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Cognition and consciousness in nonhuman species: Open Peer Commentary and Authors' Responses

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Experimental cognitive ethology. *Why stop with apes [P&W]?* The experiments described in P&W's paper provide an excellent example of experimental cognitive ethology. Most are still under way or planned for the future, so that it is not yet possible to evaluate what they may tell us. The preliminary results sound promising, and we will await with considerable interest the subsequent more complete report. Meanwhile P&W have set all ethologists an excellent example by trying an approach which, if successful, will be very informative indeed. There is no reason why comparable experiments should not be attempted with animals other than Great Apes. Obviously the experimental details will need appropriate modification, but the basic possibility deserves serious exploration. Unsuccessful and usually unpublished attempts to elicit significant responses to motion pictures have discouraged this approach, and it may well be that only a few of the most gifted species will prove amenable to it. But significant results have been obtained by Jenssen (1970 *op. cit.* G) with *Anolis* lizards and by Amlaner and Stout (1978) with gulls. Only further efforts will disclose whether a wider variety of social animals can be induced to respond to experimental simulations of various kinds in a sufficiently normal fashion to permit experimenters to establish significant two-way communication. The potential significance of the results that might be achieved is difficult to overestimate, and the tentative conclusions discussed by P&W give us a taste of what may lie ahead.

The final remarks concerning primitive and natural inferences and "intelligent" positivism should stimulate all interested behavioral scientists to consider carefully whether preconceptions or biases affect their interpretations of animal behavior. To the extent that inferring mental states in other living organisms is primitive and natural, the possibility arises that such states are widespread among social animals. Even though the content of the inferences may be simple and crude, their presence would be a significant attribute of animals. It is correspondingly important for ethologists to ascertain whether or not such inferences about the intentions of other organisms are present in various species, and if so, under what conditions. If this prospect appears fanciful, unrealistic, or too difficult to warrant serious consideration, I suggest that we ask ourselves how realistic P&W's experiments would have seemed had anyone suggested them ten or fifteen years ago.

Parsimonious intentionality [SR&B]. The ability of Austin and Sherman to communicate about tools needed to acquire particular foods provides important new evidence that chimpanzees can communicate intentionally with at least rudimentary understanding of what they are doing. There will doubtless be some, like Sebeok (1977), who continue to suspect that unrecognized cues, *un*intentionally provided by the experimenters, may be responsible for what looks so much like intentional communication. But the complexity that would have to be postulated in such a "Clever Hans" explanation makes the assumption of intentionality seem parsimonious. The ability of Austin and Sherman to interchange roles is also important; it is reminiscent of the spontaneous interchange of transmitting and receiving roles observed by Lindauer (1971 *op. cit.* G) in honeybees dancing about the location of cavities. As other commentators will doubtless discuss in detail, all the accomplishments of Washoe and her several successors can be interpreted in behavioristic terms as complex examples of discriminative learning. But since human intellectual activities and linguistic communication can also be viewed in a similar light, such interpretations are not particularly helpful in trying to decide whether or not apes are sometimes aware of the results their signalling is likely to produce.

Much will depend on the extent to which apes that have been trained to employ symbolic communication may later use it spontaneously to accomplish objectives not built into the experimental situation by the investigators. It is too early to expect ideally controlled and completely documented examples of spontaneous symbolic communication, because the acquisition of such communicative abilities is a necessary first step before they can be used in a spontaneous and creative fashion. Spontaneous behavior is by its very nature difficult to organize into a controlled experiment. If it has been anticipated by the experimenters and arrangements made for suitable controls, these arrangements themselves may encourage the occurrence of what looks like spontaneity. It may be necessary to wait and see whether apes surprise us by unanticipated uses of symbolic communication for their own purposes.

The leading groups of experimenters training apes to use symbolic communication share a general faith in the significance of the endeavor, but they are often critical of each other's specific experimental procedures. If one accepts the sharpest of these mutual criticisms, it is tempting to dismiss all of the results as inconclusive. But this type of experiment is so new, so challenging, and so exhaustingly difficult, that despite serious problems remaining to be resolved, the accomplishments of both apes and experimenters suffice to demonstrate much more versatile and enterprising thinking on the part of nonhuman animals than most scientists were prepared to recognize as little as ten years ago. It is difficult to escape the conclusion that these animals must have specific intentions, definite awareness, and at least rudimentary understanding of the relationships about which they manage to communicate.

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Commentary by **E. Sue Savage-Rumbaugh, Duane M. Rumbaugh,** *and* **Sally Boysen**
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Sarah's problems of comprehension. We concur with G's suggestion that comparative psychologists should investigate the experiential similarities between human and nonhuman brain function. The study by P&W is obviously an attempt to answer this type of challenge. Does it succeed? Do the techniques put forth by P&W demonstrate that the chimpanzee Sarah "recognized the videotape as representing a problem, understood the actor's purpose, and chose alternatives compatible with that purpose?"

P&W reach these conclusions on the basis of one animal's selection of static photographs; her selection of photographs is presumed to be statistically independent from trial to trial, although materials do not vary across trials on a given problem. No evidence, other than anecdote, is presented to indicate that Sarah (1) saw the videotaped scenarios as problems-to-be-solved; (2) perceived her choice as representing a solution to the portrayed problem; (3) understood either the elements of the problems or the nature of the solutions; or (4) if faced with the problem herself, could solve it. Such factors should not be presumed to be the inherent bases for Sarah's choices of photographs until it is clear that Sarah understood what she was choosing and why. While such binary choice tasks are of value if used appropriately, they become meaningless when the number of cognitive processes to be inferred becomes so large as to permit essentially a demonstration of anything, provided that the researcher frames his questions in a sequentially embedded fashion.

P&W's failure to demonstrate that Sarah indeed perceived the taped segments as problems allows for the real possibility that the dynamics of the scenarios were beyond Sarah's comprehension and that her photograph choices were based on strategies simpler than those imputed by the authors. If the scenarios were not perceived as problems by Sarah, then her choices obviously could not have been solutions. That she would pick the "correct" photograph – a static, single-frame sample of a "solution" – should not be construed, without corroborating evidence, to mean that Sarah saw a logical relationship between the photograph and an implied course of action which, if carried out, would solve the actor's problem.

For it to be reasoned that the scenarios were, in fact, problems in Sarah's perception, it should have been demonstrated at some point that the individual elements of the problem were meaningful to Sarah and that she understood their videotaped representation. The fact that she chose a photograph of a burning wick in response to a photograph of a furnace does not necessarily mean that she understood that furnaces must be lighted if one is to be warm. Perhaps it simply implies that Sarah knows that wicks "go with" furnaces and that keys and faucets do not.

If Sarah did *not* perceive the video scenes as representations of problems, then the question arises as to how she might have selected the alternatives as she did. It seems reasonable to conclude, given the variety of previous training paradigms Sarah has received, that she could have used relatively simple associationistic and match-to-sample strategies, none of which would require an understanding of either the problems portrayed in the videotape or that her choices represent solutions. For instance, it is clear from the video stills and photographs presented by P&W that problems 1 and 2 of the banana attainment series could readily have been solved by a straightforward match-to-sample of the photograph to the scene held on the monitor on the basis of physical similarity of the images themselves. Problems 3 and 4 present a more difficult match-to-sample choice; however, Sarah performed at chance on these. We suggest, based on numerous cognitive tests of chimpanzees (Rumbaugh 1970 *op. cit.* SR&B, 1971), that Sarah would use the simplest strategy available to her, and in the case of the food attainment problem, the simplest strategy was a physical match.

The next four problems (nos. 5–8) were object-choice tasks in which Sarah was to pick a photograph of an object that could be used to solve the actor's problem. That physical matching was precluded in this particular set of problems does not mean that Sarah did not use a physical match-to-sample strategy in the first set of problems as argued by P&W.

Sarah could have solved the second group of problems (problems 5–8) by past observational experience. It is important to note that Sarah had previous experience with this type of associative match-to-sample problem ("causality" problems, in Premack, *op. cit.* G 1976). It is more parsimonious to conclude that in the present study Sarah chose the key because she associated keys with locks, the faucet because she associated the hose with the faucet, and so forth, than to infer, as P&W do, that Sarah understood that a member of another species faced a problem, that she comprehended the nature of the problem, and that she intentionally chose a picture which represented the solution to be enacted by another individual.

We made a specific test of this possibility with our chimpanzees, Austin and Sherman, by presenting to them twenty-eight items with which they were familiar and having them select which one of two additional objects was to be paired with each sample item. The presented pairs were, in some cases, similar to those used by P&W (e.g., lock and key – see Table 1) and in other cases quite different. Five of the pairs represented items with which Sherman and Austin had worked in keyboard-related tasks and twenty-three pairs represented items with

Table 1. *(Savage-Rumbaugh et al.) Single-trial data on unrewarded, spontaneous associative match-to-sample task*

Sample items	Choice items[a]
foot	shoe *vs.* key
automatic food dispenser	dispenser tray *vs.* hose nozzle
vaseline jar	thermometer *vs.* lock
slide tray	slides *vs.* scrub-brush handle
wastebasket	plastic liner *vs.* hammer
padlock	key *vs.* scoop
scrub brush	scrub-brush handle *vs.* phone body
thermos with hole in top	drinking-straw *vs.* paper
pounding toy	mallet *vs.* dispenser tray
bolt	wrench *vs.* shoelace
head	hat *vs.* stick
can (used to hold washers)	washers *vs.* dowel (for juicer)
string (usually used with sponge)	sponge *vs.* nail
telephone body	receiver *vs.* bottle top
pencil	paper *vs.* thermometer
hammer	nail *vs.* slide
utensil rack (S)[b]	serving spoon *vs.* hat
bottle	bottle top *vs.* wrench
dipping platform (usually used with stick)	stick *vs.* straw
hasp	padlock *vs.* towel rack
magnet (S, A)	metal *vs.* shoe
hose	spray nozzle *vs.* washers
paper towels (A)	towel rack *vs.* mallet
hotplate (A)	pan *vs.* plastic liner
can	can opener *vs.* sponge
shoe (S)	shoelace *vs.* telephone receiver
food bin	food-scoop *vs.* can opener

[a]Correct item, left. Right-left positions of the items when presented to the subjects were randomized.
[b]S and/or A indicates incorrect response by Sherman and/or Austin.

which they were familiar, but with which they had experienced no previous training. Incorrect alternatives were randomly paired and right-left positioned with correct alternatives; *all* testing was given in a blind situation. One experimenter sat outside the room with the sample object and another experimenter, who did not know the sample on any trial, sat inside the room with the alternatives. The chimpanzee observed the sample, went into the room, selected an alternative, and carried it out of the room to the first experimenter. The chimpanzees received social praise for all their choices; food reward was unnecessary. The pairs of items and the alternatives were presented *once* to each subject and are listed in Table 1. Sherman and Austin were both correct on 25 of 28 trials ($p < .0001$, binomial test), though their errors were not identical. Clearly, Sherman and Austin were able to make these selections easily *even though the experimenter did not present any formal problem to be solved* – simply an object. Their choices were based on associations between items.

On the basis of these tests we believe that P&W's conclusion that Sarah's choices were solutions to the actor's videotaped problems is untenable.

The fact that in the third experiment Sarah was able to choose the key, wick, and so forth, which was the "best" in the sense that it was intact, should not be taken to imply that Sarah understood that only intact objects could aid the actor in the solution of his problem. The photographs that required Sarah to choose among these objects were presented to her *after* she had already performed the associative object match. At this point, all that was necessary was for her to recognize and choose once again the previously rewarded photograph, as opposed to the new photographs placed beside them. Such recognition responses have been shown to be well within the capability of monkeys (Overman, MS submitted to *Science*, 1978), and one would presume that this would be an even easier task for a chimpanzee.

In the final set of problems described by P&W, Sarah is described as having chosen one set of alternatives for one actor and a different set for another actor, whom she reportedly disliked. The photographs and video scenes for the "good" actor were those which Sarah had seen before; it is not surprising then that for him she chose the same photographs as she had initially. Why did she choose the wrong alternatives for the "bad" actor? P&W do not present to the reader either the static video scenes or the still photograph solutions involving this actor. Thus, it is difficult to ascertain exactly what Sarah saw and exactly what she chose. In any case, for the bad-actor video segment, the good and bad alternatives were *all new* to Sarah, and thus she would not be able to choose alternatives that had been previously correct. Therefore, if she did not understand the problem, one would expect more variability in her performance, selecting in some instances good alternatives and in others bad. This is exactly what Sarah did.

In addition to the methodological problems described above, P&W's statistical case in very weak. Test-wise chimpanzees such as Sarah can easily learn a series of two-choice problems on the basis of correctness on the first trial and consequently be right on all subsequent trials. Therefore, in paired-choice tasks such as those used by P&W it is only the first trial performance on each problem that reflects the animal's comprehensional capacity. Sarah was given only *twelve trials* (four in the banana-attainment series, four in the object-choice, and four in the good-bad actor series) in which the correct choices presented to her on a given trial were composed of items *not* used on previous trials. Although Sarah was correct on three of the four first-trial choices on the first set of problems of the banana-attainment series and all four of the first-trial choices on the second set of problems (nos. 5–8), neither of these results can be statistically significant at the $p < .05$ level. (The probability of being correct on four choices in a row by chance is $(1/2)^4$ or 1/16, a p of only .0625.) As these two sets of four choices are from qualitatively different problems, pooling them together to obtain $p < .05$ is questionable. Even if the pooling were a valid procedure, it should be pointed out that the significance hinges on the correctness of a single choice of a single animal on a single trial, for although seven out of eight is significant, six out of eight falls well short of significance at the $p < .05$ level. P&W do not report Sarah's performance on the first four trails of the bad-actor series.

In conclusion, we feel that P&W's paper does not succeed in answering questions such as those posed by G. Though P&W attempt to offer a new set of techniques for the study of the ape's knowledge about problem-solving, close analysis reveals that theirs is simply a modified version of the traditional match-to-sample paradigm, with rich interpretations replacing important controls. Explicit efforts will have to be made to take into account criticisms of the kind made in this commentary if the study of animal thinking is to be resurrected from the disrepute in which it has been for so long.

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Talkers and doers. Humphrey (1977 *op. cit.* G) and Mason (1976 *op. cit.* G) were among the reviewers of Griffin's 1976 monograph (*op. cit.* G, SR&B) who noted that his advocacy of nonhuman awareness was neither a new position nor a revival of a dormant one. Early work in the field to nonhuman cognition ran headlong into the positivism of behaviorism and the materialism of neurobiology. The collision produced "talkers" and "doers." Talkers derided both the reductionism of the neurobiologists and the biological agnosticism of the behaviorists. However, their most eloquent pleas failed to slow either, and thankfully so, for both neurobiology and behaviorism have provided major contributions to our knowledge of the animal mind.

The doers on the other hand, while few, worked steadily. Their research was neither neurobiological nor behaviorist, but it was sound empirical research indeed. They preached a bit, but always after supplying data on animal cognition. A list of doers would surely include Köhler, Yerkes, Klüver, and Menzel.

To be sure, we still do not *know* much about awareness, mental experiences, mental images, predictions, or intentions in nonhuman animals, but this is not surprising, since we really don't know much about such phenomena in humans. However, we have discovered a great deal about how different animals acquire, store, and act upon information they encounter in their physical and social environment. I regard the admittedly modest contributions of the doers to be far more constructive than continued attempts, doubtlessly fruitless, to convert neurobiologists, behaviorists, and classical ethologists into cognitive ethologists.

The SR&B and P&W papers are in the best doer tradition. SR&B provide the first demonstrated instance of symbolically mediated cooperation in nonhumans, and show that such behavior is more than the mere sum of languagelike communication and cooperation. Indeed, the various stages of symbol acquisition and cooperative deployment of those symbols illuminate the essence of such behavior. I am excited to learn that apes will learn the symbols for objects that have functional value more rapidly than those for objects with no utility. SR&B's flagging of the importance of receptive capacity in nonhuman symbolic communication is timely.

P&W provide evidence that a chimpanzee can impute purpose to humans, but I do not follow the logic of the purported demonstration of the ape's also imputing knowledge to the actors. Replication to increase sample sizes would be welcome but, as the authors note, different tapes and photographs would be required. Sarah's remarkable capacity for vicarious mischief (toward Bill) has implications for the nature of chimpanzee sociality. SR&B note that their subjects sometimes cooperated spontaneously; will they also learn selfishly to deceive?

The SR&B and P&W papers are remarkable in that they both provide ingenious and innovative probes into the animal mind, and they do so with impeccable empirical technique. They exemplify the type of research that will constitute cognitive ethology as a science as contrasted with cognitive ethology as an assertion.

P&W seem at times to equate "a theory of mind" with the ability to predict the behavior of others. I am confident that they recognize that a theory of mind is not a logical requisite for predictive ability. P&W also allude to what I think is an important point and one that I have made previously (Beck, 1975): there is no need to assume that the most pedestrian association learning is devoid of mental experience. I daily avoid burning my hand, and I do so unconsciously. However, when I choose to contemplate the relationship between my cooking fire, my flesh, and pain, I can do so with rich imagery. Cognitive automation may be an adaptation to the conservation of an organism's consciousness for use on relationships that it does not yet comprehend.

I was a bit concerned by P&W's emphases on the "power and complexity of the primate mind" as opposed to that of "lesser species." Chimpomorphism will retard cognitive ethology. We should not conclude that symbolically mediated cooperation and theories of mind are confined to laboratory-housed chimpanzees. Field literature suggests that many wild anthropoids as well as social carnivores employ such cognitive operations under natural conditions. I have provided data that indicate that even herring gulls are cognitively active (Beck, 1976). If the evolutionary continuum of cognition proves to be so extended, Griffin's reminder of the implications for our care of laboratory animals must be amplified: "cropping" an elephant is distasteful but cropping a thinking elephant is, to me, unthinkable.

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Some remarks about concepts. *"Communication" and "symbolism"* [SR&B]. Savage-Rumbaugh et al. show that chimpanzees can interact, in intentional ways, using symbols. It is good to get away from interactions between chimpanzees and humans. The mere fact that one participant is human generates a bottomless reservoir of possible contributions to the chimpanzee's performance; these are all filtered out when the chimpanzees are induced to interact with one another.

It's also valuable that the chimpanzees faced problems which, though they were set up by humans, owed none of their essential features to that fact. All the problems had the form "How can it be opened?" rather than "What does E want me to do?" In moving from essentially contrived problems to possibly natural ones, SR&B have escaped from speculations about the chimpanzee's beliefs about the trainer's desires (see my comments below on P&W); and that filters out a further possible source of perturbation.

SR&B are to be congratulated on this elegant experiment. Unfortunately, their interpretation of it is flawed by their uncritical use of certain concepts. Their work breaks new ground, they say, because it "has demonstrated that two chimpanzees have been able to comprehend the symbolic and communicative function of the symbols they use." But if they have a definite sense for the word "communicative," it is one which makes that claim obviously false; and I cannot find any one sense that they are giving to the term "symbolic."

SR&B use "communication" to mean something distinct from – perhaps narrower than? – problem-solving, and this generates for them a criticism of Premack: "Does Sarah give evidence of comprehending that she is communicating with her teachers, or that they are communicating with her, as opposed to simply solving a set of problems?" One would like to know what distinction SR&B have in mind here. This matters, not just for their importance-claim, but also for the following reason.

The performances of SR&B's subjects are analogous to *injunctions* – requests, commands, pleas, etc. – which aim to elicit *behaviour* from the other party. contrast injunctions with *statements,* which aim to produce *belief* (or awareness, or realization, or knowledge) in the other party. Now, what is wrong with saying that an animal which purposively does *anything* is thereby requesting or commanding the universe to produce the desired result? If there were nothing wrong with that, then all purposive behaviour would be the uttering of injunctions, and all practical problem-solving would be "communication." There does exist a stopgap answer to this, namely, that we always restrict "communicate" to cases where both parties to the transaction are sentient. That is surely right. But why do we want this restriction? If it is not a mere aesthetic preference, a superficial linguistic nuance, then there must be some underlying rationale for it, some further strand in the concept of communication which will lie idle unless the communicators are both sentient. What can this further strand be? One possible answer is this: "Unless both parties to the transaction are sentient, the communicator can't produce a *belief* in the other; and the latter is involved in all communication." That is not the only nonarbitrary way of stopping the notion of communication from flattening out so that it covers all purposive behaviour; but it is one plausible way, and I think it is the one SR&B would choose. They repeatedly appear to assume that communication essentially involves transfer of information, this being understood as production of belief or awareness or the like. This implies that the basic purpose of an injunction such as "Hand me the stick" is – in SR&B's phrase – "the transfer of information regarding the necessary tool"; that is, the command is really the statement, "I want you to hand me the stick."

I like that account of injunction, and the account of communication in general which goes with it. I think that in most human communication the speaker does intend to produce in the hearer a belief-change which may, but need not, be intended to have some specific further behavioural upshot. That's over-simplified (see Bennett, 1976, §41), but it will do for now. Simplified as it is, it carries an impressive conceptual load: X *intends* to make Y *believe* that X *wants* such-and-such. And on any viable account of intention, that involves: X *believes* that X can make Y *believe* that X *wants* such-and-such. SR&B write as though they were content with this. They find it "difficult to understand how Sarah could come to realize that the plastic chips could be used to communicate desires," and they apparently think that their chimpanzees did come to "realize" such things. On the most modest construal of this that I can devise, it involves a belief about a belief about a desire. See also their pregnant remark: "The initiator expected the recipient to understand why he was gesturing."

Although this is the concept of communication which SR&B manifestly employ, they couldn't be content with it if they saw what it involved. For they would see that their experimental work goes no way towards showing that chimpanzees can "communicate" – let alone believe or comprehend that they are "communicating" – in this strong sense.

It's puzzling that one should have to say these things. SR&B themselves write: "The question of whether signing chimpanzees . . . comprehend the nature, function, and symbolic power of the symbols they use becomes a question of *awareness* and *intentionality*"; and evidently they see this as a problem area which is "now being reopened." I can't understand how, if they realize that, they can have permitted themselves to use the concepts of awareness and intention in such an innocently uncritical manner, both in evaluating their own work and in criticizing that of others.

Now let us turn to "symbolic." SR&B do not offer to explain this, and it isn't self-explanatory. Sometimes they apparently use it simply to mean "noniconic"; but then what does "iconic" mean? I would explain it in terms of *natural* associations between features, in contrast to those that are *institutional, artificial, stipulated,* and so forth; but then one could hardly credit chimpanzees with making a distinction between icon and symbol – with "understanding that they could make symbolic requests to one another" or "comprehending the symbolic nature of the materials they are using" – at least not on any evidence presented in this paper. It looks as though SR&B would tie "icon" to the notion of something that *resembles* what it stands for, and that generates a feeble sense of "symbol" in which the chimpanzee probably can distinguish icons from symbols, that is, similar pairs of items from dissimilar pairs.

But neither of those senses of "symbol" makes any sense of SR&B's crucial use of the term in their importance-claim: their chimpanzees, they say, comprehend the symbolic *function* of the symbols they are using; and elsewhere items are said to have symbolic *power.* These phrases are not explained at all, and couldn't be explained by reading "symbolic" as "noniconic" on any sane account of what "iconic" means.

An item's symbolic function (or power) might be its use (or capacity to be used) to symbolize or stand for something else; but SR&B surely do not think that their results show their chimpanzees to have a grasp of the relation of *standing-for.* The chimpanzees do, it is true, know that certain symbols are *associated with* other things, but other chimpanzees have been shown to know that much. They know further that these associations can be exploited for practical purposes; but there's nothing new about that either.

The best guess I can make is that in the phrases "symbolic power" and "symbolic function," the word "symbolic" is being given the same meaning as "communicative." SR&B do sometimes write as though they took those two terms to be equivalent. Thus they write: "It is impossible to tell whether the chimpanzee is simply imitating or echoing . . . the action or object, or whether the animal is indeed attempting to relay a symbolic message." Here something which is both symbolic and communicative is contrasted with something which is neither, and there is no attempt to sort out the ingredients of the mix; so the suggestion is that "(non)symbolic" and "(non)communicative" must stand or fall together. The same thing is firmly implied when SR&B allude to "the symbolic and communicative function" – not *functions* – "of the symbols they use."

Of course I do not accuse SR&B of not knowing that the terms "symbolic" and "communicative," far from being equivalent, are perfectly independent of one another: I'm sure that they do know. But I think that they have slipped into a way of writing and thinking which belies this knowledge. If I am right, then what seems to be a double importance-claim is really the single claim that their chimpanzees knew that their behaviour was communicative. If I am wrong, and SR&B's "symbolic function" and kindred phrases mean something other than "communicative function," I am at a loss to know what that meaning can be.

Beliefs about beliefs [P&W]. If one wants to apply to chimpanzees a concept of communication as rich as SR&B's, one must first ask what evidence there is that chimpanzees can have beliefs about beliefs about *anything* (let alone beliefs about beliefs about desires). It is good news that P&W are investigating this question in a properly isolated way, getting belief-about-belief out of the undergrowth and into the open where it can be looked at squarely. Their work – achieved and projected – is so interesting and potentially important that I would do anything to be allowed to help it along. I here offer what I can: three criticisms, a comment, and a suggestion.

First, a small point which illustrates a large one. The "empathy" interpretation of the first experiment is said to "assume that the animal imputes a purpose to the human actor [but] does not grant the animal any inferences about [the human's] knowledge." That is wrong, I submit: Sarah's behaviour isn't relevant to what she thinks the human *wants* or *intends* unless she thinks that he *thinks* (perceives, cognizes) his situation to be thus and thus.

The large point is that when one is attributing mental states on the evidence of behaviour, cognition and motivation must go hand in hand. An animal's actions show what it thinks only on certain assumptions about what it wants, and they show what it wants only on assumptions about what it thinks. This goes for our attributions to the chimpanzee, and for hers to the human: if she is perfectly agnostic about the human's cognition, how could she have any thought about how his purposes would lead him to behave, or about what purposes his behaviour would manifest? If she can have neither, then she can have no thought about his purposes.

So I dissent from P&W's conjecture that "inferences about motivation will precede those about knowledge." If the inferences are to be based upon behaviour, it is *impossible* for there to be this separation (see Bennett, 1976, §15). This is worth stressing, if it is true, as a warning against devising experiments where one of the two factors is attended to while the other hovers in the background, unrecognized and thus uncontrolled.

Secondly, some of the experiments are, as P&W recognize, disturbed by the question, What is Sarah up to? Presumably she *wants* to succeed in her assigned task, but that is an empty conjecture unless we know what she *thinks* the assigned task is. Although P&W are properly cautious about this, the situation may be worse than they recognize. If all goes according to plan, Sarah thinks, in the first experiments, that she is to *predict* what the human actor will do, in the next lot she thinks she is to *express what she prefers,* and in the "embedded videotape" experiments she thinks that *she* is to predict and that *he* is to express what he prefers. I'm not optimistic.

Might it not be better to escape from experiments whose interpretation depends on what the animal thinks its assigned task to be? My objection is not that that concerns what she believes the trainer wants, and thus concerns her pschological theory; for we can surely construe "what she thinks the assigned task is" so that it doesn't automatically credit her with any psychological theory. The objection is just that this feature introduces a not easily remedied uncertainty about how the results – whatever they are – should be interpreted.

Thirdly, although P&W's current experiments on the attitudes of chimpanzees to liars and fools have the merit of not involving the concept of an assigned task, they do strike me – as clearly they strike P&W also – as being highly tenuous for other reasons. I hope that more work will be done on the rockbottom matter of belief about belief before much more effort is expended on these more complex and recherché matters – in which I include the distinction between knowing and guessing. I don't deny that these matters are amenable to experimental investigation; but I suggest that they are better left aside until the groundwork has been more completely done.

Fourthly, as P&W imply in their closing remark, the price of avoiding mentalistic concepts is an increase in the complexity of the conceptual structures needed to handle the materials; and that is what justifies using mentalistic concepts. Now, suppose we know that in the first experiments Sarah was predicting how the human actor would behave, and let us ask: Why shouldn't she have reached this prediction by purely behavioural computations, without going through intermediate stages concerning his states of mind? P&W answer this, it seems, by saying that if her route to the prediction wasn't mentalistic, it must be describable in "associationist" terms, and they object to this for reasons that I am not sure I understand. Perhaps what they are saying comes down to what I contend to be the best answer (if it is true), namely: For Sarah to get from data to prediction by a route which didn't attribute beliefs to the human actor she would need an extremely complex inference, whereas she could get there without undue complexity if along the way she had hypotheses about the human's mental states. That claim about relative complexity is needed to justify crediting Sarah with beliefs about beliefs and not mere beliefs about behaviour.

Finally, whether the relative-complexity claim can truthfully be made in connection with those first experiments of P&W's is not clear to me, because I don't know enough about what Sarah's premises were. Exact information about an animal's data would be easier to get if the experiment had to do, not with "Does she think that he knows that heaters have to be plugged in?" but rather with "Does she think that he knows that this heater is not plugged in?" That is, there would be better grounds for a belief-about-belief interpretation if the focus were on her beliefs about what the human believes about this particular situation. For then one could vary the evidence she had about his knowledge of individual features of the situation – features which were relevant to whether or how the problem could be solved. Let her see that the key is turned while he is not looking; or that the box is sneaked away after being hidden from him by a screen; and so on. This could provide strong evidence for beliefs-about-beliefs, by showing that the chimpanzee must be making inferences which are dauntingly complex when stated in purely behavioural terms but relatively simple when stated in terms of mentalistic concepts.

How is she to manifest these predictive beliefs about the other party's behaviour? I suggest the following. Let A be the agent about whose beliefs we hope the subject chimpanzee will form beliefs. Give the chimpanzee abundant evidence of A's sharing her value-system and being prepared to cooperate with her towards common ends: this is to justify us in assuming that if she has any psychological theory about A, its motivational part will credit A with roughly the motivations that she herself has. Then construct coordination problems – situations where the chimpanzee can see that *what it is prudent for her* to do depends upon *what A is in fact going to do* (see Lewis, 1969, chap. 1). Her predictions about A will then be manifested in behaviour which can be much more confidently interpreted than could any amount of photograph-selection – namely, in terms of her pursuit of her own natural down-to-earth goals. I don't mean that interpretative problems may not still arise, but merely that they aren't likely to be problems about what her motivations are, what she is "up to."

I cannot put experimental flesh on these bones; no doubt Herculean labours are needed to turn such armchair proposals into a practicable programme. Still, abstract proposals can have value, and I offer mine, hopefully, as pointing to a possible kind of experiment which (1) keeps both belief about motivation and belief about cognition clearly in view; (2) avoids the notion of "the assigned task"; (3) can be aimed at the most elemental kinds of belief about belief, leaving the complex ones until later; and (4) makes it possible to know fairly exactly what the animal's data are, and what her conclusion is, thus facilitating the comparison (in respect of complexity) between the mentalistic and the nonmentalistic routes from the one to the other.

The need for criteria [G]. I share G's interest in developing a richly mentalistic psychology and ethology; but his advocacy, persuasive as it is, would be stronger still if he were more cautious in his deployment of mentalistic concepts. That would involve two things.

1. More clarity and explicitness are needed regarding the criteria for applying mentalistic concepts. G says a good deal about this in particular cases, for example, in his good remarks about the possibility that honeybees might learn to report on fibreglass; but his campaign needs a general statement as to what sorts of behaviour support which mentalistic attributions. It would be controversial, and would need defence; but the mere statement of it would give us a better idea of what G wants us to accept.

That statement of criteria should *govern* the discussion. One feels the need of it in, for instance, G's remarks about chimpanzee-communication experiments. He seems to assume that some of those studies provide evidence of "communication of intentions" in some significant sense, unless all of them are vitiated by "Clever Hans" errors. But there are hosts of ways in which those experiments might, without being vitiated by cueing, have to be interpreted as something other than communication of intentions.

The desired statement of behavioural criteria would direct more attention to the internal geography of our system of mentalistic concepts. G tends to cut corners, as in his handling of the case of the two planes, one guided by a kamikaze pilot and the other by a heat-seeking device, where "communication . . . might well convince us that a real, live, conscious pilot was flying one machine." So indeed it might, but how and why? We are to be impressed by the pilot's responding appropriately to our messages; and presumably the "simple dialogue" mustn't be so stereotyped as to revive the suspicion that we are talking to a tape – for example, it mustn't consist just in commands from us and "Yessirs" from the pilot. So the dialogue is to provide evidence that we should get appropriate replies to most things we might say; and the only thing capable of such a feat is a human being – a mind-endowed, conscious, aware person. Now, I don't dispute that a simple dialogue could convince me that the plane was piloted by a conscious being. But two corners have been cut.

Firstly, the story is not essentially one about communication. It's true that we are in doubt about the pilot until we discover that he can *communicate with us;* but that is a large, cloudy truth which contains within it the leaner and more precise truth that we are in doubt about the pilot until we discover that he can *respond appropriately to a large range of input.* The input is provided by us, and could in that sense be called "signalling," and thus "communication"; but that is a mere accident of the example, and not part of what makes the dialogue evidence as to the pilot's status.

Secondly, G's route from "The dialogue is succeeding" to "There is a conscious being at the other end" is invalid, for a reason which has nothing to do with whether "communication" is involved. The dialogue is evidence that the plane's guide would respond appropriately to a vast number of distinct signals, but that is not *in itself* evidence of consciousness. It points to consciousness only with the help of the fact that in our region of space-time the only things which have such a large store of responses are the higher animals, and they are all conscious. What qualifies them as conscious, however, is not the size and complexity of their stores so much as their flexibility, adaptability, educability. It is true that in the animal kingdom complexity and flexibility tend to go together, with humans having a uniquely high degree of each; and this is no mere coincidence. Still, they are distinct properties of an organism, and they are not equally relevant to consciousness or mentality. If there were a tremendously complex hard-wired device which could handle the pilot's end of the "simple dialogue" but which was perfectly ineducable, it would pass G's dialogue test yet wouldn't be conscious [See Haugeland: "The Plausibility of Cognitivism" *BBS* 1(2) 1978]. I'm not saying that G's test isn't good enough ("Maybe it's not a man answering us, but rather a technologically innovative device . . ."); I'm saying that he doesn't make sufficiently clear why it is good enough.

2. When the criteria are actually formulated, they should not be too generous, that is, they should not make it too easy to establish that a given animal is conscious. This is not because consciousness should not be spread too widely: I hold no brief for the view that we must keep the other animals at a respectful distance from ourselves. Rather, the reason is that the more lax the criteria for something's being X are, the less content there is in the statement "This animal is

X," and so the less interesting such statements are. Sometimes G tends to push towards making such statements true at the price of making them uninteresting. For example, in one place he asks plaintively "what else bees might be expected to do that would provide stronger evidence of intention to communicate, given the circumstances under which their behavior has been studied so far." This mislocates the onus of proof. Until we have good evidence that honeybees intend to communicate, we should not say or think they do; otherwise our uses of such expressions as "intend to communicate" become empty and boring. (My own guess is that the evidence will never be forthcoming; and I wish G hadn't permitted himself the jibe "Is it because bees are small?," ignoring a reason which surely deserves a respectful hearing – namely that the neural organization of bees may be too simple to permit that adaptability and flexibility which many of us regard as criterial for mentality. But that's by the way.)

G's remark about bees' intention to communicate, having served to illustrate my main point about severity of criteria, also illustrates my general theme. See what precedes it:

"Smith [says] that honeybee dances communicate information 'about characteristics of the next flight the dancing communicator will make' rather than about the location of something desirable. But the distinction between predicting one's future behavior and expressing an intention is a rather subtle one that is certainly difficult to analyze in another species. It is therefore appropriate to ask what else bees might be expected to do that would provide stronger evidence of intention to communicate. . . ."

This contains two conflations. Firstly, it conflates (i) the difference between flight-prediction and report-on-*food* with (ii) the difference between flight-prediction and report-on-*flight-intention*. It will be hard to get any purchase on (i) in the apian context, and G is right that (ii) is elusive in any context; but he writes as though they were the same distinction, when in fact they are as different as chalk from cheese. I can only suppose that this conflation results from a kind of conceptual hurry that appears to be present throughout the article.

Another sign of hurry occurs in the next transition in the quoted passage. There, G glides from "expressing an intention" to "intention to communicate," as though he didn't distinguish "X *communicates that it intends to fly*" from "X *intends to communicate* that it will fly." But these two are also quite different: the question of what is communicated, for example, whether it is a message about intentions, is independent of whether the communication is intentional.

Just because I find G's campaign so sympathetic, and so many of his details interesting and persuasive, I would like to urge upon him the importance of circumspection–of a patient, careful, continuous attention to conceptual foundations.

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by **Irwin S. Bernstein**

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Awareness, intention, expectancy, and plausibility. *Animal cognition [G].* G takes the position that behaviorists are unnecessarily impoverishing their scientific world by refusing even to consider animal cognitive states. The arguments posed and the counters to be expected will remind psychologists of the battles between Gestalt psychologists arguing for insight versus trial-and-error-learning theorists, and the arguments concerning latent learning versus reinforcement theory. Although we can all agree as to what response was performed by a subject, accounting for why that particular response occurred at that particular time requires some theoretical commitment. In one case, we examine the history of reinforcement for the subject prior to the time that a response occurs. In the other case, we assume that the present motivational state draws on past experience to produce an expectancy of the outcomes of available alternative responses. In actual practice the only difference in the two approaches is the inference of a mental state in the second view.

Why does one chimpanzee making the "correct" choice in a discrimination task quietly accept the carrot slice reward while a second, previously rewarded with banana slices, rejects the reward and shows behavior often described as temper tantrums or frustration? Can we ever reduce such a question to the realm of scientific investigation?

Communication and intention [P&W]. P&W answer with a resounding yes. In considering the sequence of responses exchanged between individuals, one can think that the behavior of the recipient is modified by the sender and that the new behavior of the former recipient is now received by the original sender and modifies its behavior in turn. In such a model, each new response is determined by the previous one. If the original sender, on the other hand, modifies its signal in such a way as to guide the receiver toward a particular form or behavior, or better still toward a class of responses any of which will produce a particular outcome, then the communication may be regarded as goal-directed.

The questions raised by P&W are even more sophisticated than the simple case given above. They outline a series of studies, some completed, some yet to be run, all of which may produce results which could reasonably be interpreted as demonstrating that chimpanzee subjects are capable of identifying (correctly or incorrectly) the motivational state of another. Much as we might respond very differently to a blow received if we regarded it as deliberate, accidental, or playful, the differential response of an animal to the same response in others, dependent upon a greater context, suggests an awareness of motivation in the other, or a "theory of mind."

Plausibility [SR&B]. The difficulty in obtaining "proof" for either G or P&W is exemplified by the contribution of SR&B. They begin by examining other studies of linguistic ability in chimpanzees, recognize the criticisms of such studies, and indicate what the appropriate controls should have been. They then present their most recent studies to show that their new work does control for many of the alternate explanations that have in the past been proffered by scientists who find linguistic ability in nonhuman subjects antithetical to their view of the world. SR&B suggest that subtle "Kluge Hans" phenomena, chance events, observer bias, and complex chain conditioning, in some combination, can account for the performances of Washoe, Sarah, and similar subjects. Proper controls and systematic data collection plus refinements of experimental design can indeed discount these alternative hypotheses for existing data relevant to language acquisition in the chimpanzee.

One can, nevertheless, suggest a complex sequence of learned responses involving matching and contingencies to account for the performances of Austin and Sherman. In fact, the learning sequences described, the error scores, and the dependency on particular interactants might make more plausible such an extraordinarily complex learning chain. We would then shift our attention to the remarkable instances of food sharing, cooperation, and possible imitation learning involved in following the experimenter model.

Indeed, the search for controls will be endless. When one asks what variables need to be controlled in designing an experiment, we usually assume a consensus with regard to what alternative explanations are as plausible as our original hypothesis in accounting for the anticipated results. The question of plausibility is, however, a personal decision. Should I find it unacceptable that chimpanzees should differ from humans only quantitatively, then I will insist either that there is yet one more quality of language that chimpanzees have not been demonstrated to be capable of, or that there is yet another explanation for your data, which you may think so farfetched as to be absurd, but which I think more reasonable than the absurd conclusion that an animal might achieve language.

Although the arguments go on, and are likely to continue for some time to come, maybe it is time to forget the labeling and assess what we do know about the cognitive life of animals, decide how we can meaningfully test our concepts of cognition, and consider the significance of the data we already possess. Now, suppose an individual did anticipate the behavior of another in terms of the presumed intentions of that individual. . . . Where do we go from here?

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***Concept of mind in primates? The need for a psychological theory** [P&W, SR&B].* The experiments involving communication with and among chimps have provided an interesting alternative to child language learning as a means of studying the developmental foundations of intentional concepts. It is too early to tell how much may be gleaned from this area, partly because no clear limit on what chimps can learn has yet been established, and partly because there is no generally agreed upon theory about what they have learned. But the experiments already performed promise a variety of interesting applications.

