

COMMENTARY

Reply to Jaakkola (2014): “Do Animals Understand Invisible Displacement? A Critical Review”

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Jaakkola (2014) critiques studies that investigate nonhuman capacities to track objects undergoing invisible displacements. She states that the results of most of these studies are tainted by cuing, that conceptual understanding is lacking, and that, as a consequence, great apes are the only nonhumans to have full Stage 6 object permanence. Any critique, however, must clearly take into account all published information on the techniques being used, including more recent data that counter its negative claims. Furthermore, disagreements as to the interpretation of the underlying mechanisms, although common, need not always cast doubt on the actual findings reported. Here I present material with respect to Grey parrots (*Psittacus erithacus*) to counter Jaakkola’s critique. First, I take issue with Jaakkola’s claims of cuing, based both on data from newer studies and citations from the original material. Second, I discuss her suggestions that associative learning rather than inferential mechanisms underlie demonstrated performance, pointing out some difficulties in drawing clear lines between the 2 interpretations. In sum, I argue that Grey parrots, at least, do indeed succeed on tasks involving invisible displacement, and demonstrate full object permanence.

Keywords: Grey parrot, object permanence, invisible displacement, Stage 6, parrot cognition

Critical analyses of published research are an important part of the scientific process. Only by constantly refining our techniques and our understanding of potential problems can we ensure that the best possible science is being performed, and critical analyses assist us in this manner. Such critiques generally take two forms: a critique of the experimental design, and a critique of the interpretation of the underlying mechanisms that are responsible for the behavioral data that are collected. Critiques of the first type are important because they may shed doubt on the validity of the data presented. Researchers who perform such analyses, however, must be especially careful to perform proper due diligence before asserting claims as to the errors of their peers, lest they inappropriately cast blame. Critiques of the second type are important because they lead to interesting discussions about differing opinions and interpretations, but rarely do they actually bring into question the validity of the data. Researchers who perform the second type of analyses, therefore, open the field to noteworthy discussions but must realize that they often simply *engender*, not settle, debate by making claims for their own interpretations. Jaakkola’s (2014) review of research on object permanence with nonhumans involves both types of critiques, discusses a number of different

species and studies, and comes to the conclusion that only great apes have achieved full Stage 6 competence. I limit my response to information that, with respect to research on Grey parrots (*Psittacus erithacus*), suggests her criticisms of the first type are unwarranted, and present commentary in response to her criticisms of the second type.

Issues of Cuing

Jaakkola (2014) takes a stance on inadvertent cuing, particularly with respect to sensory and social cues. Although the issues are, obviously, of considerable importance, the critical factors are whether the organism being tested is actually sensitive to the cues being discussed and whether any researchers have, in fact, demonstrated via relevant studies that the purported cuing is not an issue under the specific conditions of the experiments in question. In her critique of object permanence studies on Grey parrots, Jaakkola (2014) does not cite articles addressing the first concern nor does she fully acknowledge efforts on the part of researchers to address the second.

Sensory Cues

Jaakkola (2014) accepts that birds do not have the same keen sense of smell that might confound studies with dogs or cats. Nevertheless, her comment that controls for such cues are “*likely unnecessary*” (italics added for emphasis) rather than irrelevant in published object permanence studies tends to sow seeds of doubt in the minds of readers. To reassure her, and others, about the lack of olfactory cuing in Grey parrot studies, note the following several points.

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In the Pepperberg and Kozak (1986) and Pepperberg, Willner, and Gravitz (1997) studies, both food and nonfood items were hidden, and many different food scents had permeated the felt liner on the tray and interacted with the covers regardless of the reward (food or nonfood) being used. Notably, Jaakkola (2014) ignores the fact that nonfood items were even used in such studies. Nonfood items (metal keys, wooden beads, corks, etc.) were handled and cleaned equally by trainers, were stored in toy boxes open to air, and were covered in invisible displacements by cups that were stored with the other toys, which were handled and cleaned as much or as little as the toys themselves. Thus, little discernible scent difference would have existed between toy-baited and unbaited cups, and olfactory cues would therefore have been irrelevant for the toy-based trials.

