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Where is the skepticism in animal metacognition?

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Abstract

The comparative analysis of metacognition may answer fundamental questions about the evolution of cognition. Although a substantial amount of research has been directed toward this goal in the last two decades, the recent development of quantitative non-metacognition models has raised questions about the existence of metacognition in nonhumans. Kornell (2013) proposes that advances in animal metacognition may be made by following emerging trends in human metacognition research, namely that animal metacognition may take the form of drawing inferences from metacognitive cues without directly assessing the strength of memories directly. A problem with this approach is noted. Because the metacognitive status of certainty judgments in animals is at the center of the dispute in the field, demonstrations of the inferential view would not provide evidence that putative metacognitive cues are indeed based on metacognition. I argue that any preparation that claims to tap into metacognition needs to be tested against leading non-metacognition hypotheses such as Le Pelley's (2012) reinforcement-learning model. Progress in animal metacognition will come from the development of new assessment techniques that offer predictions contrary to non-metacognition hypotheses. Animal metacognition will advance by applying skepticism about methods and interpretation while letting the animals (and their data) settle the debate.

Keywords

metacognition; comparative cognition; skepticism; commentary

The proposal that nonhumans represent the status of their knowledge – metacognition, broadly defined as knowing that you know some information (or not) – has attracted considerable attention in comparative psychology. Research on this topic has increased rapidly over several years, in part, because an emerging body of research suggested that refinements in assessment techniques had advanced to the point that well-established approaches became available for researchers using a variety of species (Smith, 2009). This exciting development has been overshadowed by the re-emergence of new threats to metacognition interpretations of metacognition experiments and data. A major advance in the comparative-metacognition debate comes from the deployment of quantitative models of non-metacognition data (Crystal & Foote, 2009, 2011; Jozefowicz, Staddon, & Cerutti, 2009; Le Pelley, 2012; Smith, Beran, Couchman, & Coutinho, 2008). This approach demonstrated that putative metacognition data – using major data sets that had widely been

accepted as, perhaps, the best evidence for metacognition in nonhumans (e.g., Hampton, 2001; Smith, Beran, Redford, & Washburn, 2006), as well as other metacognition data (e.g., Foote & Crystal, 2007) – are quantitatively well-described by non-metacognition models (cf. Le Pelley, 2012). These developments set the stage for requiring creative new approaches to advance animal metacognition research.

A natural source of insight for animal metacognition may be found in the primary, well-developed literature on metacognition using human participants. It is in this connection, that Kornell (2013) examines a number of well-established principles derived from human metacognition. Importantly, Kornell reviews a number of lines of evidence which suggest that humans draw inferences (e.g., based on such non-metacognitive factors as ease of processing, fluency, reaction time, familiarity, recency, etc.) when solving metacognition problems, rather than directly accessing their memories, which precludes judgments of memory strength. Kornell points out that the direct-access-to-memory-strength view has dominated thinking in animal metacognition, despite the fact, as he also points out, that the direct access view has been tested and largely rejected in human metacognition – when inference and direct-access views have been put in conflict, humans relied on inference.

These considerations led Kornell (2013) to the proposal that animals may make certainty judgments by drawing inferences (e.g., ease of processing, fluency, reaction time, etc.) without directly accessing the strength of their memory, as humans apparently do. The value of this proposal, as Kornell emphasizes, is that it is an empirical question, which may readily be addressed by novel experiment (and he outlines two approaches). Kornell is correct to note that validation of the inferential view in animal metacognition would make some of the theoretical claims ascribed to animal metacognition – self-reflection, awareness, the missing link – inappropriate.

The potential problem with Kornell's proposal is that the inferential approach may not be widely accepted as evidence of animal metacognition. Consider reaction time as an example. It is generally accepted that evidence for metacognition in animals is established when alternative explanations are ruled out. If it turned out that difficult problems are responded to more rapidly than easy problems, then it would be difficult to conclude that certainty judgments were relying on metacognition (i.e., based on knowledge of an internal state). Instead, the animal may have learned to perform one action when reaction times are long and otherwise perform a second action – a discrimination so widespread in animals that it is a weak candidate for metacognition. In this connection, Hampton (2009) provided a helpful distinction between private and public events. Animal metacognition has traditionally been aimed at demonstrating knowledge of private events, to distinguish it from many other impressive domains of animal cognition that focus on public events. As Kornell notes, his own research has adopted this skeptical stance on animal metacognition in the past (Kornell, Son, & Terrace, 2007), as have others (e.g., Foote & Crystal, 2007; Hampton, 2001).