I am not inclined to rest much weight on the question of whether the chimps really speak a language or really have a theory of mind, at least in our present state of knowledge about them. There are as yet no agreed upon criteria or

*Received too late for a Response from P&W or SR&B. See Continuing Commentary. [Ed.]

pieces of evidence which justify applying these notions beyond the paradigmatic cases of human language users. And there are striking differences between the chimps and these paradigmatic cases, in addition to the tantalizing similarities. The issue of whether the chimps employ language or intentional concepts depends on whether our systems of description and explanation which attribute these notions are optimal for theorizing about the chimps' capacities. And here we run up against the fact that psychological theories have hardly gotten off the ground. English-speaking psychology has just begun to emerge from decades of behaviorist domination, which had the effect of stifling theory. In this situation it is natural and perhaps fruitful to try out the conceptual apparatus of commonsense psychology and commonsense linguistic interpretation. Whether systems of descriptions and explanation more appropriate to nonhuman primates are in the offing is, as far as I am concerned, a wide open question. There is some point, however, in emphasizing differences between human language-use and what has so far been shown about the capacities of the chimps, particularly since researchers in the field tend to be more interested in the similarities.

Linguists have frequently pointed out that the "languages" that the chimps have mastered are not generative. The animals give no evidence of the capacity for producing an infinity of semantically distinct grammatical constructions. This difference is related to another. Any given chimp has learned to use his symbols in what may be easily described as a relatively small number of linguistic and environmental contexts. It would be glib to dismiss this difference as a matter of "degree" rather than "kind." For what is at issue is whether fundamentally simpler explanatory systems than those ordinarily applied to humans will suffice to explain the capacities of chimps. (One of the sources of the failure of behavioristic schemes was the sheer complexity of human language use and cognitive capacity.) Explanatory systems that use less than the full conceptual arsenal of commonsense psychology and semantic description are certainly promising as means of accounting for the chimp's capacities.

Savage-Rumbaugh et. al.'s experiment, to take a case in point, has the chimp associate a given symbol for a tool with three contexts: matching symbol and tool (given certain promptings), fetching a tool subsequent to "hearing" the symbol, and expecting to get the tool subsequent to uttering the symbol. Even if these contexts were increased five hundred-fold, the chimp's capacity would not begin to approach that of Man. There is no finite listing of the possible linguistic and environmental contexts in which human words may be employed. I have, of course, described one of the three contexts mentalistically. And I am far from sure that mentalistic attribution can be reasonably avoided. But the relatively narrow capacities of chimps in their use of symbols, even granted various instances of transfer, suggest that relatively simple explanatory systems deserve exploration. I think we should be particularly cautious about applying the rich systems of semantic notions that we ascribe to human language-users.

One aspect of this complexity of human linguistic skill bears emphasizing. SR&B note at the outset of their paper the characteristic that allows language to transmit "specific information in an abstract, context-free form." There is an important sense in which none of the chimp's uses of symbols are context free. All symbol uses in the experiments can be matched with perceptually relevant contexts in the animal's present, recent past, or immediate future. All symbol uses are close approximations to what Quine has called "occasion sentences" (Quine, 1960 op. cit. by *Churchland & Churchland*). Their truth conditions are relatively easily inferred from the animal's perceptions in the context of the symbol's utterance. Even such generalizations as have been taught to chimps in other experiments are easily decomposable into finite conjunctions or disjunctions of such occasion sentences. Schemes for explaining symbol use which avoid appeal to irreducibly semantic notions are far more plausible as applied to uses of such sentences than as applied to sentences without such immediate perceptual relevance.

SR&B put the matter in the right light when they suggest caution about comparing the chimp's symbol acquisition with that of a child and when they emphasize the significance of role-reversal and cooperative communicative behavior that their experiments have elicited. It is not so much the chimp's use of symbols or his ability to learn the relevant tasks that is impressive. Ability to learn the skills so far demonstrated should come as no great surprise in the light of Köhler's early discoveries, and known cases of signalling in the wild. The skills described so far are not very complex. In my view, the chief interest of the SR&B experiments lies rather in their suggestions regarding the role of social cooperation in the origins of language. Under what conditions do the chimps invent iconic signs to replace the ones they have been taught? Under what conditions are they led to invent signs for new objects or actions? What kinds of social goals stimulate the relevant cooperation? What sorts of comparisons can be drawn between the laboratory cases of cooperative behavior and known cases of social sharing among chimps in the wild (for example, in occasional ventures into meat-eating)? Can nonhuman primates not known to share food, such as baboons, be induced to cooperate as the SR&B chimps have done? These questions and a host of others invite exploration.

Premack and Woodruff's paper is inventive and stimulating, but, to my mind, unconvincing. Part of the problem stems from my general skepticism, articulated earlier, about the state of psychological theory. But I am also doubtful about particular arguments in the paper. For the sake of discussion, I shall grant (what I find plausible in any case) that in some sense, so far poorly delimited, chimps have mental states. What I shall question is whether the experiments described provide much reason to think that they have a theory, or even a concept, of mental states. The basic tests (whether or not they involve physical inaccessibility) can apparently be explained without imputing such a theory to the chimp. Assuming that Sarah understands the problem of the actor's getting the bananas and the solution (say, knocking them down with a rod), she chooses the photograph that represents what she would like to happen. Sarah's motivation for wanting the problem solved may take any number of forms. She may like the actor, or simply want such problems solved, other things being equal. In the case of the actor she does not like, the experimental results can be explained in terms of her understanding the actor's plight and wishing negative outcomes upon him.

Do these accounts of the matter entail attributing to Sarah a belief that the actors have the mental state of wanting the bananas? I do not see that they do. It is enough for her to recognize a problem for a person and want (or not want) it to be solved on his behalf. Compare my seeing a beetle stymied by an obstacle in its path. I need not attribute mental states to the beetle in order to understand its problem, and want (or not want) it solved. Similarly, the chimp need not attribute mental states, or even know what it is to have a mental state, in order to act as she does in understanding the actor's problems. Perhaps Sarah does have to have some sense (instinctive or learned) of what is good for, or bad for, a person – what improves his well-being and what inhibits his normal activity. She has to make intelligent inferences in these matters. But attributing these notions to her does not entail attributing to her a theory of mind.

The embedded videotape is also unconvincing, for various reasons, of which I will mention one. The experiments as decribed do not rule out the possibility that Sarah is basing her choices purely on her attitude toward the participant. Perhaps she simply chooses the photo that provides the solution. The information as to whether the observer likes the participant might be interpreted by Sarah simply as evidence as to whether *she* should like the participant. The role of the observer, as intermediary between Sarah and participant, would be essentially vacuous. Obviously, the experiment could be complicated to test this interpretation. (Sarah could like the participant antecedently and be given evidence that the observer does not like her and vice-versa.) But until the experiment is refined, it is doubtful whether it shows anything that the unembedded experiments do not already show.

The experimental tests for "lying" seem inconclusive in the same way that the basic, unembedded tests do. The chimp may be seen as being angry toward the selfish trainer because he frustrates her expectations. It is a simple matter for the animal to determine which of several people, in a given context, is in a position to give her the bait – or is likely to give it to her, relative to her past experience. Lower animals commonly make such discriminations. These discriminations together with frustrated expectations (and an aggressive rather than docile temperament), appear sufficient to account for the difference between the way she treats the guesser and the liar. Again, one can reasonably impute mental states to the chimp without attributing a theory, or a set of concepts, about mental states.

P&W's remarks at the end of their paper provide a vastly over-schematized choice to someone (in particular, the chimp) trying to understand the actor's activity. The alternative as posed is between a non-inferential viewpoint which contents itself with disconnected descriptions of behavior, and a theory of mental states. The former alternative would be unacceptable even to most of those who have traditionally been counted behaviorists. Behaviorists rarely content themselves with disconnected descriptions of behavior. They have typically inferred unobservable dispositions and have postulated simple theoretical (non-observable) mechanisms (e.g. association) to explain the observable evidence. Moreover, there is a range of *functional* concepts that are neither behavioristic nor mentalistic that may be invoked to account for certain sorts of activity of animals or machines.

None of the foregoing is to suggest that we know *a priori* that chimps do not have a theory of mind or a concept of the mental. But to be persuasive in claiming that they do, it is not sufficient to argue that they make relatively simple causal or functional inferences about the behavior and capacities of human beings.

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Closing the circle: The ethology of mind. I only seem able to deal with these three papers in a historical context. I think that behind all the discussion of experimental design, controls, heuristic value, linguistic clarity, operational definitions, uncited papers, and philosophical implications that these papers will surely provoke, there will be many strugglings like mine. If my comments seem off the point, that is the point; for you, after all, are not me.

We have scotch'd the snake, not kill'd it. Prior to Darwin we could allow animals their instincts and humans their minds. But Darwin argued that some of the animal was still in the human, structurally and behaviorally, and that these were inherited traits. And then he argued that some of what was thought most human was found in the animal, meaning that precursors to almost every human mental and emotional characteristic could be found in the nonhuman. This double-pronged attack, finding the Animal in Man and Man in the Animal was most ingenious. But the world wasn't quite ready for it. Scientists were still trying to come to grips with physical evolution, and Darwin's necessary reliance on limited and questionable evidence made going back to his work, when the air had cleared a few decades later, too difficult.

An experimental attitude and laboratory approach was on the rise, marked by figures such as Lloyd Morgan, Thorndike, Small, and soon Yerkes, Watson, and Pavlov. Darwin's interest in the mental life of animals became transmuted into the apparently more tractable problems of measuring intelligence in animals, itself soon demoted to learning. The study of instinctive behavior waned and largely disappeared. Those experimental psychologists who studied humans relied almost solely on introspective reports or verbal reflections on affect, cognition, or memory. Psychology to them was the study of consciousness and not behavior (Angell, 1911). Just consider the situation prior to World War I: The natural behavior of animals was being ignored, along with questions of genetically based behavior and predispositions. The concept of instinct was being flagrantly misused with people. Experimental human psychology was becoming increasingly mired in apparently unsolvable and erudite controversies between functionalists and structuralists. Workers with animals, however, were having great success in training animals, developing powerful theoretical and empirical methods relying on observable behavior and what we now call classical and instrumental conditioning. The colonels of the behaviorists' revolution, led by J.B. Watson, soon took over, although it must be admitted that there were always some rebel guerillas making quick forays from inaccessible cloud-covered mountains.

The first of the two main consequences of the behaviorist takeover was the emphasis on overt behavior. They argued that a focus on "mind" and "consciousness" was unproductive in the study not only of animals, but of humans as well. Such things may or may not exist, but they obviously cannot be studied. Thus scientific psychology lost its mind. Second, they argued that human and animal evidence on instinct was minimal and its use as an explanatory concept ludicrous. And had they not shown how important learning and the environment were to animal behavior? Thus, both instinct and mind were removed from human and animal alike, leaving a variety of mechanistic stimulus-response processes to fill the slack. Obviously this situation could not last; what amazes me is that it held on so long and so firmly. This perhaps indicates that there is much of value in behaviorism, which should not be discarded.

But back to the two evils, instinct and mind, that the behaviorists thought they had eliminated from scientific discourse. In reality they had only wounded the snake and driven it deep into its den. And it was a two-headed snake they tried to eliminate. (The left head being Instinct, the right head being Mind; thus, our lab's two-headed black rat snake is named Im). Later most ethologists were willing to grant the behaviorists their due in the emphasis on behavior; they only protested their ignoring of evolution [see Eibl-Eibesfeldt: "Human Ethology" *BBS* 2 (1), 1979]. But I'm starting to believe that the behaviorists knew what they were doing in tying the two together.

In any event, a reaction against the elimination of instinctive behavior from animal psychology arose by the late 1930s, and today the existence of important innate or genetic aspects of behavior is accepted in all nonhumans. Thus instinctive behavior was put back in the animal. It was inevitable that humans would soon be studied for similar phenomena and evidence, which did in fact happen as ethologists (reviews in Burghardt, 1973; Eibl-Eibesfeldt, 1975) and then sociobiologists (Wilson, 1975) applied their methods and concepts to people. The innate is now back in the human.

And mind made a comeback too during the 1950s. People studying humans were feeling overly restricted by behaviorism of whatever variety. New views of language, cognition, and information processing were on the rise and behaviorism, weakened, could not hold off this front either. Thus by the mid-1960s mind was back in the human. The only missing link now was putting mind back into the animal. The Gestalt psychologists tried to do this in the twenties and thirties as did Bierens de Haan, Buytendijk, Von Uexkull, and others, although they were effectively silenced. But even at that time some honeybees in Austria were dancing up a storm, and that brought new clouds for behaviorists and ethologists alike. Von Frisch's work was widely known in America during the 1950s, but as long as something as anomalous as a symbolic language was restricted to a lowly invertebrate it could be repressed in a variety of ways.

But then came the gesturing, tool-making chimps and the language training by Premack and Rumbaugh. Field-oriented primatologists began looking toward sociology and anthropology for methods and concepts, and it became increasingly difficult to ignore the subjective baggage that went along with them. For instance, in discussing the role concept, an eminent and unrelentingly behavioristic ethologist had to deal with the idea that "shared expectations" are involved in the sociological use of the term.

"Does this imply that conscious awareness is a necessary part of the use of the role concept? If so, one could argue that the term role cannot usefully be applied to animals simply because the evidence we can obtain about their conscious intentions is so much less secure than that available in the human case." (Hinde, 1974:387)

The fact that Hinde even had to raise the issue was astounding. And then Griffin's book (1976 *op. cit.* SR&B) formally and seriously addressed the issue and completed the circle. We are now in the second revolution that ethology seems to be provoking, although many ethologists and animal psychologists are themselves unconvinced. Two-headed snakes are often referred to as monsters, but can one head live alone?

Communication as a window [G]. Now the above is public history, however debatable and jaundiced by selectivity and judgment. Personally, I have always delighted in the idea that animals have complex mental processes, though I have found that it was circumspect both as student and teacher to be restrained in advocating mentalistic terminology. Possibly the fact that my empathy extended especially to snakes and lizards rather than apes had something to do with it. G's account of the resistance to "awareness" as applied to bees extends the apparent absurdity even further, and I am pleased that G did not retreat from bees by pleading one of the myriad available excuses for concentrating on primates or even just apes. Thus G does not subscribe to the gratuitous assumption explicity held by P&W, who twice on the same page assert that animals other than humans and chimps are "lesser species." And G addresses the difficulties on the conceptual, methodological, and empirical level. He has, in fact, performed an admirable service by updating his earlier reassessment of cognitive processes in animals.

But it is also important to ask what is new and what is old about cognitive ethology, as questions of animal mental life and consciousness were addressed early in the post-Darwinian era. G is aware of this but chose to concentrate on the current scene. Holmes (1911) stated the dilemma we are struggling with now (p. 3):

"Concerning the conscious life of animals distinguished from the objective facts of behavior – our knowledge rests upon an insecure foundation. We have no means of cognizing directly the conscious states of any creature besides ourselves and what we know of the psychology of our fellow human beings is based upon what we find taking place in our own minds. We infer consciousness in other beings because we are conscious ourselves, and we judge of the mental states in the minds of others, such as joy, sorrow, anger, or fear, from certain physiological manifestations which are like the accompanying manifestation of these mental states in ourselves. With beings much like ourselves our inferences may be fairly accurate. When thrown amid people of other nations or races our judgments are most apt to be erroneous. And when we try to infer what goes on in the mind of a cat or dog the difficulties are very greatly increased."

But however sensible in theory, the continuum position does not sit well with most of us who favor a greater human-nonhuman split. Washburn (1908:3) nicely made the next step: "To this fundamental difficulty of the dissimilarity between animal minds and ours is added, of course, the obstacle that animals have no language in which to describe their experience to us."

Thus as I have perused this early literature I have discovered that not only was communication between animals largely ignored in discussion of mental abilities, it was nonexistent as a separate topic of study. Symbolic language in animals was considered an impossibility, and what communication did exist, instinctive. Thus G and others in the new cognitive ethology are really opening up something new in using communication as the window in the wall that other methods could not penetrate.

Washburn understood the procedural problems in the endeavor all three of the present papers are attempting (1908:4):

"Knowledge regarding the animal mind, like knowledge of human minds other than our own, must come by way of inference from behavior. Two fundamental questions then confront the comparative psychologist. First, by what method shall he find out how an animal behaves? Second, how shall he interpret the conscious aspect of that behavior?"

Language among chimps [SR&B]. The efforts by SR&B to study communication are truly seminal in terms of being ingenious, methodologically tight, and showing strong results, although the amount of chimp-chimp versus chimp-computer communication is somewhat unclear. The suggested link between language and tool use is attractive, but given the lack both of a comprehensive survey of tool use in the animal kingdom (now being rectified by Benjamin Beck) and of an accepted characterization of nonhuman language, too much should not be made of it at this stage. Although nonprimates are ignored by SR&B, the scholarship evidenced in the paper is truly impressive and valuable, contrasting with the following piece by P&W.

While the majority of SR&B is devoted to a report of their innovative experiments, a goodly portion is a critique of earlier work, particularly the ASL approach of the Gardners and the plastic-chip work with Sarah by Premack. Most of the criticisms are convincing and useful to have together, but one wonders why they did not allude to their own earlier Lana work. If it was exempt from the criticisms leveled at the Washoe and Sarah projects, this should have been made clear, as Lana is also part of the history of this area. And, if not, critical assessment here would have been fair.

Do Premack and Woodruff have a theory of mind [P&W]? In contrast, the article by P&W is frustrating: some good ideas and brilliant work obscured by rhetoric and argument. The experiments as reported here and elsewhere are exciting, but most of the present paper reads like a verbally shrouded grant proposal. Some pilot data are alluded to, but lots of hypothetical to-be-done experiments are also outlined, complete with exhaustive if-then discussion and interpretation. Plausible argument and anecdote do not provide adequate control. For example, the TV experiments need to be done without any actor.

There are other problems as well: Throughout the later sections it is often unclear what has actually been done and what is proposed; the discussion of results is vague, with data reported as consistent or invariable with no indication of how many trials were run (2, 20, 200?). And why does a person a chimp does not like have to be given a fictitious name whereas a more favored person can be mentioned? There is too little awareness that many people have looked at these issues: Only one reference before 1973, and this to Köhler. I would suggest everyone read chapter 12 in Hobhouse (1901) on "the concept" and then reread both SR&B and P&W.

And finally, it can be answered that P&W, but not the chimps, have a theory of mind. Their preoccupation with proving it has led to experiments gauged to support the "theory," although this inadequacy tends to be obscured by the rhetoric in which the reports are embedded. The theory *may* be of value; a clearer presentation would have given the reader more understanding and faith.

The attempt and not the deed confounds us. But the question arises: How effective and necessary is the new cognitive approach? Certainly both heads of the snake were attacked by the early behaviorists with the fervor (and ignoring of evidence) we see raised today in the more socially relevant areas of sex and race differences. For to be on guard against misuse is one thing, to deny "mental" processes in animals is another, and as G notes at the end of his paper, there are indeed some moral and ethical issues raised by giving animals back their minds. Should we do this if there is still disagreement on whether discussion in these terms aids analysis (the initial behaviorist point)? Definitely.

In my classes in psychology and ethology I find it necessary to raise the issues of teleology and anthropomorphism early. Somehow, in spite of the profound moral, religious, and evolutionary issues raised, most students are willing to talk about animal behavior in subjective and mentalistic terms and cannot see the logical problems and the "as if" distinction. Why is this? Is it just a remnant of childlike identification with cute animals with anthropomorphic features? Morgan (1903:51) was of a different mind: "There can be no question that the interpretation of the actions of animals as the outcome of mental processes essentially similar to those of man amply suffices for practical needs." In other words, "it works." But just as all of us can get around in the world on a fairly primitive understanding of physics, yet nonetheless take physics and mechanics in school to prepare us for going beyond rough working hypotheses, so more rigorous approaches are needed in psychological science too.

The fact that many competent scientists did not believe von Frisch's findings on bees, not on methodological grounds but because of blinders, is unfortunate. But we should be cautious in using such instances of earlier narrowness as grounds for accepting empirical findings calling for a revival of specific mentalistic concepts. Many human ethologists might see the irony of bringing cognitive processes into the analysis of nonhuman behavior at the same time they are trying to get the analysis of human behavior onto a more objective plane. SR&B and P&W beautifully show the complex behavioral interaction and language abilities of chimps, but SR&B discuss their results in largely behavioral (but not old-line behavioristic) terms while P&W bring in mentalistic slush that undercuts worthwhile data and ideas. And this is the danger. Even competent psychologists can be seduced into fuzziness when the focus is removed from behavior.

Yet I think we need to work toward a cognitive ethology, which at this time I would like to see characterized as an open-minded concern with the subtleties and complexities of animal behavior. G is not giving us answers but raising questions and emphasizing the need for rigorous experimentation and logical thinking. SR&B and P&W are trying for answers, and perhaps at this time it is most generous not to stifle diversity of approach. I do think that the phenomenological approach as a way out of Cartesian thinking should be considered. Thines (1977) has written a useful book that apparently hasn't yet elicited a reaction from American students of animal cognition.

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EDITORIAL NOTE

*Received too late for a Response from G, P&W, and SR&B. See Continuing Commentary. [Ed.]

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How the animals lost their minds. Since the invention of radical behaviorism (and, later, logical positivism) wags have cried that we have a psychology that lost its mind. Less tiresome critics have whispered that ethology never found its brain. This issue of *The Behavioral and Brain Sciences* shows that the mind is alive, healthy, and again the permissible object of fascination, by those who wish to know what, if anything, animals think.

I wish to show that study of the animal mind, whether of awareness, consciousness, probability learning, or whatever designation human beings may contrive, was not sent packing by behaviorism; that the task of uncovering what animals think depends upon the strategies of materialism, mysticism, and perhaps phenomenology; that these strategies of studying animal thinking assume and thereby discover different properties of the mind; that the study of language as a representation of the mind is inappropriate; and that animal awareness, consciousness, or cognition is most effectively seen as a sensory filter, rather than as an independent entity.

Fundamentally, we can identify three theories of mind: materialism, which defines mind in terms of the *things* that it can classify; mysticism (in the Greek, and still best sense of the word), which defines awareness as comprehension transcending but classifying *things;* and, possibly, the developments since Kant that have attempted to solve the lovers' quarrel between mysticism and materialism by attempting to describe consciousness itself. In this commentary, I draw no important distinction between awareness and consciousness, as some do, only because the proposed differences are unimportant to the question of whether either can be described as characteristic of animal life. (But see Natsoulas, 1978, on the kinds of consciousness.)

The radical behaviorism of the 1920s developed for many reasons, among these being the fact that the study of psychology had begun to fail to make the essential distinction between hypothetical constructs and intervening variables. The mind, awareness, consciousness, and cognition have the properties of hypothetical constructs, not intervening variables. The latter "abstract the empirical relationships," but hypothetical constructs "involve the supposition of entities or processes not among the observed" (Tolman, 1932; amended by MacCorquodale and Meehl, 1948). Among the reasons for the denunciation by radical behaviorism of nineteenth-century faculty psychology, a psychology that focused on the presumed trinity of faculties of the mind (emotion, reason, and the will [motivation]), was that students of the mind had begun to confuse mind as an intervening variable with mind as an hypothetical construct. Mind, then, assumed a reality separate from its logical properties and degrees of empirical verification. Behaviorism stripped study of the mind from this methodological error but, by so doing, made it unfashionable to ask questions regarding the mind, consciousness, awareness, and like concepts.

Both G, and P&W argue for a revised code to govern the study of the animal mind. G prefers to concentrate on the possibility of animal awareness by adopting a reasoned materialistic view of the rules of scientific inquiry that provides appropriate warnings – and they are needed – regarding the sensibleness of considering animals as sensing and aware models.

I think it not true that behaviorism, or those who so describe their activities, has routinely dismissed mentalism as G contends. Certainly the work of the gestalt school (Köhler, 1927 *op. cit.* SREB), Tolman's still influential *Purposive behavior in animals and men* (1932), Mowrer's analysis of learning and symbols in animals (1960), Thorpe's analysis of purposive behavior in animals (1956, 1962), and contemporary work on information processing and signal detection, have kept sturdy the study of mental events. It was Tolman who first drew convincingly the distinction between intervening variables and hypothetical constructs and who clarified the distinction between learning and performance, a distinction that is of relevance to the work of P&W and SR&B as well as to G's analysis of how one studies awareness. Boden's excellent work on purposive explanations in psychology clarifies Tolman's influences splendidly (Boden, 1972, pp. 80–98). Smith (1971) is well aware of the mentalistic view of animal behavior in his helpful book *Purpose in animal behavior,* especially pp. 27–33. However compelling G's warnings may be, it would be unfortunate and (one must believe) contrary to his wishes if the warnings prevent the development of a biology of the mind. One cannot quarrel with the strict materialistic view presented by him, for the brief history of attempts at mentalistic explanations provides clear warnings of the dangers of straying thoughtlessly from the path so clearly plodded by those of the "nothing but" materialistic persuasions. Still, occasionally excursions from the true path are rewarding if hazardous, as P&W and SR&B demonstrate.

Apes and philosophers [P&W]. Reversing the occasional trend, P&W have made philosophers out of monkeys – or apes, to be precise. Of greatest importance for the rejuvenation of the study of mentalism in animals is their view that an *umweg* is to explore the possibility that animals reason from the general category to the particular event. This contradicts the unfortunate view that has controlled study of animal life for many years, that animals build from experiences of the particular. The evidence collected by P&W supports the view that the chimpanzee mind contains something akin to Kantian categories of judgment. If true, research focus shifts to uncovering the categories and how they are modified by experience. I cannot imagine a potentially more fruitful approach for the study of the mental states of animals, a view supported by the success of their work.

It would appear that this approach is bound to set off rounds of studies, creating the danger that in the hands and minds of the philosophically uninitiated the distinction between hypothetical construct and intervening variable may, once again, be confused. To my mind, the logical experimental steps must now make use of the knowledge we have gained from the description of mind by Fechner (1860) and Stevens (1957). Fechner's psychophysics, a system he predicted could not be destroyed because there could be no agreement on how to do so, represents the strict nineteenth-century materialistic view offered by G. Stevens, by showing the plausibility of direct scaling of mental experiences, permits one to uncover and scale mathematically the categories of the mind. The obvious success of these techniques with human beings – coupled with P&W's tentative but compelling success with chimpanzees – suggests how one may locate the nature of the categories without succumbing to the intervening variable/hypothetical construct confusion still so prevalent in theories of animal behavior.

Among the achievements of P&W's contribution is the reintroduction of the faculty of motivation as a meaningful research subject with animals. In the behavioristic rebellion against animal mentalism, motivation (and emotion) were the constructs most damaged by neglect. It is not surprising that one of the recognized and attractive contributions of modern phenomenology and the study of consciousness (albeit in human beings) is the recognition of the investigability of motivation (Moss and Keen, 1978). If chimpanzees can be "liars" or "fools," altruistic or spiteful, to human beings, depending upon categories and experiences, we have begun to appreciate the logic of the animal mind and the relationship between categories of the mind and experience. If it be true, as P&W conclude, cheek filled with tongue, that "The ape could only be a mentalist . . . he is not intelligent enough to be a behaviorist," may it also be conversely true that the human behaviorist is not sufficiently intelligent to grasp the Critique of Pure Reason?

Aping language [SR&B, G]. The fundamental difference among these contributions lies not in how each values strict materialism, Kantian phenomena, or gestalt mysticism, but in the importance and status given language. As SR&B state so clearly in their introduction, a linguistic situation consists of the transfer of information. The transfer may be verbal and acoustic, certainly, but it may also be observed in bodily motion, facial expression, and behavior. G appears to understand this fully, for his section "Testable hypotheses concerning cognitive ethology" concentrates on examples that involve communication, symbolically, posturally, or verbally. Presumably this section replies to the ideas, cited by him just previously, of Ryle (1949 op. cit. G) whose work failed to perceive the importance of the difference between learning and performance, much less the broad meaning of language, and succeeded in providing an example of how and why not to impose human categories on other species.

Both SR&B and P&W show no fright at introducing the concept of intentionality into their explanations and designs. Intentionality, long a fortress of ethological theory, can be conceived as a reasonable extension of motivation, and its return along with emotion as an area of study is overdue and welcome, so long as the concept's role as hypothetical construct is appreciated. SR&B's understanding of symbolic representations leads them to procedures and interpretations that are dramatic. Awareness (in G's word), cognition, or the mind is unlikely to be singular. If awareness or mental state is not communicated, its selective value is much restricted, and our observations tell us that animals do communicate mental states to one another and, now it would seem, to us as well. May symbolic representations be viewed with profit as a category of the mind – one, perhaps called visual representation?

Language is a potential menace. All three of the papers under Commentary agree on its importance; each differs somewhat concerning the view, stated or tacit, as to what language is and how it is best investigated. I doubt that it is true that language is the *most important* view of awareness or mental states, for mystic, materialist, and phenomenologist alike can agree that language is but one of many behavioral expressions that have little meaning to the investigator unless the expression is communicated: that is, is understood and acted upon by another being. So long as we conceive of language as something "given off" by an animal, we shall attempt to read it as a lost language requiring a Rosetta stone for interpretation. Language has meaning only insofar as it is reacted to: its meaning lies in the behavior of the observer, not in the presumed motivations of the giver. To lose sight of this distinction is to lose much that has been gained, as shown in these papers, in our understanding of productive strategies of studying the animal mind.

I propose that language, in the broad sense described, be treated as any other hypothetical construct ought to be, as an entity "not observed." To write that it is not observed is not to say that we have no observations of language: indeed, we have too much of some and not enough of others. What has not been observed systematically is the *meaning* of language as perceived by the recipient. Our approach has been to compile a haphazard dictionary of what expressions, postures, and the like "mean" to the giver. This dictionary is doomed, for it assumes that the word units of language are graven. They are not: they are alterable, evolving, and understandable only by observing the behavior of those who read them.

P&W's and SR&B's research make it evident that they appreciate this distinction, although I do not find them explicit about it. Those who follow to expand and refine this research should understand the importance of the strategy, or animals will once again lose their minds. They, no doubt, will not find its loss troublesome, but we human beings who want to peek into the animal mind will lose a splendid opportunity. Be aware.

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Internal states and cognitive theories. [G, P&W]. *Mental States: Natural kinds or functional kinds?* [G] We are in clear accord with G on the cardinal insight of the new wave, cognitivism: it is fruitful – nay, necessary – to postulate internal states and processes in order to explain intelligent behavior in humans and other animals [see Haugeland: "The Plausibility of Cognitivism" *BBS* 1(2) 1978]. But it should be emphasized that the states and processes in question are best conceived as *functional* in character. Unfortunately, G's laudable attempts to seek a philosopher's perspective on the nature of minds and mental states did not lead him to the strongest theory available, namely, functionalism (see, for example, Putnam, 1967; Fodor, 1975; Dennett, 1978). The central tenet of functionalism is that mental states are functional states; that is, they are the states that they are in virtue of the interactive role they play in the larger information-processing system. On the functionalist theory, mental states are not natural kinds, nor is there a well-defined division between things that really do have mental states and things that really do not. And as to determining how far down the phylogenetic tree things really do have mental states, according to functionalism *there is no fact of the matter.* What is an empirical matter is what functional organization chimpanzees do have, what functional organization rats have, and what bees have. We can be tolerably sure in advance that the functional organization of chimpanzees and bees will be substantially different, for the nervous system of the chimpanzee is markedly different from that of the bee, and the behavioural repertoire of the chimpanzee is much richer and more flexible than that of the bee. And if we discover that chimpanzee behaviour can be explained by a functional theory that postulates, among other things, beliefs and desires, those states will be like our own to the degree that the chimpanzee's internal functional organization is like our functional organization. That is, to the extent that our functional organizations match up, so the functional role of our "belief states" will match up, and to that extent the "belief states" in the chimpanzee and the belief states in us are similar. Beyond this, there is little point to pondering intrinsic similarities or a shared natural essence for mental states generally, for if mental states are functional states, they *have no* intrinsic (i.e., non-functional) properties essentially, and the search for any such essence to help us make transspecific identifications of mental activity is a false errand.

Dualism is the view that (a) mental states form a natural kind, and (b) this natural kind is characteristic of a nonphysical dimension of reality. Most of us, G included, have put (b) behind us. But (a) wants rejecting no less than (b). And once (a) is behind us as well, it is plain that the only relevant cross-specific similarities to be delineated are functional similarities.

Given that this is so, and turning to theories about the functional organization of chimpanzees and bees, we must ask if it is really useful to include states like beliefs and desires among the functional states we assign to them. Perhaps, so long as we are aware that the states can only be analogues of our own – very distant analogues in the case of bees, less distant in the case of chimpanzees. To the degree that the analogy is remote, the ascription of beliefs and desires is metaphorical, and in that case, it will surely be more promising to look for a quite different sort of functional theory, leaving the belief-desire paradigm for, at most, the higher animals. For this reason it is likely, in the case of very simple animals, that neuroscience will be a faster route to such a theory than will orthodox cognitive psychology. For as Pylyshyn's commentary illustrates, orthodox cognitive psychology is still dominated by the belief-desire paradigm. Whether that paradigm is appropriate for the higher animals is the question to which we now turn.

Representational states: how should we conceive of them [P&W]? Let us suppose, with P&W, that the chimpanzee does indeed have a theory that postulates inner states in humans and other chimpanzees – inner states which, on the chimpanzees' theory, are related to one another, to sensory circumstances, and to behavior in ways that permit the systematic anticipation of behaviour in others. This supposition is certainly appealing, for chimpanzees and other animals can indeed anticipate the behaviour of other creatures to a degree that seems explicable only on the assumption of their possessing something as rich and powerful as a theory of the kind described. And for our part, we do not hesitate to accept this much.

However, there are two cautions that must be entered here, for the proper conclusion to draw is in fact crucially weaker than the one P&W defend. First, the fact that the chimpanzee successfully anticipates what we would anticipate in a given situation does not guarantee that the theory he uses to do this is the *same theory* that we use. It implies only that his theory predicts more or less what ours does in that situation. Beyond their behavioural consequences in prosaic domains, their theory and ours might differ substantially. Even radically.

In illustration, consider an alien people (whose language remains untranslated) who display an impressive ability to anticipate and control thermal phenomena. Their capacity in this respect might be put down to their knowledge of the familiar corpuscular/kinetic theory of heat. But it could also be accounted for by supposing them to hold the very different caloric fluid theory of heat. Over a wide range of cases, these two theories are empirically indistinguishable, and so will be the behaviour of their respective adherents. In fact, there is a potential infinity of theories of heat that share this feature, and the gross behaviour of the aliens will be fairly well accounted for whichever such theory we assume them to hold [cf. Pylyshyn: "Computational Models and Empirical Constraints" *BBS* 1(1) 1978].

Accordingly, the chimpanzee may indeed be a theorist, and a successful one, but the evidence tendered by P&W does not show that what the chimpanzee imputes consists of *desires that P, beliefs that Q,* and so forth. The chimpanzee's theory of mind might be as different from ours as is the caloric from the kinetic theory of heat.

We do not wish to insist from this, as perhaps Quine (1960) would, that the chimpanzee's theory is ultimately inaccessible to us. But we do wish to caution against reading *our* theory of mind into *his* outlook.

To caution thus is of course to remain faithful to the idea that the chimpanzee must possess something as rich and powerful as a theory. And we agree that he must. But here a second caution wants making. To assume that what the chimpanzee possesses is actually and literally a *theory,* a set of *beliefs* in sundry general and particular *propositions,* is to suppose that our common-sense theory of mind is literally true of chimpanzees. And this is a supposition of which we should be very chary, given the lesson learned in the preceding paragraph, for the very modest success of our folk psychology – as applied to chimpanzees, and even to ourselves – need not be due to its truth. Beyond question the chimpanzee manipulates internal *world representations* of some highly sophisticated kind. But for us to represent his representings on the overtly *linguistic* model that dominates (almost constitutes) our own theory of mind is to be insupportably parochial again. Recall that it is a framework feature of our folk theory of mind that the principal mental states are always identified by reference to a sentence – John believes, desires, fears, suspects, doubts, hopes, and so on – *that P,* we say, (where some sentence substitutes for "P".) Granted, human language is so far the only model for systematic world representation we possess; but that is a defect in our intellectual situation that desperately needs remedy, arguable as it is that the linguistic model is inadequate even to represent our own cognitive activities. For extended discussions of the inadequacies of the linguistic paradigm, see Churchland, P. S. (1978, 1979), and Churchland, P. M. (1979).

In sum, it seems clear that chimpanzees are "world representers" in some sense, and also that they "represent" some things in the world as (other) "representers." But it is far from clear that they *believe* that others have, for example, *beliefs,* and it is probably self-stultifying for psychology to assume that they do. For what we need is a psychological theory that can explicate the vague notion of a *representational system* in ways that go beyond the linguistic paradigm which has so long imprisoned us.

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Intentions, awareness, and awareness thereof. *Intentions and awareness in communicating [SR&B].* According to SR&B, they have evidence that Sherman and Austin not only communicate information (even DNA and bacteriophages do that, as G points out), but also "comprehend the symbolic and communicative function of the symbols they use." Comparable evidence for Washoe and Sarah has, they believe, not been reported. But on their own account, such comprehension turns out to require beliefs about one another's beliefs and intentions (what P&W call a "theory of mind"). And the evidence for this seems no better than what might be assembled for Washoe or Sarah: "richly interpreted" anecdote or things simply taken for granted ("E responded by demonstrating to Cr, with an open-hand gesture, that he had no tools . . . after five or six trials the animals seemed to begin to comprehend").

The alleged comprehension is a question, they say, "of *awareness* and *intentionality.*" But awareness of what? And precisely what intentions? SR&B quote Mounin (1976 *op. cit.*) and Steklis and Harnad (1976 *op. cit.*) to the effect that, among others, the following are requisite: (1) the "transmitter" of a communication must be aware that his target is the "receiver"; (2) the receiver must be aware of being the transmitter's target; and (3) the gestures or symbols used must be "intended and relied upon to transmit information."

What do these mean, and why are they necessary? One plausible account runs as follows. When Cr (requester chimp) presses *wrench,* he is trying to get Cp (provider chimp) to bring the wrench. He believes that pressing *wrench* (and getting Cp to notice) is a good means for achieving this. Why does he believe this? Perhaps because, during those first five or six trials, each C realized that when he was Cr, Cp would in fact bring the tool requested. But if this is the sum of the matter – in particular, if Cr has no idea *why* Cp brings the tool requested – we cannot say there is genuine communication (that is, communication with the "comprehension" of which SR&B speak). Cr would merely be manipulating Cp as he might manipulate an inanimate object; he would not be *asking* Cp to bring the wrench. We must say that Cr does not regard Cp merely as a "black box," but as a creature with intentions and beliefs. If so, we can say that Cr understands his pressing the *wrench* as getting Cp to believe that he has reason to bring the wrench, and so getting him to form the appropriate intention. If so, we can say he is pressing the *wrench* intending thereby to transmit information to a specific target (1 and 3 above). And this can plausibly be regarded as genuine communication.

At the other end, Cp may view Cr's action as an attempt to *tell* him something – to give him the information that he (Cp) has reason to bring the wrench – or simply as an indication that he does have reason to bring the wrench. In the former case we can say he is "aware of being Cr's target" (2 above). And only in the former case can we say that he "comprehends the communicative function" of Cr's action.

(Among philosophers, this general approach to communication has been pioneered by Grice, 1957, 1969 *op. cit.* SR&B, and to my knowledge, most fully and carefully developed by Bennett, 1976.)

Can we say that Cr imputes intentions and beliefs to Cp? SR&B say that after the first few trials, Sherman and Austin had for the first time "reason to presume that the other animal *knew* and *used* [intentionally?] such symbols" (my emphasis). But no particular evidence is given that either animal actually came to this realization, rather than the simpler realization that each animal would in fact *behave* in certain ways. (Cf. Mounin's suggestion, endorsed by SR&B, that Sarah's "messages" might in fact be contentless, merely reflecting her belief that if she makes such-and-such a response, she will be rewarded.)

Perhaps it is relevant that Sherman and Austin readily reversed roles. We can imagine each thinking "when he presses *wrench* and I bring it, I do so because I believe I will be rewarded; so when I press *wrench* and *he* brings it, he must do so for the same reason." But this is plausible only if it is plausible to suppose that each imputes intentions and beliefs to *himself.* In the next section, I will explore the possibility that in one sense (though perhaps not the sense relevant here), animals do *not* make such imputations.