A literature search finds that although some parrot species may use olfactory information, olfaction is not a strong enough sense to enable them to distinguish odor in the format of the closely spaced food-baited and unbaited cups used in our studies (note, e.g., Zelenitsky, Therrien, Ridgely, McGee, and Witmer, 2011 for a discussion of olfaction sensitivity in parrots). Even Gsell, Hagelin, and Brunton's (2012) study on New Zealand keas (*Nestor notabilis*) and kakas (*N. meridionalis*)—parrots that are expected to use scent more often than other psittaciformes (e.g., Healy & Guilford, 1990)—showed that responses were less strong to food-related scents than to other ecologically relevant material such as feathers (potentially containing pheromones; note Mihailova, Berg, Buchanan, & Bennett, 2014). Moreover, in Gsell and colleagues' (2012) study, scent levels were set by the experimenter so as to be potentially noticeable and lures, open to the air, were purposefully spaced approximately a meter apart so as to carefully disambiguate scented from unscented material. Too, the authors state that after cleaning the materials used in their study, residual scents were not detected by their subjects. In contrast, materials used in the parrot object permanence studies had no scents noticeable to humans and scents would have had to have been detected through covers. Additionally, the object permanence stimuli were only a few centimeters apart (generally ~5 cm), further confounding the ability to distinguish between baited and unbaited covers. Too, as noted in the published object permanence articles, the parrots never dithered—that is, they did not look from one cover to another before choosing—which could have suggested an attempt to pick up a scent trail.

Grey parrots also performed equally well whether rewards were food items or toys. I agree that it is unfortunate that statistical tests were not presented in the Pepperberg and Kozak (1986) study to backup this claim for Alex, and that the data sheets, after almost 30 years, are no longer available for statistical analysis. Note, however, that for the Grey parrot Alex, food was not a particularly strong motivator for a search: He would often reject a food reward and request a toy after succeeding on a food-based trial (Pepperberg & Kozak, 1986). Too, a mix of food and nonfood items was also used in the subsequent developmental study with the Grey parrot, Griffin (Pepperberg, Willner, & Gravitz, 1997); Griffin also performed equally well with food and nonfood items, and, in this case, using the data presented in the journal article, on invisible displacements, a Fisher's exact tests for results for food versus nonfood tasks provides a $p = 1$.

Finally, and critically, a more recent study (Pepperberg, Koepke, Livingston, Girard, & Hartsfield, 2013) performed a

specific olfactory control—a choice between two cups, similarly handled as those used in the 1997 study, and which were spaced as were those in the object permanence task, placed on a comparable tray (the tray from the original OP study was no longer in the laboratory), with one cup baited with jelly beans (slightly more strongly scented than the nuts and other food items used in the object permanence studies) and one not. Researchers found that all four of the four Grey parrots tested (including Griffin, the same bird as from the developmental OP study) were at chance at finding the desirable treat on such tests. Furthermore, the same type of controls (using seeds and nuts) were conducted by researchers in Austria in a related study on Grey parrots (Mikolasch, Kotschal, & Schloegl, 2011), demonstrating that the lack of olfactory cuing is not specific to one laboratory. Thus, olfactory cues should not be an issue of concern in the specific object permanence studies cited and should not affect the conclusions.

Social Cues

The specter of Clever Hans-type cuing overshadows almost every study involving nonhumans that takes place outside a Skinner box (note Burghardt et al., 2012). And, as Jaakkola (2014) points out, many forms of social cuing may exist. Thus, those of us involved in such “outside the box” studies must be hyper-vigilant against such forms of cuing.

