Metacognition in animals: Trends and challenges

Jonathon D. Crystal & Allison L. Foote
University of Georgia

There is widespread agreement that metacognition is not demonstrated if alternative explanations account for putative metacognition data. However, there is less agreement on which studies are protected from alternative explanations. We have argued that existing experiments on uncertainty monitoring can be explained by low-level explanations without assuming metacognition (Crystal & Foote, 2009). The field would benefit from the development of accepted standards for what is required to produce a convincing example of metacognition in animals.

Keywords: Metacognition, comparative metacognition, uncertainty monitoring, metamemory, quantitative modeling.

Introduction

The collection of articles on metacognition in Comparative Cognition & Behavior Reviews provides an opportunity to look back at previous trends and look forward to future research and challenges for the future.

Trends

Each contribution (Crystal & Foote, 2009; Hampton, 2009; Jozefowiez, Staddon, & Cerutti, 2009; Smith, Beran, Couchman, Coutinho, & Boomer, 2009) to the series on metacognition recognized the burden of ruling out alternative explanations before concluding that a genuine metacognitive process is responsible for data. Hampton pointed out that “…there is a high bar to clear in terms of ruling out alternative mechanisms for metacognition before we can conclude that any nonhuman animals engage in private metacognition” (ms. p. 22) and “we can only infer private metacognition in nonhumans by excluding likely public mechanisms” (ms. p. 7). Smith et al. noted that “even when animals perform in a way that might demonstrate metacognition, researchers should consider carefully the alternative possibility that these performances can be explained through low-level, associative mechanisms based in stimulus cues and reinforcement contingencies. Morgan’s Canon has seldom had a fatter target to shoot at than animal metacognition” (ms. p. 5), and “BEM is also able to account for data showing that animals are able to generalize the use of the uncertain response to new tasks” (ms. p. 6). Crystal and Foote noted that “if the principles of associative learning or habit formation operating on a primary representation may account for putative metacognitive data, then it would be inappropriate to explain such data based on metacognition (i.e., based on a secondary representation); the burden of proof favors primary representations, by application of Morgan’s canon” (ms. pp. 4-5). Overall, there is widespread agreement that the burden of proof, in this domain, is on providing evidence that uniquely implicates a genuine example of metacognition (i.e., private metacognition, secondary representation), and until such evidence is provided, the cautious interpretation is to note that lower-level explanations are sufficient to explain the data.

However, the contributions differed in the assessment of the status of empirical evidence with respect to ruling out alternative explanations. Hampton (2009) and Crystal and Foote (2009) noted that the bar has not been reached in many studies (they differed somewhat on the prospects of reach-
ing this bar with existing methods). Jozefowiez et al. (2009) pointed out that although their model does not explain all aspects of the data that they reviewed, other proposals may explain it without needing to use the concept of metacognition. Smith et al. (2009) noted that the bar has not been met in several studies but concluded that the bar has been met with a selection of recent approaches; they reached this conclusion by determining that some tasks are sophisticated and therefore do not call for low-level explanations. Each article in this series noted that it is not necessary to assume uncertainty monitoring in order to produce apparently functional use of the uncertainty response in at least some experiments. There was less agreement on which studies are protected from low-level, alternative explanations.

Tests of a stimulus-response hypothesis were well integrated into the examination of alternative hypotheses. However, tests of a stimulus-independent hypothesis (i.e., the implications of a low, flat threshold with constant attractiveness across stimulus conditions, as proposed by Smith, Beran, Couchman and Coutinho, 2008), needs to be added to the analysis of metacognition. Although Smith et al. (2009) pointed out that a signal detection account of metacognition (e.g., Smith et al., 2008; Smith, Shields, & Washburn, 2003) may correspond to high-level and low-level versions, the response-strength stimulus-independent account (Smith et al., 2008) was offered specifically as a low-level alternative to metacognition (i.e., it does not have a high-level version). As we argued (Crystal & Foote, 2009), although a transfer test is an adequate test of a stimulus-response hypothesis, transfer tests do not appear to be an adequate test of the stimulus-independent hypothesis. Moreover, the operation of residual reinforcement variables (as we documented in Crystal & Foote, 2009) further complicates the ability to rule out low-level explanations. In Hampton’s (2009) terminology, generalization to new environments is not adequately ruled out by transfer tests, in our view, because an association with environmental cues is not needed to produce a low-level, alternative explanation of adaptive control of behavior based on public information.