But first, notice again that SR&B take for granted that Sherman and Austin themselves do have intentions and beliefs, whether or not they impute them to one another. Perhaps this is justifiable (since no single experiment could prove it, anyway; see my last section, below.) But what follows if we take seriously their descriptions – say, that of C as "gesturally and vocally soliciting aid if he could not orient the tool properly"? If C has beliefs and intentions, and solicits aid by means of gesture, he must believe that the gesture is likely to result in aid being tendered him. And if it is really proper to describe C as "soliciting aid" rather than merely doing something he has reason (what reason?) to believe will cause E to help him, then again we must suppose that C imputes intentions and beliefs to the target of his gesture. He has learned that E is generally benevolent, and will offer aid if he (C) gives him (E) reason to believe he (C) needs it.

If SR&B are willing to stand by this description of C as "soliciting aid" (and similar descriptions), then they must regard their whole experiment as showing little beyond what they took for granted right from the start. And if they are not willing, they ought explicitly to address the question of what *in their experiment* shows that Sherman and Austin really had the intentions and awareness necessary for genuine communication. Certainly *not* the mere fact that they employed symbols rather than "iconic" gestures. For in those first trials, they might have learned to rely on one another's behavior without having or making use of any insight into *why* they could do so.

In any case, notice that all the intentionality and awareness requisite for genuine communication *could* have been present in the case of gestural communication. The "issue of iconicity " seems to be relatively unimportant, contrary to what SR&B say.

Awareness of intentions, awareness, etc. [P&W]. An organism has a theory of mind if it imputes mental states to itself and others, say P&W, but virtually all the experiments they describe at best yield evidence that their animal subjects impute mental states to others.

It is worth taking seriously the possibility that there are organisms which impute mental states to others but *not* to themselves. Perhaps Sarah's choice of photograph really does reflect her belief that the actor *wants* to get the bananas and *believes* (or would realize if he stopped struggling long enough to look around his cage) that stepping on the box would be a good way to get them. (I have misgivings, however. How did P&W manage to "ask the animal to indicate how she thought the human actor would solve his problem," and how sure can they be that she got the message? Cf. Mounin's criticism of other experiments with Sarah, quoted by SR&B.) It remains possible that Sarah has no beliefs about – no awareness of – her own wants and beliefs, or any other of her mental states.

If we grant that Sarah *has* beliefs and is capable of acting intentionally and intending (for the distinction, see Davis (1979)), we would probably also have to grant her beliefs about – awareness of – what she is doing and what she expects to do. But present and future actions are not mental states, present or future. A chimpanzee making a termite-probe might know that he was doing so, that he was going to catch termites with it, and even that he was doing the former in order to do the latter, without imputing any mental state to himself. G speculates that if asked what he was doing, such a chimpanzee might answer "that he was contemplating the capture or eating of termites." Now contemplating may be a mental *action* rather than a mental *state,* but it is still entirely mental, and so we would have to accept this answer as refuting the hypothesis that chimpanzees do not impute mental states or actions to themselves. But of course no such answer has yet been obtained, so we may continue exploring this hypothesis. (Nor would it be easy to be sure we had gotten such an answer. How could we ascertain that something the chimpanzee said had this meaning? Difficult, but perhaps not impossible. See Bennett, 1976.)

P&W suppose that if Sarah could be taught to say whether she knew something or was merely guessing, this would show that she did impute mental states to herself. But strictly, they envisage her applying "know/guess" to her *actions.* Conceivably she could come to discriminate actions based on her relevant knowledge from actions performed despite the absence of relevant knowledge, while at the same time failing to have any belief about the presence or absence of this relevant knowledge. (Parallel remarks apply to "truth/lie".)

Our understanding of mental states is essentially flavored by the fact that we do impute them both to ourselves and to others – to others on the basis of behavioral criteria, and to ourselves, in many cases, on the basis of no criteria (Strawson, 1959; Shoemaker, 1963). If Sarah really imputes no mental states to herself, then (a) those she has differ from ours, at least in not directly causing awareness of (i.e., belief about) themselves, and (b) what it is that she imputes to others would not quite consist of mental states as we understand them, but merely of what P&W suggest: a system of inferred states useful in predicting the behavior of other individuals. Perhaps we should say she imputes "psychological" states rather than "mental" states, so that P&W's experiments would show that Sarah has a psychological theory of the behavior of others, rather than a theory of mind. And perhaps in the light of (a) *we* might say that Sarah herself does not have "mental" states, but only "psychological" states. But it would be dangerous to make much of this distinction without a good deal of supporting argument. I am thinking in particular of possible claims that the distinction has *moral* significance. Suppose for example that Sarah, when injured, is in a "psychological" state that is just like our "mental" state of pain except that she lacks "direct awareness" that she is in it. (She might have *indirect* awareness based on her awareness of the injury and of her behavioral tendencies.) Could this difference make a difference so far as the moral injunction to relieve pain is concerned? (Cf. the last section of G, and Davis, 1974.)

Final Comments [G]. G discusses the presence in nonhumans of what I would call (i) beliefs and thinking, (ii) acting intentionally and intending, and (iii) direct awareness of one's own mental states. Some points on which I would agree or disagree may be gleaned from my comments on SR&B and P&W, especially as regards communicative behavior, and my distinction between "mental" states and "psychological" states. But several further points should be made.

1. "One necessary . . . condition for the occurrence of awareness and thinking is the presence in the brain of patterned images. . . . " G allows that these images may be "coded in a variety of noniconic ways," but they still sound too much like actual pictures in the head. Surely his point could better be made in terms of concepts, or information and its storage, rather than in terms of images.

2. G often uses the phrase "conscious intent"; to me, the phrase suggests "intention which the organism is aware of having," and so I want to stress again that we might have plenty of evidence for animal intentions of various sorts but much less evidence for supposing they are aware, directly or even indirectly, of having them. In the same vein, G quotes himself on the presumed "adaptive advantage" conferred by "awareness." This is plausible if it is specifically awareness of – beliefs about – the organism's environment, its own bodily state, what it is doing, what it will do, and so on. I am much less convinced of any such adaptive advantage to awareness of its own "psychological" states (an awareness which, if direct, would render them "mental" states), outside of a fairly sophisticated linguistic community. But work by Gordon (1978, 1979) may be relevant here.

3. Favorable results of G's proposed experiment to test whether honeybees intend to communicate by means of their dances (hence "comprehend the symbolic nature and communicative function" of the dances, as SR&B might put it) might indeed support the claim that they do. But there is a major proviso. Alternative interpretation of such results is possible, as we have already seen. What inclines us to interpret specific chimpanzee behavior in terms of communicative intentions is our general readiness to interpret and explain *so much* of chimpanzee behavior in terms of beliefs and intentions. And this general readiness stems from the fact that so much of this behavior fits the very natural hypothesis that they have beliefs and intentions, and fits so well that this hypothesis must be counted our "best (only?) explanation" of the behavior. (For this reason, the wealth of anecdotal evidence disparaged by SR&B yet tacitly relied on by them is actually of great and legitimate importance as background support for their interpretation of the isolated behaviors on which their report focuses.) G's envisioned experiment could never convince us if the waggle-dances remained the *only* area of the honeybee's life for which explanation in terms of beliefs and intentions seemed plausible. (For more on the kinds of behavioral evidence needed to support claims about beliefs and intentions in languageless creatures, see Bennett, 1976.)

4. I cannot understand the alleged significance of Gallup's experiment, cited by G. If we are at all willing to impute beliefs to the chimpanzee, we must surely allow him to believe that someone is stepping on his toe if, indeed, someone is stepping on his toe. So he must have the concept of his toe, and a concept of himself. (He does not respond by attacking someone stepping on *another* chimpanzee's toe!) Perhaps this is what G is saying in pointing out the absurdity of "awareness of everything but me." But then obviously chimpanzees have self-awareness, and Gallup's experiment teaches us nothing except that chimpanzees can acquire beliefs about themselves *via* mirrors *as well as* in other ways. We already knew they could acquire them in other ways. An interesting kind of self-awareness would be awareness of one's own mental states; but Gallup's experiment is totally irrelevant to this.

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Animal cognition without human consciousness. The *BBS* issue on cognition and consciousness in nonhuman species contains three distinct papers. SR&B have combined the traditions of studying tool use, both from laboratory and field observations, with what they have learned about communication and cooperation in chimpanzees. The result consists of data and logical arguments supporting "symbolically mediated exchange of goods and information in a nonhuman species." P&W's paper describes the halfway point in their long-term program of research on inferences about the chimpanzee's mind by human beings, and, more interestingly, the reverse. A progress report like P&W's is intriguing not only because it presents completed studies, but also because it suggests plans for future research. Thus one excitedly awaits the papers in press and the next installment. Since SR&B and P&W approach animal mental life empirically and empirical-programatically, respectively, G's paper should have been a good review of the literature on cognition in nonhumans, or at least a systematic sequel to his 1976 book on animal awareness. He makes lengthy quotations from his book, but presents little that is new. Ultimately, he attacks a straw man, "behaviorism," and advocates what we need least, another neologism, "cognitive ethology."

Although *Premack & Woodruff* reassure us that the ape "is not intelligent enough to be a behaviorist", they (and, less so, SR&B) can be faulted for failing to tie their exciting data and projections to what we already know about nonhuman awareness. For example, P&W employ distinctly dressed actors to convey *purpose, knowledge, belief, doubt, liking,* and so forth, in stopped film strips. In this, they are similar to (perhaps even more sophisticated than) Hebb and Thompson (1954), who had actors in distinctive suits and masks play the roles of a bold or timid man. Chimpanzee subjects were quite able to make inferences about the mental states of the masked actors, and the actors played their roles sufficiently well to be able to interchange suits and receive corresponding differential treatment by the apes. Another point of P&W's discussion involves their developing concern with the primitiveness of perceived causality. One hopes that they will take advantage of the fact that their animals are movie-trained so as to employ cartoons embodying demonstrations which elicit perception of cause in human beings (see Michotte, 1946). Shaping might be needed, but the demonstration would help P&W avoid a problem, which they recognized elsewhere, of language-dependent mental perceptions.

Savage-Rumbaugh et al. make significant methodological advances in the study of animal-animal communication problem solving, but they owe an historical debt to earlier studies of observational learning in monkeys (e.g., Darby and Riopelle, 1959). If one monkey can benefit from seeing another make an error, it is entirely possible that the tools could be matched samples to their associated functions for apes. One must remember that chimpanzees have the cognitive capacity to match across modality (Davenport and Rogers, 1970), so the match from tool to function is a reasonable possibility. This does not detract from the clever experiments of SR&B and of P&W, but I would have preferred more context from the rich literature on animal cognition.

Griffin presents no new data and omits much of the progress in animal cognition over the past twenty years. Also, he plays the intellectual game of shooting down a theoretical position which is already dead and inventing an imaginary field. This generalization obviously omits his own brilliant work on navigation in birds and echo-location in bats, and his more complete earlier attempt to raise the question of animal awareness (1976 *op. cit.* G, SR&B).

Behaviorism was never as monolithic as G's straw man. It lacked adequate technology, data, and theory to justify expending much time or effort in contemplating nonobservables when there were so many interesting things to observe, measure, and control. However, as technology advanced and data were collected, many theoretical structures associated with behaviorism had to be discarded. This resulted in a neobehaviorism with many ideas concerning animal cognition. Much of this work on animal cognition was done with the behavioristic penchant for careful control and was even conducted in alley mazes (Gleitman and Steinman, 1963) and Skinner boxes (Gleitman and Bernheim, 1963). Space does not permit full development of this argument, but a few examples may suffice to illustrate that comparative psychologists have been studying cognitive problems during the past twenty years and are much more in need of a theoretical structure than a fictional field such as cognitive ethology or sociobiology.

To illustrate that my own contention is not just an attack on another straw man, the following are areas in which psychologists have recently studied cognitive experiences by animals: (1) cross-modal transfer; (2) surgical and optical sensory recombination; (3) several hundred papers on memory and its development, including reinstatement, rehearsal, and the use of surprising cues; (4) transposition; (5) conditional reaction problems; (6) perceptual constancies; and (7) the perception of barriers and detours. Probably I should also have included Harlow's (1949) formation of learning sets, which bridges the gap between incremental learning and insight. This is a considerable commitment to a comparative and evolutionary analysis of cognition. We need to organize animal cognition into a coherent theoretical structure using the presently abundant data.

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The second time around. The behavioral sciences appear to be coming full circle. There was a time, many years ago, before the dominance of behaviorism, when questions about animal minds and emotions were considered to be quite legitimate, and indeed they are still seen as such by people not schooled in the behaviorist tradition. Then, in the early part of this century, the methodological self-discipline of behaviorism took over, and most ethologists and psychologists even up to the present day have fought shy of considering questions of consciousness and mental awareness on the grounds that they are not accessible to scientific investigation and should be left severely alone. Now times are again changing. Subjective phenomena are once more being seen as legitimate areas for scientific study. But whereas this appears to be simply a reversion to an earlier view, a counter-revolution to bring us back to a more enlightened time, it is in reality far more than this. The second time around for scientific consideration of subjective phenomena in animals is characterized by a rigor, a determination to consider alternatives, and above all by attempts to make predictions and test hypotheses, all of which was previously lacking.

Griffin sets the stage by arguing forcefully for a cognitive ethology and attempting to meet head on some of the objections to it. For example, it is often argued by skeptics that postulating subjective experiences in animals does not lead to any specific predictions at all – it would not seem to make any difference to an animal's behavior whether or not it was conscious. In other words, the existence of subjective experiences is something for which there can be no rigorous test. G answers this objection in two ways. Firstly, he argues that open-minded agnosticism is more appropriate (and indeed more scientific) than flat denial, and that while it is true that we have no rigorous proof of the existence of subjective experiences in animals, neither are we able to claim rigorous proof in many other areas of biology (as is the case with the far-reaching and productive theories of sociobiology). G's second argument, which follows from this, is really the more exciting one, namely, that it *is* possible to formulate testable hypotheses about animal subjective awareness. The two papers on chimpanzees by P&W and by SR&B, which accompany G's in this issue, illustrate this point.

Premack & Woodruff set out to investigate something that appears at first sight to be quite out of bounds as far as respectable science is concerned, namely, whether chimpanzees ascribe mental states to other animals, such as human beings. The importance of this paper seems to lie not so much in whether it actually shows that chimpanzees do have a concept of mind as in the method of approach. There seem to be some objections to the evidence that is actually presented, but in thinking about these objections, one is forced to the conclusion that it would still be possible to devise an experiment which would be convincing. This is a far cry from saying that the phenomenon is one which is in principle *not open* to investigation at all.

In fairness to the authors I should be more specific. In the first experiment, the matching between the actor's stance in the video sequence and his stance in the "correct solution" photograph is acknowledged by the authors as being a confounding factor and considerably weakens the conclusion. This result is so crucial to the idea that the chimpanzees are really "assuming that the human actor wants the banana," as opposed to just completing a sequence, that it is a pity that it suffers from this objection. The later experiments with inanimate objects which are said to rule out physical matching suffer from the fact that although familiar sights cannot easily explain the results, the effects of familiar *sequences* do not seem to have been ruled out as completely. In any case, it is unfortunate to have to use a later experiment to justify the conclusions of an earlier one without simultaneous controls. But, as I said before, this is not the important point. The fact that controls could have been introduced and means could have been devised of more completely eliminating simpler explanations does not detract from the conceptual leap forward which the authors have taken in daring to ask their questions.

Savage-Rumbaugh et al.'s very valid criticisms of earlier experiments on chimpanzee communication (for example that there were often inadequate controls for experimenter bias) actually reinforce the point that it is in principle possible to devise rigorous experiments. Picking small holes in existing evidence is in fact a means of developing new and more watertight experimental paradigms, rather in the way that experiments on pigeon homing have been improved by the realization that more controls have to be introduced than was at first thought.

The second time around for the scientific assessment of animal consciousness is, as illustrated by the three papers in this special issue, characterized by adventurous questions and by the realization that it is possible to make determined attempts to answer them. It is not a time when "anything goes" and one interpretation is as good as any other. As G points out, the prospects for a cognitive ethology are much better now than they were in the days of Darwin and Romanes. At long last we can look forward to a real understanding of subjective awareness in animals, one of the profoundest mysteries in the whole of biology on this, the second time around.

by **Daniel C. Dennett***

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***Beliefs about beliefs** [P&W, SR&B].* Because of its intrinsic interest – indeed its fascination – it is easy to lose track of the point of this kind of research. Getting a chimpanzee to talk takes on the aspect of sending a man to the moon. Suppose you succeeded. Then what? Presumably behaviorists would have to claim to be unimpressed, as unimpressed as they are by the verbal abilities of – themselves, for instance. So suppose we grant for the sake of a superannuated argument that *in principle* a suitably complex version of behaviorism can "handle" all ape behavior (and all human behavior too). That version of behaviorism will of course be scarcely distinguishable from mere mechanistic materialism – with micro-events in the brain being viewed as responses, for instance – and there is scant reason to oppose that creed, at least at this stage of our knowledge. The issue that remains is, on a first pass, how fancy a cognitive structure is required *in practice* to predict a chimpanzee's behavior. That is, granting that in practice it is desirable to intentionalize our account of chimpanzees (by attributing beliefs and desires, or belieflike states and desirelike states, Dennett, 1971, 1976), which beliefs and desires will it be useful, predictive, illuminating to attribute? In the present instance, will we find it valuable to attribute second-order beliefs and desires – beliefs and desires about the beliefs and desires of others? If so, then chimpanzees have a theory of mind in the requisite sense, for they use the concepts of belief and desire (or concepts importantly analogous) in their own action governance. If they turn out to have humanlike theories of mind, they will have use of even higher-order intentional attributions; they perhaps believe someone wants them to believe something, or want someone to believe they want something, and so forth. But how can these suppositions be put to the test?

I think the issue is analogous to the current controversy about mental images. What the growing literature on mental images shows is that whatever it is to which we may in the end "reduce" mental-image talk, there can be no doubt that there is *a* level of description of the phenomena at which imagistic characterizations are perspicuous because they are richly predictive of a surprisingly wide variety of behavioral effects. Talking of mental images may be a *façon de parler*, but it is no "mere" *façon de parler*, because taking the talk (quite) literally keeps on leading to confirmed predictions. This is undeniable even if it is also true that talking about mental images is itself in dire need of explanation – and even of ultimate elimination if one supposes that mental images cannot be taken *dead* literally as 3-D pictures in the brain.

What must be shown by *Premack & Woodruff*, analogously, is that imputing a theory of mind to chimpanzees (whatever that comes to literally, in the end) is richly predictive. As P&W note, any single test, however consonant its results with the theory-of-mind hypothesis, can be given a deflationary redescription by associationists *et al.* What one wants is a panoply of results elegantly predicted by the theory-of-mind hypothesis and only predictable with the aid of ad hoc provisions by its competitors. P&W do not yet have these results, as they grant, but while the experiments they are now undertaking would favor their hypothesis if the results were positive, they seem somehow slightly off target. P&W are searching for evidence that chimpanzees have expectations of the behavior of others that are better explained by supposing that they are (tantamount to) predictions derived from the chimpanzee's beliefs about the beliefs and desires of those others than from supposing that they are derived from either habits (of thought) or beliefs about other features of the world *(e.g.,* experienced regularities in the behavior of others). But the very training required to bring an animal into P&W's test situations seems to provide the relevant experience for engendering such alternate habits or beliefs. P&W are aware of this, and much of the complexity of the tests they have designed is dictated by their desire to make this alternative hypothesis less plausible. But in becoming so devious, the tests seem – to me – to sacrifice the most interesting hypothesis: it would be much more exciting to discover that chimpanzees normally have (naturally acquire in their lives) a theory of mind than to discover that chimpanzees can have a theory of mind dinned into them eventually. Bears can ride bicycles – a surprising fact of elusive theoretical interest. But when one tries (as I have, now, for several days) to dream up better experiments for P&W to run, one begins to appreciate that it is very hard to think up direct, natural, plausible tests. Why should this be?

Very young children watching a Punch and Judy show squeal in anticipatory delight as Punch prepares to throw the box over the cliff. Why? Because *they know Punch thinks Judy is still in the box.* They know better; they saw Judy escape while Punch's back was turned. We take the children's excitement as overwhelmingly good evidence that they understand the situation – they understand that Punch is acting on a mistaken belief (although they are not sophisticated enough to put it that way). Would chimpanzees exhibit similar excitement if presented with a similar bit of play acting (in a drama that spoke directly to their "interests")? I do not know, and think it would be worth finding out, for if they didn't react, the hypothesis that they impute beliefs and desires to others would be dealt a severe blow, even if all the P&W tests turn out positively, just because it can be made so obvious – obvious enough for four-year-old children – that Punch believes (falsely) that Judy is in the box.

But suppose we are uncertain how to interpret the children's glee; how can we go about strengthening the hypothesis that they believe Punch believes . . . ? We can ask them questions, particularly "why questions," but others as well ("What do you think Punch would have done if . . . ?"). But are there nonverbal tests we can also employ? It is hard to think of any that would be decisive that wouldn't be too difficult for the children. This is because of the complexity of the "thought processes" one has to impute to any person or animal who acts on the basis of such a prediction from a theory of mind. So far as I can see, the *minimally* complex pattern has the following format:

1 C believes that E believes that p.
2 C believes that E desires that q.
3 C infers from his beliefs in (1) and (2) that E will therefore do x, and so, anticipating E's doing x,
4 C does y because
5 C believes that if E does x, then unless C does y, C won't get something C wants, or will get something C wants to avoid.

(This is the minimally complex pattern for doing something because you believe someone believes . . . ; doing something in order to get someone to believe something in order to get him to do something . . . has a different but equally complex scenario.)

The ideal experiment to establish the use of such an explanatory format will have the following features:

a. E's anticipated action x will be a (relatively) novel action, or at least an action that (arguably) could not be anticipated by C under the circumstances simply by virtue of being habitual for E or oft-repeated in just these circumstances. (An elegant way of accomplishing this is to ensure that the belief attributed to E in (1) is false *(cf.* Punch and Judy), for then E will be expected to act inappropriately to the circumstances, and hence, in all likelihood, not the way E has typically acted in the past.)

b. C's action y will also be an action as much as possible from C's natural repertoire, rather than a highly trained artificial response, for again, arduous training procedures almost inevitably provide grist for the associationist's mill (Dennett, 1976).

c. The perceived (by C) dependence of y on x should also be natural and obvious, so that C's belief in it (5) can be attributed to C on the basis of C's straightforward observation of a relatively novel circumstance, rather than on the basis of extensive training.

Trying to design experiments to meet these conditions soon reveals the difficulty. Conditions (1-3) are relatively easy to meet – one would think. For instance, suppose there is a key that E, the experimenter, uses to open the banana locker. One day two boxes, one red and one green, are placed in the scene, and C sees E put the key in the red box and leave the scene. Then C sees Sneaky Pete come in and move the key to the green box. When E returns to feed C, *ex hypothesi* C believes that E believes that the key is still in the red box, and hence C expects E to go to the red box (since he believes that E wants to get the key). But now, how can things be rigged so there is something C might see to do that is appropriate to C's expectation (meeting conditions (4) and (5))? P&W's solution at this step is to train C to perform a sort of proto-speech-act, a *prediction* by choice of photograph, with the assumption that, for predictions, truth is its own reward (thus satisfying C's desire in (5)). But this is gimmicky. One would prefer to have C's action y interact more meaningfully with E's action x, but reflection reveals that this is hard to set up without resorting to another sort of gadgetry: artificial dependencies created between x-type actions and y-type actions that C might be *trained* to recognize.

The conclusion that seems borne in on one is that unless there is a great deal of normal interaction – either competitive or cooperative – between C and E, there is just no way for C to come to perceive his own actions as meshing with E's in the tight way required at step (5). One can rig it up – *e.g.,* C could be taught that he will get a shock if E opens the red box unless C, anticipating this, moves to a particular location – but this requires training that removes the desired novelty listed in (a). This objection to gimmickry is not just aesthetic, of course; the more artificial the test circumstances, the more restricted the range of predictions available to the theory-of-mind hypothesis, and as noted at the outset, predictive fecundity is of the essence in this investigation.

It appears that except in tricky environments that require extensive training to produce familiarity, the only act-types that naturally meet the conditions are *communicative* acts, such as C's warning E, or requesting something from E, or asking E a question; and so the problem of the training factor now pertains to the training up of communicative act-types. In this regard *Savage-Rumbaugh et al.'s* format with Austin and Sherman looks much more straightforward and promising *if* the communicative mode of interaction between Austin and Sherman can be extended to relatively novel situations without (much) additional training. But still the conclusion that would follow success in such experiments would be at best that chimpanzees can be put in complex artificial environments (artificial for chimpanzees, not for people) in which they eventually develop a theory of mind. In their natural environments there seems to be no clear need for them to develop a theory of mind about each other, and hence no compelling reason to impute it to them. But perhaps further ingenious experiments will find a way of meeting the desiderata listed and make a believer out of me.

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EDITORIAL NOTE

°Received too late for a Response from P&W or SR&B. See Continuing Commentary. [Ed.]

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Animals and the rest of us: Descartes versus Darwin. Topics as complex, ill-defined, and open-ended as those to which this special issue of *BBS* is devoted cannot possibly be resolved, if at all, within the compass of a few thousand words. One can at best select those aspects of the problems that have personal appeal, relying on differences of interest as well as opinion to provide the needed breadth of coverage. Since Griffin, in his contribution and earlier book (1976 *op. cit.* G, SR&B), elaborates the kinds of questions that are partially addressed by the experiments in the other two contributions, it seems logical to begin with a discussion of his views.

Are the meadows of cognitive ethology really greener [G]? Since the views of Darwin, a firm believer in continuity, are often cited as forming one of the major pillars of modern ethology (cf. Eibl-Eibesfeldt, 1975; see also Eibl-Eibesfeldt: "Human Ethology" *BBS* 2(1) 1979), it seems a bit odd that G feels the need to sound a battle cry against Cartesian discontinuity – a doctrine that would appear to have been laid to rest in an earlier age (at least as far as ethology is concerned). Indeed, Darwin's brilliant defender, T. H. Huxley, discusses this issue at length using arguments from neurological continuity very similar to G's to affirm continuity of consciousness (1874). To imagine a genetic saltation suddenly resulting in a phenomenon as complex as consciousness (or human communication for that matter) appeared as nonsensical to Huxley as it does to the British philosopher, Mary Midgley, who, in a recent work, points out that it requires us to assume "a quite advanced point in animal evolution when parents who were merely unconscious objects suddenly had a child which was a fully conscious subject (1978, p. 217)." Indeed, it appears that ethologists do not deny the continuity of subjective states per se, only that one can say anything specific about them (cf. Eibl-Eibesfeldt, 1975). Thus, continuity does not seem to be the question; rather it is whether such vague terms as *cognition* and *consciousness* can be defined with sufficient explicitness to allow for the formulation of testable hypotheses.

What one will find, I believe, is that such terms do not designate entities that an organism either possesses in their entirety or not at all, but rather a mosaic of structures, skills, and knowledge that, like human communication, does not develop in children as a whole, does not disappear in cases of pathology as a whole, and undoubtedly did not evolve as a whole. But what is consciousness? Is it to be contrasted with unconsciousness or subconsciousness? Is it the same as thinking, self-awareness, ability to learn, volition, perception of relationships, a private world of mind? Each of these definientia yields different results in terms of the continuity question. If self-awareness is chosen as a distinguishing characteristic, perhaps only hominoids can be said to be conscious; if learning is chosen, then even one-celled organisms would appear to evince consciousness. It is not clear to me that terms like *cognition* and *consciousness* even define a coherent network of functions in the way I assume a term such as *communicative behavior* does (cf. Dingwall, in press). To postulate a neurophysiological basis for consciousness in terms of neuronal cell number and degree of connectivity as Rose (1976) has recently done is not based on any firm neurobiological evidence [see also Puccetti & Dykes: "Sensory Cortex and the Mind-Brain Problem" *BBS* 1(3) 1978] and implies a *scala naturae* of consciousness with the primates at its apex, which is already known to be unsupported in the case of some indices of learning (Hodos, 1970).

At this point, I'm inclined to agree with the position of ethologists such as Eibl-Eibesfeldt concerning the vagueness of these terms. It is not the case, one should hasten to point out, that ethologists have shied away altogether from postulating theoretical constructs, often in terms of hypothetical neural structures (e.g., *motor coordination center, releasing mechanism,* etc.); indeed, as Konishi (1971) has pointed out, many of these constructs have been shown to have neurophysiological bases. It is the program of research which involves careful observation of behavior, (either in the field or in the laboratory), isolation of critical aspects of this behavior which can be provided with plausible neurological correlates, followed by neurophysiological testing for such correlates – it is in such a program (which might be termed: *neuroethology*) that I would place my hopes for progress, rather than in a *cognitive ethology,* which may well reveal itself a chimera. Incidentally, the use of animal surrogates in the investigation of communicative behavior, which, in my view, is likely to be one of the most productive of G's proposals, is clearly compatible with the program outlined above.

No end of wonders [P&W]. Premack & Woodruff, who show themselves to be well aware of the need for careful definition of terms as well as for the careful evaluation of alternative explanations of experimental results, have provided a fascinating demonstration of how much remains to be learned about the mental states of chimpanzees (as well as of other animals). It will be most interesting to discover the level of performance of other populations mentioned (viz., normal and retarded children) on these same tasks. They may well prove to be significantly lower than those of such a sophisticated subject as Sarah.

Rather than raising quibbles concerning details of the experiments reported, it seems to me more important to address a theoretical point brought up at the very beginning of the paper. Since neither associationism nor rule systems of the type proposed by linguists have provided a viable theory of language, on what basis, I wonder, do P&W assume that these will prove adequate for a theory of something as poorly defined as mind?

In the beginning was the word. [SR&B]. If P&W's contribution shows us how much remains to be learned about the chimpanzee's concept of problem, then the contribution by SR&B shows us how much remains to be learned about this great ape's communicative capacities. The sophisticated analysis of the role of function in word-learning and of levels of wordness constitutes a major contribution of this paper and provides interesting parallels with similar research involving children. As in P&W's experiments, it would be interesting to have direct evidence of normal children's performance in the tasks involved. The keyboard-off control condition provides an important supplement to the work of Menzel [q.v.] on the efficacy of the chimp's "natural" communicative behavior in transmitting information.

It is perhaps important to note at this point that the gap between what is known of the great apes' communicative behavior in the wild (which is very little as yet) and the languagelike capacities demonstrated in the laboratory is often cited as evidence for the discontinuity view. This involves a misunderstanding of evolutionary theory, for it is not the minimal abilities of an animal that are important in the struggle for existence but rather their behavioral potential for dealing with new or unusual situations.

There are a number of minor yet important statements made in the course of SR&B's contribution which may be misleading. Let me mention three. (1) It is an error to suggest that Myers (1976 *op.cit.* SRB) or anyone else who has investigated the neurological bases of nonhuman primate nonverbal (*sic*) communication has failed to notice that the limbs are under volitional control and differ significantly in this regard from the substrates of such communicative behaviors as vocalization and certain emotion-linked facial expressions. (2) The statement that the elements that make up the lexigrams of Yerkish are analogous to phonemes should not lead one to believe that such a level of analysis in the processing of Yerkish has been empirically demonstrated in either humans or chimps, which is, as far as I know, not the case. (3) Finally, the link between tool use and the emergence of language is tenuous at best; the only evidence cited by SR&B in support of this view, namely, the putative constraints on apes' manipulation of objects, is considerably weakened by the recent demonstration that an orangutan is quite capable of making and using flaked stone tools (Wright, 1978).

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Some considerations in the philosophy of mind. *Why the language of P&W distresses me.* I have a mixed response to the paper of P&W. I am very interested in, and admire, the experimental work that the authors report; but I am distressed by the language they use to do so. Because I am a philosopher, it is proper that I should concentrate on their language. But this will have the unfortunate general effect of overemphasising my disapproval, and greatly underemphasising my approval, of their paper.

According to P&W, to say that an individual has "a theory of mind" is to say that "the individual imputes mental states to himself and to others"; and to do this is (apparently) to have and to exercise "a system of inferences." But P&W themselves do *not* have a theory of mind in this sense; for it is not the case that P&W must logically impute a state of mind to themselves, or to others, in order for them to claim to know that they can and do think, can and do feel pain, and so forth, and to know the same of their wives and others. I only impute a state of mind to Smith when, for example, I accuse him of being jealous of my success; I only infer a state of mind in another, when I do some detective work and conclude that Nixon really does intend to make a comeback. And I certainly do not generally impute states of mind to myself. On the contrary, I just take it for granted, or just presuppose, or assume (P&W's word), or something of this sort, that I can think, feel pain, and so on, and that others can do the same.

Now, do P&W want to claim that the chimpanzee really does do some detective work in their experiments and can, therefore, be said correctly to impute mental states to himself and to the humans involved? Or do they think of the chimpanzee, not as a private eye, but as just taking it for granted that the animal itself, and the humans involved, have certain mental states in the circumstances described? Presumably, the latter, since this is the more lowly achievement. I will now rewrite P&W accordingly.

We are told that the animal solves certain problems by just assuming that the human actor wants the banana and is struggling to reach it, and by then making use of this assumption to solve the problem. This is a mysterious claim and, as it stands, not particularly helpful.

a. To assert that "the animal assumes that . . . ," is to ascribe a propositional achievement and attitude to it. For this ascription to be true, it must also be true that the animal has the ability to exercise concepts, such as "want" and "struggling to reach," and actually exercises these concepts in the relevant experiments. What evidence is there that the animal has this ability and manifests it here? It is not clear to me that P&W vouchsafe the evidence to justify us in asserting that the chimpanzee has this propositional achievement to its credit.

b. P&W seem to be unconcerned about this problem, for they appear to argue that this and similar claims must be correct, since they embody the only satisfactory way of explaining the animal's achievements in the experiments.

Now, if this were true, it would be very puzzling indeed. For then we would all be logically committed to maintain that the chimpanzee has acquired certain concepts, and related rules for correct application, without (apparently) learning to do so by the aid of any public means of expression. That is to say, the animal has acquired its own, quite private nexus of concepts. This is all rather mysterious. Indeed, there are well-known considerations, stemming from Wittgenstein, that go to show that this claim is incoherent when made about humans. It would be very unfortunate if comparative psychologists found themselves driven into incoherence about animals.

c. But *is* the P&W claim (that animals assume that . . .) the *only* way of explaining the experimentally discovered facts? This is not a question that I as an outsider, am in a position to answer. But I should be happier if P&W were to pay more attention to other possibilities. For example, perhaps the animal is built to react in appropriate ways to the patterns of input coming from the purposive behaviour of other organisms like itself; and perhaps it solved the experimenter's problems redintegratively with the aid of some experimentally acquired and internally produced mediating cues or aids. I suspect that it may be helpful to look upon the comparable achievements of the small child in a similar way. Then in the course of its next few years, the child comes to acquire the relevant nexus of concepts, and other abilities, which are necessary preconditions for the formation of propositional attitudes about his own purposive behaviour, and that of others. The chimpanzee, on the other hand, may or may not be able to get as far as the child in these respects. But until we obtain evidence that it can do so, we are not justified in asserting *simpliciter* that the animal assumes that the human actor has purposes, it has purposes, and the like.

I suspect that P&W have been misled by our everyday ways of talking. These ways seem to have restricted their scientific imagination into supposing that, if we cannot talk about Sarah as psychologists have done in the past (for example, in terms of association), then we are *compelled* to speak in the way we *ordinarily* do for everyday purposes. This does not seem to be true; the disjunction is not exhaustive. What I hope P&W will do is to press on with their empirical studies of the abilities of animals and combine these with theoretical work, which will help to describe and explain the structure and functioning of these abilities.

Why the language of G pains me. G produces a case for the prosecution of research in a certain field of animal life and activity. This is a very interesting and, no doubt, important field, and I wish him and his colleagues every success. However, I am forced to confess that I do not think G has presented his case as satisfactorily as it might be done. For he seems to have described the field in a confusing way, and he could be accused of overvaluing the relevance of some of the experimental work he uses to support his case.

He tells us that he is concerned with "the mental experiences" of animals. Do they have any comparable to our own? But what is "a mental experience," in contrast with "a nonmental experience," or "a bodily experience," or just "an experience?" His use of the expression "a mental experience" seems to be pleonastic. What he wants to explore, presumably, are "the experiences" of animals. If an animal can be shown to be aware of something, to think, to intend, and the like, then on each and every occasion when it does think (etc.), it can be said to have an experience.

Very well. But what has to be done *to show* that an animal is, for example, aware of x, or has an intention to do k? G does not tell us clearly, if at all, what the criteria are that have to be satisfied before we would be justified in claiming that, for example, "That animal intends to do k." Nor does G appear to offer us any stipulative criteria. This is a pity. In his book (Griffin, 1976 *op. cit.* G) he says that "an intention involves mental images of future events." But, alas, this answer just will not do.

i. Smith may have some mental imagery connected with an intention. But this does not entail that he has some *particular* item, or items, in his mind's eye, as he would have if he were an eidetiker projecting some particular picture onto a background. So what we are saying of Smith here is the usual very vague story that we produce when we speak ordinarily about imagery.

ii. More serious, for an act of Smith's to be intentional, it is not necessary that it should be preceded or accompanied by imagery. He can perform an act intentionally as part of a habitual routine, with his mind on other things.

iii. Nor is it sufficient *merely* to have some "picture" in the mind's eye. If this "picture" is to fuction for me as an image of, for example, Nelson's Column, I must also know that it is Nelson's Column I am imagining. I must be able to use the image to represent the Column, and so on. Consequently, to say that Smith or a chimpanzee has an image of Nelson's Column, or of some future act, is to say that Smith or the animal can achieve propositional functioning and therefore can enter into a range of propositional attitudes in respect of the column or the act. I think G should, and would, accept this consequence. But I am uncertain that he is aware of all this.

Elsewhere in his article G has a different suggestion to offer. Insofar as human thinking is "closely linked to language," and "insofar as animal communication shares (the) basic properties of language, the employment of versatile communications systems by animals becomes evidence" that they can think, can form intentions, and so on.

In my judgment, this is a much better suggestion to pursue. The power to use language and to communicate is, in an important way, a matter of degree; and this implies that the power of animals to think (etc.) is also a matter of degree. This suggestion coheres with G's own emphasis on the continuity between men and animals. Moreover, it also enables him to sidestep completely the confusion generated by our ordinary concepts of consciousness, awareness, and so forth, and it allows him to get on with his job as an ethologist. If it turns out that the animal's communication can only be explained by postulating a power to conceptualize sufficient for the achievement of propositions and propositional attitudes, we can then use its communicative behavior as evidence of the sort of internal world the animal possesses.

However, I suspect that the skeptical psychologist will hesitate to agree that the experimental work to which G refers does oblige us to postulate propositional powers. Of course, if we are *required to describe* the animals in Gallup's experiment as G does, viz. that "they recognized the mirror image as a representation of their own bodies," then we do seem to be committed to postulating that the animals have the connected conceptual and propositional powers. But why are we required to use G's description? Why should we not say instead, for example, that an animal has learned to react to the mirror input by treating it as a set of cues for reacting to the corresponding part of its own body? This description – or something much better and more sophisticated along these lines – does not commit us to ascribe conceptual and propositional powers to the animal. This story, or some better story of this type, does seem to deserve discussion.

I think it is important that workers in this field not succumb to a zoophilism that drives them to upgrade their subjects, and to groom these, in fantasy, for presentation at their mother's tea party, lika a latter day Eliza Doolittle. It is also important that they should not denigrate the skeptics with the pejorative use of labels (e.g. "behaviorist"). All such propaganda is apt to be counterproductive, especially in Great Britain. An ounce of good theoretical and experimental work is worth a ton of polemic and propaganda.

Is there balm in SR&B? Yes. As an outsider I find myself commending the critical and relatively detached tone of this paper, and its attempt to elucidate the concept of symbolic communication (and related notions) – an attempt that exhibits some of the complexities involved. Even though the particular experimental findings that are reported may not be very surprising, it is such findings, surely, that help to shape the future of knowledge. *Res ipsa loquitur.*

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I am grateful to Dr. R. Passingham for his comments, but it must not be assumed that he agrees with anything I have said.

EDITORIAL NOTE

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by Howard Gardner

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A social synthesis. Hegel would take satisfaction in the course of discussions about the powers of animals. As against the vitalist thesis that the full spectrum of mental capacities can be found in the lowliest vertebrate, and the humanist's antithetical horror at the prospect of a conscious ape, the recent spate of studies on animal communication has inspired a much more reasonable and informed synthesis. Most authorities now concur that infrahuman primates are capable of significant cognitive and communicative activities but that the exact line of demarcation between animal and human powers remains to be drawn.

Griffin. The three statements presented in this symposium can be organized in terms of increasingly strong (and controversial) claims. With reference to G's call for open-mindedness, there is little basis for quarrel; my eyebrows were arched only by his suggestion that bees may well intentionally interrogate one another.

