
Biology and Philosophy **17:** 695–702, 2002.

Book Review

A Skeptic's Progress

COLIN ALLEN
Texas A&M University
Department of Philosophy, 314 Bolton Hall
College Station, TX 77843-4237
USA

A review of Daniel J. Povinelli, with James E. Reaux, Laura A. Theall, and Steve Giambrone, *Folk Physics for Apes: The Chimpanzee's Theory of How the World Works*, Oxford University Press, 2000, 400 pp., (Hb) ISBN 0-19-857220-4, £49.50.

Seven chimpanzees in twenty-seven experiments run over the course of five years at his University of Louisiana laboratory in New Iberia, Louisiana, are at the heart of Daniel Povinelli's case that chimpanzee thinking about the physical world is not at all like that of humans. Chimps, according to Povinelli and his coauthors James Reaux, Laura Theall, and Steve Giambrone, are phenomenally quick at learning to associate visible features of tools with specific uses of those tools, but they appear to lack cognitive access to forces and other invisible causal features of those tools. Povinelli's chimps apparently rely on a trial-and-error strategy to learn whether a particular tool is suitable for a particular task, and and having mastered one task they appear unable to generalize to other tasks on the basis of tool properties that are not directly visible. Thus, for instance, Povinelli's research subjects did not immediately recognize that a tool that had been demonstrated to be non-rigid would be unsuitable for dragging a piece of food towards them. When presented with a choice between a rigid, T-shaped "rake" that they had used many times previously and a rake with non-rigid arms, Povinelli and Reaux found, over the course of eight trials, that their chimps chose the non-rigid rake as frequently as they chose the rigid one (experiment 9, chapter 7).

One animal, Jadine, provided an exception, choosing the rigid rake a perfect eight times out of eight. But the researchers recalled that Jadine was particularly fearful of rubber snakes, and wondered whether her success in obtaining the food was simply due to an aversion to the arms of the flimsy rake. To test this idea, they constructed two hybrid rakes, rigid in one arm and non-rigid in the other, and presented Jadine with a choice between pulling on

a rake with food next to its rigid arm or pulling on one with the food placed next to the non-rigid arm. Over eight trials with this set up, Jadine's performance dropped to chance (experiment 10). Jadine's choices thus appeared not to be based on the idea that contact between the object and a rigid arm is required for successfully retrieving the object. The transition between these two experiments is illustrative of the strong internal logic running through all twenty-seven of Povinelli's experiments. Through these experiments, sophisticated interpretations of the animals' abilities to manipulate tools and other equipment are repeatedly undermined by showing that they fail to generalize successful manipulation in the next experimental set up.

What, though, of the external logic? What does the fact that these seven chimpanzees do not appear to understand the importance of features such as tool rigidity imply about chimpanzees in general? It is here that some serious questions can be raised, and have indeed been raised by reviewers in the major scientific journals *Science* (Hauser 2001) and *Nature* (Whiten 2001). Lack of generalizability would follow if Povinelli's chimps are in some way atypical, particularly with respect to the development of their physical understanding of the world. One obvious suggestion is that there is some difference between the experiences of Povinelli's captive chimpanzees and their wild counterparts that could account for differences in their comprehension of the physical principles related to tool use. After all, wild chimpanzees have been observed to modify sticks to make them more effective for fishing for termites, and they are known to select, guard, and carry over long distances stones that have particularly desirable properties for the purpose of cracking open nuts. In the final chapter of FP, Povinelli addresses this objection, pointing out that his animals have no trouble learning how to use tools given enough exposure to them, and arguing furthermore that it is possible his captive animals have had *more* opportunity to learn about tools than animals in the wild. Yet this may not settle the matter, for as both Marc Hauser and Andrew Whiten point out in their reviews, field data from Tetsuro Matsuzawa's studies of chimpanzee tool use (see Matsuzawa 2002) argue for the importance of age-critical processes that may not have been fully completed by Povinelli's chimps given their unusual rearing from infancy in a peer group without adults. Hauser also suggests that Povinelli's animals, who were all studied between the ages of five and ten, may have initially been too young to show full competence with tools, confounding the early results.