Challenges

It is striking that there does not appear to be an accepted standard for what it would take to produce a convincing example of metacognition in animals. We like Hampton’s (2009) Table 1, which provides a systematic, thorough, and logical approach to analyzing candidate explanations and evaluating if each has been ruled out. There may be other explanations (as Hampton acknowledges and Jozefowiez et al. (2009) point out), but these explanations should be proposed and the systematic analysis extended. As Hampton points out, it will only be by ruling out alternatives that metacognition can be convincingly claimed.

Our view is that the systematic analysis (and the development of convincing evidence) will be accelerated by a complete application of low-level explanations to existing methods. If we do not recognize the limitations of existing methods, then we will undercut the motivation to develop new approaches that might have the potential to provide a compelling case for metacognition. It is a big challenge, but concluding that we have already accomplished it (if we have not) is a barrier to overcoming the challenge.

The challenge for the future is to develop new techniques that document the use of a secondary representation (in our terminology; Crystal & Foote, 2009) or private metacognition (in Hampton’s 2009 terminology). Importantly, such a new method would need to produce data that is not predicted by application of a primary representation, given the array of existing stimulus-response and stimulus-independent hypotheses.

References


It is clear that there is no shortage of uncertainty about metacognition in nonhuman animals. The four papers in this special issue have raised questions about the evidence for metacognition from many perspectives, ranging from concerns about whether existing control procedures unambiguously specify sources of stimulus control, to models that appear to generate metacognitive patterns of performance without explicitly metacognitive components, to critiques of the entire effort on the grounds that there is no mechanism specified for metacognition. The area cannot be criticized for a shortage of critical thinking. Many of the critiques put forward direct attention to aspects of this problem that need more attention, but some of these concerns are more central than others.

The notion that metacognition is “complex” or a form of “higher” cognition is widespread, and concerns about invoking “higher functions” to explain nonhuman behavior were raised directly or indirectly in each paper. But this concern is relevant only if performance in metacognition tasks requires “new” cognitive mechanisms not employed in other behavior. It is not clear that metacognition is more complex than cognition. Metacognition may involve mostly the same mechanisms as other cognition even when it can be shown that metacognition takes as input private mental states rather than publicly observable stimuli. For an analogy, consider that secondary reinforcement is not necessarily more complex than primary reinforcement, although it cannot occur without primary reinforcement. Both primary and secondary reinforcement are useful constructs in a mechanistic account of behavior. The important question is whether metacognition is similarly useful in describing and explaining behavior, although it is unlikely to have such broad explanatory power as secondary reinforcement. Labeling some mechanisms as “higher” and others as “lower” may interject unhelpful and anthropocentric value judgments into evaluation of our accounts of behavior.

Progress in the study of metacognition in nonhumans may best be advanced through focus on stimulus control. Which stimuli control metacognitive responding (e.g. use of a decline test response, search for more information, accurate confidence judgments) and how is such control established? Are all controlling stimuli public, or can private states such as assessments of memory strength or the vividness of memories also come to control use of a decline test or other metacognitive response? The research reviewed in these papers clearly identifies some interesting behavior not previously documented in nonhumans. This justifies further work on metacognition in nonhumans. In accord with Jozefowicz, Staddon & Cerutti (2009), I think we should further analyze how these performances come about, without devoting disproportionate effort to developing additional behavioral or theoretical criteria for metacognition. Experiments can proceed and information can accrue without a precise mechanistic definition of metacognition, as in the case of research on “learning” or “perception” which are also difficult to define precisely. In fact, these phenomena are probably
It seems unlikely that substantial progress will be made in elucidating the processes underlying metacognition by trying to eliminate the influence of reinforcement (Smith, Beran, Couchman, Coutinho & Boomer, 2009). Reinforcement, in one form or another, is the only way in which instructions can be given to nonverbal species. Unless it is useful in maximizing reinforcement in one form or another (increasing quality or quantity of food, decreasing delays or effort) there is simply no reason for an animal to use a metacognitive response. Indeed, as indicated by Smith et al., our job is to “come to understand the cognitive representations and processes that allow this maximization to occur.”