Why curtail this skepticism now? Kornell (2013) argues that inferential processes are at work in human metacognition and, this work is widely accepted as metacognition in people. This perspective, when applied to animals, requires some unpacking. The proposal is that perhaps animals have metacognition of the sort that people have. People use inference in

metacognitive assessments, and we tend not to doubt the existence of metacognition in people. So, the implication here is that perhaps animal metacognition can advance by focusing on inferences – without establishing that animals have direct access to internal states such as memory strength. But if the standards for establishing metacognition are relaxed in this way, how can we be assured that the animals have any access to internal states? How can we be assured that their decisions are based on private, rather than public, events? If the answer is that we cannot be assured in these situations, then why accept these demonstrations as evidence of animal metacognition?

Although I agree with the intuition that useful guidance may come from exploiting developments from human cognition, there is a breach in this instance. Arguably the best demonstrations of animal metacognition can be modeled using non-metacognition representations (Le Pelley, 2012). Consequently, the metacognitive status of certainty judgment is at the center of the dispute. Judgments in human metacognition have not faced the same problem. Perhaps we are less skeptical of metacognition in people because we tend to accept the self-reports of people ("I don't know...") at face value – whether this is advisable or not is a separate matter. To use Kornell's (2013) examples, we do not doubt that people are describing their internal states when they tell us: "I am bad at remembering names" or "what was I thinking?" Slow, hard to remember, difficult-to-answer questions may well provide important inferential cues that people readily use in metacognition experiments. Yet, we tend to trust someone who tells us, for example, that she does not know the capital of Uzbekistan. For several years, comparative psychologists were beginning to trust uncertainty responses from animals as a window into what these animals know (or do not know). The development of quantitative models that produce putative metacognition data without metacognition returned a measure of skepticism.

Consider Kornell's (2013) first proposed approach. Suppose, for example, that nonhumans were trained to use certainty judgments and in the course of the experiment, perceptual fluency was manipulated (e.g., reducing the contrast of studied items, as in Kornell's first proposed approach). It has been shown that certainty responses can be based on associative learning—without metacognition (Le Pelley, 2012); thus, any reduction in the quality of perceptual input may influence the response strength for the certainty response. Kornell proposes that manipulation of perceptual fluency will occur without manipulating performance on the primary task, which he predicts cannot be explained by reinforcement-based models. Although the insight may come from considering perceptual fluency and the inferential perspective, what would make the data convincing is that it would constitute a demonstration that these putative metacognition data cannot be predicted by non-metacognition models.

Some degree of caution is also needed when considering non-metacognition models in isolation from other cognitive processes. In this connection, consider Kornell's (2013) second proposed approach. Here, increasing the relative frequency of a target item within a session may bias confidence judgments that the target was presented in the current trial, and Kornell proposes that this judgment may be manipulated without impacting accuracy in a symbolic matching task where the target is used to select one item from a series of distractors. In the absence of evidence that the certainty judgment measures metacognition

(Le Pelley, 2012), the predictions offered in this approach may be obtained without reference to metacognition. For example, target frequency effects are well known in animals (Blough, 2000). Moreover, dissociation of multiple measures of performance in these tasks are not unprecedented – target frequency differentially affects reaction times and accuracy in pigeons (Blough, 2000).

Overall, Kornell's (2013) proposal to study metacognitive errors is valuable. However, what is needed – and I argue currently absent (Crystal, 2012; Crystal & Foote, 2009, 2011) – are techniques with animals that firmly establish that the performance is based on metacognition (i.e., one that cannot be explained by non-metacognition models such as Le Pelley's (Le Pelley, 2012) and others). With such techniques in hand, it will be valuable to assess metacognition errors and adaptive (non-error based) metacognition performance. What is needed is development of the currently missing techniques. My own view is that a sprinkling of skepticism is an essential ingredient. Any preparation that claims to tap into metacognition needs to be tested against leading non-metacognition hypotheses such as Le Pelley's model. It would be valuable if a quantitative model of metacognition was also developed. With both metacognition and non-metacognition models in hand, one could test predictions of the models under a proposed novel experimental protocol before conducting the experiment with animals. If the models make contrary predictions, the protocol would make an interesting, and critical, experiment to conduct, and would offer a major advance to the field of comparative metacognition.

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