Matsuzawa's work may be relevant in another respect not mentioned by the previous reviewers. As he describes it, successful tool use depends on young chimpanzees observing the behavior of adults for a long time during infancy (Matsuzawa 2002). The opportunity for observational learning, whether from adults or peers, was entirely missing from Povinelli's set up. For

instance, prior to experiment 9, pairs of chimpanzees were allowed "to freely play" with the tools for twenty minutes a day for each the four days leading up to the experiment. These short play periods occurred before the animals knew what they were going to be asked to do with the tools. Being tested singly, they never have the opportunity to observe each other's successes or failures in using the tools. And while the humans involved in the experiment spent a certain amount of time attempting to draw the test subjects' attention to relevant features of the tool (at the beginning of each trial a human demonstrator obtained the animal's attention and demonstrated the rigid and flimsy properties of each the two rakes "in an exaggerated manner for 15 seconds" (165)) there was still no demonstration to the animal of how this feature of the tool interacts with the problem of retrieving a food object. (Hauser also complains in his review that it is impossible to tell from the descriptions in FP exactly what kind of modeling was provided by the humans, and how this might have biased performance.)

In order to resist the suggestion that his animals have an atypically deficient understanding of tools Povinelli would likely fall back on the claim that "Our chimpanzees had no trouble whatsoever learning to use the tools we presented to them" (327). But this seems beside the point, for the issue is not whether the same proficiency in using tools can be attained somehow or other, but how it is attained. The New Iberia chimps were raised from infancy in a peer group, without benefit of adult modeling, and in an austere concrete and chain-link environment that lacks the sheer variety of manipulable objects found in natural environments. This situation, which some primatologists regard as inappropriate for chimpanzees, could quite plausibly affect development and understanding of tools, even if other learning strategies enable the animals to achieve a certain level of competence with tools by other routes. Indeed, there is a certain irony in this last point, for one of the chief axes that Povinelli and coauthors grind throughout FP is a philosophical critique of what they call "the argument by analogy" from similarity of behavior to similarity of cognition. It is striking that Povinelli is willing to assert that his chimpanzees are not all that different cognitively from their wild counterparts, based on a relatively superficial similarity – both groups use (different) tools competently – when this very same similarity is judged an insufficient basis for making that claim about similarities between human and chimpanzee cognition. Likewise, I believe that it would be hypocritical for a defender of Povinelli to fall back on another possible response that appeals to the apparently quite robust nature of *human* cognitive development across a variety of unusual environments to argue that despite the unusual circumstances in which Povinelli's chimps were raised, one should expect them to develop an understanding of tools similar to that of chimpanzees in the wild.

I'll return to the critique of the argument by analogy below, but before doing so, what is the bottom-line assessment of the scientific component of FP? Hauser, in his review for *Science*, provides the most scathing assessment, pointing out that none of the twenty-seven experiments in FP had been published in peer-reviewed journals, and writing, "Had I been asked to evaluate *Folk Physics for Apes* for a peer-reviewed journal, I would not have recommended publication." Too harsh perhaps, but even if publishworthy there is a salutary tale related to Povinelli's earlier foray into skeptical primatology, in the area of theory of mind. In previously published work (extensively reviewed in the first chapter of FP by Povinelli and Giambrone) Povinelli and colleagues showed that the same set of chimpanzees were surprisingly unable to generalize appropriate food begging behavior across different conditions where the choices were between begging from humans who could or could not see the chimpanzees. These results led Povinelli to conclude that the chimpanzees' understanding of seeing is nothing like that of humans age 5 and up, who are able to reason about what others do or do not see. Instead, he argues that chimpanzees apply a hierarchical set of rules concerning bodily and facial orientation of the target of the begging.

More recent experimental work by Brian Hare and colleagues (see Hare and Wrangham 2002 for an overview) suggests that under certain conditions, chimpanzees do seem to know what others do and do not see. For instance, Hare and Wrangham write that "when two pieces of food were placed in view of both competitors, the dominant subject retrieved the majority of food. If one piece of food was hidden behind an occluder from the dominant while the subordinate could see both, as subordinates, subjects preferred to retrieve the hidden piece of food that the dominant could not see. In addition, if one piece was hidden behind an occluder from the subordinate but the dominant could see both, as dominants, subjects preferred to retrieve the visible piece of food first to assure they obtained both pieces" (2002, p. 366). Hare and Wrangham argue that Povinelli's own negative results are due to the use of a "cooperative-communicative paradigm" that is less natural for chimpanzees – that is, Povinelli asks his chimps to engage in cooperative communication about food, whereas competition for food is the more normal problem that they face. Hare's experiments employ a "social-competition paradigm" which, Hare and Wrangham argue, tests cognitive abilities in circumstances that are closer to the conditions for which they are adapted. As Whiten puts it, "If Povinelli's gigantic prior analysis of chimpanzees' folk psychology can be overturned by an elegant experiment more intuitive for chimpanzees, what of the prospects for the current, equally voluminous onslaught on folk physics?" (Whiten 2001). The jury is still out on this question, of course, but philosophers would be well advised not to take Povinelli's results on