Three of the four papers on this issue make extensive use of modeling in an attempt to understand the performances observed. The Behavioral Economic Model (BEM; Jozefowicz, et al., 2009) formalizes competition between responses and therefore shares some core explanatory features with the much less formal account of the performance of monkeys searching for food in the “tubes task” offered by Hampton, Zivin & Murray (2004). At least two relevant questions arise from these analyses that emphasize competition. First, if these analyses are correct, do they invalidate the conclusion that the subjects’ performances demonstrate metacognition? Or do they rather provide a possible explanation of some human and nonhuman metacognitive performances? Second, can competition models work when the opportunity to decline tests is offered before presentation of tests and the two types of response are not in direct competition (as in Hampton, 2001)? If not, what does this tell us about the decision process underlying such prospective metacognition?

Finally, it will be interesting to see more explicitly comparative work on metacognition, particularly experiments that use identical procedures with different species. It might be expected that associative and competitive process like those formalized in the BEM model would be shared by many distantly related species. It is early to tell but it appears that some species (humans and old world monkeys) may more readily show a metacognitive pattern of performance than do other species (pigeons and new world monkeys) that would be expected to share the mechanisms articulated in BEM (Beran, Smith, Coutinho, Couchman & Boomer, in press; Basile, Hampton, et al., 2009; Sutton and Shettleworth 2008). There are, of course, many reasons that any given test of metacognition would be negative. However, if a pattern of species differences is established that appears to have some cognitive (rather than motivational or perceptual) basis, other comparisons between those species will help identify the necessary conditions for metacognition and the selective pressures responsible for its evolution.

References


We return to the issue of reinforcement addressed by Smith, Beran, Couchman, Coutinho & Boomer. We argue that their concerns are unfounded because (a) reinforcing the ‘uncertain’ response need not undermine an otherwise convincing demonstration of animal metacognition, even for skeptics, (b) with or without metacognition, in the absence of the appropriate reinforcement contingencies, animals will not choose at all.

Keywords: Metacognition, comparative metacognition, uncertainty monitoring, metamemory, quantitative modeling.
tive abilities, there must be some incentive for picking the uncertain response. In other words, the uncertain response must be reinforced in one way or another. In the absence of risk aversion, the payoff for the uncertain response must be more than half what the subject would earn by responding randomly to the test question.

Delaying reinforcement by giving trials in blocks with the reinforcement at the end, as Smith et al. (2006) did in an ingenious experiment, doesn’t really solve the problem. The procedure simply impairs the correlation between real uncertainty (assessed by actual percent correct) and the animal’s assessment of uncertainty (assessed by its choice of the uncertain response). Thus, in Smith et al.’s experiment, the monkeys were equally likely to respond “uncertain” to stimuli that were in fact responded to correctly 95% and 24% correct. These data may still be explicable as a sort of winner-take-all competition between a low but nonzero tendency to make the uncertain response (what Crystal & Foote, in press, term a stimulus-independent hypothesis) competing with the weak tendencies to make either discrimination response in the confusable range.

In other words, any model of the task used to demonstrate animal metacognition must involve the subject making a decision about which response to make based on some information to which it has access. The critical point regarding metacognition is not the process by which the decision is made – which necessarily will involve the reinforcing effects of the payoffs for the various responses available – but the information it uses to make this decision.

To take a parallel example, in studies aimed at demonstrating numerical abilities, the animals obviously perform the way they do because of the contingencies of reinforcement implemented by the experimenter. But this does not affect the real issue, which is: what information does the animal use to perform the task, numerosity or some other stimulus dimension correlated with numerosity but not numerosity itself?