Savage-Rumbaugh, Rumbaugh, and Boysen. The study by SR&B seems a convincing demonstration that chimpanzees can cooperate with one another in the pursuit of desired ends and that the possession of a symbol system can aid significantly in this process. The fact that the Rumbaugh group is so hard on its rivals in the simian semantic sweepstakes tempts one to apply equally critical calipers to the work reported here: but I am not certain that would be appropriate in view of the conservative claims about its significance.

Premack and Woodruff. As always, P&W's work is ambitious, bold, somewhat programmatic, and somewhat problematic. The cleverness of the studies devised should not deter us from invoking Occam's razor in their interpretation. In this spirit I must voice reservations about certain theoretical distinctions (for example, appreciation of an organism's motives as against appreciation of that organism's knowledge). And I must also cavil about some interpretations. The fact that primates may select photographs appropriate to the solution of a problem, and assign different photographs to different agents, seems interpretable along at least half a dozen dimensions, some of which may prove difficult to disentangle from one another. Nonetheless, P&W's demonstrations continue to provide fascinating insights about the unsuspected capacities of Sarah and her circle as well as the forbidding difficulties entailed in assessing any organism's intellectual capacities.

A Stock-taking. This provocative set of studies encourages some stocktaking concerning the enterprise of primate intellection. Efforts to demonstrate that primates are linguistic in the same sense as humans seem less prevalent nowadays: parallels to human syntactic and phonological capacities seem less than striking, and so authorities are properly focusing on the cognitive and pragmatic aspects of primate communication systems. It is instructive to note that each of these papers highlights a dimension which has hitherto been minimized: the *social aspects of communication.* In a manner reminiscent of the Chicago school of social knowledge (Baldwin, Cooley, Mead), researchers are focusing on the capacities of animals to understand and adopt the role of conspecifics as a royal road toward an emergent awareness (or consciousness) of themselves, their minds, their "societies." And, indeed, it is in beholding "symboling" primates as they aid one another in gathering food, or indicating which of a set of options would aid a fellow organism in distress, that I feel an interspecies tie with them. (Conversely, it is because I cannot – for whatever reason – empathize with the waggle dance, and because I do not find interesting syntactic parallels between Lana's performance and those of young children, that I am inclined to minimize cross-species analogies in these areas.)

Just where we ultimately draw the line between human and infrahuman capacities will depend, it seems to me, on *the ease with which,* and *the extent to which,* other animals acquire the kinds of cognitive, linguistic, and symbolic behaviors which human beings universally acquire. In this regard, the problems faced by G, P&W, and SR&B in assessing the progress of their charges is not fundamentally different from those confronted by my colleagues and myself as we trace the development of symbolic competences in children. We are inclined to credit children with "genuine" symbolic competence, consciousness, intention, and other human virtues, when we find them engaged in generative or inventive behaviors (for instance, using a symbol system in a comprehensible yet nonimitative manner); capable of utilizing a range of symbolic vehicles (language, picturing, gesturing, numbers) to express or refer to a given entity; able to adopt a playful or experimental (as opposed to a rigid "preprogrammed") approach with materials; inclined to reflect upon their own activities (or those of others), using symbolic vehicles to make "meta" comments. Conversely, to the extent that behaviors (1) appear only when elicited by strong training models, (2) recur in virtually identical form over many occasions, (3) display little experimental playfulness, (4) exhibit restricted coupling to a single symbolic system, or (5) fail ever to be used to refer in "meta" fashion to one's own activities, we are inclined to minimize their significance.

No organism will ever reach a level of total consciousness, full awareness, or constant intentionality – these are emergent capacities in terms of which it becomes, at best, increasingly acceptable to describe organisms as the latter, come to exhibit more and more of the kinds of behaviors alluded to above. My reading of the literature of animal communication to this point suggests that it is the much longer period of immaturity in man, coupled with the proclivity to express and understand himself via a much greater range of symbolic vehicles, which brings about the principal differences between human beings and other higher primates.

by James L. Gould

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Behavioral programming in honeybees [G]. The source of the knowledge behind an animal's behavior has always been a question of great interest. Spalding (1873), for example, marveled at the seemingly prescient behavior of a female wasp who would work tirelessly to "gather food . . . she never tasted" to feed "larvae she would never see." The insights of three pioneering ethologists, however, have removed much of the superficial mystery surrounding behavior. In the early part of the century, Karl von Frisch (1967 *op. cit.* G) discovered that each animal lives in a rich, species-specific sensory world to which evolution may have denied us admission. Hence, bees can navigate by means of patterns of polarized, ultraviolet light in the sky to which we are doubly blind. The subsequent discovery of species-specific sensory-information processing emphasizes the problem with reading our own limitations into the behavior of other species.

Later, Lorenz and Tinbergen (1938) discovered that seemingly intelligent behavior may often result from the mindless workings of a machine. The most remarkable example for me is the egg-rolling response of ground-nesting birds.

The animal spots the orphan egg and rolls it back into the nest. One possible interpretation of the behavior is that the bird knows what it is about, but a little discreet tampering with the situation reveals that this is not the case. For example, the bird will retrieve anything even vaguely round, beer cans and volleyballs for instance, but recognizes them as inappropriate once they are in the nest and discards them. More striking still, the object may be removed once the bird has begun the retrieval, and the *now-imaginary* egg will be gently rolled back into the nest nevertheless. The bird is simply a well-programmed machine, wired to recognize one or more simple but normally diagnostic cues for "eggness" and to execute a complicated motor program in response.

A host of ethologists, beginning with Lorenz, Tinbergen, and von Frisch, discovered that learning may be programmed to occur only with the appropriate combination of context, time, and cues, and can be used to build hard-wired motor programs. For example, many birds learn how to sing, but can learn to sing only their own species' song (Marler, 1970). The bird recognizes its own song and ignores those of other species on the basis of certain diagnostic cues. During a critical period in the life of the bird, the song is memorized. Months later, males begin to practice until they learn to manipulate their vocal muscles in a way that will produce a good copy. This motor program becomes fixed, so that an adult male may be deafened without affecting his song.

The lesson from these discoveries is that complex and seemingly inexplicable behavior may be the consequence of an animal's use of unexpected sensory windows, elegant programming, or "instinctive learning." Most of animal behavior may be explained in this way. We reject these explanations of much of human behavior, though, in favor of the elitist notion that our special niche in the world is one in which things are consciously reasoned out with brute intelligence. G raises the ever-intriguing possibility that this strategy may not be confined to our species – that the creatures which throng the stage of life around us may not all be simply the elegant, microcomputer-equipped robots of classical ethology, and that somewhere inside their brains may be an abstracted self-image, and an ability to know what they are doing.

By its very nature, the knowledge of what is going on in a mind is private. The three lessons of ethology mentioned above caution us that mere complexity is not itself a reliable clue. The novelty of G's approach is that is suggests two general categories of *tests* for self-awareness. One sort looks at what animals do when presented with problems which evolution could not have anticipated, so that any intelligent output from the animals must represent its own analysis of the problem rather than evolution's. The other method is to engage in a dialogue of sorts with the species in question, and to look for telltale signs for a disembodied consciousness on the part of the other party. The judgements in either case are largely intuitive, but so they often are at the leading edge of science (Kuhn, 1962).

G concentrates on two groups of animals in his arguments: the higher primates, and the honeybees, each the intellectual apex in their respective phyla. The evidence he cites in the first case is already intuitively satisfying, but it is difficult to imagine consciousness being even possible in bees. Nevertheless, in all fairness I must admit that there are aspects of bee behavior which, in our present state of knowledge, lend themselves to the consciousness hypothesis at least as well as to the robot theory (reviewed in Gould, 1975, pp. 187–194). For example, during training with respect to an artifical food source, there comes a point at which bees begin to "catch on" that the experimenter is systematically moving the food further and further away, and Frisch (1967 *op. cit.* G, p. 17) recalls instances in which the trained foragers began to anticipate subsequent moves and to wait for the feeder at the presumptive new location. It is not easy for me to imagine a natural analogue of this situation for which evolution could conceivably have programmed the bees.

Another example revolves around honeybees' hatred of alfalfa. These flowers possess spring-loaded anthers which give honeybees a rough blow when entered. Although bumble bees, who evolved to pollinate alfalfa, do not seem to mind, honeybees, once so treated, avoid alfalfa religiously (Lovell, 1963). Placed in the middle of a field of alfalfa, foraging bees will simply fly tremendous distances to find alternate food sources. Modern agricultural practices and the finite though surprisingly long flight range of honeybees, however, often bring the bees to a grim choice between foraging alfalfa or starving.

In the face of certain starvation, honeybees are said finally to begin foraging alfalfa, but they rapidly learn to avoid being clubbed. Some bees come to recognize tripped from untripped flowers, and frequent only the former, while others learn to chew a hole in the back of the flower and to rob untripped blossoms without ever venturing inside (Reinhardt, 1952; Pankiw, 1967). What has analyzed and solved this problem: evolution, or the bees themselves?

I find G's suggestions for language-related experiments too technically challenging to be practicable, but I have another experiment to propose. In his charming book *Rationality*, Bennett (1964 *op. cit.* G) develops logical criteria for real, self-conscious rationality. He uses bees as the counter example, though by the time he wrote the book they had been found to do most of the things he says they cannot (reviewed in Gould, 1975, pp. 187–194). His arguments lead him to propose as the unique characteristic of rationality what he calls an "R-denial": denying the truth of a statement because, logically, it *cannot* be true – that is, the ability to recognize an abstract lie as a lie. Now bees do not normally lie – in view of their close genetic relationship to one another and their common goal of sustaining the hive and its queen, it would be maladaptive to do so. Under special circumstances (Gould, 1976), however, foragers may be made unwittingly to lie about the direction of a food source. Bees learn the topography around the hive before beginning to forage. If a colony were placed next to a lake and forager dances made to indicate a familiar food source out in the middle of it, would experienced recruits be fooled into leaving the hive, or, having left, would they search seriously in the lake? If they did, would a subsequent set of dances to another food odor that was apparently still in the lake again elicit a machinelike response? Since throughout evolution foragers have never lied, it seems unlikely that the bee's on-board computer could have been programmed for this eventuality.

My own combination of biases and intuition leads me to doubt that bees know what they are doing. If the examples mentioned above can be taken at face value, however, I must suppose that evolution is capable of such subtle feats of intellectual engineering to deal with unpredictable situations that it is difficult or impossible at present to distinguish the programming of a 1-mg bee brain from some sort of insect free will. If this is the case, how, I wonder, can we talk so confidently about any qualitatively different sources of human behavior?

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Developmental processes in the language learning of child and chimp *[SR&B].* I shall be approaching this commentary primarily from the point of view of a developmental psychologist, comparing the linguistically mediated tool use and exchange by chimpanzees described by SR&B with comparable developments in human children.

An interesting contrast with children is the apparently greater difficulty for chimps of simply labeling an object in comparison with naming the same object when it is needed as a tool. Indeed, my research has indicated that the linguistic encoding of an instrument or tool is extremely rare in the one-word stage of children, and its first appearance is months after the first appearance of a simple label (Greenfield and Smith, 1976 *op. cit.* SR&B). Our study of the development of linguistic functions in two children in the one-word stage also found that the earliest labels precede the earliest instances of naming something in a request context.

All of this would seem to indicate that the difficulty of the two sorts of semantic function is reversed in children and chimps, with chimps (1) more easily learning to use language to request than to name and (2) showing more interest in tools than children do. The first contrast is the more interesting, for it suggests that a primary difference between chimps and people is the chimps' difficulty with symbolization *per se* – forming arbitrary relations between signifier and signified, making one thing arbitrarily stand for another. For the chimps, there seems a relatively long period in which they learn more easily when the word to be acquired is embedded in or part of an action context. The behavior of the more language-experienced Lana does indicate, however, that awareness of arbitrary symbols eventually develops even in chimps, for Lana was immediately able to transfer her tool words from the request to the labeling context without further training.

As SR&B point out, the stage of action-embedding parallels our description of the pure performative stage in child language. In pure performatives, sounds are part of action contexts; sound pattern and referent are not clearly separable. The arbitrary connection between sign and referent does not yet exist. The parallel should not be stretched too far, however; the chimps' requests for tools involve using a word to trigger an action of another person in a specific situation. The chimp's early tool vocabulary is less tied to the animal's own action than the child's pure performative (e.g., saying "bye-bye" while waving). The chimp's vocabulary does appear, however, to be more tied than the child's to the *total* context in which a new word is introduced. Children seem to have a greater tendency to abstract a *part* of the context in which a word is introduced. They then use this abstraction as the basis for further uses of that word, correct or incorrect. But there may be an earlier stage in which human children do not abstract either; Piaget (1951) describes the earliest word uses of his children as totally tied to one particular context.

There are, however, a couple of other possible explanations for the chimps' difficulty with labels. From the procedural information presented, it seems as though the chimps had to produce labels in response to a question like "What's this?" (or some other verbally presented request for a name). In the original tool-request situation, in contrast, the chimps were to name the tool in response to a nonverbal situation: seeing a hiding place baited with food. In our study, we found that a child's spontaneous use of a given semantic function in one-word form occurred first in response to a nonverbal context, only later in response to a verbal one. For example, the children in our study could spontaneously label entities before they could use the same words to answer the question "What's this?" If this same progression exists in chimps, it could also explain why labels were so difficult for them to learn under the conditions of this study.

Another possible explanation of the chimps' difficulty in learning object labels lies in the role of extrinsic versus intrinsic reinforcement in language learning. In the label-training procedure the chimp was asked to name an object and rewarded with praise or food if correct – an *extrinsic* reinforcement condition. In the tool-request situation, in contrast, the chimp was given the tool he had named (even if it was the wrong tool for the situation); here, the consequences had an *intrinsic* relation to the chimp's language behavior. In the naturally occurring language acquisition process of children, extrinsic reinforcement seems to play almost no role at all (e.g., Brown, 1973). At the same time, students of child language have pointed to the potential importance of intrinsic feedback that gives the child information about what he has been taken to mean (Ryan, 1974). This type of intrinsic feedback is provided in the tool-requesting situation, where the chimp is given a tool corresponding to the name he produces on the computer keyboard. In the object-labeling situation, in contrast, he could be given food as a reinforcer, no matter what object name was produced. If this extrinsic reinforcement was interpreted by the chimp as intrinsic, the procedure could actually be confusing. The chimp might conclude that the referent of *blanket,* one of the labels in the study, was the food reinforcer. Finally, after-the-fact reinforcement for correct symbol selection in the label-learning procedure seems to have replaced an initial stage in which symbol and referent are systematically paired. Such a stage existed in the tool-request procedures, but not in the object-labeling one.

Each of these different explanations for the greater ease of learning and using vocabulary in the tool-request procedure would have different implications for the language acquisition process in chimps and its comparison with its human analogue. But more information from the authors about the object learning procedure is needed before it is possible to rule out any particular explanation.

A parallel between chimps and children appears in the concepts implicit in their errors of word use during the acquisition of particular lexical items. Thus, SR&B report a confusion between words denoting members of the tool category (e.g., between *key* and *stick*), but not between tool names and food names. This pattern indicates the functional category "tool" as the basis for the lexical confusion. Similarly, Braunwald (in press) reports examples where her own child spontaneously extends tool names to other tools that fulfill a similar function (e.g., *broo* for broom is extended to refer also to dust mops). Function is certainly not the only basis of children's lexical extensions and, in fact, it is often difficult to separate function and form (as in the broom/dust mop examples). What is clear, however, is that the surface behavior of child and chimp is not very different in some cases of lexical extension.

Perhaps the most striking parallel between child and chimp is the necessity for a prelinguistic sensorimotor understanding of various forms of action and communication for the symbolic encoding of actions and desires to take place. Evidence on this point continues to accumulate for children. For example, using the child's response to offers in order to study the transition from sensorimotor to linguistic communication, we found that offers (of an object or an activity) were initially made by the mother on the sensorimotor level alone, then simultaneously on both the linguistic and sensorimotor levels, and finally on the linguistic level alone (Zukow, Reilly, and Greenfield, in press). Correlatively, at the early stages, children would generally not respond to offers unless all the sensorimotor elements were present (e.g., the mother says "Do you want a cookie?" while holding out the cookie to the child). Response to a linguistic offer depended on having the sensorimotor information simultaneously available. Recently Bruner (personal communication) has found the same pattern of development from sensorimotor to linguistic for the child's expression of requests to the mother. In the interanimal communication experiment reported here, the animal differs from the human child in not having prior experience in which a second chimp fulfills his requests. Hence, it was necessary for the human experimenter to direct one chimp's attention to the other chimp, in order to get the chimp to address his request to another animal. Here the experimenter acted like the mothers in our study, using attention-getting devices to transform initially unsuccessful communications into successful ones.

These parallels and divergences between the developmental processes of child and chimp are important in establishing the full nature of linguistic communication and in identifying what is uniquely human therein. Knowledge of parallels is also important in preventing premature conclusions about chimpanzee language-learning limitations. When many of the chimp's limitations of today turn out to have been analogous to early stages in the child's acquisition process, we should not be surprised when tomorrow the chimp follows the child in taking the next step on the road to mature linguistic communication.

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Basic concepts for cognitive ethology. Ethology, as Griffin (1976 *op. cit.* G, SR&B) has argued, was founded under the aegis of behaviorism. But behaviorism was Cartesian dualism with its mental sector atrophied. Now that experimental psychologists, as well as some philosophers, have undertaken investigations that bypass the Cartesian dichotomy and analyze the cognitive powers of animals, including ourselves, without that embarrassing impediment to understanding, the need to articulate adequate concepts to guide such work brings the interests of philosophers into convergence with those of experimentalists.

1. The conversations of Sherman and Austin [SR&B]. Work of the kind reported by SR&B represents not only "a large step" for their experimental animals, but for human theorists as well. Concepts like "intentionality," "propositionality" (from Steklis and Harnad, 1976 *op. cit.* SR&B, p. 451), "comprehension," and "symbolic representational capacity" should indeed become pivotal to the study of cognition. The context in which they are used and the development of experimental design under their guidance illustrate, for this commentator, the fruitful interaction of theory and experiment that a fresh perspective in science can encourage, and offer, at long last, support for the biologically biased epistemologist in the study of animal cognitive behavior. For a philosophical account of intentionality that parallels SR&B's usage, see, for example, Føllesdal (1969) and Searle (1979).

"Awareness" seems to me rather more difficult. Granted, one no longer wants to deny awareness to other animals, any more than to human beings. Granted also, "awareness," being vaguer than "consciousness" or "subjectivity," smells less than do the latter terms of the ghost of the ghost in the machine (Ryle, 1949 *op. cit.* G), and therefore also less of the ghost of the machine itself. The reasons why it seems to me a questionable concept, at least in some contexts, will be mentioned in my comments on G below. In connection with SR&B, I would only add that their excellent survey of previous work confirms the hunch that both the "objective" and "subjective" halves of the Cartesian duality have haunted previous studies of apes' use of language. Signing could be studied "objectively" and then there was something "subjective" left over that could only be hinted at. To study "symbolic representational capacity" as distinct from signing, on the other hand, is to move from the behavioral, *not* to the mental in some "privileged access" sense, but to a complex context of the kind referred to by SR&B in their conclusion (see Holloway, 1969 *op. cit.* SR&B). An attempt to show how human intelligence is grounded in a complex network of animal competences was made many years ago by Polanyi (1958). It may be that his long-neglected work will prove to have some bearing on future research in animal and human cognition.

2. What Sarah knows [P&W]. Given the post-Cartesian position that I welcome in respect to SR&B, I have obviously no overall comments on P&W. For the sake of my commentary on G, I want chiefly to take both SR&B and P&W as examples of work in cognitive ethology already in progress. Nevertheless, a few points, chiefly terminological, may be worth making. First, although I know it is habitual with linguists to use "theory" as liberally as P&W do, I cannot help finding this usage strange. Not being a linguist, I don't "have" a theory of English. Being a competent user of language is not being a theorist of language. Nor is understanding "minding" identical with having a theory of mind. Although Sarah may well be a genius among chimpanzees, she is no more a learning theorist than I am. Or are what philosophers of science call theories in fact metatheories? There is too much slippage of logical levels here for philosophical comfort.

The same sort of (admittedly purely terminological) uneasiness overcomes me in the face of P&W's "three interpretations." "Physical matching," "association," "theory of mind," and "empathy" would count as three (+) methods by which Sarah might be held to solve her problems. To "association*ism*" would have to be matched three other theories (or metatheories?) of animal problem-solving embraced by psychologists.

One, more substantive, question: the bracketing of positivists with young children as "parties that would hold a noninferential view" is both charming and instructive. But does the analogy with causality really hold? Causality identified with constant conjunction, whether or not it is narrowed to necessary and sufficient conditions, seems to some thinkers an "unnatural" empiricist contrivance. I would like to ask developmental psychologists whether childish thinking really is of the constant conjunction kind, and to suggest to P&W that even "filling in all four cells in the contingency table" does not in itself produce a causal explanation. (See Grene, 1974, pp. 1–12; Scheibe, 1970; Cartwright, MS.)

It is to be hoped that P&W will comment on SR&B's criticism of the experimental design of Premack (1976 *op. cit.* G).

3. Mental images, plans, and the life of the hive [G]. The difficulties I find in G's presentation are twofold.

First, while both SR&B and P&W present work in progress in cognitive ethology designed in the light of appropriate regulative concepts, G, like the earlier workers discussed by SR&B, seems to consider the program for this discipline from a perspective still haunted by Cartesian thought and the empiricist approach descended from it. G is interested especially in "mental images," or "representations" in the sense of copies of external objects (as against Sarah's presumptive "symbolic representational capacity"). These, he asserts, are "particularly important components in mental experience." Space does not permit me here to marshal evidence against this thesis; I can only assert, with respect, that as a generalization it is patently false. Imaging is a relatively minor and uninteresting aspect of mental life. In addition, G mentions "intentions," evidently in the sense of plans. Maybe. That there is "purposive" behavior in other species no reasonable person ought to deny. But the most promising novel subject matter for cognitive ethology is *intentionality* as SR&B and P&W use the term, not intentions in the sense of plans. Rather than taking refuge in "internal images" to escape behaviorism (a still Cartesian device), it would be more fruitful, in my view, to rely on concepts like those employed by SR&B and P&W (as well as by many philosophers) and escape altogether the old alternative of outer-public-nonmental and inner-private-mental. (Hence my uneasiness about SRB's use of "awareness.") It is of course true, as G points out, that mind is not (yet) definable as exactly as a chemical compound is. Nevertheless, there are approaches to the study of mind, neither behavioristic nor subjectivistic, which can guide research in this area — as, once more, SR&B and P&W demonstrate.

Such approaches, moreover, would support the notion of a causal relation between the development of mind and the degree of organization of the nervous system. This is where my second difficulty arises. For G both explicitly asserts and, at least implicitly, denies this kind of relation. At least he seems to deny that the difference in the brains of a primate and an insect makes any important difference to their mental powers. Is it because bees are small, or such distant kin to us, he asks, that we deny them "intentions"? Of course not; first and foremost, it's because of the organization of the arthropod nervous system as compared with that of primates. G himself has told us that it is "parsimonious to assume that mental experiences are as similar from species to species as are the neurophysiological processes with which they are held to be identical." And as different! Even if the firing of a single neuron is indistinguishable in bee and chimpanzee and man, the organization of the nervous system is very different indeed in the first case from the other two, and somewhat different in the second and third.

In looking for formulations to take account of such differences, I had thought that A.J.P. Kenny's definition of mind, quoted by G, was a good candidate (Grene, 1976, 1978). It seemed to me a radically non-Cartesian statement that applied to the kind of mental life apparently correlated with especially highly organized nervous systems, with the full development of the organisms possessing them dependent on the use of artifactual symbol systems of the kind most strikingly characteristic of human natural languages. Clearly, however, I was mistaken. That G can apply this definition so readily to apian communication must rest, I admit, in part on the inadequacy of the definition itself. The "symbols" in the case are not meant to be just signings (see SR&B) and the "activity" is to be understood (a) in terms of competence, not performance, and (b) in intrinsic relation to the achievement of the power of responsible choice. In trying to refine Kenny's statement appropriately, it will be useful to consider such work as that reported in SR&B and P&W. As SR&B suggest, it is probable that more can usefully be said not only about the likenesses, but also about the differences between symbol use in chimpanzees and children. In any event, it seems to me essential for the careful development of a cognitive ethology in general that the massive differences in neuroanatomy, proportion of learning to "wired-in" behavior patterns, and so on, between primates and arthropods be kept in mind (cf. table in Popper and Eccles, 1977 *op. cit.* G, p. 58). G's insistence on overlooking such differences appears on the whole to weaken, rather than to reinforce, the foundations for the comparative cognitive ethology he wishes to support.

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What does it mean to be conscious? *The proof of the pudding [SR&B].* The meaning of the symbol use experiments with apes has been endlessly discussed; the experimenters themselves – the Gardners, the Rumbaughs, Premack, Patterson – tend to regard the behavior as truly linguistic, while their detractors – Bronowski and Bellugi, Sebeok (*op. cit.* by SR&B and G) – find fault in the experiments themselves or in their interpretation. Sebeok even proposes that the experimenters have fallen victim to the "Clever Hans" fallacy, an interpretation which I have little hesitation in rejecting. More perceptive critics accept, by implication if not quite openly, an evolutionary continuum from "not-language" to "language," but erect barriers across the road at some point, on one side of which is language and on the other side mere brute intellect; this type of response is reminiscent of the misapplied ingenuity that went on in the 1960s to define "man," when tool-use, tool-making, tools-to-make-tools, and so on fell in rapid succession to the onslaughts of chimpanzees, yet the lesson still was not learnt that where there is an evolutionary continuum *any* threshold or barrier set up is bound to be artificial. Instead of trying to fix a gulf between "man" and "animal," it is surely much more useful to ask just how much further along the track of hominization a given ape would theoretically have to travel; or indeed, since living apes are not merely proto-humans, what aspects of a chimpanzee's or a gorilla's psychological capabilities are peculiar to that species and are *not* along the track of hominization.

Nonetheless, some of the first experimenters, in their understandable enthusiasm, may well have overstated their case, and left howling gaps into which the perspicacious human chauvinist can insert a crowbar (or, let us say, a monkey wrench). It is to SR&B's credit that they review previous work and pick out some of these gaps. From the standpoint of their own criticism, the experiment they report comes through unscathed. I think it answers all potential criticisms: have the chimpanzees internalized their symbol use system? Will they still use it when no experimenter is present? Will they use it to each other?

The subplot of SR&B's experiment, which the authors do not stress but which is surely almost as remarkable in its implications, is the food-sharing. The two chimpanzees were not mates, nor were they brothers: no question of Inclusive Fitness. They did not have anything tangible to gain from sharing the food: the sorts of factors that seem to operate in the meat-sharing in the wild – such, presumably, as dominance, hunting partnerships, oestrous females – are quite inoperative in the laboratory. Though speculation about the relationships between the two chimpanzees outside working hours would be possible, the most parsimonious explanation is simply "Thanks for your help."

The biter bit [P&W]. This paper is less satisfactory than SR&B's, because of its incompleteness. SR&B describe in foolproof detail a single experimental series, and follow it up with a satisfying theoretical discussion; P&W pass in review a whole gamut of experiments, clearly the product of someone's fertile and enterprising imagination – but in only one case is there anything that can be called a result, and most of the experiments have not even been performed yet! It is disappointing to read of experimental set-ups with most exciting potential, only to read on and discover that it is all in the future.

The one experiment that has some claim to completeness, that of the chimpanzee finding the solution to her handler's problem, is very cursorily described. How, for example, did she understand what she was to do when the film was stopped? The argument that she had a "theory of mind" seems a little tenuous. Certainly simple association is not, on the description offered, altogether excluded as an explanation; nor even, in this case, is "Clever Hans." I am bound to say, though, that the disasters in which she placed the handler she dislikes add a great deal to the conviction of the experiment, and do make the reader turn to a "tit-for-tat" explanation. Revenge is sweet, after all: or are we anthropomorphising too much? And does a chmipanzee, despite many a theologian's categorical assertion, know right from wrong?

Does the noosphere inevitably reflect [G]? I would like to make my feelings quite clear: I think, with G, that self-awareness, or consciousness, is one of those attributes which has up to now been far too glibly put down as a human species-specific characteristic (see, for example, Teilhard de Chardin, 1959), simply because it has not been sought in other species (also, of course, because of that human chauvinism which has for so long been permitted to retard interpretations of nonhuman psychology). On evolutionary grounds, I would expect it to exist, in however rudimentary a form, in very many nonhuman species. Yet I am not satisfied that G's article – or even his book *The Question of Animal Awareness* (1976 *op. cit.* G, SR&B) – has really attacked the problem more than superficially. The review of his book by Humphrey (1977 *op. cit.* G) is on target here; and Humphrey also asks whether, and in what sense, it might matter whether a given species is self-conscious or not: a point to which I will return later.

The obvious question to ask, which G has hardly touched, is "What would actually be evidence for awareness in animals?"; and this in turn demands that we define awareness very closely – no easy task, for it is one of those concepts that recedes as we examine it. If awareness means simply an image of self, an ability to imagine one's self, then are not dogs self-aware in their dreams, as they whine and bark and – if we are to follow the popular interpretation – chase imaginary rabbits? Such an explanation springs to mind, but I would not accept dreaming in dogs as evidence of awareness by any means. More acceptable are the mirror-reaction experiments with apes, as discussed by Slobodkin (1977): adjusting one's appearance in a mirror implies the existence of fantasies about oneself, and indeed recognizing that a mirror image *is* oneself seems sufficient to indicate awareness of one's own existence.

In a U.S. Primate Center, Dr. Peter Reynolds (personal communication) observed a juvenile gorilla playing by itself, eyes tight shut, making a play-face. This seems to me a very thought-provoking observation. To whom was the play-face directed? To an imaginary playmate? To itself? Either way, a self-image does seem to be a necessary part of the explanation. Another example might be found in the nature of orangutan food-finding behaviour, which virtually requires the ape to have a conscious mental map of its home range (Rijksen, 1978). I suggest that experiments could be designed in which the solutions would depend on the animal's capacity to imagine itself; I am not sure that even the SR&B and P&W experiments fulfil this requirement.

The subject of self-awareness, of course, lends itself to loose thinking, and it is hardly surprising that even the discoverer of echolocation in bats can lose his way in the utter darkness surrounding the topic. Culture is an associated concept; strikingly, the very first attempt to give a formal definition of culture in terms of the behaviour it involves (rather than of the species it is supposed to characterize) dates from June 1978! (McGrew & Tutin, 1978); and it would be simply too facile to link the two by saying that culture depends on awareness, or indeed vice-versa. It could be argued that innovation, the first step in cultural development according to McGrew & Tutin, demands awareness, but I am sure that even this step could be quite unconscious. One of the major adaptive functions of culture, as Humphrey (1976) makes clear, is to extend the range of activities that can be performed with least effort, either physical or mental.

It may be true, as G suggests, that it is the cavalier rejection of the possibility of a self-awareness in nonhumans that has cleared the way for physical abuses of them; yet remember that the very rationale for biomedical research is that nonhumans *do* work in the same way as humans, that there *is* evolutionary continuity. The nineteenth-century dog-vivisectors, even the unspeakable Claude Bernard, were in complete agreement with today's primate "users" over this. I think the explanation lies not in their rejection of the concept of awareness – is pain any the more terrible if one knows that one is in pain? – but in making a decision that they will not follow through the logical implications of their basic premises. The point is, as Jeremy Bentham put it, "not, can they *reason?* nor, can they *talk?* but, can they *suffer?*" That is a much simpler question to answer, and (contra Griffin, if I understand him right) is the only question that can possibly be relevant for a human being in deciding how to interact with members of other species.

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***by* Gilbert Harman**

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Studying the chimpanzee's theory of mind. *Purpose and knowledge [P&W].* P&W say, "Not even the chimpanzee will fail tests that require him to impute wants, purposes, or affective attitudes to another individual, but he may fail when required to impute states of knowledge." This cannot be right. Purpose and knowledge are interrelated concepts. A chimpanzee can have a conception of desire or purpose or goal only if it also has a conception of knowledge or belief. The concept of a desire or goal or purpose is the concept of an attitude toward something that can lead a creature with that attitude to do what it knows or thinks will promote the existence of that thing.

P&W say what they say because they take a chimpanzee to have a conception of knowledge only if it distinguishes *know* from *guess* or realizes that adults have more knowledge than children. But distinguishing knowing from not knowing would be enough – for example, the chimpanzee does not expect a second chimpanzee to approach some partially hidden bananas until the second chimpanzee has caught sight of them.

Furthermore, a chimpanzee might distinguish *know* from *guess* or *merely believe* without satisfying the strong condition P&W propose, namely that the chimpanzee should be able to distinguish knowledge from correct guess or true belief. I have had students at Princeton University who distinguish knowing from merely believing but have trouble distinguishing knowledge from true belief or correct guess. There is no reason to require more of a chimpanzee than of an adult human being. Suppose that a subject chimpanzee sees a second chimpanzee watch a banana being placed into one of two opaque pots. The second chimpanzee is then distracted while the banana is removed from the first pot and placed in the second. If the subject chimpanzee expects the second chimpanzee to reach into the pot which originally contained the banana, that would seem to show that it has a conception of mere belief.

Expectation and discrimination [P&W]. Now it is important that, as P&W stress, a chimpanzee's theory of mind, if any, will be manifested in its predictions or expectations about others. Expectations or predictions are not just discriminations that the chimpanzee makes. A chimpanzee might discriminate between X's being followed by Y and X's being followed by Z without expecting an actual X to be followed by Y (or by Z). The fact that the chimpanzee makes certain distinctions, no matter how complex, does not show that it has the relevant expectations and so does not show that it has a theory of mind. P&W's experiments using videotapes and photographs test a chimpanzee's ability to make certain distinctions but do not clearly test what it expects to happen, so it is not clear what such experiments can show about a chimpanzee's theory of mind.

A chimpanzee manifests its expectations by relying on them in its own purposive behavior. It manifests its expectations about what another chimpanzee will do if in obtaining its goals it relies on the other chimpanzee to do the thing in question. The cooperative behavior described by SR&B may be a good example.

Manifesting or acquiring a theory of mind [SR&B]? SR&B report that "Prior to the training that promoted learning of tool function and cooperative sharing of the obtained foods, mutual requests for aid were not observed between the animals. . . . Now they regularly employ prelinguistic gestures of this sort in their interactions with one another." This should remind us that in asking "Does the chimpanzee have a theory of mind?" we might mean either "Does the chimpanzee in nature have a theory of mind?" or "Can the chimpanzee acquire a theory of mind, for example by learning cooperative sharing, and so forth?" We must allow for the possibility that these questions receive different answers. Our studying a chimpanzee's theory of mind might be what leads the chimpanzee to develop such a theory.

by **D. O. Hebb**

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***Behavioral evidence of thought and consciousness** [G].* There is no need of dissent from G's conclusion that animals (or some animals) are conscious and have representational processes and intentions; and there is a reasonable case for the idea that apes may have a concept of self. The trouble with G's paper is that it is an anachronism, at least as far as comparative psychology is concerned, and I would be sorry to think that ethology was lagging so far behind. The argument would not have been out of place in 1935 when the great continuity/noncontinuity debate was getting under way, concerned with the question whether cognitive processes must be postulated to account for the way in which animals learn. But comparative psychology has moved on, with the noncontinuity or cognitive position tacitly conceded following the important unifying paper of Meehl and MacCorquodale (1951). It is true that some learning theorists (to whom G refers, apparently, when he speaks of strict behaviorists) still prefer to avoid the problems of imagery, intention, and insight, though they do not deny that such things exist; but they are not at all representative of comparative psychology – or indeed, of behaviorism generally. Lashley and Tolman both called themselves behaviorists and were certainly strict enough in behavioristic method, but both spent their careers in the attack on Watsonian and neoWatsonian ideas.

Familiarity with the development of that line of thought in comparative psychology, besides showing that Watson need not be disproved all over again, would have made it easier for G to find objective criteria of what used to be called thinking but is now called cognition, and to show the objective, behavioral meaning of terms like consciousness *and* mind without appealing to self-awareness or having to resort to the commonsense definitions of a nontechnical dictionary. Such an approach has been made elsewhere (Hebb, 1949, 1960, 1972; see also Puccetti & Dykes: "Sensory Cortex and the Mind-Brain Problem" *BBS* 1(3) 1978). It depends on a distinction between cognitive behavior, thought-controlled, and behavior controlled by conditioned as well as unconditioned reflex, which is sense-dominated or environmentally programmed – a distinction that would immediately prevent the error of regarding the communications of honeybee and chimpanzee as similar, except in complexity. The ordinary communications of the untrained chimpanzee are cognitive and different in kind from the reflexive "language of the bees."

To recognize that fact is not to abandon objectivity. Dichotomies *are* necessary in biology. Evolution has produced qualitatively new physical structures, different in kind from what preceded; in a comparable way, in the development of the nervous system, qualitatively new kinds of behavior have emerged. These can be thought of as produced by the attainment of critical mass (or a critical level of complexity) in the nervous system. Cognitive behavior has been demonstrated in mammals, even in the laboratory rat. There is little evidence of it elsewhere.

The objective approach to these questions might run as follows. Consciousness is a state of reactivity to the environment that is characterized by the presence of representational processes. That is, consciousness requires the presence of thought. The essence of thought is the representational process, image, or idea. The representational process is an activity of the brain that is initially elicited by stimulation from some object or event, but can later occur in the absence of that stimulation. In effect, it may be thought of as a perception excited by an associative process instead of by the adequate stimulus. This conception was made fully operational and objective by Hunter (1913, 1917), who showed how to demonstrate the presence of representational activity in animals and young children by means of the delayed response, without any appeal to verbal report. He thus gave us the means of finding out which species are capable of cognitive activity.

The perfect demonstration was made by Tinklepaugh (1928) in Tolman's laboratory, using the delayed response with monkeys. The monkeys liked lettuce, but liked banana better. When they saw lettuce put in the food cup, and after the delay period found lettuce there, they took it and ate it. But when they saw banana and then found lettuce (the experimenter having made an exchange during the delay period), they disregarded this second-class reward and hunted in and around the food cup for the missing banana – or had a temper tantrum instead. Here the evidence for the existence of a memory image and expectancy is unassailable. Together with Köhler's (1927 *op. cit.* SR&B) classical work with the chimpanzee, it leaves no reason for debate in the question of thought and consciousness in the primate; and the work with rats in Tolman's laboratory (e.g., Tolman, 1948) tells the same story, even at this lower evolutionary level.

Now the crucial point is that such evidence is almost totally restricted to mammals. It seems likely that the larger-brained birds also have cognitive capacities and that their nest-building or broken-wing behavior is more than a compulsive situation-guided and controlled set of responses, but the necessary experimental analysis has not been done. It seems clear, however, that there is no basis at all for attributing representational processes to the insect, and hence no basis for regarding the communication of ant or bee as the same in kind as the intentional communication of the chimpanzee. Intention or purpose includes an anticipatory idea of a future state of affairs that will result from the intentional act.

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by **John Heffner**

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***Perception and animal consciousness: the philosophical context** [G, P&W].* Unless our conclusions are to be constrained unnecessarily by method or ideology, there is little reason to doubt that mental activity in humans forms a continuous spectrum from sensory processes to high-level abstractions. Behavioral and anatomical evidence make it quite reasonable to conclude that many animal species experience roughly similar spectra, even though theirs probably do not extend as far into the abstract part of the range as the human spectrum. In treating this subject, ethologists are concerned primarily to ascertain and compare the ranges of the various spectra, whereas philosophers are concerned primarily to articulate criteria by which the ethological judgments can be made. Both concerns overlap, but their different aims suggest caution in the use of philosophical literature by ethologists and in the use of ethological studies by philosophers.

Much twentieth-century Anglo-American analytic philosophy has been constrained by method and ideology. It has been constrained methodologically by a rigid separation of data and theory, and, in its virtual exclusion of empirical considerations, by conceptual and other a priori analytic techniques. Ideologically, it has been constrained by its narrow interpretation of philosophy as a study of the attribution of meaning to various linguistic forms. These constraints apply equally, although in somewhat different ways, to both the positivistic (Shaffer, 1975 *op. cit.* G; Edwards and Pap, 1973 *op. cit.* G) and the ordinary-language (Ryle, 1949 *op. cit.* G) branches of analytic philosophy. Philosophy has accordingly tended to be unconcerned with specifically empirical questions, naive in its use of empirical data, and historically myopic. Because its doctrines have usually been molded by its constraints, its literature should be used cautiously. And because it tends not to treat the problem of animal consciousness as an empirical problem, its basic assumptions probably are not compatible at bottom with the programs of P&W or G. What G refers to as a "customary" denial that animals have experiences comparable to our own, therefore, should be read specifically as being customary within the Anglo-American analytic approach to philosophy. Some other approaches to philosophy make the same denial (e.g., Cartesianism) and some do not (e.g., much of traditional empiricism, pragmatism, and some forms of Platonism and Aristotelianism), but in each case the reasons for the specific doctrines, which vary greatly among the various approaches, are probably more important than the doctrines themselves.