either second-order mentality or folk physics to be definitive. (Hare's findings should likewise be treated as preliminary.)

Despite these worries, FP remains an impressive work of sustained experimentally-based argumentation. In this respect I'm inclined to side less with Hauser and more with Whiten who describes FP as "a rigorously documented set of internally consistent results" (Whiten 2001) and I agree with him that anyone in the field of chimpanzee cognition will be required to come to terms with Povinelli's challenge to those who would defend attributions of sophisticated cognitive processes. Nevertheless, FP remains a curiously uneven book, and nowhere is this more apparent than in the more "philosophical" parts, and in particular in the critique of what the authors call "the argument by analogy." Povinelli's coauthor in these parts of FP is Giambrone, a philosopher and colleague at the University of Louisiana.

In the first sentence of their chapter near the beginning of FP, Povinelli and Giambrone set out to "expose the logical weakness in assuming that the similarity in the natural behavior of humans and chimpanzees implies a comparable degree of similarity in the mental states which attend and generate that behavior." Of course, the fact that arguments by analogy are not deductively valid is hardly news, but it is far from clear what they mean by "logical weakness" if it is not the failure of the premises to imply the conclusion, especially given their own use of the word "implies". Things get murkier still when their exact target of their attack seems to shift, just two sentences later. They assert that they "are not indicting arguments by analogy in general" but that their target is "'the argument by analogy' for the existence of other minds" (9). However, the question of whether other minds exist is rather different from the question of whether the minds of other animals are like ours, and, as Povinelli makes very clear elsewhere in the book, he is not in any way challenging the claim that others have minds, even when the others are chimpanzees. Povinelli and Giambrone seek to relate the two different arguments by claiming that Bertrand Russell's strategy in arguing by analogy for the existence of other minds is a special case of a more general strategy of arguing for similarity between human minds and other (animal) minds. But even if this were true it would seem to be a mistake to take the special case as a model for the general, as they do in the final section of their chapter. In particular, the more general argument does not need to rely on introspectively derived premises in the same way that Russell's argument must, for the more general argument need not confront the skeptical problem of solipsism, and so their criticism based on the limitations of introspection (see esp. p. 69) seems beside the point.

Such carelessness about what exactly "the argument by analogy" is arguing for could perhaps be forgiven if Povinelli and Giambrone had a clear

point to make about the logical weakness in arguments by analogy for similarity between chimpanzee and human cognition. Unfortunately no such point is forthcoming. A major claim towards the end of their mammoth 64 page chapter turns out to be that they "have presented empirical and theoretical reasons to doubt the argument's validity" (69). But as noted before, no one should have thought validity is the right issue. Focusing a bit more charitably on their claim that careful experimental analysis shows that superficially similar behaviors have different cognitive causes produces no additional enlightenment. Inductive reasoning is non-monotonic: new information, in this case derived from experimentation, can cause a revised assessment of the likelihood of the conclusion without contradicting either the original premises or the assessment of the inductive strength of the argument when it was based on just those original premises. The results of the experiments on seeing show that chimpanzee experimental subjects have a different learning profile from human children run through the same experiments. The respective learning profiles are just more behavioral data, and in this case there is a marked dissimilarity in performance between the Povinelli's chimpanzee subjects and his human subjects which causes us to reassess claims about similarities between the respective cognitive processes. Far from pointing out a special "logical weakness" in the argument by analogy, what we learn is that the more behavioral information we have, the more accurate our assessments of cognitive similarity are likely to be. No news there.

