be-learned items. For the control to be effective, it must be able to change the object-level processes and change them in adaptive ways. The effectiveness of control processes is partially dictated by two factors: (1) the monitoring effectiveness, and (2) the choice of good strategies.

The differences between metacognitive monitoring and control have not been explicitly addressed with respect to animal cognition. As suggested by other domains, animals may also show poor control in situations in which they have accurate knowledge (Boysen et al. 1996). We suspect that animals will not only vary in the degree to which they behave metacognitively, but in the kinds of metacognition they demonstrate as well. We also suspect that some species will excel at monitoring tasks (perhaps those frugivores who have to make fine distinctions between fruit colors), whereas others might control behavior better (e.g., those that need to outwit larger conspecifics). Like Smith et al., we endorse a broad comparative approach using a large range of species. Using foraging ecology to generate hypotheses has greatly benefited the study of spatial memory (e.g., Burke et al. 2002; Platt et al. 1996), and we believe that such investigations will also help us elucidate other issues of metacognition as well as the differences between monitoring and control. We contend that being able to differentiate between monitoring and control processes – in addition to explicit/implicit monitoring and knowledge-of-uncertainty/uncertainty responses – may help researchers define the boundaries of metacognitive abilities, and thus is consistent with Smith et al.'s call for a comparative study of metacognition. Animal metacognition is still an open book with untold interesting questions and methodologies to be discovered.

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Uncertain what uncertainty monitoring monitors

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Abstract: Smith et al. present a model that they suggest may clarify aspects of the phylogenetic distribution of metacognition, based on observation of what they call uncertainty monitoring. Although they suggest that their model is supported by data collected using monkeys and dolphins, their interpretation that nonhuman animal behaviors parallel thought processes in humans may be unwarranted. The model presented by Smith et al. is inconsistent with current theories and empirical findings on the comparative aspects of metacognition. We present three oversights of the model and extend our critique to include a brief discussion of animal self-awareness, as well as current neuropsychological perspectives on metacognitive processing in humans.

The model suggested by Smith et al. is interesting, but only with respect to human participants. The experimental tasks, although interpreted as paralleling human thought processes, are wrought with methodological issues when applied to nonhuman animals. The first problem (briefly acknowledged by the authors) is the fact that the comparative uncertainty responses may simply be trained; that is, the animals simply could be learning a third behavioral response, which are associated with rewards (e.g., shorter timeouts) in the data. The authors, however, interpret this behavioral response as an uncertain response. The authors appear to attribute verbally communicated human thought processes to nonverbal, nonhuman animals! Even in verbal humans this response may not be what it appears: Humans, for social desirability and lack of a complete understanding of their mental processes, may attribute undeserved meaning to what are, essentially, trained responses.

Our second contention with the paradigm is the lack of ecological validity. Although the authors state two requirements for correctly executing this type of paradigm, “create perceptual or cognitive difficulty for the animal in order to stir up something like an uncertainty state” (sect. 4) and “provide a behavioral (i.e., nonverbal) response that lets the animal comment on or cope adaptively with that state” (sect. 4), they fail to test the model under conditions that are likely to be encountered by their subjects in the wild (e.g., uncertainty with respect to monitoring dominance position, alliances, face perception). The authors assume that what creates uncertainty in humans also creates uncertainty in monkeys, but differences in environment and environment-specific functioning suggest that the stimuli of uncertainty would also be different.

Our third issue is the authors' definition and use of the term “metacognition.” The authors define the term as “thinking about thinking,” and although we agree with this definition, the authors fail to adequately address the vast literature investigating metacognitive capacities in nonhuman primates (e.g., Hare et al. 2000; 2001; Povinelli et al. 1993; 1998; Tomasello & Call 1998), which has consistently provided support for a model developed over a decade ago by Gallup (1982) that theory of mind (i.e., metacognition) can only emerge in organisms that can first conceive of themselves (e.g., self-awareness; Gallup 1988; Gallup et al., in press). This model has gained increasing support in the last decade (e.g., Frederick & Platek, under review; Shillito 2002; Shillito et al. 1999; see also Amsterdam 1972; Gallup et al. 2000; Perner & Wimmer 1985). The authors also fail to acknowledge that the mark test (Gallup 1970) has been well supported by empirical data (see Shillito et al. 2002) from across the world and from almost every living species of nonhuman primate. The data strongly favor the notion that only great apes can pass the mark test. By using a limited phylogenetic distribution of animals and a very low sample size, the authors fail to adequately accomplish what they set out to do – examine the comparative psychology (i.e., phylogenetic distribution) of metacognition. Furthermore, the vast liter-