G notes correctly that psychology, no less than philosophy, has been constrained by method and ideology. Methodologically it has been constrained by too great an emphasis on radical behaviorism, and ideologically it has been constrained by its implicit naive materialism (Campbell, 1969). To view the problem of animal consciousness as an empirical problem, broadly construed, is to go far by way of eliminating both straw men and straw ghosts. To do so, moreover, as indicated in G's quotation from Whiteley (1973 *op. cit.* G), is to return to a more traditional philosophical perspective, in which the scientist and the philosopher are considered to share certain general questions about nature. This more traditional, more naturalistic perspective denies a radical distinction between data and theory. It sees an understanding of data as leading to theory, and it sees theory as a useful basis for the interpretation of data. Data and theoretical concepts are thus considered to be related dialectically by mutual criticism and support, so that neither assumes absolute epistemological precedence over the other (James, 1904; Shimony, 1971; Campbell, 1974). Philosophical theories are thus considered to be continuous with scientific theories and also to have empirical implications. This philosophical perspective, which is implicit in G's program for cognitive ethology, is epistemologically unobjectionable, provided only that recent Anglo-American analytic philosophy is not taken for orthodoxy. When the problem of animal consciousness is viewed from this broader perspective, it becomes a relatively uncontroversial problem which has empirical implications. Both the philosopher and the ethologist can focus on the interesting question, which is not whether animals have minds, but what specific cognitive structures they may have.

Perception is a rich source of clues about these cognitive structures. Because they affect phenomenal fields themselves, it is misleading to divide perception into mental photographs plus interpretative inferences (Heffner, 1976). Spatial vision, for example, is highly organized by memory, classification, and other cognitive structures, which affect the visual field itself and not just the use which the perceiver makes of visually received information. It is reasonably certain that the spatial vision of many animal species closely resembles human spatial vision, which argues strongly for a similarly close resemblance of the relevant cognitive structures.

In their treatments of intentionality in animal cognition, both G and P&W touch a matter of great philosophical interest. Intentionality is not limited to conscious planning. In logic the term designates the definition of class membership by characteristics rather than by enumeration, and in philosophy of mind, especially in the phenomenological movement begun by Husserl (Spiegelberg, 1971), it designates the cognitive processes by which experience is referred beyond its own phenomenal content to external objects. By virtue of its organization, human perception is intentional in both senses, even though it is not usually voluntary. The extent to which animal perception is similarly intentional could provide clues about the higher cognitive processes in animals. The further issue of intentionality as volitional, of course, is also interesting in its own right. P&W's efforts to gauge various modalities and epistemic states are particularly impressive in this regard. They seem to circumvent some of the difficulties that arise when communication, language, and speech are taken simplistically to be coextensive. Much linguistically oriented philosophy is not this simplistic, but it is important to supplement its tendency to rely exclusively on verbal examples. The results obtained by P&W seem to indicate that at least some primates besides humans have at least a limited ability to make and use symbols. These results imply corresponding mental activities, and accordingly they should interest philosophers as examples of experimental data which have epistemological implications.

Impressive though P&W's results are, they finally suggest a philosophical caveat. One ability that humans have undoubtedly to a far greater degree than other species – if, indeed, other species have it at all – is the ability to think meta-linguistically. To manipulate symbols, and even to invent them, is not necessarily to comprehend their nature as symbols. Chimpanzees may learn to communicate, but they probably will not become grammarians or logicians. To have mental activity in various modes is not necessarily to recognize them as such, just as many humans who have never studied logic can nevertheless think logically. Chimpanzees may learn sophisticated ways of communicating and of coping with the world, but they probably will not become ethologists or philosophers. To say that chimpanzees attribute states of mind to other organisms is not to say that they have even implicit theories of mind. "Theory" usually designates a meta-language, and it really is too well embedded in the vocabulary of science to receive an impromptu definition.

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In a manner of speaking. If we compare the vocabulary between early and late Greek texts over the first millennium B.C., or between early and late Hebrew texts over the same period, a dramatic change is obvious. Early texts have no mental words. The referents of words are concrete, indicant, objective, touchable, watchable. But, in a few centuries only, the human lexicon is suddenly aglitter with a network of new subjective words that to us are equivalent to mind, belief, know, remember, imagine, aware, purpose, intention, and so forth. The referents of such words are only observable, if at all so, by a new kind of mental process currently called introspection, previously reflection, or simply and more fuzzily thought. And the peculiar quality of these words, in contrast to cup, run, green, river et al. is that they create their own referents on the basis of metaphor, becoming those analog behaviors which we call consciousness.

This new way of talking about human happenings became its own theory of human behavior. Previously, it might have been said that a man seeks shelter. Now, a man seeks shelter because he wants shelter – a vacuous tautology which says almost nothing, but whose function is perhaps to help us look "inside" the man and pay attention to his consciousness rather than his motions. We like dealing with the "insides" of others, their intentions, ideas, thoughts. It is safer. It makes behavior more predictable. Most of our social interactions are now on this level. In fact, talking about other's consciousnesses is itself an inherent feature of consciousness. Even in this peer commentary journal, we are talking about ideas which we locate in the heads of others and ourselves. Nothing of the sort could have taken place 1000 B.C.

This mentalizing lexicon was so useful that it spread out to describe almost all human action, even when not warranted. A person after consciousness developed may indeed want X or have an intention to Y or be aware of Z, but not all X's and Y's and Z's are preceded by wants, intentions, or awarenesses. The attribution of all our behavior to wants, purposes, intentions, and so forth, became so habitual that it left us with the false conviction that consciousness governs all and is responsible for everything from concepts to learning and speaking.

So excessive did this way of speaking about ourselves become that it spread to all animate behavior, until during part of the first millennium A.D., animals could be tried in court of law and proven guilty or innocent of willful misbehavior. And then it engulfed even inanimate behavior. In the Renaissance, because magnets had the ability to move and be moved, they were thought to have souls. Or even rivers seeking valleys. And after Copernicus showed that the earth really moved, no less an assembly than Campanella, Kepler, and – at one time – Newton believed that the universe was what we would call "cognitive." Even today, contemporary physicists such as John Wheeler and Eugene Wigner are making the same confusion of metaphor and actuality, proclaiming that consciousness has to be brought in as a force in the universe because of certain astonishing findings of quantum mechanics.

This wisp of history is not beside the point here. It is meant to show that we have inherited a group of mentalistic words that are as slippery as live fish and just as difficult to hold still. Moreover, there seems to exist, from their first significations to conscious processes, a hierarchy of levels of metaphoric application as we descend from human adults to rivers or magnets. I think it is obvious with this preamble that I am about to suggest that the three papers under discussion are using these mental terms on a metaphoric level, not as they pertain to actual human consciousness.

Let me immediately say, however, that my criticism is in a sense unfair, since none of the writers stresses the word consciousness. I apologize, but only halfheartedly. For I think the average reader will certainly assume that it is a consciousness like his own that is being talked about with such cognate terms as awareness, intentionality, or theory of mind.

Let me take just one part of *Griffin's* extremely interesting paper, his description of Gallup's experiment. That a mirror-educated chimpanzee immediately rubs off a spot on his forehead when he sees it in a mirror is not, I suggest, "clear evidence for self-awareness," at least in its usual sense. Self-awareness usually means a consciousness of our own persona over time, a sense of who we are, our hopes and fears, as we daydream about ourselves in relation to others. Our conscious selves are not our bodies, although our bodies, particularly our faces, are often emblems of ourselves. We do not see our conscious selves in mirrors. Gallup's chimpanzee has learned a point-to-point relation between a mirror image and his body, wonderful as that is. Rubbing a spot noticed in a mirror is not essentially different from rubbing a spot noticed on his body without a mirror. The animal is not shown to be imagining himself anywhere else, or thinking of his life over time, or introspecting in any sense – all signs of consciousness.

As for mental representations or images, it depends what our precise referents are. A dog seeking a particular stick thrown by his master into a high hayfield certainly has a persisting brain-representation of the stick, a visual-olfactory-tactile complex by which he will recognize the stick. But he does not introspect upon this brain-representation as we do with our conscious images.

When two chimpanzees are communicating with each other, we have a vastly more complicated situation than one chimpanzee communicating with a mirror. The experiment by *Savage-Rumbaugh et al.* is methodologically elegant and exciting in clearing out extraneous variables. The reader is almost impatient to suggest further studies with the same paradigm. But I do not understand the necessity of emphasizing such difficult terms as "intentionality" and "symbolic." I am reminded here of Sir John Lubbock, who, in 1888, hearing of the methods used in the training of the human handicapped, trained his dog to "read." The dog would bring to him in appropriate situations any of several cards on which were printed "food," "out," "bone," "tea," and do this in a way that simulated human speech expressing human wishes. I would call this symbolic and intentional behavior, but explainable on a far simpler level than consciousness.

Even more complicated is the relationship of a chimpanzee to a videotape of actors in problematic situations. *Premack's* papers are always so articulate about these issues as to leave one full of doubts about one's own ideas. As he well knows, so much depends on definitions and the referents of the terms of those definitions, and also on the connotations of the terms used. "Theory of mind" certainly conjures up human intellectual processes. If the term simply means the recognition of a particular mental state in another animal and by mental we do not imply conscious, I do not disagree. But then we can apply such a phrase much more widely: to a dog that cowers to his master's scolding tone or wags his tail to praise; or to a four-year-old child who can choose appropriate gifts for a two-year-old. Both dog and four-year-old are recognizing the mental states of others, and I suggest that this is more automatic than introspective.

Although this is not explicit, all three papers seem to say that demonstrating that animal behavior can be made to simulate aspects of human behavior, simultaneously demonstrates a similarity to human conscious functioning. It is the same argument used with that other method of simulation of cognitive processes, computer intelligence [see Pylyshyn: "Computational Models and Empirical Constraints" *BBS* 1(1) 1978]. But – to use a wildly dissimilar and probably inaccurate example – because Mickey Mouse looks and behaves so humanly on a screen does not mean that a celluloid film is conscious; it means Mickey is made to look conscious.

In no way do I mean to diminish the very real breakthroughs of these ingenious studies in what, with G, we could call "animal cognition." They point to a new era in studying the primate mind. But we must not be misled by our labels into thinking the results identical with that metacognition we call "introspective consciousness" or what Alexander Bain long ago called "the awareness of awareness." It would be interesting in this connection to do a reverse simulation, to run these experiments with human subjects, particularly if they had been trained in giving introspective reports by Külpe back in Würzburg.

***by* Alison Jolly**

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The chimpanzees' tea-party. The editors of *BBS* go out of their way to ask commentators to argue. It seem only fair, then, to begin by stating one's own position. I take the naive view that animals are conscious, and that animals that look and act like ourselves probably have awareness rather like our own. The alternative, that consciousness is uniquely human, which has seemed plausible to many followers of the Judeo-Christian tradition, strikes me as un-Darwinian, unparsimonious, and (to quote G) conceited. Another alternative is that consciousness does not exist because it is hard to define – or if it does, let us not mention it. I can see no point in trying to converse with people who ignore consciousness, though they may be suitable cases for therapy.

Which said, I respect the conclusions of all the authors, but feel they have sometimes worked too hard to prove, if not the obvious, at least the likely. The real excitement would be if we could worry less about whether an animal is enough like ourselves to be called conscious, and ask instead how its mind differs from ours in its own right, qualitatively or quantitatively. When will psychologists outgrow the chimpanzees' tea-party?

Griffin attacks the central problem: how can we decide whether a creature is aware of itself or of its intentions? How would we distinguish a guided missile piloted by computer from one with a human kamikaze pilot, and how would we apply the same criteria to a dancing bee?

There are at least three parts to the answer: criteria derived from an organism's actions, those from its structure, and those from our own ethical views.

In spite of G's analysis, I am not fully convinced that we can decide simply from an organism's actions, even with the possibility of two-way communication. Even if we leave aside the question of whether other humans are conscious, we could now install a computer in the guided missile that did not just play patriotic songs, but would answer back. Weizenbaum (1976) describes a sample of conversation between a patient and a psychotherapeutic computer. The computer reflected the patient's own statements and put them together as leading questions, in a sympathetic baritone. Eventually the patient declared, "You remind me of my father!" Of course, she may have been right and the machine's program may indeed have resembled her father's, but it seems increasingly difficult to decide that even a machine is not conscious. Any robot can be equipped with pain circuits – feedback devices to help it preserve itself from noxious stimuli. If a computer uses a thermostat to adjust the temperature of its room, it behaves much like a thermoregulating animal, and if the temperature fluctuates out of control so that it flashes a warning light, it is acting like a social animal that cries for help. All we would need to simulate the apish emotions of its guardians would be a tape-recording instead of the warning light – perhaps a series of tapes that responded to rising temperature in ever more urgent tones, ending with a scream.

This may seem to argue against the thesis that common sense is right in guiding us to believe that other beings are conscious, for computers could now trick common sense. We do not believe the computers because we know we made them. The question of the origin and mechanics of the organism seems to be crucial. If an unknown device, which was rational and socially responsive, arrived from outer space, we should probably conclude that it was alive and aware. When we ourselves succeed in synthesising a moving, respiring cell from biochemical components, will we not admit it is alive? If we could make a biochemical computer that conducted a two-way conversation, might we not call it conscious?

The fact that chimpanzees are constructed like ourselves, and that we share a common origin, makes it highly probable that they also think in similar ways. The presumption that chimps think more like us than bats do, and bats than do bees, is plain zoology. I shall return to ape mentality in discussing the other two articles. G, however, is more concerned with our approach to bats and to bees, where our evolved repertoire of communication does not bridge the species gap.

Here it seems that we are on ground where we can only use the criteria that we would for a totally strange machine. We can be tricked in the same ways. The machine may keep silence or may not speak our language, or it may have a complex responsiveness that we do not wish to call consciousness. Whether we call such an alien mind conscious seems to me to reflect our value system, rather than objective criteria. Perhaps only humans have foreknowledge of death, but shall we deny that other creatures know the terror of death? Whether we call it that depends, not so much on our logic, as on whether we think the feelings of a computer, a bee, a bat, or a laboratory ape, actually matter.

We can be delighted that G has not only raised and analysed the subject of animal awareness, but that he also links the logical, evolutionary, and ethical criteria, whichever turn out to be most fundamental. His article, and book, should inspire much more research that reflects his own respect for the capacities of animals.

Savage-Rumbaugh, Rumbaugh, and Boysen show, to a new degree, the interrelation of symbolization, tool use, and interanimal cooperation. They have shown that their two young chimps understand the symbols they use, in the sense that the symbols are both produced and received as utilitarian communication.

It is true that they have shown all this with rigor and sophistication. The cleverness, however, is the experimenters' not the chimps'. Cooperative multimale hunting in the Gombe Stream, or the joint intimidation of Evered by the brothers Figan and Faben, seem to me more sophisticated than the handling of tools, even named tools, through a partition. The fundamental importance of interanimal attention when learning language and role reversal is hardly new to this experiment, but is common to theorists from De Laguna to Bruner, in practice to human mothers, and, it must be admitted, to people who make pets of their apes while teaching them language. The authors will not be pleased if I suggest they have rigorously arrived where the Gardners started.

I hope that SR&B will now use the enormous potential of their system to build their experiments into still different spheres. Apes that request tools are a model of human interests. Is the emergence of "wordness" significantly more human when a chimpanzee must discuss tools than if he must instruct another to turn somersaults, drum on the walls, or distract an experimenter so that his accomplice can steal food? Until we compare words in different spheres we cannot make too much of the relation between symbols and tool use. Might it be possible to allow the computer-language chimpanzees still more productivity, to give us a cross-check on the ASL chimps that would reveal and analyse what one ape *wants* to say to another?

We are back to G's concern with the quality of minds that differ from our own. At the circus we applaud with awe as the human acrobat brachiates above ground. Then we chortle when bediapered chimps waddle in on their hind legs and sit down to tea. Could the authors now devise an intellectual equivalent of letting the chimpanzees loose on the trapeze?

Premack and Woodruff's experiments reach for the trapeze. As always, it is possible to quibble – to think that the ape's view of differences in ability between chimps and humans might be as interesting as the differences between children and adults, or that the ape may make a very wide distinction between "pretend" and "lie," for one is likely to be playful and one malevolent. On the whole, though, both logic and experiments in this paper are fascinating.

Two comments on their concluding remarks. It does seem reasonable that apes and young children will find it easier to solve problems about wanting than knowing. The distinction between wanting and knowing, though, probably arises through several stages in young children. Even adults may slide from one interpretation to the other, as in whacking a tired child because it didn't know enough to go to bed an hour ago, or as in "The car didn't want to start so I kicked it."

P&W's overintelligent behaviorist who believes that consciousness is in principle beyond the reach of science is here dismissed to join the mystic who believes that consciousness is in principle uniquely human, and so beyond the reach of biological analysis. P&W put primates' theories of each other's minds firmly back into evolutionary context. They open the door again to talking of human origins not just in terms of our technological breakthrough in understanding tools, but in terms of the evolution of our understanding of each other.

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Social knowledge and mental acts. Imagine that the positivist and radical behaviorist movements did not exist and that events had to be interpreted in terms of a different paradigm. My grandmother, for example, would have little trouble explaining the following events: our kitten, earlier taught to retrieve a small black ball, later spontaneously brings the ball to one member of the family; she places it at their feet, and when it is picked up and thrown, runs after it and brings it back. This game goes on for many trials before either the kitten or person tires of it. My grandmother would explain the kitten's behavior by saying that "the kitten *wants* to play." Whether or not the kitten had such a plan, or has a plan before she sees the ball, it is clear that having obtained the ball (and possibly the association from it) she does seek out someone to play with. In fact, everyone at home accepts as correct that the kitten has intention and, as a result of this belief, plays with her so as not to disappoint her. SR&B cite a similar incident, from the Hayes and Hayes (1954 *op. cit.* SR&B) reports, in this case referring to the behavior of Vicki, a chimpanzee.

That such incidents, across a wide range of species, do occur, and that we have failed to give them meaning – indeed, that we have assigned them meaning devoid of mental action – speaks to our bias. The three accompanying target articles seek in various ways to question the bias of radical behaviorism and positivism – G by logical analysis, SR&B by demonstrating that chimpanzees are capable of symbolically mediated exchanges of goods and information, and P&W by demonstrating that chimpanzees are capable of role-taking, which is affected by their knowledge of the situation and of another individual, in this case a human. The exciting feature of all three papers is not only the attempt to refocus our interest on the mental life of nonhuman (and by inference, human) animals, but at the same time to suggest empirically compatible tests of mental operation and action. In reading these papers, I am concerned by the relative lack of inclusion, at least in two of them, of the work of scientists who, with other nonspeaking organisms (human infants and young children), have likewise attempted to study mental life. For me, there is a clear analogy between the problems of demonstrating intention, awareness, empathy, and so forth, in chimpanzees and in human young. The analogy appears to rest on the same assumptions. First, and most important, is the belief that infants and animals have little or no mental life, even if mental life can be defined and demonstrated for adults of the species. It should come as no surprise that intentions on the part of infants appear to many as farfetched as intentions in animals. Thus, the problem explored within these three papers is similar to the problems explored by Baldwin (1894), Piaget (1963), Merleau-Ponty (1964), G. H. Mead (1934) and, most recently, by Lewis and Brooks-Gunn (in press) [see also: Brainerd: "The Stage Question in Cognitive-Developmental Theory" *BBS* 1(2) 1978]. There is a long history within the study of developmental psychology which addresses these problems and because of this they beg for a developmental consideration.

Indeed, my most serious concern with the SR&B and P&W papers (in fact, with most studies of this sort) is that the authors tend to spend too little time on the developmental aspects of their work. For this they cannot be faulted. Clearly, the problem they pose is directed toward the behavior of an animal not in its infancy; nevertheless, there is some similarity between the training procedures used to shape the responses these investigators are interested in studying and the developmental issues themselves, if only that development and learning may have some processes in common, although over a different time span. If we share an interest in intention, awareness, the use of self as in empathy (empathy being the ability to put *oneself* in the place of the other), it becomes important to ask how the complex set of behaviors we use to infer mental life comes about; that is, the history of learning or the developmental course becomes critical to the very heart of the argument.

One aspect of the development/learning of these abilities appears to be the relationship of the chimpanzee to its social world. One is impressed that in both papers the point is made of the existence of a special relationship between caretaker and chimpanzee and how this relationship interacts with the phenomena under study. This would not surprise those of us interested in the developmental aspects of these abilities since, for example, Lewis and Brooks-Gunn (in press), in their study on social cognition and the acquisition of self, place a special importance on the interaction of the infant with its social world and how through that interaction the child acquires at the same time knowledge of itself and another. That the acquisition of self, and thus agency, the prerequisites of intention, (1) have a social interaction derivative, (2) are related to knowledge through interaction, and (3) are not specifically shaped, plays an important role in assigning them "mental" status. The social origin of the mental abilities discussed by SR&B and P&W finds additional support within both papers. In particular, the fact that for chimpanzees as for children, words and concepts are at first closely and therefore concretely linked to function, suggests that early cognitions are socially derived from the organism's adaptation to its environment. For both apes and man, the most critical aspect of this environment is social; thus, we must support Levi-Strauss's (1962) contention that symbolic behavior is a social contract between conspecifics.

The social environment also acts on responses, once they are emitted, and may thus contribute to the latter's meaning. It appears to be the case that humans, at least, impute intentionality to the action of others, even to animals, in varying degrees. The effect of such attributions on the quality and nature of emitted acts is certainly unexplored but it is difficult to imagine that it should not differentially affect the meaning of the action to the actor. As G. H. Mead (1934) and Cooley (1912) have pointed out, we are what is, to some degree, determined by how others think of and respond to us. That the caretakers attribute various meanings to the chimpanzees' behavior should affect the nature of the interaction as well as the meaning attributed to it by the chimpanzee. The acquisition of self and intention may require that young organisms *be treated as if* they do possess these attributes.

If we care to pay attention to social interactions as part of the process by which mental life develops, two special issues are raised by these papers. The first has to do with the nature or content of the symbol system being taught to these animals. We may make the mistake (as Washoe herself is reported to have done in thinking that chimpanzees were people) of assuming that since chimpanzees have a "mental" life, the content of this life is or should be similar to our own. This latter assumption may be unwarranted, since it is possible to believe that chimpanzees can both think and think "chimpanzee thoughts." We assume, as does a child's mother, that the content of thought should be the same; this is the process of socialization by which they in fact *become* the same.

Might it not be better to teach chimpanzees to communicate about those things that are more chimpanzee than human? Surely the behavior of free-roaming chimpanzees is well enough known for us to select more species specific problems. Although P&W suggest that chimpanzees are being used as actors in the problem-solving scripts, why not adapt the context of the problems to them as well? Likewise, SR&B might consider chimpanzee problems and exchange, rather than human tool use. A confusion between "content" and "mental" needs clarification. Are we being told that (1) chimpanzees are like humans, or (2) chimpanzees have a "mental" life, or (3) "mental" life is a valid explanation for some classes of events? While we have evidence that human infants have a self concept or intention, we would not assume the content, and perhaps the process, to be the same as that in adults (Lewis and Brooks-Gunn, in press).

A second question concerning the "mental" abilities in question constitutes a metaproblem or an epistemological issue, namely, how the chimp knows what you want, and what it is that he knows. In the chimpanzee's understanding of cueing (SR&B) and his answering of questions (P&W), a prerequisite is the preexistence of a social interaction, a sharing of symbols, and a shared meaning system that has to be predicated on what the chimpanzee already knows about you and what you want.

In both cases, it is the social interaction of the chimpanzee and his caretaker that sets the stage and provides meaning for any subsequent mental act. We would hold, therefore, that the origins and sustaining vehicles of "mental" acts are social acts. Not until the prior social task is mastered can the child and chimpanzee develop further, and abstract "mental" actions from social experience. In reading the accompanying target articles, one cannot fail to be impressed with the attempt to provide a broader theoretical framework for the explanation of complex events than that provided by radical behaviorism. As others have pointed out, however, such cross-paradigmatic issues cannot be solved by data (Reese and Overton, 1970). Instead, it is necessary to demonstrate the heuristic value of alternate views. The utilization and integration of the social origin of these actions seem to me to serve as some basis for an integrated theory of the mind which connects mental acts with the organism's total experience. As such, mental life constitutes only one aspect of the organism's interaction with its environment.

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by **John Limber**

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Good-bye behaviorism! An ancient psychologist returning from a sixty-year sabbatical in Katmandu or thereabouts and reading this issue of *The Behavioral and Brain Sciences* would experience an extraordinary sense of *déjà vu,* taking him back to his graduate school days. That was a time when everyone was concerned with cognition and consciousness in nonhuman species – and not just that of the recently discovered great apes. Romanes (1885), for example, had already located the origins of consciousness somewhere between the *Coelenterata* and *Annelida.* Others vigorously and seriously debated the implications of demonstrations of learning in protozoa (e.g., Walkin, 1899) for theories of mind. The question of language in nonhuman species, of course, had continually been in the air from Descartes to Max Muller, and indeed for all we know our apocryphal centenarian psychologist might well have fled to the East to avoid speculation on the subject, rampant then as now:[1]

"A great deal has been published in both magazines and newspapers during the past few years about the so-called "language" of animals, especially apes . . . in consequence there was given the widest publicity to an immense amount of the veriest nonsense, from which the average person who depends entirely upon his newspaper for his information is likely to have formed an entirely false conception of this very interesting matter." (Gladden, 1914, p. 307.)

In any event I am certain our psychologist would be familiar with most of the issues raised in this volume as well as impressed by the research discussed in the three papers. He might be inclined, however, to wonder what all the fuss was about; why devote a special issue to such mundane topics? We should then have to tell him about behaviorism and how behaviorism tried convincing us, successfully in many cases, to forget about mental life, mind, and consciousness and to concentrate on pure behavior. Pragmatically it advised its adherents to forget about common sense and not to bother with anything done by nonbehaviorists. We should also be pleased to point out that behaviorism had been on the decline for several decades under pressures from such things as ethology, cognitive psychology, Noam Chomsky, John Garcia, "autoshaping," and "constraints on learning" – not to mention an occasional mutiny (Herrnstein, 1977). Indeed, the present volume might well be said to commemorate the end of behaviorism [see also Bindra: "How Adaptive Behavior is Produced" *BBS* 1(1) 1978; and Haugeland: The Plausibility of Cognitivism" *BBS* 1(2) 1978].

Other nonhuman minds [G]? The paper of G sharpens the thesis developed in his book, *The question of animal awareness* (1976 *op. cit.*G, SR&B). On the one hand he calls for an end to the "behavioristic Zeitgeist" that inhibited inquiry into the mental life of organisms, and on the other he suggests that extension and refinement of two-way communication between ethologists and the animals they study offer the prospect of developing a truly experimental science of cognitive ethology. I am in considerable agreement with G on both of these points. If anything, I think he understates the case against behaviorism; I have in mind the restrictive effects of removing hypothetical, causally effective constructs from the tools of scientific analysis and the practical difficulties in ascertaining such things as "behavioral repertoires," "effective reinforcers," or "histories of reinforcement." Many psychologists simply do not see the point of giving up useful explanatory concepts, however mentalistic, for the sake of prescriptive scientific ideology. As P&W – certainly no strangers to behaviorism – remark in their paper, how could they avoid mentalistic notions in their explanation of Sarah's behavior?

One thing I missed in G's paper is some recognition that behaviorism did not in fact completely snuff out all research into the mental life of animals. For example, I see no mention of E. C. Tolman's (1932) purposive behaviorism, with its suspiciously mentalistic notions of such things as cognitive map and hypotheses. It is my impression that Tolman's work not only gave a certain intellectual respectability to the rise of behaviorism but also today serves as an important link between pre-behaviorist psychology, contemporary cognitive psychology, and G's cognitive ethology. Elsewhere – since behaviorism was primarily an American affliction – comparative psychologists and ethologists continued to seek empirical answers to the traditional questions about the minds of organisms (e.g., Katz, 1939). Contemporary Soviet studies on the orienting response also seem to be directed at similar matters. It is just possible that G gives behaviorism more discredit than it deserves.

The most important features of G's paper are his hortatory efforts to get psychologists and ethologists to look at the behavior of animals with an open mind and to stimulate research that might tap whatever mental life exists. In addition to what G has to say on the importance of two-way communication between experimenter and animal, it may be useful not to overlook a cognitive interpretation (Tolman, 1932; Dulaney, 1968) of the traditional instrumental/operant conditioning paradigm as a communication paradigm. As Tolman pointed out long ago, the reinforcer, for example, food pellet, has both affective and informational elements to it that are typically confounded in the behaviorist account.

Probably what excites G and others most about the recent developments with Washoe, Sarah, and Lana is the possibility that much more sophisticated quasilinguistic human animal communication is just around the corner. One might imagine, although G was not so bold as to suggest it, that at some point we shall have direct Cartesian evidence that certain animals have a mental life analogous to ours. One might even fantasize that the structure of their quasi-language should reflect the structure of their quasithoughts, much as it is sometimes supposed that the structure of human thought is reflected in the structure of human speech.

At the present time, however, there is very little reason to be overly concerned about any of this. First of all, there are serious unanswered questions about the accomplishments of Washoe et al. (See, for example, Sebeok, 1978, and SR&B, this issue). Even if we accept at face value the glosses given to the symbol-using apes by their interpreters, there seems to be a striking lack of cognitive or intentional verbs such as *want, think,* or *decide.* This may be only a temporary vocabulary gap; on the other hand, it may prove extremely important in the search for nonhuman minds. For it is just these verbs and their sentential complements (e.g., *think that such and such*) that are the foundations of the traditional linguistic analysis of other minds (e.g., Margolis, 1978). Moreover, it is just these complex constructions that inevitably mark the beginnings of syntactic speech in children between two and three years (Limber, 1973).

I surely do not want to suggest on the basis of language alone that apes do not want, decide, or think. For example, it is quite plausible that when Washoe signs "Gimme banana" she actually wants a banana. At a pragmatic (but not syntactic or semantic) level her signs may be functionally equivalent to the eighteen-month-old child's "wa nana" or the three-year-old's "I want a banana." I do, however, think these linguistic factors raise the possibility that apes may not have conscious access to their mental life in the same way that children do. Similar notions have been expressed quite independently of any specific linguistic considerations. Katz (1939, p. 253) for example, says "Man not only has consciousness, but he knows that he has it." While Washoe may truly want a banana, she may not know it!

One should be careful about jumping to conclusions about the underlying basis for any putative consciousness level differences among primates. While these may exist independently of language as intrinsic cognitive differences, it may also turn out that language itself is in some way causally implicated in these differences. If this should be the case, training an ape in quasilinguistic skills may do more than let us study its mind as G suggests; it may in a sense create that mind.[2]

Do behaviorists have minds [P&W]? I have maintained over the past few years to my students that probably the most significant effect of the various efforts to train quasilinguistic apes may be on the thinking of their behaviorist trainers. P&W bear me out in an interesting paper that reports not only on an important research program into the mind of Sarah but also provides an unusually reflective account of how the authors, on pragmatic grounds, were driven to formulate a primate cognitive psychology. The paper should be required reading for anyone interested in the traditional conflict between behaviorism and mentalism.

I have relatively little to say about the paper itself; the videotape paradigm seems most promising and I can only agree with the authors' conclusions about the incompleteness of behavioristic accounts of behavior. Nonbehaviorists long ago recognized the difficulty of fully characterising even the simplest behavior without some intrinsically mentalistic concepts like purpose, want, or attention. P&W suggest that the ape is not intelligent enough to be a behaviorist; in a sense, neither is *Homo sapiens.* Behavior even in the simplest situations is a function of the organism's *interpretation* of stimulus conditions. Only with a model of the organism's mind can we generate predictions of its behavior in novel situations. Only through our theories of others can we overcome the egocentricism of childhood and positivism.

I do have various doubts about the P&W program for examining "epistemic states" in the chimpanzee but at this point in time it seems proper to praise appropriate behavior rather than to criticize it. P&W, I suspect, will have their hands full dealing with their colleagues and the ghost of Lloyd Morgan.

Some implications of symbol use by chimpanzees [SR&B]. The paper by SR&B takes the trained apes and their acquired symbolic skills far beyond moot and irrelevant debates about whether apes can learn language. As I have remarked elsewhere, the promising aspect of this research – the effects of symbol availability on animal behavior – have not yet been realized (Limber, 1977 *op. cit.* SR&B). Now SR&B have reported what may be the first account of the utilization of arbitrary symbol systems in animal behavior. They have demonstrated that chimpanzees trained in a suitable symbol system can indeed use that system cooperatively to achieve ends well beyond their means without it. I found the SR&B paper thought-provoking and will try, with some difficulty, to limit myself to brief comments on two interrelated issues: one concerns the functions of human language, the other its origins.

SR&B, like most others who deal with animals and many who do not, take communication to be the "true adaptive function" of language, "enabling man to transmit specific information in abstract, context-free form." Now it would be absurd to deny the importance of the communicative function of language; yet there is another function traditionally ascribed to human language, typically by logicians, philosophers, and cognitively oriented psychologists. This function concerns mental representation and thought itself. Without going into details here, I find it premature, to say the least, to suppose that the communicative advantages far outweighed any representational advantages in the evolution of human language. It seems, for example, that the representational capacity of English far outstrips its use in everyday interpersonal communication (cf. Limber, 1976). What is exciting about the SR&B studies is that for the first time we can begin to think about experimentally studying the effects of mental representations themselves on animal behavior. How would variation in the symbol system affect cooperation in the SR&B paradigm? SR&B's discussion of functional and nonfunctional naming capacities in chimpanzees immediately suggests intriguing experiments in the realm of "functional fixedness" (Duncker, 1945). Numerous possibilities for investigating the recalcitrant problems of linguistic relativity and determinism suggest themselves. Many of the classic paradigms of experimental psychology, for example, the delayed reaction experiment, double alternation problems, associative cue effects in problem solving, might be adapted to examine the effects of arbitrary symbol systems on behavior.

To sum up this point, the mental representational capacity inherent in human language seems at least as much of an adaptive advantage as does the communicative advantage. The SR&B paradigm provides the potential for experimental study of adaptive advantages that accrue to organisms having symbol systems of varying complexity.

It is not impossible that a focus on the representational aspects of language will also give us a chance to break into the notorious problems of language origin. (Even formulating the issue adequately is difficult [cf. Kenny, 1973]: among the traditional answers to this question at least Süssmilch's "divine origin" theory lays the cards on the table!) There seem to be two fundamental obstacles to solving the problem. One is to understand how the rule-governed, arbitrary symbol system that is human language came to exist among a *population* with no previous system; another is to enumerate whatever conditions – biological, cognitive, social, environmental – support human language, yet might have evolved in its absence. If, however, we reasonably suppose adaptive advantages came to those organisms with increasingly complex intrapersonal representational systems, the fabled "private language" or "language of thought," then we can postpone, for a while, a consideration of the really tough problem of the externalization of that system into the social phenomenon of language. On this problem it may be that the SR&B and P&W papers supply some small pieces of the puzzle. It appears that SR&B's chimpanzees, once given their symbol system, had little difficulty using it; there seemed to be a preexisting emphatic relationship, initially developed between experimenter and animal, then readily generalized to other chimps. This suggests exactly what P&W clearly demonstrate, namely that chimpanzees have quite elaborate theories of others. Now the adaptive advantages of such theories seem obvious; they enable an organism to understand the behavior of others, more importantly they probably underlie the enormously advantageous process of observational learning. Learning from another surely requires an elaborate conception of another in relation to one's self. Observational learning, which perhaps evolved in connection with mechanisms of socialization (see also Beck, 1974 *op. cit.* SR&B), is important in connection with language origins in that its processes of rule induction and rule following resemble those processes required in the acquisition and use of language. In contrast to natural laws causally governing behavior, rules can be violated, modified, and become objects of awareness. For all I know, one of our clever ancestors, whose population had independently acquired a certain representational capacity, communication skills at an animal level, and empathic learning ability, just invented language serendipitously and it then spread like wildfire. Naturally the invention need not have been structurally like ours; considerable subsequent cultural evolution may have taken place.

NOTES

1. We cannot be entirely certain of this; he might just as well have gone off to investigate the language of the Yeti.

2. I have hinted at some of these issues (1977 *op. cit.* SR&B) and discussed them at greater length in my monograph in preparation on language among primates. Everyone should also read Vygotsky (1962, chap. 4).

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Speculations on the adaptive significance of cognition and consciousness in nonhuman species. It would appear that the social and biological sciences, and in particular psychology, are once again concerned with the mind/body problem (e.g., Boring, 1957). The issues are not as philosophical as they were near the turn of the century, but the questions being addressed are similar in intent. As before, the most salient inquiry is whether humans are unique among the numerous species of animals in possessing self-knowledge, in being aware of their own existence and in using such detachment to some survival advantage. Although the gist of the answer is less decidedly a no than it was some sixty years ago, the discourse still has the flavor of human centricity more than of science. Whatever the reasons – for example, more positive data, a concern for the welfare of animals, or a need for people to feel a part of nature – nonhuman animals (particularly the chimpanzee) are being championed as possessing cognition (as well they might) and, with less certainty, as being part of a continuum in this regard which culminates in *Homo sapiens.* As with any obvious change in scientific dictum, there comes an overemphasis of the new position, with straw arguments (or dated information) put forth so that they may be struck down by the weight of "new evidence." For example, such a tactic was being employed when G stated that "The standard response of a behaviorist is to insist that only external contingencies of reinforcement are of any interest, at least to him."

However, it is not so much what is being said in these exchanges that is disturbing, as what has been omitted, both in the debates of earlier times (out of possibly a lack of data) and in the more recent discussions for which there is no ready excuse. No reference has been made as to the adaptive significance of cognition and consciousness in nonhuman species. The focus has been either to show that certain animals to have mental processes akin to man's (e.g., P&W) or to use animals as models (e.g., SR&B) whereby some understanding of human communicative mechanisms (essentially verbal language) may be derived. Little if any effort has been directed toward seeking answers to questions as to why and how natural selection may have operated to predispose certain species to "think" and apparently not others, or as to what significance such capabilities may have in the perpetuation of those animals. As in the quest for "general laws of learning," the hunt has begun for "general laws of mental processes." While motivation of this sort may be admirable and may, in time, even lead to a better understanding of brain circuitry, the importance of biological evolution is once again largely ignored.

What an animal is, or if it is capable of cognition and consciousness, is a function of its ancestry and the ecological selective pressures of the past that have shaped its genotype and influenced its phenotype. The behavior or physiology of an animal is independent neither of its proximal experiences nor its distant genetic history. In retrospect, therefore, it is not surprising that chimpanzees are unable to acquire most verbal symbols (Hayes and Hayes, 1954 *op. cit.* SR&B), given their particular vocal anatomy, while ASL signing (Gardner and Gardner, 1969 *op. cit.* SR&B) has met with somewhat greater success. Anthropoids such as chimpanzees have an evolved repertoire of gestures and grimaces that could preadapt them to the learning and performance of other signals involving similar musculature and memory storage. In the same view, while SR&B's criticism of Premack's employment of plastic chips (as being in essence artificial symbolic instruments) seems reasonable, SR&B's research results might have had a less favorable outcome if the training tools utilized in the learned symbolic associations had not been "extensions of the hands." There is a body of literature from field studies indicating that foliage (sticks, leaves, grass) is used by chimpanzees to obtain items (mainly food) out of reach or not accessible directly (e.g., Goodall, 1972). While it is insightful of SR&B to have proposed (and to have found) that experience with the objects to be symbolized is important for the recall and consistently accurate deployment of the symbols, the field data had already suggested such an outcome. For example, whereas the chimpanzee may be genetically predisposed toward some tool use, the efficient stripping of a blade of grass for termite "fishing" is gradually learned by the young chimp through watching its mother or older siblings and imitating their behavior. In spite of the thoroughness of SR&B, the essential biological point has still been overlooked in their captive-animal research.