I previously mentioned the irony of rejecting the analogy argument while relying on behavioral similarities between the tool-using behaviors of wild chimps and Povinelli's captive chimps to argue that the captive chimps are cognitively similar to their wild counterparts. Here, there is at least some prima facie plausibility to the claim of cognitive similarity, because the comparison is between members of the same species. The same cannot be said, however, about the weight that Povinelli and Giambrone place on a computer simulation of chimp social behavior to support their deflationary account of chimpanzee social cognition. Citing work in an unpublished doctoral dissertation, they maintain that the software is able "to produce realistic simulations of chimpanzee societies" (63). Given the crude state of all current behavioral simulations, any similarities between the simulated behavior and the behavior of real chimpanzees are going to be partial at best, and pretty much in the eye of the beholder at worst, and in either case inadequate to support their contention that "each individual artificial chimpanzee can be thought of as having a mind, but no 'theory of mind' " and that this is sufficient to "generate the social complexity necessary to provide a realistic approximation of chimpanzee political struggles" (64). This apparent double-standard regarding when it is appropriate to rely on the argument by

analogy (especially on the basis of work that has never been peer reviewed) is troublesome.

Another curious feature of FP generally, and the attack on the argument by analogy specifically, is that its targets seem inappropriate. Hume, Darwin, and Romanes are named as abusers of the argument by analogy, but it is unclear that any one of them is guilty of the particular sin that Povinelli and Giambrone directly target: that of erroneously attributing second-order mental states to animals. All these authors thought that many nonhuman animals have minds, but so does Povinelli. In the final, single-authored chapter of the book, Povinelli names the BBC and *National Geographic* as purveyors of a cognitively anthropomorphic view of chimpanzees. But is unclear that these should be considered serious academic targets. Similarly, it seems churlish on the last page to use a hyperbolic quotation from Sue Savage-Rumbaugh (Kanzi's trainer) that was given in an interview with a feature writer for *Ms. Magazine* as a serious indication of her detailed assessment of the degree of similarity between the Kanzi's cognitive abilities and her own. This is the only mention of Savage-Rumbaugh's impressive work with Kanzi in the entire book, and if Povinelli wishes to demonstrate that she makes inappropriate use of the argument by analogy in her scientific work, a more scholarly treatment (with particular attention to the differences between bonobos and common chimpanzees) should be demanded. A similar complaint can be raised against the other brief and sometimes elliptical references to the work of other primatologists to be found in FP.

Perhaps a clue to the zealousness of these attacks can be found in Povinelli's preface to FP, which begins with the confessions of a former romantic. "My earliest impressions of chimpanzees were, to put it mildly, rather absurd," he writes (ix), and he goes on to blame his early anthropomorphic views of chimpanzee cognition on everything from television science programs to *Planet of the Apes*. No one now would accuse Povinelli of remaining a romantic (unless it is about computer simulations), but his apparent suspicion that anyone who does not adopt his skeptical stance toward chimpanzee cognition is as romantic as he once was strikes me as unrealistic as his former view (which is not to say that there are no romantics).

Our closest living relatives, chimpanzees are widely believed to be the most cognitively similar creatures to human beings. In one sense this is likely to remain true no matter what we discover about chimpanzee cognition, for no matter how dissimilar they are from us, we are unlikely to find anything closer. But of course chimpanzees are very different animals whose cognitive capacities have been shaped by three or four million years of divergent evolution from the common ancestor we share with them. Despite my complaints, FP does a commendable job of making some possible cognitive differences

more salient, and it contains some interesting suggestions about the evolutionary origins of the differences between chimpanzees and other primates, including humans. But it must be noted that these evolutionary speculations have not themselves been subjected to the kind of rigorous testing that Povinelli holds up as the necessary standard for progress in primatology.

References

Hare, B. and Wrangham, R.: 2002, 'Integrating Two Evolutionary Models for the Study of Social Cognition', in M. Bekoff, C. Allen and G.M. Burghardt (eds), *The Cognitive Animal*, MIT Press, Cambridge, MA, pp. 363–369.

Hauser, M.D.: 2001, 'Elementary My Dear Chimpanzee', *Science* **291**, 440–441.

Matsuzawa, T.: 2002, 'Chimpanzee Ai and Her Son Ayumo: An Episode of Education by Master-Apprenticeship', in M. Bekoff, C. Allen and G.M. Burghardt (eds.), *The Cognitive Animal*, MIT Press, Cambridge, MA, 189–195.

Whiten, A.: 2001, 'Tool Tests Challenge Chimpanzees', *Nature* **409**, 133.