Notably, the most obvious cues (at least for humans), those of pointing or human gaze, have been shown to be irrelevant in studies with Grey parrots when objects are closely spaced. Specifically, a study has been performed to test for such issues of pointing and gaze in Grey parrots, a study that Jaakkola (2014) does not cite. Giret, Miklósi, Kreutzer, and Bovet (2009) found that for the only instance in which Grey parrots did respond to human gaze, the two objects of interest had to be 1.6 m apart and the human face had to be deliberately turned in the direction of one of the objects. In that experiment, the bird could easily distinguish the line of sight, unlike the situation in the studies of object permanence, where objects were always less than 10 cm apart (two objects, ~5 cm apart; if three objects, the first and third object would be ~10 cm apart), and the human face was deliberately centered. Thus, Grey parrots would not be sensitive to issues of gaze in the object permanence studies cited (and experimenters clearly did not point to the correct choice); such matters would therefore not affect the conclusions. Nevertheless, mirrored sunglasses were actually used in a control task in Pepperberg et al. (1997); Jaakkola (2014) failed to mention our use of that control, and the relevance of that control for other trials.

Issues of body tension, breath holding, or other subtle cues still remain. As implied by Jaakkola (2014), such cues, indicating where the object was present—or indeed absent—and where birds should search, would be involuntary and impossible for the humans to control. Were Grey parrots sensitive to such human cues, however, they would have responded above chance in the Pepperberg, Koepke, Livingston, Girard, and Hartsfield (2013) previously cited controls as well as on other controls (using nuts) in that study and in the Mikolasch, Kotschal, and Schloegl (2011) research noted above, but they did not. I also note that in Pepperberg and Brezinsky (1991), no difference was found in the parrot Alex's accuracy on single versus double-blind tests for questions involv-

ing stimuli again spaced ~5 cm apart, showing that Grey are not sensitive to social cues at small interobject distances.

Should controls for these issues have occurred at the time of the original object permanence experiments? Given the complete lack of behavioral indications that would suggest that the parrots used such cues—that is, their lack of dithering, the speed of all of the birds' responses, their total lack of checking back with the experimenter during the process of choosing (they either responded immediately or refused to engage; see Pepperberg & Funk, 1990; Pepperberg & Kozak, 1986; Pepperberg et al., 1997)—implementation of controls for such cuing was considered unnecessary. Control trials at the time would indeed have strengthened our claims; however, given that additional published, accessible data now do exist to counter the possibility of such cuing, specifically in a quite similar task and with the same parrot as in the object permanence study (Griffin; Pepperberg et al., 2013), Jaakkola's (2014) arguments about social cuing are unwarranted and do not affect the conclusions of the original experiments.

Associative Learning

Without question, almost any task can be reexamined and the results interpreted as being based on learning by simple associational rules; with equal likelihood, such an interpretation can subsequently be countered by alternative, so-called "higher order" explanations (e.g., Heyes, 2014; response by Scott & Baillargeon, 2014). In some instances, the interpretations come down to a matter of opinion; in other instances, the debate serves to sharpen the issues involved. Regrettably, often what constitutes one researcher's stringent requirements for a response to be based on more than associative learning may be seen as additional training by another experimenter, and the two might find themselves simply agreeing to disagree. Such may be the case with respect to the object permanence studies.

Jaakkola's (2014) initial claim, that exposure to simple types of tasks aids the subject in learning how to solve more difficult ones has merit, except that in developmental studies, there is sometimes little choice. One cannot avoid testing simpler tasks before challenging a subject that is in the process of maturing with the more difficult ones. However, note that in Pepperberg et al. (1997), we specifically omitted some simpler tasks (invisible displacements involving one and two cups) to see if the parrot tested, Griffin, could succeed on a complex invisible displacement (three cups) without experiencing the simpler ones. As we note (p. 66), "This shift also showed that Griffin was not being trained in a stepwise manner, because success at a more complex task was then independent of experience with simpler tasks," particularly as he succeeded in the invisible task with three cups. Jaakkola (2014) seems to miss our point.