Perhaps one of the most succinct statements of the application of evolutionary theory to laboratory research has been made by Mayr (1974). He addressed what he called "opened" and "closed" genetic programs. He did not intend a dichotomy per se but only a relative emphasis as to whether certain behaviors are more or less genetically fixed. Notice that individual behavioral categories are at issue and not the animal species as a whole (although there is obviously some correlation between the two). He suggests that ecological selective pressures of the past (including those from other species, or even conspecifics in the case of social species) predetermine whether specific animals are likely to be highly invariant in some of their behaviors, or if survival instead requires a more open genetic program. For instance, the genetic strategy in song acquisition of parasitic birds (e.g., coo-coos) is likely to be different from nonparasitic avian species whose biological parents are those attending the brood. Further, in social species in which individual recognition is important, an open genetic strategy such as imprinting assures the bonding of offspring to their mothers and provides a template for species recognition in mating, whereas a closed program already genetically hard-wired might be more appropriate for nongregarious, solitary animals (as in the case of species recognition displays in certain lizards). In other words, in seeking a research direction to ascertain whether cognition and consciousness are attributes of some nonhuman species, the first question is whether such open genetic programs would be biologically advantageous for particular animals given their specific social organization and ecological niche.

Taking the position of Mayr, the rest of this commentary will be concerned largely with speculations as to the adaptive significance of mental processes and animal awareness, or lack thereof. For the purposes of this discussion, cognition will be regarded as necessary but not sufficient for consciousness. Moreover, the argument will not in the least be exhaustive in terms of those species which may eventually be shown to be cognizant of either their surroundings or of themselves as entities. These speculations will merely be a framework in which the study of such processes is likely to be biologically reasonable and scientifically fruitful. Also, in no way will it be suggested that the cognitive processes which nonhuman animals might possess are similar to, or simply less complex than, human "thinking." Each species is a biological unit, and whereas closely related species may have homologous processes, such generality is not the intent of this message – in fact, the opposite emphasis is desired. If a general principle is to be gleaned from this thesis, it is that animal species are molded by ecological determinants, and convergent behaviors may be frequent occurrences (e.g., Wilson, 1975).

The first of two hypotheses to be presented considers those social species in which individual conspecific recognition is essential for survival and suggests that in those animals which also have relatively altricial offspring, an extended infancy period, male/male competition for limited resources (e.g., habitats or females), and a gregarious social organization, cognitive processes are likely. In other words, it is proposed that species which may be capable of cognition are those that are long-living, ecological generalists (Wasser, 1978) and depend upon conspecifics in rearing offspring, seeking out food resources, or predator defense. Most social carnivores (e.g., felids and canids), some primates (e.g., langurs, baboons, and chimpanzees) and several marine mammals (e.g., porpoises and whales) meet many of the suggested criteria. These animals are likely candidates for possessing cognitive processes, since it would be either genetically difficult to have built in all of the ecological contingencies necessary for their perpetuation, or it would be essentially impossible to predict genetically the outcome of competition among individuals for positions of dominance within each species.

The second hypothesis proposes that in those cognizing species where social deceit (see, for example, Wallace, 1973; Dawkins, 1976) is adaptive (and is not already manifested in the physical phenotype), the capacity for consciousness, or, perhaps more importantly, its possible antithesis, subconsciousness, is probable. If in deceiving conspecifics one is more successful (in terms of inclusive fitness, see Hamilton, 1964) when self-deceived as to the actual motive for their deception, possessing a subconscious would indeed be adaptive. However, self-deception itself, without the intent to deceive conspecifics, may be an asset, in terms of reproductive success of the unaware organism, particularly if cognition were under a fairly strong selective pressure for some other function. For example, since parents and full-sibling offspring have only one half of their genes in common, if parent/offspring conflict (Trivers, 1974) were to become excessive, parents self-deceived into continuing to provide care for offspring, no matter what the conflicts, would reap statistically greater reproductive success (all else equal) than parents not self-deceived [cf. Rajecki, Lamb & Obmascher: "Toward a General Theory of Infantile Attachment" *BBS* 1(3) 1978]. Therefore, of those animals possessing cognition, species likely to have consciousness as well would be ones where the mating system was polygamous and the infancy period quite extensive. In other words, animals that compete for mates, where the male and female mating strategies are dissimilar and where the rearing of offspring may often have to be borne by one parent either by default (the death of the other parent) or by design (desertion by the other parent), are good candidates for possessing consciousness, and, by inference, subconsciousness. Until more is known of the intricacies in social organization of land mammals such as elephants or marine mammals such as porpoises, it may well be that the greater apes (apart from humans), and in particular the chimpanzee, may be the few extant species which are likely to possess an awareness of themselves as physical beings, apart from what they are feeling and doing at the moment.

In summary, a few specific comments are in order about each of the three papers which stimulated the present discussion. The study by SR&B (and in light of their succinct review of the status of the literature on symbolically mediated behavior in chimpanzees) may well constitute, apart from descriptive field data, "the first instance of symbolically mediated exchange of goods and information in a nonhuman species." However, it is surprising that they may have expected their subjects to perform better than they did with the keyboard turned off. Given that these researchers demonstrated the importance of experience in symbolically mediated behavior, the outcome of only some 10 percent correct with the keyboard off should have been predicted by them. If their subjects were to be given training in improvising, either by means of alternative symbolic media or iconic gestures, these authors might well find that chimpanzees have the capacity to learn to employ such detours and even make up some of their own.

The paper by P&W is an example itself in creative thinking. Without the experience themselves in trying to outsmart the chimpanzee, the ingenuity in research design shown in their article seems unlikely to have materialized. From the point of view of an animal behaviorist, however, how much more esthetic it would be if the test tapes involved conspecifics solving more "naturalistic" problems for the chimpanzee than the everyday trivia and frustrations of humans. If for no other reason than that it would appear to be less anthropomorphic, symbolic mediation demonstrated between two or more chimpanzees, rather than a chimpanzee's interpretation of staged human behavior via videotape, is somehow more desirable.

As for the paper by G on the prospects for a cognitive ethology, the objective is academically worthy, but the interpretation of the evidence he rules as supportive is largely intuitive. For example, many ethologists would undoubtedly not concur with his alternative explanation of the bee dance. Moreover, a lack of theoretical parsimony seems to permeate many of his arguments. His personal philosophy that "animal awareness, if it occurs, is also important for our definition and understanding of the human condition" is not scientifically debatable. It is, though, *not* the usual reason expounded by colleagues for studying animal behavior, including cognition. In this regard, it may be that G is less self-deceived as to what motivates behavioral scientists than they themselves are willing to admit, at least in a public forum.

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***Evaluation as an indicator of intention** [G].* I agree with so much of G's commonsensical analysis that I have only a little to add by way of qualification.

1. The case for "reopening these long neglected questions" seems to me unanswerable. If the behavioristic *Zeitgeist* has in some quarters been allowed to smother them as "unscientific," this only highlights the dangerous extent to which the Church Scientific has (in these quarters) allowed itself to become priest-ridden.

2. To characterise the approach sketched by G as "materialistic," however, would I think be misleading. His main point seems to be that questions about mental experience in animals can be scientifically pursued by methods which use essentially *mechanistic* concepts. I agree (MacKay, 1965, 1972). But as the literature of "Artificial Intelligence" bears witness, it is possible to take a mechanistic approach to cognitive processes even at a "software" level, to which the dogmas of metaphysical materialism are totally irrelevant; and I believe that the adoption of such a methodology is no more incompatible with theistic than with materialistic assumptions (MacKay, 1954, 1966, 1978).

3. This point is well illustrated in relation to what G claims that "thoroughgoing materialists" believe or accept as a working hypothesis. A "thoroughgoing theist" could be no less ready to assume that the events we call "mental

experiences" have direct correlates in physical brain processes, and to look at a mechanistic level for the difference between brain processes that are and are not correlates of conscious experience. I would argue, however, that the appropriate level at which to look for such differences is not that of brain physiology as such, but rather that of the *information-flow-structure* sustained in and by the physiological activity (McKay, 1966, 1978) [cf. Puccetti & Dykes: "Sensory Cortex and the Mind-Brain Problem" *BBS* 1 (3) 1978].

This choice of starting level can crucially affect the kind of hypothesis that suggests itself for scientific exploration. Those who start with the mass of simultaneously ongoing physiological processes tend to speculate whether it could be the anatomical location, or the chemical constituents, or perhaps the pattern of firing, that offer certain material processes the privilege of "generating mental experiences." Suppose, however, that we start at the level of our conscious experience itself, and then ask what *programmatic* correlates we might expect at the system-engineering level of our CNS. The whole enterprise is now a different one, and moreover one to which the details of our conscious experience can be expected to make scientifically useful contributions as we discover the relevant information-engineering questions to ask of it.

To reuse a well-worn analogy (MacKay, 1965), I am suggesting that looking for the physiological correlates of conscious experience is rather like looking for the electronic correlates of some abstract mathematical property of an equation being solved on a computer. To start at the level of the transistors would be scientifically inept, not because *in principle* it could never succeed, but because methodologically it would be a bad gamble. Only by starting at the mathematical end, and exploring hypotheses as to the programming structure and the encoding scheme, could we have a reasonable hope of finding what distinguishes the requisite physical correlates from all the other physical activities in the mechanism [cf. Haugeland: "The Plausibility of Cognitivism" *BBS* 1 (2) 1978].

4. I agree with G that we should try to design tests for mental experience in lower animals by analogy with those found appropriate in our own case. Although it would be nice to use the growing communicative repertoire of apes, however, I do not see this as an essential experimental tool. I suggest that the key feature of conscious mentality in ourselves is not communication, or activity in view of ends, or even evaluation of situations, but rather *evaluation of priorities,* or if you like, *self-evaluation* on the part of the evaluative system. This is what makes possible a rational (as distinct from an irrational or an unconscious) change of our priorities.

Unless there were reason to hold *a priori* that this process has no analogue within the information systems of subhuman species, it would seem unwarranted to deny them some degree of mental experience. The hypothesis I would favour is that what distinguishes the human brain is its capacity for forming and manipulating *representations of representations.* This does not exclude the possibility that nonhuman animals might "know what they are doing," but does set presumptive limits to the degree of insight they might have into what they are doing – the level of abstraction at which they could *think about themselves* as agents and above all as evaluators.

5. In these terms, the key test of "intention to communicate" would be a variational one (MacKay, 1972): In what way, and to what extent, would the sender's internal *evaluative* indices be disturbed by evidence that his signals were *not* having the "intended" effect on the recipient? This again is essentially a mechanistic criterion, which in due course might even hope to have a direct physiological application.

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Mentalism and methodology. *A rigorous, behavioristic cognitive ethology [G]?* G's article has many cogent arguments supporting the position that the influence of behaviorism has led to a rather myopic view of behavior by limiting its examination to those aspects that are not considered to be mentalistic. In our opinion G is certainly correct in this position. Naive behaviorism has severely limited the questions a behavioral scientists could examine with impunity. As a result, many important and interesting behavioral characteristics of nonhuman species have traditionally been ignored. G is careful to note that when studying behaviors previously considered mentalistic one should be very careful to use proper methodology and controls. In other words, although behaviorism has inhibited scientific investigation in the past, it has at the same time developed a rigorous methodology that is invaluable to worthwhile research. This methodology should never be comprised, otherwise the research will have no more scientific value than the anecdotal method as used by Romanes, which was the stimulus against which behaviorism reacted.

Would you mind a theory [P&W]? P&W's article can be viewed as an attempt to follow G's suggestions. It is a refreshing reminder of the early Köhler experiments on the mentality of apes and it is certainly a welcome return to an area of research that did not gain the acceptance or establish the scientific traditions that other behavioral approaches did. P&W are certainly examining interesting questions with regard to the chimpanzee mind. Unfortunately they seem to have been carried away by the exciting and interesting prospect of exploring heretofore taboo topics of theory of mind (e.g., intentionality and empathy), while forgetting the cautions of G and the valuable legacy of behaviorism in terms of providing well-controlled experiments using proper methodology. As a result, P&W's interpretation of the results of their experiment is questionable because of certain methodological oversights.

For example, P&W presented Sarah with four different problems six times each. In the first set of four problems Sarah missed one and got three correct. The expected chance level of responding would be two correct and two incorrect. P&W collapse across the six presentations, presumably assuming that they were independent presentations, when in fact they were not. Only the first set of four problems would be independent; after this Sarah is now informed and could simply respond by choosing the same alternatives she chose on the first set (which is exactly what she did on the second and third presentations). SR&B indicate that Sarah could respond correctly after one presentation on ten separate discrimination problems, supporting the contention that after the first trial the choices are not independent. As a result, the probability value for Sarah's performance is .25 rather than the rather impressive $p < .001$ as reported by P&W.

Another problem is that P&W did not run the proper control conditions. Sarah should have been pretested with the two-picture choice problem prior to testing with the videotape. This would have established what pre-experiment bias these pictures had for Sarah. For example, a picture of a key might have intrinsic value for a caged chimpanzee. Had this been done, these choices could have then been compared to choices Sarah made under testing conditions.

A final point concerns P&W's method of controlling for social cues. The method was for the trainer to take a box holding the two pictures (counterbalanced for position) into Sarah's cage, set it down, and leave. Sarah then opened the box and made her choice. P&W do not state who placed the two pictures in the box prior to taking it into Sarah's cage; nor is this stated in P&W's *Science* article (1978 *op. cit.*) where the experiment is also reported. If the trainer who carried the box in was also the person who placed the pictures in the box or even knew its contents, then this procedure would not qualify as a control for social cueing. For example, it would be easy for such a person to cue Sarah inadvertently as to which side of the box held the correct picture. Likewise, it would not be difficult for Sarah to remember this until she opened the box.

It is unfortunate that these errors and ambiguities occurred in such a pioneering experiment. It is likely that tough-minded behaviorists will view this as nothing more than a continuation of the sloppy mentalistic experiments of the past, which is a justified conclusion because an exciting research topic cannot replace sound methodology.

Observations, anecdotes, and monologues with a computer [SR&B]. SR&B present an interesting analysis of how the meaning of a word may be acquired by a chimpanzee. The word "key" first represented food in an adjacent locked room; then, with the introduction of a new food site, it was applied (incorrectly) to this site, so that it was then relegated to applying only to the "food in the adjacent locked room" situation. The chimp did not use the word for key when a locked box was introduced, although he did use a key to try to open the box. After learning that the word "key" could be used to obtain a key for unlocking the box, the chimp successfully used the "key" word to represent a key in new test situations. Lana, a much more experienced chimp, did not go through these stages in learning what the word "key" meant, but was able to acquire immediately the appropriate meaning of the tools, although no definitive reasons for her ability are offered. Eventually, two chimps were able to learn the names of six objects and to request them through a keyboard button press or to deliver them to an experimenter when requested to do so.

Unfortunately, the main point of the SR&B report seems to be chimpanzee-to-chimpanzee communication. The experiment fails to demonstrate any such communication, except for one reported anecdotal episode. The experimental procedure consists of having the chimpanzee request a tool from the other chimpanzee, the requester then obtaining the food and sharing it with the tool-provider. That a chimp would willingly share any amount of food with another chimp is surprising in itself, although not germane to the question at hand. The "control" to show that the two chimpanzees were actually communicating with each other was to turn off the keyboard. This seems much like teaching a person to drive a car and then removing the steering wheel. It tells us absolutely nothing about chimp-to-chimp communication. To find out the degree to which the chimps were interdependent on *each other* (as opposed to communicating with a machine), what should have been done was to have an automatic dispenser send the requested tool down a chute into the requester's grasp and to have the computer programmed to request tools from one of the chimps so that if the chimp put the appropriate tool through a hole in the wall, it would deliver some food to the chimp a little later. Would the performance accuracy be affected in any way? We doubt it. The communication between chimpanzees is limited to monologues, with one chimpanzee requesting and the other complying. One piece of evidence that the presence of another chimp is important (as opposed to a computer or a tool delivery mechanism) is an anecdote:

". . . on one trial Sherman requested *key* erroneously when he needed a wrench. He then *watched carefully* as Austin searched the tool kit. When Austin started to pick up the key, Sherman looked over his shoulder toward the keyboard, and when he *noticed* the word *key,* which he had left displayed on the projectors, he rushed back to the keyboard, depressed *wrench,* and tapped the projectors *to draw Austin's attention* to the new symbol he had just transmitted."

This richly-interpreted anecdote (italics added to emphasize the richness of the interpretation) is the best evidence SR&B have to indicate that the *chimps* are communicating with one another, as opposed to the computer. SR&B incorrectly state that such anecdotes are the only data "suggesting that Washoe and other signing apes are producing anything more than short-circuited iconic sequences."

The vitriolic attack by SR&B on Project Washoe might seem puzzling to the reader familiar with the published reports of the Gardners and Fouts, and seems rather difficult to explain purely in terms of objective science. It is ironic that SR&B present their most interesting result as an anecdote (see above). Apparently one investigator's "observation" turns into an anecdote when seen through another's eyes.

NOTE

1. Order of authorships was determined by the flip of a coin.

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Should mentalistic concepts be defended or assumed? Although we consider ourselves cognitivists of sorts, and although we would have been at least 75 percent favorable toward any one of the target articles taken separately, together they had a cumulative effect which has left us uneasy and wondering what is in store for animal psychology in the future. Indeed, the harder we have struggled with the authors' treatment of awareness, intentionality, language, symbolism, iconicity, self concept and animals' theories of mind, the more inclined we are to concur with Popper: "One should never quarrel about words, and never get involved in questions of terminology. One should always keep from discussing concepts. What we are really interested in, our real problems, are factual problems, problems of theories and their truth" (Popper, 1972, p. 310). Perhaps the difficulty is that all-or-none questions such as "Do animals have awareness?" or "language?" create qualitative if not artificial dichotomies of what are psychological continuities, and run the risk of obscuring as much as they clarify.

Are mentalistic concepts useful? Until the advent of behaviorism and ethology it was generally assumed that the study of behavior was of interest only insofar as it could be shown to shed some light upon mind. Today the situation is to some extent reversed. The prediction and control of behavior is one of our major goals (for some it is the sole goal), and there is a tendency (illustrated in all three articles) for people interested in the functioning of minds either to try to make their questions sound as behavioristic as possible or to try to set them up in direct opposition to behaviorism. Is it really necessary, however, to bow to or to flail at this demon? Why not simply say that what constitutes a useful, interesting, or fruitful question depends upon one's point of view? From our point of view, mentalistic questions or concepts need no more defense than behavioristic questions need defense for a behaviorist. As a matter of fact, even if it should prove possible to predict and control 100 percent of the variance of behavior as a nonmentalist defines it, this would not necessarily tell us anything we care to know. Consider, for example, a man who is falling from a tower. Where is he going? From the point of view of physics, a Newtonian account would be just as accurate and complete for the man as for a stone (assuming, of course that the man has no parachute); but it would say nothing about whether or not the man was going to his death (a question that we would not even pose in the case of the stone and that could not be answered by a Newtonian physics), let alone whether he was aware of this fact but perceived it as going to meet his maker and to a new life (questions that we would not ask about most animals or even about an anesthetized human).

The most important function of theories in our present state of ethology and psychology is not that they summarize knowledge already obtained but that they serve a guiding function, affecting the sorts of new hypotheses we propose and the sorts of new data we collect. In this respect cognitive approaches need no defense – or at least, no more than any other approach.

Defining mentalistic concepts. The idea that it is possible to identify mind (or awareness, or what have you) is not substantially different from a naturalist's belief that it is possible to identify "life," a behaviorist's belief that it is possible to identify "real behavior" as opposed to simple motion, or a linguist's belief that it is possible to identify "language." None of these terms refer to physical objects as such; they all refer to processes that we attribute to some objects or beings and not to others. While G initially suggests that it does not seem necessary to be overly concerned with formalistic definitions (and we agree), he then seems to encourage this very formalism. Ostensive definitions probably work just as well, but it is important to remember that at least two such definitions are possible and each alone has serious limitations. On the one hand, following the logic of Turing (1950) one can simply produce or point to one or more specimens (A) to which most scientists would unquestionably attribute mind, intent, or what-have-you. Other specimens (B) about which we are unsure are then assessed against this norm. To the extent that one would find it difficult or impossible to discriminate between the performances of competencies of B and A, we would accept the null hypothesis of no difference and (by definition) attribute to B whatever specific process in question we attributed to A. Today, of course, the accepted norm (A) is a "normal adult human being" (some would even say a linguist), and B is anything else, for example, a computer, a one-month-old child, a chimpanzee. As we see it, all students of "chimpanzee language" operate on this Turing logic (Menzel, 1978). The target articles by SR&B and by P&W are cases in point.

On the other hand, following the inverse of Turing's logic, we might produce or point to a normative specimen (A) to whom we would definitely *not* attribute the internal process in question. A stone or a gas molecule that is moving purely in accordance with the known "laws" of physics, chemistry, or "chance" is a simple case in point; a computer simulation is a more complicated case. The performances and competencies of an unknown specimen (B) may then be compared against those of A, and if we cannot discriminate between them, we assert that there is no basis for rejecting the null hypothesis of "no difference, therefore no mind (etc.)." Williams (1966) says we know we are dealing with life when, in order to achieve an adequate explanation of what we see, we are forced to invoke principles above and beyond those of physics and chemistry, which apply to all objects in general. To expand on this, we know that we are dealing with *sentient* life when we must invoke still more specialized principles, above and beyond those that apply to all objects in general, and to all living things in general, including plants and . . . (here already the list grows problematical).

Quite obviously, whether we use the performances of adult humans or of rocks as our definitional criteria for abstract concepts rests on conventions which are apt to change from time to time and on which there is less than 100 percent agreement at any one time. These conventions (including what performances on the part of A and B are critical to an issue) are philosophical and sociological as well as scientific. Which of these criteria is most fruitful may depend on the issue being addressed; however, it is safe to say that keeping both in mind is apt to be less limiting than concentrating on either one alone.

Can one animal take into account the behavioral and cognitive capacities of another? It is hard for us to imagine how any animal could survive otherwise. We therefore concur with P&W that this question should be answered in the affirmative. We have developed our own argument in some detail elsewhere (Menzel and Johnson, 1976 *op. cit.* G). It should, however, be noted that even nonmentalistic accounts of evolutionary epistemology would have no quarrel with such a position. In the evolutionary sense, animals, and even plants, are problem solvers. The conjectures and tentative solutions that they incorporate into their anatomy and their behavior are (if one chooses to use such terminology) biological analogues of theories, and vice versa (Campbell, 1974; Lorenz, 1977; Popper, 1972). Just where one is justified in attributing to animals phenomenological understanding of others, or theories of mind in the literal, anthropomorphic sense, is an interesting and open question; and there are not many animal researchers who have addressed themselves to this question as directly and explicitly as have P&W. However, we would suggest that the important issue is not *whether* animals have "theories of mind" but what these "theories" are, how they came to be "formulated," and so on. We would emphasize that certain concepts, seen in the overall context of the animal's natural behavior, including its economic situation and biological and social history, do not seem so strange or mystical as they might when proposed in the abstract.

As another example, take the question: Can animals withhold information or lie? If by "withholding information" one means inhibiting one's response under circumstances in which another animal might profit more than oneself from this response (or in which one might stand to lose a good bit), then withholding of information is common, if not ubiquitous (see Dawkins and Krebs, 1978). Why else, for example, do many animals "freeze" at the first sign of a predator? Here, as in many other "mentalistic" issues, the presumed *special* status of information withholding – or prevarication, for that matter – tends to obscure the similarity between this problem and the general one of assessing perceptual and conceptual categories and capacities of animals, or indeed of humans.

Is human language the archetypic communication event or the archetypic cognitive event? All three of the target articles seem to imply that human language is the archetypic communicative or cognitive phenomenon. In our opinion, such a position generates a number of pseudo-questions that deflect research away from more interesting and fundamental questions. One of these pseudo-issues is "What is language?" Quite possibly because they are so concerned with this issue, students of "chimpanzee language" (see also the symposium on this topic in Harnad, Steklis and Lancaster, 1976 *op. cit.* G) seem to be the most severe critics of each other's work. SR&B in particular seem to require far deeper understanding on the part of other investigators' chimpanzees before they use the terms "communication," "language," or "intent" than could easily be demonstrated for most humans.

Adequately operationalized concepts are, of course, critical for studying psychological processes. However, one can operationalize away the main point of an idea. For example, SR&B emphasized the importance of finding context-free demonstrations that the chimp has knowledge about arbitrary relationships between signs and the things they stand for, which sounds to us like S-R psychology revisited. They also seem to imply that reliance upon recall cues make a performance less like language. This is part of the source of their criticism of American Sign Language as a vehicle for studying language in the chimpanzee – the signs resemble the things they stand for (iconicity). It is as if they require the animal to talk out of context and hopefully without reference to memory before pronouncing the act "language." Suppose an Englishman visited France and was able to communicate by constantly referring to an English-French dictionary (or a vocabulary list). Or suppose our traveler has used some mnemonics to learn words and could remember them only when external cues reminded him of his mnemonics. We would probably say that the person was not very facile, but not that he wasn't engaging in language. The issue of "iconicity" is a particularly big Pandora's box because it assumes, with no evidence, that we know the basis on which animals judge similarity (and how judgments of similarity are made is probably a more fundamental question than whether a particular communication meets a formalistic definition of language).

Part of the impetus to focus on language probably comes from the prevalent view that linguistics has rescued people (and might similarly rescue animals) from behaviorism. Thus there is a tendency to pit ideas like *rules*, which have figured saliently in linguistics, against ideas like generalization, which have figured saliently in associative and behavioristic theories. But these are not mutually exclusive ideas. A number of people working within associative frameworks would be quite comfortable with the notion that rules are applied to information that builds up according to associative principles. With respect to P&W's discussion, we would also suggest that the idea of rules is really no more specific than the idea of generalization. Finally, recent work on natural categories (e.g., Rosch, 1975) highlights the fact that many of our concepts do not have a neat rule structure. The idea, therefore, that understanding and communication are governed by rules may not *by itself* take us much further than the idea that they are governed by generalization from past experience.

In a similar spirit, the SR&B paper seems to imply that appropriate *use* of signs or symbols is somehow not a good criterion for inferring what they call "semantic comprehension." From a functionalist point of view, it is hard to imagine a better criterion. However, it is generally true that the adoption of linguistic-style analyses of meaning for psychological purposes has tended to lead toward an overly "taxonomic" view of meaning. For example, suppose a man is shown an apricot, some wheat, and an apple, and asked to choose the odd item. If he grew up on a farm, he might say apple is odd because apricots and wheat both ripen in the spring and apples in the fall. We wouldn't want to say he responded nonsemantically because he didn't respond according to conventional taxonomic categories of fruits and grains. It would seem to be reasonable as a general approach to simply *assume* that animals are responding "semantically," and then to try to find out the basis of their responses. That is, how do they organize the world? An argument can be made that all signs are symbolic (even the dinner bell) and the interesting questions are: what do such signs signify to various individuals or species? how did they come to signify what they do? what are the implications of a given cognitive structure? and so forth.

Would any new ethical questions arise if animals could be shown to "have language" or to be "aware?" We see no reason why ethical considerations should wait upon, or be subordinated to, "openminded" scientific research that is conducted in strict accordance with Lloyd Morgan's canon. Indeed, in our treatment of animals as well as people we see every reason to adopt the opposite of Morgan's canon: "Assume until proved otherwise that others are just as intelligent, complicated, and so on, in their own way as you are in yours. And be very skeptical of your own motives and intellect if you think you have proved otherwise." As a matter of fact, even in some areas of scientific research this constitutes the more appropriate "null hypothesis." The study of "animal language," after the fashion of the target articles, may have, if anything, tended to increase rather than decrease expectations of human chauvinism and presumed "biological superiority," especially in the popular press, where it is more and more often suggested that chimpanzees, gorillas, and perhaps dolphins may deserve special consideration based on the outcome of research projects demonstrating their similarity to humans. The poet Robert Frost, noticing the apparent fear reactions of a paper mite, spared its life and wrote, "I have a mind myself and recognize / Mind when I meet with it in any guise." While we would question the usefulness of his statement if it were given as the principal *conclusion* of a scientific paper, it does not seem inappropriate as a *starting point* for ethics, or for a cognitively oriented ethology or animal psychology.

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by **Julius M. Moravcsik**

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Can the concept of cognition bear the weight psychologists place on it? *[G, P& W]* Today there is a lot of interest in experiments testing the so-called cognitive skills of animals. These experiments typically involve planning, tool using, complex expectations, and responses mediated by the use of what are allegedly symbol systems. The interest, however, centers often not on the intrinsic value of the work, but on its alleged connection with sweeping questions concerning the uniqueness of the human species, ethical issues, and the evolutionary hypothesis. The articles by P&W and G give us an opportunity to place the work on animal cognition into proper perspective.

P&W's results and "theories of mind." The experiments described in this paper show that a chimpanzee can find solutions to someone else's problems, and she can ascribe good or bad possible alternatives in a hypothetical case to an agent, depending on whether she likes the agent or not. P&W interpret the data as showing that the chimp ascribes mental states to herself and to others, and thus has a "theory of mind."

The difficulties lie not in the experiments, but in the interpretations. There are two distinct grounds for attributing mental states to ourselves. One of these is experience and introspection. The other is that such attributions might help to account for behaviour and capacity. P&W ignore this distinction, and concentrate only on the latter ground. But it remains to be shown that imputing mental states to others would be a "natural" way to account for behaviour even if we did not have the direct experiential evidence in our own case. Thus, from the mere fact that the chimp seems to attribute purpose to agents in certain situations we cannot conclude that in the chimp's case, too, this involves the attribution of mental states to others. P&W argue that behaviourism is an "unnatural" view for humans, requiring considerable sophistication. But this does not show that behaviouristic interpretations – especially of purpose – might not be more natural to the chimpanzee. It depends on whether the chimp has the same direct experiential base for concluding that it has mental states as we do; but that is precisely the issue in question. Mere behavioural evidence will not settle the matter.

A similar overinterpretation is to be found when P&W say that Sarah's choice "may be intended as an answer to any of three questions: What would the human actor do if in this situation? . . ." The crucial issue is, however, not whether we *may* interpret the response in this manner, but whether we *must* do so. A small child can exhibit the skill of knowing how another child would solve a jigsaw puzzle without necessarily having the capacity to process questions of the sort P&W pose here.

I also have doubts about the attempted behavioural contrast that is supposed to compare "guess" with "know," but lack of space prevents me from articulating the doubt. (Roughly, the point about whether someone knows or not is not simply whether he has the required evidence available but whether he uses it; and in P&W's proposed setting, nothing guarantees that.)

To sum up, my main question is why experiments that have obvious intrinsic merit need to be tied to such nebulous notions as "theory of mind" and "imputation of mental states."

Animal psychology and the "big questions" [G]. The article by G answers the question raised at the end of the previous section. For he claims that determining the nature of animal thinking will have crucial bearing on the following issues: (a) human uniqueness and our place in the universe; (b) whether general learning principles apply across human and animal learning situations; and (c) ethical issues concerning the treatment of animals. Furthermore, G sees as the critical issue whether or not "the differences between men and animals in this respect are qualitative and absolute. . . ."

I find the notion of "qualitative and absolute" unclear. Suppose that we find that in terms of formal complexity there is a striking difference between the grammars of natural human languages and animal communication systems. Suppose, furthermore, that in terms of logical complexity there is an equally striking difference in semantic expressive power. Would such differences count as "qualitative and absolute?" If not, why not? Why is it necessary to link the phrase "qualitative and absolute" to such hazy and ill-defined questions as: "can animals (or humans?) think?" or "can animals (or certain humans) use language?"

There is another problem with G's methodological assumptions. He writes as if the only choices were either behaviourism or the view that mental states are known directly through introspection. There is a third alternative, however, and it seems to me to be more plausible than either of the two that G considers. According to this view, some of our key cognitive processes, such as calculation or language processing, are not fully knowable directly, either via introspection or via behavioural evidence. Hence we can study them best indirectly, by concentrating on the properties of the abstract objects that these processes can deal with.

Returning to the "big questions," I find their link to work on animal thinking doubtful. It is one thing to say that we are interested in what are, for humans, species-invariant capacities, and another to lay such heavy stress on uniqueness. Why is uniqueness so important? I shall not value human freedom and dignity less if it turns out that we share of what we take to be unique features with dolphins, chimps, or beavers. Furthermore, if the differences turn out to be on the order of what I have sketched above, that will be quite sufficient to show that general principles of learning do not carry across species. Finally, my care and concern for animals in general, and endangered species in particular, or for matters of the ethics of animal experimentation, would not diminish if we found what G regards as "absolute and qualitative" differences. Philosophically, I would argue for a general respect for life in all of its manifestations. The expression of this respect will, obviously, depend on what we happen to know at any given time about different types of living creatures.

Conclusions. Cognition is a many-splendoured thing. It includes a variety of competences such as concept formation, reasoning, information processing, symbolic representation, and so forth. These capacities are not only conceptually distinct; it is an open question as to how tight the causal interrelations are. Today there is a lot of talk of cognition, cognitive science, and the like [see Haugeland: "The Plausibility of Cognitivism" *BBS* 1 (2) 1978]. If this means simply that the capacities listed above will be subjected to interdisciplinary study, then it is all to the good. But this must not mean that terms like "cognition," or "thinking" will be key terms in scientific theories; these terms are neither sharp enough nor tied to empirical evidence with sufficient clarity to bear that kind of a conceptual burden.

Work on animal psychology would probably benefit from being no longer tied to the "big issues" that G lists. Physics benefited from being no longer considered as having grave relevance to theological issues. Why should one not draw a lesson from the history of neighbouring sciences?

Finally, even if my advice were to be heeded, work like that of P&W would still be of interest to philosophers. For philosophers should ponder over the cases that P&W describe, and ask: "if these are not cases that we would regard as cases of thought or language processing, what is missing?" Thinking of this sort might help us eventually to substitute a series of clearer and more sharply defined notions in place of the current stock of concepts that haunt philosophy and animal psychology alike.

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What to look for in comparing species. Suppose that some sign-using chimpanzee of the twenty-first century, perhaps the result of special breeding and evidently the result of a very special education, receives her Ph.D. in psychology (comparative) with a thesis on twentieth-century speculation about the cognitive capacities of her species. What will she make of papers such as those of SR&B, P&W, and G? No doubt she will praise their intention; no doubt she will find them a little patronising. But what will she think of the attempt that these papers make to think through, in relation to the small amount of evidence we now possess, the extension of some of our most impressive, and, as we once thought, characteristic, qualities to other species?

Of course, to begin to tell the story this way is to beg the question. Our culture, our science, and the skills that allow us to manage them may be so peculiar to us that in order to have enough of them even to talk and interact socially in anything like a human manner an organism would have to *be* human. Evidently, to answer these questions we would have to understand basic essential facts about human cognition, language, and sociality. All the grand old questions lie in waiting along the way. It is tempting, then, to want to formulate clear definitions of those attributes of people whose extension to other species is conceivable and then with these definitions in mind to formulate a feasible research program towards answering some of the basic questions. In this regard the conjunction of the papers by SR&B, P&W, and G is extremely instructive, for G shows an acute form of the temptation I just mentioned, and SR&B provides some interesting reasons for resisting it, while P&W poses questions that might seem premature, were it not for SR&B's evident need for someone to formulate them.

Grammar versus intentions [SR&B]. Savage-Rumbaugh et al. are concerned that the impressive data on vocabulary size and appropriateness of use of this vocabulary, and the rather more ambiguous data on acquisition of syntax, found in the chimpanzee work of Gardner and Gardner, Premack, and others, does not show that chimpanzees are capable of assigning meaning to symbols in the way that people do. The concern is that an animal's use of a sign in the presence of, as a result of a desire for, or even because the animal is thinking of, an object or situation, does not establish that the animal is using it with any communicative intent. The animal may not intend that the signs be taken by an audience as grounds for believing that the animal intends to refer to the object or situation in question. (The exact formulation of this requirement is tricky; many variations on it are due to the philosopher Paul Grice and his pupils.) SR&B have therefore devised experiments in which the presence of such communicative intentions is the best explanation of the chimpanzees' sign-using behavior.

Note in this connection that the necessity of an independent confirmation of such intentions is in part due to the uncertainty of conjectures about the syntactic capacities of chimpanzees, for appropriate use of much of a language like English requires that such intentions be present; they are not a further addition to using the right sentence at the right time. For example, if one says "listen to me and try to do exactly what I say" appropriately, then one is addressing an audience – setting up a situation in which their response to what one says is what matters – and presupposing that the audience is ascribing an intention to communicate to one. But sentences like this are clearly far beyond the output of any present sign-using animal, and therefore the experiments of SR&B, and also P&W, are necessary.

If the possibility of an animal that can produce the output of a rich transformational grammar but not mean anything by what it produces is thus very unlikely (and the sketchy argument in the last paragraph could at most show that an animal is unlikely to produce *and use appropriately* a sufficiently rich output without having semantical intentions), several consequences of the need for separate verification of appropriate production and communicative intention remain striking. The most important, I think, is the possibility that the various syntactic, semantic, and social abilities that go into human use of language may have no inherent unity. SR&B suggest that this may in fact be the case, by showing that object-naming is considerably harder for their chimpanzees to learn when the objects are presented just as perceivable objects than it is when objects are presented as tools, as solutions to problems of manipulation. Certain functions that objects may serve seem easier for these two chimpanzees to name than the objects themselves. Is this part of a more general fact about chimpanzee cognition? Does the communicative competence that these animals exhibit depend on a connection with this particular range of tasks, manipulating objects to achieve definite rewards? More work is needed to answer these questions. The point that seems clearest is just how important and open a question is the one concerning the connections among the ability to manipulate objects, the ability to cooperate with others in manipulating objects, the ability to learn sign-object correlations, and the ability to recognize the intentions of others to have one cooperate with them. The openness of the question is obvious. Its importance derives from the possibility that the relations among these skills may not be the same in humans and in chimpanzees. If they are significantly different, then for all the similarities there may be in what we can do, there may be no single thing called "linguistic competence" that we share.

Does the human have a theory of mind [P&W]? To have a theory of mind is to impute mental states to oneself and others, according to P&W. I imagine that I qualify as having a theory of mind, according to this definition, but I am rather less sure what this theory is. When I try to state it I only come up with trivialities. These trivialities (and various profounder things the expression of which I usually botch) may, for all I know, be tied together in a comprehensive implicit theory. I expect they are, but it is possible also that all that social psychology will ever find in the likes of me is a scattered bundle of inclinations to say that this person and that is in this state or that. Given that this is probably false, and that different people with different socializations have probably imposed different organizing schemes on their inclinations to ascribe states of mind, we can ask what kind of processes organize these ascriptions and how specific to our species they are. I take it that this is the central problem of social psychology.

The experiments P&W describe provide what seems to me very strong evidence that the chimpanzee they were working with had some relatively complicated representations of the states of mind of particular humans. Their sophisticated nature is significant, for to ascribe, say, an intention to get at some bananas by means of a stick, is of an order of complication beyond, say, ascribing rage, and it is precisely these more complicated ascriptions that are needed for language use to be humanlike in the way SR&B stress. And ascriptions of at least this complexity are required in order to have a representation of even the simplest form of social rule.

It is not as clear, though, what the form of the ascriptions or imputations is, what the animal's relations to representations of states of mind are. Does Sarah represent the actor as looking for the right key *because* of the latter's fear of the approaching lion? Does she explain his behavior by reference to his state? If she takes an experimeter to be a malicious liar, is she taking him just as a negative force in her search for truth and reward, or is she representing him as acting as a result of an intention to produce false beliefs in her? I find that my uncertainty about what is supposed to be involved in the ascription of a state of mind to an agent makes it hard for me to decide to what extent the alternative explanations P&W suggest for the data presented in the earlier sections of the paper are real alternatives; and I am also uncertain as to quite what is being conjectured in the later sections with regard to Sarah's possible differentiation of guessing from knowing or of fools from liars. One reason why my lack of understanding on this point disturbs me is that the kinds of ascriptions that SR&B take to be essential to full linguistic competence require more than a simple assignment of an intention to a speaker, for in recognizing an intention to communicate one must take the speaker to believe that the sentence he produces will cause the audience to believe that he produced the sentence to make them agree to it. Here the use of beliefs and intentions in an implicit pattern of psychological explanation, the sort of pattern that holds together our scattered everyday ascriptions of states of mind, is as essential as the complex content of the states ascribed.

Universal structures [G]. Griffin's paper throws out possible explanations and possible experiments right and left. I won't discuss most of them, since my general sympathy should be clear. One very basic premise of his argument seems to me misguided, though. It is the idea that we should take the human commonsense categories of consciousness, language, and so on, and, after elaborating them somewhat into definitions, try to discover to what extent they are applicable to other species. These categories are elaborations by philosophers of the human theory of mind that P&W are concerned with. This naive theory tells us almost nothing about the underlying causes of consciousness or language, and it is in comparing these underlying causes that we are likely to find surprising resemblances and differences between species. Or, to put the point in the idiom of recent philosophers of science, an investigation of the *meaning* that mental concepts have in their present use is not going to tell one anything essential about them. Rather, one should try to see what kinds of thing these concepts *refer* to. And in this enterprise, which begins just with a description of the cases to which the concept applies and then asks the factual question of what these cases have in common, the essential nature of commonsense concepts is likely to be revealed in their reduction to the scientific concepts that causally underly the phenomena they explain.