Put in yet another way, because tasks used to demonstrate animal metacognition are discrimination tasks, any account of performance in these tasks will be an associative one where the animal associates reinforcement of a response to specific stimulus values. This is not the issue. The issue is what the stimulus values are. Hence, the fact that monkeys meet the behavioral criteria for metacognition while pigeons do not has no implication about whether or not the monkeys’ performance require us to invoke metacognition or can be explained by low-level models. It just shows that both animals are using different kinds of information to make their decisions, even though reinforcement plays a role in both cases. In monkeys, that information is correlated in some way with its chance of success on that trial, i.e., with an effect of its past history with similar (in a way to be discovered) trials. (Of course, sensitivity to past history is also a property of models such as BEM, which do not resort to the faculty of metacognition.) Pigeons, however, apparently do not show this kind of sensitivity (Inman & Shettleworth, 1999), though which information they use instead is not yet clear.

Moreover, as our game-show example shows, you cannot escape reinforcement: there must be a reason why the subject picks the uncertain response and there is no other candidate but reinforcement. Even though Smith et al. claim to have “provided strong evidence that the reinforcement structure of this task did not determine the animal’s uncertainty-response strategy,” the question remains: if not reinforcement, then what? How does the monkey set its decision criteria if it does not use reinforcement? It’s almost a logical necessity: cherchez le renforçateur, as Crystal and Foote did, and you will find it.

Hence, using complex procedures, such as the one developed by Smith et al. (2006) or Beran et al. (2006), will just make the life of the researchers unnecessarily complicated. First, it may not be possible to use such procedures with other species. It is pretty amazing that one of monkeys from the Smith et al. (2006) study continued to respond in the new trials. (Of course, sensitivity to past history is also a property of models such as BEM, which do not resort to the faculty of metacognition.) Pigeons, however, apparently do not show this kind of sensitivity (Inman & Shettleworth, 1999), though which information they use instead is not yet clear.

References


*doi:10.1037/0096-3445.135.2.282*

*doi:10.3758/PBR.15.4.679*


(*www.psycrit.com*).
The Curious Incident of the Capuchins

J. David Smith
Department of Psychology and Center for Cognitive Science, SUNY Buffalo

Michael J. Beran
Language Research Center, Georgia State University

Justin J. Couchman, Mariana V. C. Coutinho, Joseph B. Boomer
Department of Psychology, SUNY Buffalo

Keywords: uncertainty monitoring, metacognition, comparative cognition, decision making.

In the mystery Silver Blaze, Sherlock Holmes draws the detective’s attention to the curious incident of the dog in the night-time. The detective reminds him that the dog did nothing in the night-time. Holmes replies: That was the curious incident. The incident is an important clue to the mystery’s solution.

We draw everyone’s attention to the curious incident of the capuchins. Beran et al. (in press) compared capuchins’ ability to make a perceptual response to middle stimuli and an uncertainty response to difficult stimuli. Capuchins completed a Sparse-Uncertain-Dense (SUD) task in which any trials of the monkeys’ choosing could be declined through an uncertainty response. The uncertainty response let them avoid difficult trials, avoid associated error timeouts, and greatly increase their reward efficiency. They also completed a Sparse-Middle-Dense (SMD) task in which correctly made middle responses to middle stimuli were rewarded. Capuchins used the middle response easily (Figure 1A) but not the uncertainty response (Figure 1B—see also Figure 6, Smith et al., present volume).

That the capuchins showed almost no uncertainty responding in the SUD task yet used the middle response so perfectly in the SMD task is an important clue in developing a psychological theory of animals’ uncertainty responding. It points this new field toward the theoretical developments that will make its next phase rich and scientifically productive. This is why.

You can’t explain the Middle-Uncertain dissociation by claiming that animals emit the behavior that is associated with the higher payoff. Then, capuchins would certainly have responded Uncertain for difficult stimuli so as to avoid timeouts.

You also can’t explain it by having animals respond to minimize the average delay to reinforcement. That delay would have been sharply reduced by adaptively responding Uncertain on difficult trials to avoid the frequent timeouts.