Jaakkola's (2014) arguments concerning the "drop-first" versus "drop-last" tasks—controls used to avoid cuing the subject to respond to a particular site during an invisible displacement—are more complicated. Almost all creatures try to make sense of their world; that is, they try to extract possible rules that enable them to cope with their experiences. Discerning patterns, looking for any sort of regularity, is a basic ability critical to survival, which is why associative learning is indeed so prevalent and can be used to explain so much. For those reasons, however, almost any behavior patterns can, with enough work, be explained in associative terms,

from activities involving mirror neurons (Cook et al., 2014) to metacognition (Le Pelley, 2012), with equally energetic rebuttals (respectively, to the former in multiple responses to the cited target article in *Behavioral and Brain Sciences*, and to the latter in Smith, Couchman, & Beran, 2014). Such debates center on the underlying mechanisms responsible for the behavior patterns rather than the observed data, and, in general, are not easily resolvable. Such is the case with the object permanence studies. For example, if, as Jaakkola suggests, subjects must be given "drop-last" trials in addition to "drop-first" trials so as to avoid using rules like "go to the last indicated container" and "pick the first container touched," the likely outcome is simply *another*, possibly even *simpler* associational rule: "Look at site used just prior to being shown that the displacement device is empty, whatever else is going on." Such was the reason we did not use the drop-last control: Based on Jaakkola's reasoning, this step would not be an associative cue *control* but rather simply another associative cue. Note that if Griffin had gone for the "first container indicated in any way" (Jaakkola, 2014), in Trial 14a he would have gone for the displacement device, which had been placed near the objects used as covers (Pepperberg et al., 1997). And, of course, the addition of more trials, even those involving what could be considered controls, increases the likelihood of training a response to the task.

Jaakkola may wish to argue that the best way to control for associative learning is simply to have more associational rules, so that the subject must discard some to use others, or to make the situation complex by the addition of more rules—that is, ostensibly to add cognitive complexity. Her arguments do raise the possibility of a very interesting dialogue as to exactly how many and *what type* of rules are required to counter claims against a task being simple associative learning. Although I very much agree with Jaakkola that responses based on fairly simple perceptual associations must be excluded so that inferential claims can be made (note, again, the controls not only for sensory cuing but also for associational explanations, Mikolasch et al., 2011; Pepperberg et al., 2013), she must realize that completely eliminating arguments for associative learning is not possible. Quite likely, no matter what our procedures, some of our colleagues would still find ways to explain whatever was presented in terms of associative learning, merely leading to additional (if interesting) debate, and not necessarily ever allowing a conclusion as to whether nonhumans understand invisible displacements.

Conclusions

I agree with Jaakkola's (2014) basic argument that it behooves researchers to backup their claims appropriately, because that is the basis of the scientific method. Equally important, however, is that scientists who are inclined to dispute their colleagues' claims need to be exceptionally careful that the issues being raised are indeed relevant and have not been discounted by additional research, to ensure that seeds of doubt are not planted unnecessarily. A careful review of the literature concerning Grey parrot abilities would clarify that they are not sensitive to cues—olfactory, gaze, body position, and so forth—at the small interobject distances used in the published object permanence studies. I leave it to my colleagues studying other species—birds, mammals, nonhuman primates—to defend their studies and present whatever arguments they deem appropriate. In contrast to my strong rebuttal with

respect to Jaakkola's critique of experimental design, I take a more conciliatory stance with respect to her interpretation of underlying mechanisms. Although I counter Jaakkola's (2014) arguments against associative learning, I realize that she is just as likely to counter my arguments. Thus, any debates we may have about all but the simplest associative learning explanations, although intriguing, will not, to any noticeable extent, bring researchers any closer to agreement as to what capacities (object permanence or otherwise) exist in nonhumans. Overall, given the additional data reviewed here and the reiteration of material in the published articles, I argue that Grey parrots, at least, do indeed understand invisible displacements, and that enough evidence exists to discount Jaakkola's (2014) claims to the contrary.

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