What I would suggest then is that what psychology should do, and what SR&B and P&W are actually beginning to do, is to see what similarities of *behavior* between humans and animals can be discovered or induced, and then to investigate the similarities and differences among the processes and skills that underly similar behavioral capacities. When the facts are in, we shall no doubt seem naive – and the chimpanzee reviewer of my first paragraph will no doubt note this as a piece of typically human arrogance – in thinking that the ways we think and speak and conceive of ourselves are the ways all thinking, speaking, social creatures must.

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Stop already, my mind is made up *[P&W].* P&W are forced into doing a large set of experiments in order to prove what they must already know. How could they not be right? I do not believe it possible for any intelligent organism (human or nonhuman, animate or artificial) to learn complex tasks and problem-solving activities without imparting cause, purpose, and intention to the participants in the problem. But then, who am I to speak? I am a confirmed mentalist, so I am fundamentally biased in favor of the position of P&W.

Why do P&W even bother? As they know only too well, a confirmed behaviorist is confirmed. Nothing seems to change behaviorists' mental behaviors, and even if they change their minds, by their own definition that is quite irrelevant. The true behaviorist does not believe in the necessity of mind for anything, not even for humans, not even for themselves. P&W do not have a chance.

Nonetheless, what P&W have done is important. I see emerging a comparative theory of intelligence. Such comparative studies have long been talked about, but all the existing studies in the literature are, to my prejudiced mind, unusually uninformative. I simply do not know what to make of the comparative study of animals solving various mazes. And the normal experimental tasks in the literature are singularly uninteresting. P&W are developing techniques that might let us get at a taxonomy of intelligent performance. I would love to see similar studies performed on a range of animals, from the human (especially the human child), through the gorilla, chimpanzee, and other primates, and then to the general intelligent mammals.

Consider the dog. The household dog, jointly evolved with man for human domestication, probably exhibits some subset of the characteristics talked about by P&W. Now, the dogma about dogs would have them incapable of watching television and getting much out of it. But this would not stop the clever experimenter, for I think the type of experimental condition devised by P&W could still be applied to dogs, with perhaps some clever variations on the method of presentation. And from there we can go to other animals. We might finally get some experimental statement of the ranges of intellectual capacity in animals. Today we simply do not have that. We know whether animals can solve various mazes, we know when animals fail at double-alternation, or at various delayed tasks. But these do not get at the fundamental basis of mind: the ability to have different levels of consciousness and different levels of knowledge about the world.

In doing their experiment, although P&W should act as competent experimental psychologists, I urge them to abandon the attempt to convince the behaviorists. The behaviorists cannot be convinced. Forget them. Any attempt to draw up a series of experiments that logically can hold no other interpretation but that of mentalism is doomed to failure. Worse, it is likely to distort the very experiments themselves, forcing so many controls and so many contrived situations, that the entire phenomenon may itself be destroyed. I am quite willing to agree with my behaviorist colleagues that I cannot prove that my thought patterns mean anything, or that my distinction between conscious and subconscious states can really be demonstrated. I cannot prove this: I have tried and tried. I reside in a department with simultaneously one of the best cognitive psychology groups in the country and one of the best operant learning groups. The contrasting views held by our two groups provide an interesting study in the sociology of science and in the manner in which fundamental positions are formed and held. Please, P&W, don't waste your time trying to convince those whose entire livelihood and entire reputation resides in intelligent interpretation of data and of intelligent discovery of possible artifacts so as to avoid having to become convinced.

I am a student of human intelligence and of artifical intelligence (the latter in part deriving from the belief that I can understand intelligent behavior better if I can learn how to create intelligent machines). In order to engage in dialogue, one finds it absolutely essential to have a theory of the mind of other participants. Real language simply does not describe all the thoughts and concepts one wishes to communicate with another. Rather, we rely heavily upon the large, shared knowledge base of the other communicant. It would be impossible to understand stories if we could not impart intent, motive, and so forth. (Can a chimp understand a story? I bet he can. Actually, the simple scenarios devised by P&W are short stories.)

I have argued strongly that it is simply not possible to learn without some causal inference. I believe that the basic law of learning is what can be called *the Law of Causal Effect: an organism will come to learn the relationship between its actions and an outcome when it believes there to be an apparent causal relationship between the action and the outcome.* Many readers will recognize this as a modified form of the standard "Law of Effect," one of the early axioms of behavioral learning theory. The major change is the phrase "apparent causal relation." By this, I mean that the organism must come to believe that what one does produces some result. It does not matter at all whether this be true or not. It helps if the action is located near in time or in space to the result, but it is certainly not necessary. If I flip a light switch and a light across the room goes on, I can learn this relationship. If I go to a friend's house for dinner tonight and tomorrow feel sick, I impart the sickness to my friend's cooking, even though considerable time and space have elapsed (and even though my sickness may have nothing whatsoever to do with that food). Moreover, I believe that the very same need to interpret people's and animal's actions in terms of their mental states is required when we interpret the actions of the world. Thus, it is a general tendency across all cultures to impart "mental states" to the action of the weather, or the gods, or the general environmental happenings. As P&W point out, it matters not whether the statement about mental activities is correct; what is of interest here is the necessity for an intelligent creature to make such assessment.

I have argued these things elsewhere, better, and at greater length. Alas, the arguments reside in an introductory textbook (Lindsay and Norman, 1977). Let me close by commenting briefly upon why my best arguments and best analyses of mental phenomena are put in introductory textbooks, and not in a literature where my professional colleagues would normally read (and benefit from?) them. It has to do with the same problem that P&W face in making their argument so long and prolonged. I could not publish these ideas in the normal journals of my discipline for two contradictory reasons. In the psychology literature, my colleagues would claim that my statements are unproved, possibly unprovable. My behavioristically trained friends would say, "but look, you have left out the critical control condition. You have ignored the possible interpretation that. . . ." If I try to publish these thoughts in journals of the discipline of cognitive science, the paper would most likely be rejected as being so obvious as not to need saying. You cannot prove the existence of mental events to the behaviorist, and you need not prove it to the mentalist.

Thank you, P&W, for a fascinating set of experiments. Please go on to develop an intelligent assessment of mental capacity. Please expand the range of animals tested. At the very least, please include human children so that we can make some comparative assessment between the abilities of your research animals and normal human children. But please, do not protest too much. The behaviorists are too intelligent to be convinced, so aim at a lower level: talk to us mentalists.

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Consciousness, classified and declassified. The three papers on cognition and consciousness make somewhat different contributions. All are devoted, as requested, to whether conscious cognitive processes are to be attributed to animals. The most conservative statement is G's, the most radical is P&W's. G urges that the behavioristic *Zeitgeist* which had inhibited inquiry into cognitive processes in animals is past and that, "with appropriate caution," we might "weigh the likelihood that animals sometimes know what they are doing." P&W insist that it is obvious when one watches animals that they know what they are doing – in fact, that it takes a very intelligent being to come up with a theory which does away with "mind" as has been done in operationalism and the resulting behavioristic approach to psychological problems. SR&B go further than G in presenting incontrovertible data on the use of symbols by chimpanzees, but do not go as far as P&W in drawing the conclusion that chimpanzees must therefore have a "theory of mind" in mind when they use symbols.

These three contributions to this issue of *The Behavioral and Brain Sciences* represent the first steps in a cognitive ethology, in comparative linguistics, and in a science of animal mentality. First steps are always difficult, and it is easier to fault them than to take them. The papers are based on a prodigious amount of hard and worthwhile experimental work which, even without the carefully phrased interpretations, would stand as major contributions. In the remainder of this commentary I want therefore to add some data in the form of pertinent reminiscences and provide some analysis of concepts not covered in the papers which are, however, germane and even critical to the problem of whether it is possible to construct a comparative cognitive psychology ("psyche" implying consciousness), as contrasted with a science of comparative behavior.

In 1956 I was asked by *Science* magazine to write a lead article on my work. I did so, and submitted a paper on "The Neurology of Thinking." The paper dealt with experiments on monkeys with a variety of brain operations tested in a multiple choice situation. The introduction pointed out that problem solving and thinking were not synonymous, but that there was evidence in their prolonged reaction time and the gestures (tentative reaches, stroking of the chin, and pulling of an ear lobe) that our monkeys were thoughtfully approaching the more complex portions of the problem.

The paper was promptly rejected, with only two comments: (1) the paper "did not reflect the type of work being done in the field"; and (2) "it was generally held that animals could not think, and that anthropomorphizing as exemplified by this paper was dangerous and misleading." The paper was published in *Behavioral Science* instead (Pribram, 1959), and also incorporated into an article in the *Handbook of Physiology* (Pribram, 1960). Over a hundred additional cognitive experiments on monkeys, using the multiple choice apparatus (which is very similar to the graphics display used by the Rumbaughs), have been successfully accomplished and published since (see, for example, Pribram, 1969 and 1975).

Shortly thereafter, Miller, Galanter, and I wrote *Plans and the structure of behavior* (1960), in which we declared ourselves *subjective* behaviorists. The book received damning reviews but seems to have been influential in allowing cognition and consciousness to return to *human* psychology. Now, after two decades, evidence is being presented that perhaps some animals do think, are conscious, and may even go about their business of behaving with a mind of their own! As the Brelands (1966) pointed out to Skinner many years ago, no one who has ever trained animals can come to a different conclusion.

Were the behaviorists, then, completely wrong in what they attempted? I do not believe so. Giving operational definitions to concepts is not a trivial pursuit. Nor is the tackling of the complex psychological problems that they eschewed for their time. I believe that Griffin, the Rumbaughs, Premack, and Pribram have all benefited from lessons taught by behaviorism, and have "kept the faith" in the care with which they have defined the inferences which they draw from the behavior of animals. When those inferences are drawn from animal behavior that is identical (or at least *very* similar) to that from which mental phenomena are inferred to take place in man, is it not reasonable to use the same concepts for the inferences? There is a danger, of course. Mental terminology comes bag and baggage with a host of connotations which may not be appropriate. These connotations must be diligently carved away to leave only precise statements. Physicists have accomplished this: Newton took such terms as force, field, and energy from psychology and gave them precise physical meanings. No one today faults nuclear scientists when they deliberately attribute flavors, colors, and charm to quarks. There is humor, of course, but also a deeper reminder that, as noted by Wigner (1969), modern physics deals with "relations among observations, not observables." Quarks are inferences. So are the cognitions and consciousnesses of animals. Both physics and psychology go about their science in like manner.

Just what *do* we mean when we imply that animals cognize and are conscious. Has anyone ever objected to the statement that animals re-cognize an object, person, or environment? Recognition is a cognitive process. Animals learn to identify objects and patterns by discriminating between cues – the literature is full of discrimination-learning experiments. Object identification is the test used in clinical neurology to determine whether a patient is suffering from an agnosia. Agnosias are disturbances of "gnosis," that is, cognition. Animals communicate, and some, such as the apes, are adept at symbolic communication, as shown by the Gardners, Rumbaughs and Premack. No one doubts these data, or their importance. But are such communications linguistic?

What is at issue is whether the processes discerned in animals are different in kind or only in degree from those of humans. If the processes are different in kind, then terms such as "cognitive" and "linguistic" should be reserved for the human case and not used to describe the results of animal observations and experiments.

"Cognitive" and "linguistic" fare somewhat differently when the question is asked in this manner. As noted, the operations that define cognitive processing in animals are not readily distinguished from those that define them in humans. But the root of the terms "language" and "linguistics" is "lingua," the tongue. Studies of animal communication have thus far not demonstrated much similarity between human and nonhuman speech. This is taking a purist position, but until everyone agrees to use the term "linguistic" to refer to structured patterns of communication (or some similar definition) irrespective of modality, there is danger of being misunderstood. When Washoe and the Gardners (1969 *op. cit.* SR&B) communicate by gestural signs, is this language? When Bellugi-Klima (1972) and her deaf and dumb children communicate by gestural signs, is that language? If American Sign Language is a "language," is symbolic communication using a computer programmed with Yerkish, a "linguistic" communication as claimed by the Rumbaughs? There are no immediate answers to such questions because the answers demand a consensus on an acceptable definition of language. But the problem can be noted, and care can be taken when terms are used that are undergoing a transition in usage – and the question can be raised as to whether the meaning of the term "language" is in transition.

Mind is another construction that raises similar issues. Do apes have a "theory of mind" ask P&W. Mind is derived from minding, attending, as Ryle noted in *The Concept of Mind* (1949 *op. cit.* G). Is there any question regarding the fact that animals attend selectively, focally, and even dividedly? Or that they mind their owners? And if they mind, do they have "a mind"? As P&W have shown, their behavior is uninterpretable if one does not infer that apes construct representations of their environment. Sokolov's (1963) experiments on dishabituations of habituated orienting reactions, when used with animals as in our experiments (see, for example, Pribram and McGuinness, 1975), demonstrate with physiological indicators not only that such representations are formed, but that they reach considerable complexity. Is minding – attention – then a function of such representations? The orienting reaction is the simplest case of attending – so the answer to that question is yes. What P&W must mean, therefore, is that representations, since they determine minding, make up mind. What P&W say is that we infer representations to guide behavior in humans when we see them observing and attending. So P&W infer them to be present in animals. How can anyone object to that?

But objections will be raised, no doubt, because "mind" connotes much more than "minding." In fact, in other languages, such as German and French, a comparable term does not exist. Mental is ordinarily translated into words that are much closer in meaning to our "spiritual." Are P&W advocating "animal spirits"? Of course not, but once again the warning should be raised, lest implicitly held connotations destroy the force of the contribution.

Finally, what about consciousness? G addresses the problem of animal awareness and intention. A layman reading either his book (1976 *op. cit.* G, SR&B) or his paper in this issue of *BBS* might well react as did Granit in an earlier commentary on another paper [Roland: "Sensory Feedback to the Cerebral Cortex During Voluntary Movement in Man" *BBS* 1(1) 1978, p. 152] by wondering why it is necessary to "break down open doors." Not only do animals mind, but of course they are aware of their surroundings. We can quickly discern whether an animal is conscious or unconscious, and, if conscious, what state of consciousness he is in. Why could not the behaviorists let sleeping dogs lie? To deny their sleep state was not their intention, but to deny dogs an intent to go to sleep was. Intentional states were to be denied not only to animals but to humans as well. Why?

As Skinner makes patently clear in *Beyond freedom and dignity* (1971), intentional states imply an element of unpredictability. I would put it more specifically: we infer intentional states when behavior becomes unpredictable ("the road to hell is paved. . . "). How can reliable inferences be reached from unreliable data? Von Neuman (1956) wrote a paper on how reliable outputs could be achieved by a machine composed of unreliable parts, and probability theorists have constructed a whole statistical technology to deal with such inferences. Why shouldn't animal psychologists use these tools in their conceptualizations as well as in analysis of their data? Would not a Bayesian approach (see, for example, Edwards, Lindman, and Savage, 1963) to inferring "intentions" be as respectable a scientific endeavor as the probabilistic approach which spawned the currently popular information processing?

G's argument, perhaps more than the others, loses force because of its failure to make explicit the hidden connotations of the terms it advocates. Of course animals are not ordinarily unconscious. No one would argue that point. But whether they are intentional, self-conscious, and self-reflective remains open to investigation (Pribram, 1976a).

The issue may seem trivial but it is not. Weiskrantz has recently reported on patients who exhibit "blind-sight" (Weiskrantz et al, 1974). Despite repeatedly avowing a loss of vision in the field contralateral to a surgical excision of the occipital lobe, these patients are able to identify instrumentally the location and shape of large objects placed within the "blind" portion of the field. H. M., Brenda Milner's famous patient with a medial temporal lobe resection, has displayed a complete "loss of memory" for over thirty years (Milner, 1970). Still, when tested instrumentally by Sidman (Sidman, Stoddard, and Mohr, 1968) H. M. showed complete retention of learned operant behavior. I have reported other incidents of dissociation between verbal reports of introspection and observed behavior (Pribram, 1965); and even in animals different behaviors will be displayed under slightly different conditions. Thus, rats with lesions in the area of the ventromedial nucleus of the hypothalamus will overeat when food is available ad libitum, but when an inch-high barrier is placed between rat and food, these same animals will starve [see also Toates: "Homeostasis and Drinking" *BBS* 2(1) 1979]. Originally, the lesions were thought to impair "drive," a respectable but untenable behavioristic concept. An inference that does fit the data is that the lesion impairs "effort," defined in terms of work attempted and accomplished (Pribram, 1970). Do rats make an effort? What is the difference between a memory only instrumentally displayed and one accessible to awareness? I would like to know and want to use animal models to find out. If animal awareness is ruled out by behavioristic fiat, I may never gain the necessary control over variables so dear to the behaviorist, and thus never have the opportunity to ask and answer the question properly.

States of consciousness appear to delimit ranges of behavior in both man and animal (Pribram, 1976b). The contents of consciousness are processed within those limits. Experimental psychologists, including those working with animals, have, since William James, dealt with this issue in terms of "span" – the temporal and spatial span of attention, awareness, and short-term memory (e.g., digit span). The processing per se has classically been the province of psychophysics and perceptual psychology. As noted by G, these disciplines have now admitted Images and Plans as explanatory concepts. Animals apparently perceive; to what extent are their perceptions similar to those of humans? Both G and P&W propose that the similarity is much greater than would be allowed by a strict behaviorism. My view is that once the strictures are lifted and carefully made, operationally valid inferences are admitted, the question becomes *experimental* and subject to testing in the laboratory and clinic. It is this spirit of experimentation which informs the three papers in this issue and makes them so valuable.

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When is attribution of beliefs justified? [P&W] The purpose of this commentary is to offer a few remarks concerning the conditions under which it might be legitimate to use certain mentalistic or cognitive terms like *believe, want, think that, intend,* and so on, to describe an organism's behavior. Such language has traditionally been frowned upon as unscientific, or at least as being eliminable in favor of a naturalistic vocabulary. Even those who favor the use of cognitive terms to characterize human thought processes may be reluctant to apply them to nonhuman organisms. Morgan's canon urges that the attempt to formulate a naturalistic account of animal behavior should not be abandoned in favor of the easier road of anthropomorphism. While this is no doubt sound advice, one must also guard against mere phylocentric chauvinism by attempting to be explicit about when it might nontheless be legitimate to use such language. It seems clear that not every case of apparently adaptive behavior, or behavior that *could* be described teleologically, merits a cognitive explanation. A river flowing to the ocean by a path of "least resistance" (i.e., minimum potential energy) is a case in point, as are many examples of useful reflexive behavior in organisms. What we need is a general characterization of the class of behaviors for which a cognitive account is the appropriate form of explanation. Note, however, that we need not require anything as strong as a set of necessary and sufficient conditions for the use of cognitive terms. The lack of a precise and decisive criterion for when such language is appropriate would be no reason to abandon it entirely. In many cases such criteria are not even available for obvious distinctions (for instance, the fact that we can't say exactly how many hairs it takes to qualify as a beard or a head of hair does not prevent the notions of "beard" or "baldness" from being legitimate).

For the purposes of this commentary I shall assume that the facts regarding chimpanzee behavior are as P&W and others have reported them – i.e., that there are no major methodological flaws in the experimental design or in the reporting of the observations. If these reports are correct, I see nothing problematic in accepting the cognitive explanation of chimpanzee behavior. Although terms such as *believe* and *want* carry a certain mystique because of their role in many traditional philosophical discussions of intentionality (say, in the works of Brentano), their use in contemporary cognitive science is much more straightforward. In this context they refer to certain formal properties of representations and an organism's (or a device's) relation to these representations. Thus, they are used whenever the explanation of some behavior, under the description which gives it maximal systematicity and coherence, requires that we posit internal *representational* states of various types (e.g., belief-states, goal-states, percept-states, and so on). It is the representation-governed nature of the behavior that is at issue when we concern ourselves with the use of cognitive terms. What I would argue is that representations need to be posited whenever behavior can be shown to be significantly plastic and stimulus-independent (or at least independent of the physically described stimulus situation).

First, however, one might note one or two reasons why hypothesizing such states in humans is somewhat less problematic than in nonhuman organisms. In humans, representational states can be created and altered very nearly at will, and in many cases their consequences can be observed rather directly through the use of language. Human behavior is extremely plastic because it can be influenced to an unlimited degree (though not always in predictable ways) by purely linguistic events. We are led to posit belief and goal representations in order to account for the fact that the semantic content of an utterance can have arbitrarily far-reaching effects on subsequent behavior.

The chimpanzee case also differs from the human one in that there is a deeply entrenched need for us to describe *our* behavior in cognitive terms in order that it make sense to us. In fact *everyone*, including the behaviorist, talks about human behavior under a cognitive description. We simply cannot avoid speaking of actions such as eating, going somewhere, or telling someone something, instead of describing raw behaviors such as moving forks, legs, or lips. What makes a particular piece of behavior "saying something," rather than making sounds by moving one's lips, is that it is the former, the cognitive interpretation, which allows us to account for certain systematic consequences of these acts [see Haugeland: "The Plausibility of Cognitivism" *BBS* 1(2) 1978]. This, in turn, is because it is not the behavioral movements *per se*, but rather the behavior under one or another interpretation (as a meaningful act) that has explanatory force. We can't, for example, say that the young woman blushed because of the way the boys moved their lips and tensed their vocal chords, because it is presumably *what they said* that explains her blushing. But as soon as we admit that it is not the physical movements themselves but our interpretations of them that matter in explanations of behavior, we are really placing the explanatory burden on the subject's represention of these actions. *We* routinely speak of *our* reasons for doing things in terms of beliefs and intentions, and expect our explanatory theories also to appeal to such constructs, because we want the explanations to address our actions under our *intended interpretations* of them.

While the linguistic route to such notions is not available in the case of chimpanzees and other organisms (at least not in the creative sense of language), other independent criteria are available. If it can be shown that the chimpanzee's behavior is responsive to how he *interprets* (and thus how he represents) a situation, then it would follow that the use of cognitive terms would be appropriate. We would then, for instance, be justified in saying that the chimpanzee's behavior was determined by what he thought or believed or took to be the case. But what would count as a demonstration that an interpretation of a situation rather than a causal consequence of the situation was the appropriate characterization?

Here it seems to me that there are two primary criteria for what might be called *epistemic mediation* between a stimulus event and subsequent behavior. These are (a) the arbitrariness and (b) the informational plasticity of certain aspects of the relation between the event and the ensuing behavior. A relation can be said to be arbitrary if *under that particular description of the event and of the behavior* the relation does not instantiate a natural (nomological) law; there is, for example, no natural law relating what someone says to you and what you will do – the latter depends on what you know, believe, infer, want, and so on, and might have to appeal to arbitrary rules and conventions. A relation can be said to be informationally plastic if it can be radically altered by information – that is, if it can be systematically varied by a wide range of conditions which have nothing in common other than that they allow the valid inference that, say, a certain state of affairs holds (though this range of variation will not be as large in the case of nonhuman organisms as in human, since the latter involves the unbounded number of ways in which information can be linguistically imparted).

Now it seems to me that unless even the older reports of chimpanzee behavior that have been circulating are all radically in error, the case for the arbitrariness of the stimulus-response relation is already clear, since these reports have suggested that what a chimpanzee will do when confronted with a situation (e.g., an out-of-reach banana) depends on how he interprets such things as sticks, chairs, the trainer, and so forth. It is only by interpreting the trainer as a tall, climbable object that the chimpanzee could solve the problem of obtaining the bananas in the classical Köhler story. In fact, even the reported ability of the chimpanzee to imitate humans can be taken as evidence for their representational ability. For if the set of behaviors produced in imitation is anything like the set which a human would produce (as is implied by the anecdotes of chimpanzee imitations), then what the chimpanzee is doing is not merely producing a trajectory of movements which under some uniform similarity metric comes close enough to being the one that is being imitated; rather, he is producing some behavior that can be given the same *interpretation* as the original (e.g., "walking across the room and opening the door").

Although I believe that such reported behaviors show that there is a sophisticated representational component governing chimpanzee behavior, the case can be made all the more impressive if we can demonstrate that the chimpanzee has multiple access, and in particular a *reflective access,* to these representations so that he can, as it were, "mention" as well as "use" them. This would argue that the representational system is available for a variety of purposes apart from directly determining the immediately relevant behavior, such as to represent possible (contrary-to-fact) states of affairs, to compare a desired state of affairs with one that obtains, and to infer certain representational states in others. The last is perhaps the most interesting, for it means that the chimpanzee's behavior is determined not only by the fact that he represents a situation in a certain way, but also by his ability to represent the *representing relation* itself: he not only represents the belief B but also the notion of "a belief that B." In the case of humans, of course, this is evident from the existence of such constructions in the language. The studies P&W report are an attempt to show that not only can the chimpanzee be in the kind of relation to a representation that Russell called "relations of propositional attitude" (e.g., believing that B, wanting it to be the case that B, expecting that B, wondering whether it is the case that B, or even considering what would happen if B were the case), but also that he can represent these relations themselves – or specifically that he can represent that other organisms are in these relations to their representations. In other words, the studies argue that we must not only posit beliefs in order to account for certain of the chimpanzee behaviors, but also beliefs about beliefs and intentions of others (and, presumably, about the chimpanzee himself).

That this kind of meta-representational ability occurs in nonhuman species is not as obvious as the existence of representations per se, though nothing prohibits it in principle. The fact that chimpanzees do not possess a generative expressive language predisposes us against assuming that they have such meta-descriptive internal representational capacities. If, however, the kind of evidence discussed by P&W can be sustained, the lack of expressive language in the chimpanzee will clearly have to be explained in terms other than limitations upon their *conceptual* apparatus. The P&W interpretation clearly points to the chimpanzees' having an extremely rich internal representational system or "mentalese" – certainly much richer in expressive power than any communicative code they have ever been taught by human trainers. In particular, it suggests a capacity for at least some limited recursive embedding of propositions, an expressive power even beyond the reach of first-order logic. In fact, it might be argued that this interpretation has even more radical consequences. There are those who believe that the recursive meta-representational capacity is to be identified with the notion of consciousness itself. Certainly, if concepts like lying (or "deliberate") are among the kinds of relations between an organism and its representation that can themselves be represented by the chimpanzee, one must take seriously the idea of a chimpanzee as a moral agent.

That P&W's interpretation of their observations has such far-reaching consequences does not in itself cause any difficulties for their view. Indeed, one of the common results of taking one's findings and theoretical commitments seriously is the discovery that they entail counterintuitive consequences. Far from being a *reductio ad absurdum,* such consequences are the usual way in which radically new ideas enter a scientific enterprise.

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Who cares if the chimpanzee has a theory of mind? *Griffin.* The behavioristic *Zeitgeist* as invoked by G is a scary *Geist* indeed. If this is the atmosphere in which ethologists have been working for the past fifty years, then it is certainly time to get rid of it. If, as G implies, the behavioristic *Zeitgeist* has caused ethologists to identify mental experiences with neurophysiological processes, to hold that man is discontinuous with other animals, to refuse to study complex behavior, to reject verbal report as a scientific datum, and to study only behavior which can be elicited by known stimuli, all psychologists will join their voices to G's and plead that this ghost be exorcized. But it is important to realize that what G calls the behavioristic *Zeitgeist* has nothing at all to do with behavioristic psychology. I doubt whether even Watson held many of these views; Skinner certainly holds none of them. The major problem with G's argument, however, is not that it attacks a behaviorism which does not exist but that it proposes a cognitive *Zeitgeist* that has about as little relation to current cognitive psychology as the supposed behavioral *Zeitgeist* has to current behavioral psychology. The view of S-R psychology attacked by G is actually closer to current cognitive psychology than to current behavioral psychology. The most salient characteristic of cognitive psychology today is its reliance upon models, mostly taken from the operation of digital computers, of how information is processed in the nervous system. A cognitive ethology that had any relation to cognitive psychology would propose models for information processing in other organisms which differed, perhaps, in certain elements from that of humans. For instance, humans and chimpanzees might have similar or different input or output buffers or storage systems or programming codes, etc. From a behavioral point of view a program to uncover and compare such mechanisms in various organisms would be unfruitful but it would at least be a reasonable cognitive ethology, in line with cognitive psychology. What G proposes is a return to the anecodotal method of Romanes. By virtue of clever behavior which, as Disney has shown, may be adorably similar to that of humans, he proposes that we endow other animals with honorific titles. One imagines a Wizard-of-Oz-like scene in which G, as the wizard, awards to the chimpanzee or the bee a "Certificate of Consciousness" for having demonstrated, in its behavior, *awareness* or *knowledge* or *courage* for that matter.

This is a game that may be played at any level. With a little effort one can imagine a cognitive physics. A gallon of water, for instance, is: (a) infinitely complex; represents the environment (b) externally by taking the shape of its container and (c) internally in the form of pressure and temperature gradients which, in turn, affect the environment in terms of subtle alterations in magnetic, electrical, or gravitational fields; communicates its intention (d) to boil by first steaming and then exhibiting little bubbles, and (e) to freeze, by increasing in density, and so on. It is true that a pail of water has its own language, which must be interpreted by us, but so do the French and I've never heard of an attempt to deny them consciousness for that reason. With two examples of infinite complexity there is no justification for holding that one is more complex. In arguing that a pail of water has degrees of complexity similar to that of humans, I am not proposing panpsychism but just that *consciousness* be seen for what it is – an honorific word for a particularly human complexity of behavior which, if it is to have any meaning at all, must be spelled out more specifically than has heretofore been done.

Referring to Lindauer's work with bees, G cites the finding that bee-W does not usually imitate bee-N's (more vigorous) dance directly but first visits cavity-n, and then dances as bee-N did. An obvious next step is to see whether there are any conditions under which bee-W will imitate bee-N's dance without first visiting cavity-n. This is the sort of question a good researcher of any persuasion might ask. It does not require (as G asserts it does) first postulating that bee-W be thinking, "Very good cavity up north at such and such a distance."

Words such as *intention* are not meaningless. To test whether a certain person has an intention, however, we first have to define the word and then observe whether a person's behavior (including verbal behavior) conforms to the definition. If it does there is no reason to attribute mystical qualities to the person. We could decide in advance that words such as *intention* are useful to us only in social interactions with other humans and thus restrict the referents of such words to human behavior. Or we could arbitrarily define the behavior to which we want to apply the word and look at the behavior of other species for conformity to the definition. We should then be prepared to find technical conformity among any species or even among inanimate objects. Such conformity does not in itself imply any special connection between humans and other exemplars of the behavior. The conformity might prompt us to look for a common structure, a common function served similarly by different structures, or a common reinforcement history. But why should we postulate mental events in the object under study before we begin the search?

Savage-Rumbaugh, Rumbaugh, and Boyson. The papers of SR&B and of P&W both show that chimpanzees can exhibit behavior that looks remarkably similar to human behavior. In both cases, however, the cards are stacked. There are at least three sorts of environment in which to study the behavior of a nonhuman species:

a. Natural or naturalistic environments;
b. Extremely simple environments;
c. Humanlike environments.

The advantage of natural environments is that, in its own biological niche, the complexity and adaptiveness of the behavior of an organism is most likely to be revealed. The advantage of extremely simple environments is that important variables may be varied on one dimension at a time. The advantage of humanlike environments is that in them the activities of animals are entertaining. But it is dangerous to draw scientific conclusions on the basis of such behavior; the always-present temptation to anthropomorphize is exaggerated when, for instance, apes are using keys to unlock locks rather than using straws to hunt insects as they would do in their natural environments, or pressing levers as they might do in extremely simple environments. The use of the keys, money, and wrenches in the experiments of SR&B seems designed more to impress the reader than to study the behavior of the chimp.

The game being played by SR&B and P&W is exactly the one prescribed by G. One player sets a criterion or a series of criteria drawn from human behavior and the other tries to see if chimpanzees' behavior can be manipulated to conform to the criteria. The more humanlike the atmosphere the better. Then the first player, acting as devil's advocate, performs a behavioristic analysis of the second player's experiment whereby the *awareness* or *intention* or other cognitive attribute is shown to be only an "illusion." The cognitive attribute is then defined in stricter terms and (forgetting the arbitrariness of the initial definition) the game can begin again.

The nature of this enterprise is most clearly revealed in the discussion section of SR&B's paper, where Project Washoe and Project Sarah are criticized for not decisively testing the "symbolic capacity" of chimpanzees. But the present study of SR&B can be criticized on the same grounds. SR&B will undoubtedly be taken to task by psycholinguists for having abandoned linguistics experiments for social-psychology experiments. The *communication* between the two chimpanzees in their experiments was of the simplest kind (one chimpanzee pushes a single button which lights up the corresponding button of another chimpanzee). This communication satisfies the quotation (indirectly from Bloomfield) at the head of the article but many communication experiments of this kind have been done with pigeons, rats, and other organisms supposedly deficient in linguistic ability. Indeed, if one reads the quotation literally, it is satisfied by the communication between a piston of a car (A) which communicates with the piston linkage (B) to do something impossible for the piston to do itself (splash oil from the oil pan) which is adaptive with respect to A (in the sense that it lubricates the cylinder wall and prolongs the piston's life). It is heartening to see that the nature of linguistic ability is being defined by SR&B in this functional sense. But then why are they still concerned that Project Washoe and Project Sarah did not reveal *awareness* and *intentionality* in the chimpanzees? Bloomfield's "fundamental linguistics situation" quoted at the head of the article (with which the experiments are wholly consistent) says nothing about awareness, intentionality, or other cognitive states. It defines linguistics wholly in behavioral terms as a certain sort of interaction between two individuals – that is, as a social event.

And it is as social psychology that the experiments have their interest. I do not know in which direction a cognitive psychologist would take future experiments in this area. But what is unusual here is not the simple communication between the two chimpanzees but the fact that they came to share the food. With pigeons, in my own laboratory, I have not found sharing except when each pigeon could punish the other for not sharing. Similarly, with humans, prisoner's-dilemma games reveal that sharing only occurs under special circumstances. What caused these chimpanzees to share their food? It seems unlikely (but certainly possible) that chimpanzees occupy a particularly altruistic ecological niche between the selfishly primitive pigeon and the selfishly sophisiticated human. More likely, there was something about the training procedure that caused each chimpanzee to give up some of its food. Perhaps this was the alternation of roles of the chimpanzees or the presence of the experimenter. These variables could be investigated without attributing to the chimpanzee any particular internal linguistic events (such as: "If I no give him my food, he no give me his") as G might demand. I am not saying that such internal linguistic events cannot occur. It might even be possible to externalize them by means of Rumbaugh's graphic system. I am saying that such a search would be less interesting and less fruitful than a search among the environmental contingencies for what caused the chimpanzees to share their food.

Premack and Woodruff. The experiments of P&W are more complicated on the stimulus end (and less complicated on the social end) than those of SR&B. As the latter can be thought of as social experiments, the former can be thought of as perception experiments. Attributing a theory of mind to the chimpanzee tends to obscure what is really interesting about these experiments. The procedure consists of showing a chimpanzee a videotape and then a group of pictures and asking it to choose one of the pictures. The tapes and pictures have been selected by P&W so that a given picture either represents a solution to a problem posed in the videotape or does not. The chimpanzee, usually on its first try, chooses the picture representing the solution.

Now, as P&W say, it is clear that most adult humans would also solve the problem. One might now ask, how do they solve it? I submit that a paper such as P&W's with an adult human instead of a chimpanzee as subject would be rejected for publication by any respectable psychology journal because *it does not seriously consider this question.* It would not be sufficient (with adult human subjects) to attribute the correct solution to the fact that subjects had a theory of mind. One then wants to know how the theory of mind works, how the videotape and respective pictures operate within the person's theory of mind, and why the mind accepts one picture and rejects the other. With chimpanzee subjects, no less than for human, such an analysis ought to be provided. It is not enough to say, "You would do the same if you were a subject in this experiment." What is needed to analyze this experiment is a sort of "grammar" for the videotapes in relation to the pictures, and a training procedure with the grammar. Perhaps one might start with "Gestalt laws" such as symmetry, closure, *pragnantz,* or common fate (not, obviously, in terms of rudimentary physical properties but in terms of structural and functional aspects of the situation. Even pigeons can quickly learn to discriminate one person from another and, presumably, a shivering person from a nonshivering one, and a locked-up person from a free person. The interesting question here is what in Sarah's experience caused her to choose the key when she saw the locked-up person, not what her mental state was at the time she did it. The particular pictures used by P&W make such an analysis difficult because it is not clear how they are related to the experimental and nonexperimental history of the subject. The fact that P&W (and this commentator) can think of no clear-cut analysis does not mean that the chimpanzees have a theory of mind. If we cannot figure out how the chimpanzees do their tricks (in terms of, say, a "grammar" for the videotapes and pictures), then we should proclaim our ignorance rather than the chimpanzee's mental powers.

The experiments of SR&B and P&W are fascinating, instructive, and provocative. I am by no means arguing that they should not have been done. But questions for now are: What do these experiments mean? and, What should we do next? So long as we continue to test the hypothesis: "Chimpanzees are like human beings" and then, having prearranged conditions to get an affirmative answer, conclude: "Chimpanzees have this or that mental power," we will be hindering ourselves in our attempt to discover what sort of creature the chimpanzee is.

by **Austin H. Riesen**

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Responses versus cognitions. In the late 1940s Hebb (1949a, p. 192) identified two "great discontinuities" in the theoretical structure of psychology. One had to do with the micro-anatomy and physiology of learning and the other concerned the "intuitive judgments" on which knowledge of social perceptions, cognitions, and emotions were based. Social behavior and the study of social behavior were so dependent on intuitions, he argued, that methods for quantification and objectivity were a crying need of the time. Many psychologists were in the mood for rejecting rather than facing the problems of research in these areas. The three accompanying target articles to which this commentary addresses itself are happy reminders that progress has been made on the second theoretical front, and we find assurance that the first discontinuity is also yielding to direct attack.

Hebb [q.v.] has himself been a most creative contributor toward the breaking down of both discontinuities. The one at issue here has to do with the "private experience" of not always so private emotions and cognitions. Hebb (1946) made the study of inferred mental states in nonhuman species both respectable and theoretically fruitful. He showed that observers could judge animal emotions and individual differences in temperament with a high degree of reliability. These studies helped to turn the tide toward acceptance of feelings, motives, and cognitions as subject matter for students of animal behavior.

Premack & Woodruff's use of choice behavior with Sarah to answer objectively new questions about thinking, purpose, and intentions, provides a tool for asking what the animal infers about human intentions, and so forth. The method was used in a different form by Robert Fantz (1958) with chimpanzee and human infants as a means for asking questions without having to rely on language behavior. The studies that P&W give us are breaking new ground, in the sense of providing new evidence for expectation and for the ability of a chimpanzee to impute purpose in another primate. They also illustrate once again the desirability of having objective methods (such as Hebb was asking for) for validating human judgments about mental states in animals. This is the answer to arguments against studying consciousness that behaviorists were putting forth so strongly in the days of Watson and Thorndike and Hull.

The newer studies of capacities for language behavior in chimpanzees and gorillas add a further dimension to advance the study of mental states and problem solving in animals that have succeeded far beyond expectations of even a decade ago. When a gorilla or a chimp even tells a lie by ingenious use of sign language, we are reminded that behavioral checks are necessary even in the use of linguistic avenues to the investigation of what animals feel, know, and "manipulate" in their heads.

We are sometimes asked whether it is natural for animals to use "typewriters" or to "sign" for desired objects and actions. How would learning to do this help in adjusting to their African environments? The question is irrelevant to the study of animal capabilities, although it may be relevant to questions of ecology and evolution. Even these reservations about relevance must be dropped when one considers possible future developments. Changes that are going on now lead us to doubt that future environments can be predicted. Students of animal behavior know well that capabilities of animals and human beings are often beyond the requirements of environments in which the lives of many organisms are now being lived.

Griffin's paper extends the questions about mental activities and experiences to nonprimate species. Is it indeed futile to ask whether birds experience colors in their visual world? We obviously cannot know whether they experience them exactly as we do. Since birds show preferences (in choice situations) and execute action patterns when appropriate colors are part of certain visual patterns, reasonable guesses are that they do experience colors in some fashion, and furthermore, that certain emotional experiences go with the visual. To deny some forms of conscious experience to dogs, cats, horses, birds, and so forth, would gain nothing. The question at issue remains: do such experiences mediate between environmental inputs and the behaviors that we see as being responsive to the input?

When nervous systems differ greatly from those of higher primates we are understandably reluctant to ascribe similarities of conscious processing. Furthermore, we see behavior in ourselves that is unaided or even hindered by awareness. Simpler neural circuits suffice to mediate bodily orienting, postural adjustments, and adaptations to metabolic requirements. But we are not yet exactly clear on what the neural substrates of being conscious are. G is right about our need to keep an open mind. Laymen, neurobiologists, and psychologists will not have open minds, however, if they see no alternative to ascribing intention to every goal-directed behavior. Behaviorism taught an important lesson that needs to be appreciated. The lesson of objective behavioral investigation is not obvious to all, and for many research purposes it is the preferable approach. For some, it is the only approach, and we can afford not only tolerance but strong support for those who prefer to work without assuming consciousness as a mediating process in behavior.