You can’t explain it using some overall-reinforcement-rate explanation. Capuchins lost thousands of seconds of time-on-task through penalty timeouts by not responding Uncer-
Figure 1. Mean percentage of sparse responses (blue dotted line), dense responses (red dashed line), and uncertainty or middle responses (green solid line) by capuchin monkeys (Cebus apella) in Beran et al.’s Sparse-Middle-Dense task (A) and Sparse-Uncertain-Dense task (B). The results shown are from Beran et al.’s Experiment 2. The similar results from Beran et al.’s Experiment 1 were shown in Smith et al. (present volume, Figure 6). From “The Psychological Organization of ‘Uncertainty’ Responses and ‘Middle’ Responses: A Dissociation in Capuchin Monkeys (Cebus apella),” by M. J. Beran, J. D. Smith, M. V. C. Coutinho, J. J. Couchman, and J. B. Boomer, 2009, Journal of Experimental Psychology: Animal Behavior Processes, in press. Copyright 2009 by the American Psychological Association. Reprinted with permission.
Then how should we explain the dissociation? Probably capuchins used the Middle response appropriately because it is a perceptual response to a discrete stimulus class—just as the Sparse and Dense responses are. Probably capuchins did not use the Uncertain response because it is not grounded in a discrete stimulus class, because it is not like the Sparse and Dense responses, because it is structurally a second-order response about the judged failure of the primary Sparse-Dense discrimination. One already sees this qualitative task difference empirically in the Middle-Uncertain dissociation. Its explanation lies in the psychological difference between the tasks.

Recent research with macaques reinforces the idea that the uncertainty response plays a distinctive role in psychophysical tasks and deserves a distinctive psychological interpretation. Macaques do not need trial-by-trial reinforcement to make adaptive uncertainty responses (Couchman et al., submitted; Smith et al., 2006) because uncertainty responses are decisional processes that are not dependent on reinforcement history and conditioning feedback. Macaques do not need their uncertainty responses to bring any immediate, tangible reward (Beran et al., 2006, Smith et al., 2006) because uncertainty—definitionally and psychologically—is not a reactive, reward-based phenomenon. Macaques spontaneously respond Uncertain on Trial 1 of new discriminations, but then not on Trial 2 after grasping the discrimination’s basis (Washburn et al., 2006). This result highlights the agility and flexibility of uncertainty responses that conditioned responses would never show. Finally, macaques respond uncertain adaptively when facing abstract memory and relational-judgment problems (Hampton, 2001; Kornell et all, 2005; Shields et al., 1997; Smith et al 1998). One sees from this that macaques can make difficulty assessments even about abstract and derived mental representations.

Even the species difference between capuchins and macaques reinforces the distinctive psychological interpretation that uncertainty responses deserve. Hampton (present volume) noted that capuchins seem to be on the outside looking in with regard to tests of animal metacognition (Basile et al., 2009; Call & Carpenter, 2001; Hampton et al., 2004). Despite being equal to or better than macaques at many tasks that allow for associative, learned response patterns, they seem unable to match the macaque in this domain. The obvious implication of these results is that uncertainty responding is not associative, or else the capuchins would show it. This is also the implication from the dissociation shown in Beran et al. (in press).

Thus, many lines of evidence recommend focused research to understand the distinctive psychological role of the uncertainty response in discrimination tasks. In a sense, this was also the recommendation of Josefowiez, Staddon, and Cerutti (present volume) who pointed out that the field could worry less about what is (not) metacognition and focus instead on the processes and representations that allow animals to respond adaptively to uncertainty in the referent tasks.

Their suggestion is important, timely, and paradigm shifting. This approach means letting go the grip of formal mathematics. The mathematics is psychologically silent, and it can block one from thinking psychologically about uncertain situations. It means letting go the grip of stimulus/reinforcement, which are not applicable to recent uncertainty-monitoring findings and which do not explain task and species dissociations. It means focusing on uncertainty monitoring as a controlled process (Shiffrin & Schneider, 1977), an executive process, perhaps a conscious process. If we find the most illuminating psychological level of analysis, there will be many intriguing theoretical questions to consider. In our view, this is the pathway to the strongest theoretical advancement in this area, even though it means that we all will have to wrestle with our higher (cognitive) angels. We are optimistic about continued theoretical progress in this field, given the sharp interest in it and given the insightful comparative scientists exploring it whose work and contributions we admire and respect.

References


monkeys (*Macaca mulatta*) discriminate between knowing and not knowing and collect information as needed before acting. *Animal Cognition, 7*, 239-246. doi:10.1007/s10071-004-0215-1