On the other side of the coin, the question of anticipatory mentation gives behaviorists problems with which they have struggled. As G points out, the training of language skills should give us important new information about this. Do the great apes plan ahead? Spontaneous communication by chimpanzees (see Menzel and Johnson, 1976 *op. cit.* G) indicates an affirmative answer. *Savage-Rumbaugh, Rumbaugh, and Boysen* find strong support for such planning in the interchimpanzee communications that involve requests for an appropriate tool. How far can these requests be extended in time?

Hull (1952) developed his ingenious "anticipatory goal response" and "antedating defense reaction" constructs to explain expectant behaviors in rats. To others the same behaviors support the concept of "cognitive maps." Rats, dogs, apes, humans are all endowed with cognitive maps or anticipatory goal responses (each with both, in this commentator's opinion).

Is there any need for keeping anticipatory behavior in the motor (response) portion of the nervous system? Hebb (1949b) persuaded many of us that central circuitry was in the brain to keep cognitions and maps organized and ready to function during periods when sensory inputs were absent or inappropriate. Goal-directed use of such maps may reach its peak in human activities, but their use by apes and other animals is different in degree, rather than kind. At some as yet unknown stages between amoeba and humans, differences in kind will be discovered.

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by **Harvey Sarles**

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One new theme and . . . One supposes that G is being direct in claiming that the "basic question at issue involves a comparison of . . . brain function" having to do with possible "mental experiences" in various species. However, this claim is complexly bound up with many "whethers" and "whats" of "mental experiences." Each of these papers uses a different definition of "language," which then serves as a foundation for its framework.

Griffin's paper takes what I find to be a reasoned and reasonable position: that we must remain open and "agnostic" even to begin to discuss the questions at issue. This paper is a shortened and interestingly elaborated version of his book on animal awareness (Griffin, 1976 *op cit.* G, SR&B). In order to discover the very nature of what we are discussing, we have to wonder a little about our metaphysical commitments to particular views and questions about human and animal nature. If, as he and the other authors seem to say, it is clearly true that chimps et al. are much "smarter" than we would have supposed, why is it that we have supposed as we have? It appears that our theories about language and mind never really need much "evidence." The presentation of "evidence," instead of raising consciousness, tends instead to decline into arguments about methods and unknowables.

Premack & Woodruff's essay uses the "creativity" argument to situate the "problem" in tractable dimensions. "No theory of generalization will explain the comprehension and production of structurally novel sentences," they claim. Many readers will recognize this argument to be one that is often used to distinguish humans from other species on qualitative grounds (Sarles, 1977, Chaps. 1,4). Once this move is made, most of us who have considered the problem think that attempts at comparison are gratuitous: the *essential* uniqueness of humans is already presumed. Only certain "boundary issues" are open to debate; probably none, seriously. If the "creativity" argument indeed has these inbuilt presuppositions, is it possible that the claim to be doing serious comparative work carries other agendas?

In their abstract, P&W state: "An individual has a theory of mind if he imputes mental states to himself and others." This sets up so many levels of puzzle that one finds it difficult to sit still and think. Why a "theory of mind?" In what sense does an individual "have" one? Is it possible *not* to "have" one? Why is this a "theory?" Is it "because such states are not directly observable, and the system can be used to make predictions about the behavior of others" (P&W, sentence 2)? While hardly anyone would disagree with the "predictions" clause, it does not necessarily help the first. In fact, one wonders if there is a subject matter here at all, if it is not "directly observable" in fact (or in principle?). Frankly, it seems to me to set up the possibility, even the likelihood, that a "theory" is properly for "nonobservables." Maybe it accounts for the recent increase in the perceived lack of boundaries between behavioral science, parapsychology, and other forms of mysticism.

After the presuppositions about individuality and human nature and the claim about what a theory is and is for, I am puzzled by the remainder of this essay. In some senses, it is about "other minds" and toward some theory of discourse, mutuality of understanding, and sociality. But the invocation of a chimp "theory of history" to account for one experiment, the association of other species and human "retarded" persons, and the concluding remarks about the very nature of what is and is not "natural," suggest to me that much more is going on than is actually expressed in this essay. What is it? What is it for? Is it behaviorism redefined?

Savage-Rumbaugh, Rumbaugh & Boysen's essay seems to be much like P&W's, even though it is severe in its criticism of Premack's work. It (also) invokes a definition of "language" which confines and prescribes a qualitative sense of human uniqueness: "the true adaptive function of language lies in the ability it confers upon man to transmit specific information in an abstract, context-free form."

How could we know what the "true" function of "language" is? To invoke the term "adaptive" seems to "biologize" the definition, but what does it explain? Doesn't this definition depend on a prior view of "language" which is approximately like "logic" – an "infinite" set of ideas or sentences?

Are humans capable of "context-free" exchanges? The idea of "abstract" seems to derive *from* the human/animal debate itself, and is thus circular. This sort of definition serves principally to claim that, in some aspects of our being, we are extranatural; "language" is a cover term for features of our being which are *assumed* to be unique.

The language argument is used to criticize chimp/human communication studies, and to define intraspecies communication as the "proper" locus of nonhuman communication studies. This seems more than reasonable, except that it is then re-placed in the context of what "symbolic" communication is – as if this is a clear discussion.

To say, for example, that a (laboratory) animal "*cannot* use symbols" seems to prejudge; to claim that an animal "does not" might be reasonable. But to claim understanding of what is "prelinguistic" and what is "symbolic" is merely to enter the mind/body, human/animal controversies in the name of yet another metaphor. The fact that SR&B's methods seem very reasonable (even to me) should not blind us to the fact that the issues are embedded in old dualisms. The authors seem defensive, not critical, as they claim and ought to be.

The "state of the art," as represented in these three papers, sees a clash between those who are involved in serious attempts to solve problems by the usual experimental/empirical means, and those who have come to a new, also serious, position which might be called "methodological agnosticism." (Penn, personal communication). Although I too have come to G's position of having to rethink what this area is all about, it is not difficult to appreciate the ordinary attempts to solve interesting problems. Whereas in most areas of inquiry philosophy follows periods of problem-posing and solution, G and some others believe that there is already a great deal of intellectual baggage which embeds the very ways in which we tend to ask questions.

Surely this is a fascinating debate, and one to which we must become educated – not only in methods, but also in the history and sociology of these ideas. Many of them are, of course, ancient, and have become the common sense through which we judge their reasonableness.

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Isn't the answer obvious? [P&W]. *Preliminary comment.* P&W ask: Does the chimpanzee ascribe purposes, knowledge, and other mental states to itself and others? Their answer is affirmative, on the ground that only thus can we explain how Sarah selects the correct solution to various problems encountered by another (human) agent. The problems are presented to Sarah as sequences of pictures of the agent confronting and attempting to solve the problem, and Sarah is required to select, from among several alternatives, the picture that depicts a correct solution.

At a first level, P&W consider the competing associationist and nonassociationist explanations of Sarah's ability. The *associationist* maintains that Sarah selects moving the box and standing on it (as the solution to the problem of reaching the suspended banana) by completing a behavioral sequence like those previously performed or observed by her. (The elements of the behavioral sequence become "associated" by repeated co-occurrence in a certain order.) The *nonassociationist* argues that the above explanation fails where the correct completion produces a behavior sequence unlike any experienced before, and that Sarah is capable of such completions. For example, she selects a key as the solution to the human's problem of getting out of a locked cage (and presumably she has never herself been, nor seen anyone else, imprisoned in quite this way). We must, the nonassociationist argues, posit rules in Sarah that generate the sequence containing the key, in just the way that rules must be posited in children to explain their generation of structurally novel sequences of linguistic behavior.

At a second level, P&W consider the "theory-of-mind" explanation of Sarah's ability (a confusing label, since both the associationist and nonassociationist views are theories of mind), but fail to consider its true competitor, the no-theory-of-mind explanation. The *theory-of-mind* explanation is that Sarah selects correct solutions by ascribing purposes and knowledge to the human agent and choosing the behavior consistent with her ascription. For example, she ascribes to the human an intention to get the banana and the knowledge that there is a box nearby. The *no-theory-of-mind* explanation is that Sarah chooses the behavior that would constitute a solution for *her,* given *her* purposes and knowledge, which she ascribes neither to herself nor to the human. (I do not know where in this classificatory scheme to locate the "empathy" explanation considered by the authors.)

P&W fail to consider the no-theory-of-mind explanation because they erroneously assume that the associationist explanation is the competitor of the theory-of-mind explanation. They thus run the two levels together, treating the associationist and no-theory-of-mind explanations as equivalent, and the nonassociationist and theory-of-mind explanations as equivalent. But these equivalences do not obtain. The nonassociationist, who hypothesizes rules in Sarah that generate her completion of allegedly novel behavior sequences, can consistently maintain that Sarah does not ascribe purposes, knowledge, or any other mental states to herself or to others. The associationist, who holds that Sarah completes novel behavioral sequences by "generalizing" previously experienced sequences, can consistently maintain that Sarah ascribes purposes and knowledge to herself and to others. He must, of course, provide a suitable associationistic analysis of these mental states; but having done so, he can consistently maintain that Sarah ascribes these states to herself and others (though Sarah does not understand the analysis).

Main comment. It seems obvious to me that chimpanzees, and many animals less like humans than chimpanzees, ascribe purposes, knowledge, and feelings to others, and sometimes to themselves. The cat believes that *I intend* to scold her, for she knows that *I believe* that *she intended* to take the ham slice on the counter, and that *I want* her not to take it. (The italicized expressions indicate the cat's ascriptions of mental states: other-ascriptions, where the expression contains "I", self-ascriptions where it contains "she".) And that is why she cringes as I enter the room. Most persons well acquainted with domesticated animals will find such explanations of animal behavior entirely natural. Why, then, do our authors find it necessary to deploy their considerable experimental resources to prove what must seem obvious to them – that chimpanzees do ascribe purposes, knowledge and other mental states to themselves and to other animals?

The main reason, I think, is connected with the confusion of levels discussed above. P&W assume that their result is incompatible with the (still powerful, though declining) behaviorist-associationist theory of mind. I offer some remarks on the possible sources of and errors in this assumption.

Behaviorist associationism defines purposes, knowledge, and other mental states as (learned, adaptive) dispositions to emit (sequences of) behavior and ideas. Behaviorism eliminates ideas from the definition – they, too, get defined as dispositions to behave – and notes that, since purposes and knowledge thus defined are theoretical entities (behaviors being the observables), they are no more introspected by the animal who has them than by the animal who infers them in others.

1. It may be supposed that behaviorist associationism denies the existence of purposes and knowledge, in the ordinary sense, and thus implies that Sarah cannot be ascribing these to anyone. Such an interpretation would be mistaken. Contemporary physics is a theory that defines such ordinary terms as "sour" and "yellow" in terms of pH level and light-wave frequency. It does not deny that lemons are sour and yellow, or that humans and animals ascribe sourness and yellowness to lemons. Similarly, behaviorist-associationism is a psychological theory that defines such ordinary terms as "want" and "know" in terms of dispositions to behave. It does not deny that animals want and know things, or

that chimpanzees ascribe purposes and knowledge to themselves and to others. Note that Sarah need not be a psychological theorist to ascribe mental states to animals, any more than she need be a physicist to ascribe sourness to lemons. Note also that if the behaviorist-associationist definitions implied that Sarah could not ascribe purposes and knowledge to animals, they would also imply that humans could not ascribe those states to animals.

2. It may be supposed that behaviorist associationism is a theory of the subhuman animal mind, not of the human, and that it countenances the following argument:

I, a human, know (directly) by introspection what purposes and knowledge are. So I am able to infer (i.e., know indirectly) that some other animals have these mental states. But Sarah, whose mind is described by the behaviorist-associationist view, cannot introspect her own purposes and knowledge, if indeed she has any at all. Hence, she does not know what these mental states are, and cannot infer that other animals have them.

Now, behaviorist associationism is a general theory of mind, applying to the minds of all animals, human as well as subhuman; and it does not countenance the argument above. It does maintain that neither Sarah nor I introspect our purposes and knowledge (these are unobservables). But neither Sarah nor I need know mental states by introspection to ascribe them to others: we can know them by inference, by constructing them from the behavior of others and (perhaps) our own.

3. It may be supposed that subhuman animals are only capable of observation, not of inference – at least not of inferences to the existence of unobservable, theoretical entities such as atoms and magnetic fields – and that subhuman animals are therefore incapable of ascribing unobservable, theoretical states to systems. This assumption, combined with the behaviorist-associationist view that mental states are unobservable, theoretical states, leads to the conclusion that Sarah cannot ascribe mental states to herself and to others. But the assumption is false. Although Sarah may be incapable of ascribing a magnetic field to the earth, she is obviously capable of ascribing purposes and knowledge to other animals. Perhaps psychological theoretical states are easier to grasp than physical theoretical states and entities. (I must confess that it is almost as easy to argue that the assumption is true, and behaviorist associationism partly false. For behaviorist associationism claims that the distinction between observable and theoretical entities is sharp and useful, and that mental states are theoretical entities; and both claims are controversial.)

It may be unfair to suspect P&W of harboring any of the assumptions discussed above (especially the first two). It would certainly be unfair to suggest that they have done nothing more than provide a question with an obvious answer.

For even if it is obvious that chimpanzees do ascribe purposes, knowledge, and other mental states to themselves and to others, it is by no means obvious how far this ability extends. Do chimpanzees ascribe lying, guessing (as opposed to predicting), embedded intentions, and so forth to others? Here P&W provide us with information and tentative answers which are genuine additions to the common stock of knowledge.

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Cooperation, cognition and communication. To a social scientist interested in the biology of human behavior, it is astounding that primatologists formulate their research inquiries in terms of such questions as whether "the chimpanzee's concept and use of tool names [provides] insight into the factors that determine and promote evolving, complex forms of *word usage*" – presumably among chimpanzees (SR&B, emphasis added); or "whether or not the chimpanzee imputes mental states to others," which evidently is deemed by its authors to be an operationalization of their speculation "that the chimpanzee may have a 'theory of mind,' . . . not markedly different from our own" (P&W). What is wrong with both questions is that they appraise the relative excellence of nonhuman cognitive abilities by measuring the extent to which these conform to those characteristics of our own species – which impresses me as a very unbiological approach. And that raises questions about what we should understand to be the relevant theoretical significance of what these human experimenters, as well as their chimpanzee subjects, are doing.

I shall undertake to comment upon each paper in turn, but before I do so, two prefatory caveats are needed. Whatever may be true of chimpanzees, it certainly is the case that humans can and do make many imputations in complete innocence of any kind of cognitive theory, to say nothing of a theory of mind. On the other hand, theory of the human mind has yet to achieve the degrees of cohesion, integration, and consensus meet to its functioning as the criterion for modeling the cognitive processes of other species, at least at the grandiose level of "theory of mind" and "states of mind."

Training primates to cooperate [SR&B]. "Words" are elements of either human speech or else human written language; and their postulation as a variable in chimpanzee cognition is neither necessary nor sufficient, nor indeed even helpful, to the elucidation of how two such animals were trained to cooperate in the sharing of tools and food. The latter kinds of social behavior were surely critical in the evolution of hominids and eventually the human species, but there is no evidence that either tool or food sharing depended or depends upon the prior acquisition of verbal language; on the contrary, language probably developed to meet the social needs of bands of protohumans whose ancestors already had been sharing tools and food for millions of years (Lancaster, 1975, p. 78; Pfeiffer, 1977, p.50). Moreover, both tool and food sharing sometimes occur within feral bands of chimpanzees (Lancaster, 1975, p. 77; McGrew, 1977 *op. cit.* SR&B; Wilson, 1975, pp. 128, 207; Pfeiffer, 1977, pp. 48–49), so the training here enhances what are already evolved abilities (if not traits) of *Pan Troglodytes*.

SR&B state that Austin and Sherman "distinguished these *words,* as well as additional tool *words*" (emphasis added); and I take exception to this kind of linguistic anthropomorphizing, particularly in a paper that purports to discuss cross-species language transfer. The paper is replete with statements that exemplify my point, such as several in its conclusion: "the chimpanzee's comprehension of single words" is discussed there, together with "word acquisition" by chimpanzees, and then the remark about "complex forms of word usage" quoted in the opening sentence of my introduction. This kind of attribution of "word" as a concept of chimpanzee cognition may not be characteristic of the authors' own thinking but it certainly permeates their discussion here.[1] Yet it is abundantly clear from the data presented that what Austin and Sherman responded to, and what they initiated their other nonverbal (including oral nonspeech) communications with, were the lexigrams of the customized computer terminal that was their constant robot companion. These lexigrams are symbols, color-coded combinations of from one to four among nine design elements, all linguistically arbitrary from the perspective of human language; but they are not words in either English or any other natural language. As native speakers of the English language, SR&B apparently found it easiest to transliterate the lexigrams, which identify the keys that symbolize the computer language that the keys activated, to the English in which it is most probable that they thought about the behaviors with which the lexigrams had been associated by or for them. The lexigrams were words to the humans who directed and participated in the research, and who had a sophisticated understanding of the artificial Yerkes language and the computer programming that some of them had spent much time and effort to develop, as the medium through which chimpanzee-human communication might better take place.[2]

But irrespective of whether chimpanzees have a "theory of mind," no evidence is presented to suggest that chimpanzees have a theory of *computer*-minds. Pushing a lexigram key, and monitoring one's own (or another's) performance by observing a lighted display of the same lexigram, is a function qualitatively indistinguishable from that of pigeons pecking or rats pressing levers to obtain food (or surcease, or whatever). The observation and interpretation of more than one lighted lexigram, in sequence and in context, certainly does have implications of notable import for a theory of chimpanzee cognition; but the chimpanzee's role in the interaction process neither requires nor entails understanding on its part about how computers work. The Cs (chimpanzees) know lexigram symbols as identification signs not atypical of the strange, mechanically human environment in which they find and adapt themselves. It is not helpful, in discussing their behavior in so doing, to attribute to them the cognitive concepts of natural language.

An explicit demonstration of the kind of unnecessary difficulties that the SR&B perspective on words invites is found in their having attached, for their own purposes (and conceivably, psychic benefits), the word "straw" to the piece of plastic tubing that is photographed in their Figure 1. This word usage, I submit, is not merely semantically incorrect; what is worse, it is patently an affectation. It is semantically incorrect, because flexible plastic tubing is not referred to as "straw," at least not by native speakers of the English language. It is true that a tertiary meaning of "straw" is a hollow paper tube, used in drinking certain beverages such as an ice cream soda; but the primary meaning is a single stalk or stem, especially of certain pieces of grain. It is an affectation, because the latent image implied by the use of the word, in the context of the research, is that of a feral chimpanzee, indulging in the frequently reported tool-behavior of using a plant stem, twig, or long stick, to dip for termites or ants. This is a natural tool-use by the animal (McGrew, 1977 *op. cit.* SR&B). So here we have an example of what Kenneth Burke (1937) once called "secular prayer," as SR&B – whether intentionally or not – cash in on the overtones of the word, at least for other primatologists (although probably not for Austin or Sherman). Calling their plastic tubing a straw permits SR&B to take a free ride in the minds of their human readers, on the associated idea of how natural it is for the research animals to be using the tubing. Of course, Austin and Sherman could not care less whether or not SR&B call a spade a spade; they would have responded no differently if SR&B had decided to call the tubing "snake" instead of "straw" – unless and until, of course, both the analogy and the research were extended to include live representatives of the order *Ophidia*. It would probably have been much simpler, as well as more accurate, to have just called the plastic tubing "plastic tubing."

There is other evidence of a proclivity for hyperbole, as in SR&B's claim that if Austin and Sherman "could comprehend the function and *intentionality* of their communications and, through joint symbolic communication, *share* their access to tools and the food obtained through tool use – then, by all definitions of human culture, they would surely have taken a large step" (emphasis in original). This claim of "all" definitions of human culture is both literally and figuratively a gross exaggeration. Even a rudimentary acquaintance with cultural anthropology shows that not only many, but indeed most, definitions of human culture demand more than the sharing of tools and food through nonverbal communication – which is what Austin and Sherman did, no matter what "words" were attached to lexigrams by the experimenters. What cultural anthropologists typically mean by culture is an elaborate complex of cognitive associations, together with both linguistic and other symbolic representations thereof, built upon the use of natural human language. A critical element in anthropological definitions of culture, which is both conspicuously and oddly missing from the one proffered by SR&B, is evidence of transferability from one generation to the next. It may be that Austin and Sherman will succeed by some means of social communication in teaching their progeny how to cooperate in the sharing of tools and food obtained through their use. But they haven't done so yet, and the requirement that they succeed in doing so is a minimal element in most definitions of human culture. We surely can speak of the culture of preverbal hominids, or of other primates, or indeed of other animals (Mainardi, 1979); but to identify with *all* definitions of culture the exchange of tools and food through the use of nonverbal communication is unacceptable to most anthropologists (Hall and Sharp, 1978; Weiss, 1973; Chapple, 1970), to say nothing of other social scientists or humanists (who think that they also know something about the meaning of culture).

A more serious problem concerns the administration of the major test. In part the exciting questions raised are rhetorical: "could [Austin and Sherman] perceive the necessity of requesting tools from one another?" This question is not answered by the tasks in fact performed because the experimenters evidently chose not to be daring enough. What would have happened if, after the animals had been trained in the requisite skills and placed in the experimental situation, one crucial variable had been omitted: the presence of the experimenter in the baited room? The chimpanzees' performance would have been extraordinarily more impressive if they themselves were able to figure out, interactively with the computer and each other, how to solve their problem. Such a performance would indeed have answered the question raised by the experimenters about the test that they in fact administered; but then one more thing would have been essential. It is remarked that before the test situation, Austin and Sherman "had never observed one another use tools or employ tool symbols to request tools, and thus had no reason to presume that the other animal knew and used such symbols or would cooperate with requests for tools." No explanation for such a research policy is given; one infers that an assumption was made that such foreknowledge would "contaminate" the experiment, but it is by no means apparent why this would have been so. All social skills are learned and shared through social interaction by feral bands of chimpanzees; and it would be unnatural in the extreme for one such animal to be able to use tools (or do anything else) without some other animal in the band having observed his behavior. I think that a mistake was made in the research design, at precisely this point. It would have been much better to have gone the other way and socialized this pair of animals in their reciprocal knowledge about skills in the requesting and use of tools in interaction with the experimenters; and *then* to have set up the crucial experiment to ascertain whether they could voluntarily transfer that knowledge and induce equivalent interaction with each other. If that stage of the experiment failed, even after a fair test with role reversals and an adequate number of trials, it would still have been possible to undertake the explicitly directed training in cooperation that in fact was done. Indeed, it is entirely possible that the principal accomplishment of that training was to show the two chimpanzees each other's abilities to use tools and to use the keyboard symbols to request them.

One's puzzlement that these animals were never given a chance to show what they might do is enhanced by noting Menzel's remark "that there is already good evidence that group-living chimpanzees are capable of communicating a good bit of information to each other about the environment even without the benefit of extensive human training" (1978, p. 891; 1973a *op. cit.* SR&B). The spontaneity of such "cultural" innovations as sweet-potato washing and grain cleansing by *Macaca fuscata* (Itani and Nishimura, 1973) is an example familiar to thousands of undergraduates through courses in comparative psychology and introductory ethology; and because *Pan Troglodytes* is considered to be more intelligent than the macaques (Chevalier-Skolnikoff, 1977), grounds for the missing experiment in serendipity surely were present in the professional literature of primatology (cf. Menzel, 1972 *op. cit.* SR&B). However, as Menzel (1978, p. 890) has remarked, "The problem of 'animal genius' has . . . received almost no scientific attention . . . [although] I have been repeatedly impressed . . . by how often one can find a single odd-ball monkey that does something quite out of the ordinary for other members of [the same] population. . . . [But the] spread of a behaviour requires good receivers as well as good senders, and for this reason the group-as-a-whole went nowhere."

This leads us into a closely related point, not discussed by SR&B but manifest in their data: the significance of individual differences in the intelligence (Gibson, 1977) of these juvenile research animals. SR&B do recognize that "Austin . . . did attend to the tools closely from the beginning" and "Austin initially generalized from food-naming skills more rapidly than Sherman" and "Austin attended closely to E's statements and did well from the beginning of this task"; and Table 2 certainly supports these appraisals. But Austin is one year *younger* than Sherman; and it is therefore unlikely that the difference in learning speed between them can be explained on developmental grounds, or at least those of age. It is true that other differentials in the training or experience of the two animals might be sufficient to account for Austin's superiority (Chevalier-Skolnikoff, 1977); but no hint of such information appears in this paper. On the evidence presented, Austin is a more intelligent chimpanzee than Sherman; and if this is true, that finding could have been – but apparently was not – used in the design of the "tool transfer" tests, irrespective of whether these were to be used, as I have argued they should have been, for an exploration of the ingenuity and intelligence of these animals. Intelligence could have been a useful variable even if the Cs were only going to be taught the standard operating procedure, for which purpose it would seem reasonable to have anticipated a higher standard of performance from Austin, both in speed and accuracy, than from Sherman.

SR&B claim that their tests are "blind"; but to what extent does this appear to be true? Implicit in the role of the (unidentified) experimenters during various of the tests are two problems. One concerns possible differences in the emotional attachment of each of the two Cs, towards each of the Es. The other is a Clever Hans problem, and involves the possible effects of unwitting nonverbal cues, communicated differently but perhaps consistently by different E's. SR&B acknowledge that "Changes in personnel inevitably resulted in performance decrement at all stages of training"; and this implies not only that there was some information loss (mostly, no doubt, nonverbal) when one E replaced another; it implies also that different cues were communicated by different Es. P&W point out in their paper that "Sarah's choice was affected by the actor's identity"; and their paper reports details of the results of her fondness for her regular trainer and her lesser affection for Keith's substitute. We are not informed about the dispositions of either Austin or Sherman toward E_1 and E_2; nor do we know the identity of the experimenters in the roles discussed in the reports of the tests. Randomizing the assignment of E's, as well as reversing Cs, in the test roles during their repetition, would at least have tended to control for whatever effect differential attachments may have had. SR&B state that "Both animals did very well on these tasks (Table 3), thus demonstrating that their abilities were not dependent upon cues from E." But aren't affective cues "cues"? Moreover, "In the naming task, E again stood outside the room and held up a tool so that it was visible to C through a lexan wall, although neither C nor E could see one another." But if C could see the tool well enough to distinguish it, he could also see at least part of E's hand, and perhaps part of his arm(s) too. How much more information does a chimpanzee need to identify which human (among the small sample of available alternatives) he was dealing with? There is also an entailed problem of equity in the invocation of standards for appraising the reliability of these particular research results. For reasons that are not divulged, each of the blind tests "was administered only once." Of course, this magnified the opportunity for either of the two types of extraneous communication discussed above to have influenced the results; but there is the additional consideration that the Gardners are criticized for their acceptance of "a weaker criterion of initial acquisition . . . [with only] *one* correct spontaneous usage per day," the apparent point being that little confidence should be placed in data of such putatively low reliability. Evidently, what is sauce for the goose is not sauce for the Gardners.

SR&B recognize that there is an obvious alternative explanation, and one that ought to be preferred on philosophy-of-science and methodological grounds: that Austin and Sherman cooperated as they had been trained to do, and after they were trained to so behave, by the experimenters. Perhaps, say SR&B, Austin and Sherman's "use of the keyboard merely reflected the continuance of behaviors that they had been conditioned to emit by E"; but this straw man is immediately knocked down, or so the authors presume, by the next sentence and those that follow it. They remark that "Observations of trials on which Cs were in error suggested that this was not the case" – but the adjective "anecdotal" is omitted from the pride of place that it evidently deserves in the sentence quoted, because all of the evidence presented is precisely that. If these authors are to discount the research findings of other primatological linguists on the grounds that the evidence supporting the latters' work is merely anecdotal, as they do in their critiques of other projects, there seems no reason why we should accept a lesser standard of proof for their own claims.

Do Premack and Woodruff have a theory of mind [P&W]? Theories of mind get pretty esoteric even for human subjects; and they are more so in relation to a species whose natural language is at the more difficult level (Menzel, 1978, p. 891) of nonverbal communication. Having endeavored to employ a theory of mind to study certain social behaviors of elite human political decision-makers (Schubert, 1965, 1974), I am convinced that there must be a more parsimonious way to tune in on the cognitive signification of a particular chimpanzee's photographic preferences, without vaulting to the more transcendental levels of cognitive theory.

There is at least a logical problem with the explication of the theory profferred (P&W: "can be understood"), as a possible "explanation" of Sarah's photo choices. The authors' preferred "theory of mind" is defined as Sarah's imputation of "at least two states of mind to the human actor, namely, intention or purpose on the one hand, and knowledge or belief on the other." Yet in the concluding remarks, in the context of further speculations about the possible findings from as yet unanalyzed data comparing chimpanzees with both normal and retarded children, it is conceded that chimpanzees may well be *in*capable of making imputations about knowledge. On the basis of the data reported, therefore, it is more parsimonious to reject these authors' "explanation" of Sarah's behavior, in favor of the one more consistent with the authors' own supportable imputation about chimpanzee minds: that Sarah was guided in her choices primarily by her affective stance towards the human actors.

Alternatively, and these authors to the contrary notwithstanding, it remains possible that Sarah's discriminations were based on what is described as the simple matching of physical elements. It is conceded that one of her three correct choices might be explained by physical matching (the more upright posture of the actor); but it is argued that the same explanation cannot account for her other two correct choices, *because the actor's posture was not upright in them.* Surely this misses the point of what is implied by "the same explanation." Because P&W's own discussion is anecdotal, it seems fair to point out that there are an infinite number of other possible physical matches between the content of the photographs and the videotape, depending upon what is perceived to be relevant and important; and it is Sarah's perceptions, not the authors', that count. But even if no such unwitting cues were present (perceived by Sarah) in either of her other two correct choices, her discrimination (which P&W call, her "comprehension of problem solving") is down to two right and two wrong for this series, on P&W's own concession; and success ratios that match chance are not impressive evidence of comprehension. Nor does the remark, that "physical matching is ruled out . . . even more [for other series to be reported] later," save *this* series. It may well be that "chimpanzees . . . can solve problems with strategies more sophisticated than simply matching physically identical or similar items"; it may also well be that, not unlike humans, chimpanzees do things the simple, easy way when they can. In any event, we ought to assume that if Sarah could have solved these problems by physical matching she did so, leaving her keepers to worry about the complexity of the primate mind.

The evolutionary continuity of communication [G]. I associate myself wholeheartedly with the general tenor of G's thesis. In particular, I agree with his admonition that "Defining mental experiences as uniquely human discourages inquiry into the possibility of their occurrence in other species"; and his suggestion of the more useful hypothesis "that thinking and experiencing are related in comparable ways to the functioning of central nervous systems in various species." Such cross-species analyses of psychoneural relationships would, of course, need to be done in the context of all that we know about the brain systems as well as the behavior systems of each species concerned, pursuant to Menzel's advice (1978, p. 892) that in "conducting a field study of communication" among chimpanzees, one should "start at the level of general cognitive, societal, or even ecological considerations and then work backwards towards the data that might be of more concern to" cognitive ethologists. Furthermore, "communication is part of the general information-processing activities of an organism . . . [and] beneath the 'deep structure' of human language and human thought there are indeed 'deep-deep' structures that we share with other species, and . . . it is on these structures that our linguistic abilities are predicated" (Menzel and Johnson, 1976 *op. cit.* G, p. 140).

The qualitative evolutionary continuity discussed by G refers to development that is species-specific consequent to divergence, even for such closely related species as humans and chimpanzees. To take as an example our own experience, for which the evidence of dynamic change during the past ten million years is more impressive than that for chimpanzees, we can distinguish among three levels of communicative ability: nonverbal, verbal speech, and written language. We can appraise these three modes of human communication in terms of two dimensions: (1) recency of evolution; and (2) relative efficiency (and/or use) as a carrier of affective, as distinguished from effective, messages. The ordinality of these modes is indubitable, and even the interval estimates for the first dimension are sufficiently disparate to preclude extended discussion for present purposes. Nonverbal communication developed prior to verbal among humans, and at least some aspects of it are very much older than the ten million years to which our knowledge about some characteristics of protohominids can arguably be stretched. Oral speech evolved much more recently, perhaps a million to half a million years ago, and in phase with the doubling in size of the brain. At least on our evolutionary time scale, written language is an exceptionally recent acquisition and probably it is less than ten thousand years old; the earliest known writing dates from 5500 B.C., but that threshhold may well be pushed back by future discovery and research. Conversely, and although their number and variety are diminishing rapidly, many bands of preliterate, human primitive societies have been studied throughout the present century: all of these lacked written language, but were articulate in both verbal speech and nonverbal communication. Our interest in, and possible admiration for, the adaptive or esthetic virtues of such other modes of animal communication as whale or bird song, or bee dancing, cannot sway our appraisal that all of these – indeed, all known modes of nonhuman communication – are nonverbal, at least in the sense of written language. On the other hand, many mammalian oral communications, and certainly those of many canids (Harrington and Mech, 1978) as well as nonhuman primates, may very well exemplify some of the cross-species psychoneural relationships adumbrated by G. Certainly, we should anticipate at least as broad a spectrum of such relationships for nonverbal human communications, because of the potentially larger number of homologous species with which they might be shared.

MacLean's model of the brain (1958; and cf. Lancaster, 1975, p. 62) implies that affective messages must have been communicated for a very long time before any animals had developed brains of sufficient complexity to receive or transmit messages with effective content. Perhaps this is what P&W mean when they say that "motivational states seem more primitive than cognitive ones" and that "inferences about motivation will precede those about knowledge, both across species and across developmental stages." Assuming that to be true, we might hypothesize that the ratio of affective to effective message content will in general be maximal for nonverbal human communication, and minimal for messages in written language, with verbal speech in the middle;[3] but that components of *both* affective *and* effective content usually will be present in all three modes for human communication. Certainly the hypothesis is not inconsistent with such observations as that "The nonverbal communication of nonhuman primates has traditionally been characterized as species-specific, emotionally-based, and nonintentional" (SR&B); or that "there is little evidence that the ability to devise non-verbal codes derives from the prior possession of verbal ones" (Menzel, 1978, p. 890).

G certainly is correct that "ethology has made a contribution of fundamental importance by discovering a rich variety of nonverbal communication in many kinds of animals," and "many social animals communicate by systematic codes which convey information and often lead to predictable changes in the behavior of the animal receiving the message." We now have available more than a generation's such work from the students of Tinbergen alone, as exemplified by the sophisticated models of social communication constructed by the first of these students, based upon a lifetime of field studies of herring gulls and related species (Baerends, 1975, 1976a, 1976b); and also by the flourishing of studies of human nonverbal communication (Hinde, 1972; Argyle, 1975; Key, 1979; Morris, 1977; von Cranach, in press). There is even a beginning of studies that undertake to analyze all three modes of human communication simultaneously, as diverse and interacting dimensions of the same social communications (Schubert, in press).

To the extent that we can study the communicative abilities of chimpanzees and other animals, with our own minds open to the possibility that in the process of so doing we may learn as much about our own modes of communication as we do about theirs, we can begin to explore the evolutionary continuity of mental experience that is the subtitle of Griffin's book (1976 *op. cit.* G, SR&B) and that he has postulated to be the proper task of cognitive ethology.

NOTES

1. The Rumbaughs are much more careful to distinguish between words and lexigrams in their concluding chapter to the book that they edited (Savage-Rumbaugh and Rumbaugh, 1977); so it may be that what we confront in the present paper is a writing problem – but that makes it no less a problem. The difference between Yerkish lexigrams and English words is explicitly discussed on p. 96–97, and the design of lexigrams is explained in detail on p. 92–95, in von Glasersfeld (1977).

2. All three of the authors, plus several other researchers involved in the design or administration of the LANA project, spoke at a symposium on "Language Formation Studies with Apes and Children," on September 7, 1978, at the second annual meeting of the American Society of Primatologists convening on the campus of Emory University. I was present and heard the presentations.

3. The modal qualities of speech are illustrated by voice stress analysis (e.g., Wiegele, 1978), which provides information about emotional arousal that is manifest in speech even though undetectable in written transcripts of the same communications.

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Fantasy and communication. I am delighted to have the opportunity to comment on this series of papers on the inner life of nonhuman species, but in doing so I shall start from a somewhat different frame of reference. If any general conclusions can be drawn from human subjective reports, it is that the inner world of humans is only indirectly and remotely related to the real world around them. We do react to the stimuli in the surrounding environment, but most of our time is spent in fantasy, thinking about things that have been, might be, or never will be. Most of us keep this process under control, but some do not, and fantasy has consequently been associated principally with maladaptive behavior, and as such has been much studied by psychiatrists and clinical psychologists. Because of its association with madness, fantasy has connotations of uselessness. I suggest, on the contrary, that this kind of activity is extraordinarily adaptive, enabling us to solve problems more efficiently and directly than by trial and error, and to create new, useful, and enjoyable objects and activities.

One of the side effects of the evolution of human language is that it makes it possible for one person to share his fantasies with another, in stories, through the written word, through television, through music, and other forms of artistic expression. Obviously, living in someone else's fantasy world is highly rewarding to most of us. Objectively considered, even this present exchange of scientific communication is a method of sharing our various fantasy worlds.

What is the nature of fantasy? Subjectively reported, some of it is verbal. We speak, listen, and even write in imagination. However, this is not all. A very important part of human fantasy is visualization; we actually see things in imagination. Similarly, we can create things and events through any sensory modality: hearing, touch, smell, kinetic sensation, or any combination of these. With this process we solve problems, we create new things, and we activate ourselves, responding to much more than immediately concurrent stimuli. All this is part of being an organism with the properties of a living system rather than a mechanism.

These observations raise the problem of whether similar capacities are present in other species than man. First, does any nonhuman species possess all the capacities necessary for verbal language? The answer to this question is no; there is no other talking animal, although the research with anthropoids reported by SR&B shows that our close biological relatives have, at least in latent and primitive form, capacities to manipulate symbols similar to those capacities that may have preadapted our own species to evolve verbal language.

A second question is, Do other animals have nonverbal capacities to fantasize, such as visualization? The answer is probably yes, from the evidence of the complex problem-solving abilities that have been well demonstrated in a variety of nonhuman research animals, including the primates investigated by P&W.

A third question is whether or not nonhumans can communicate their inner fantasy world, either within species or across species. Could a chimpanzee, for example, given sufficient training in manipulating symbols, communicate to a human experimenter a "thought" independently of immediate stimulation from external sources and internal needs? Finding the answer to this question is the most difficult of all, as it must involve a great deal of indirect inference and subjective judgment.

The three papers in this series comprise three quite different approaches to this general problem of the inner world of nonhuman animals. That by SR&B is a straightforward attempt to extend the capacities of symbolic transfer of information in chimpanzees to a social situation involving two chimpanzees and includes an excellent critical review of the field. Their demonstration of communication between two trained chimpanzees brings up the question of whether this ability is ever used by chimpanzees in natural situations. I am aware of no reports of chimpanzees or gorillas sitting around and exchanging reciprocal series of symbols, but this may have simply been overlooked.

The two papers by P&W and G are different in that they make frequent use of the concepts of "mind" and "mental." Historically, these concepts are a part of philosophical dualism, usually attributed to Descartes, but also part of our cultural tendency to dichotomize and arrange phenomena into bipolar opposites. Whatever the limitations of the behaviorists, they at least enabled us to escape from this straitjacket. I can do no better than to repeat the remark of Robert M. Yerkes, who once said, "I never saw a disembodied mind."

The paper by P&W comes the closest to dealing with the third problem listed above, namely the transfer of the inner world of images, but it becomes confused with the philosophical problem of what is consciousness [see also Puccetti & Dykes: "Sensory Cortex and the Mind-Brain Problem" *BBS* 1(3) 1978]. To me, consciousness presents no great conceptual difficulty – it simply means that one is able to put something into words and so have a dual or multiple awareness of a particular phenomenon. P&W directly approach this problem in nonhumans, attributing human verbal concepts to animals that we know do not have words. While it is not impossible that a chimpanzee could somehow have acquired such concepts, the notion is inherently a dubious one. This does not exclude the possibility that animals could be conscious in the sense of being able to separate various nonverbal sorts of fantasy modalities.

G presents a stimulating call for ethologists to attack the problem of cognition in nonhumans. There are two cases of communication that approach that of humans – the dancing bees and the signing apes. Of the two, bee language is like human language in that bees can convey information about a third object to another bee, something which is yet to be demonstrated in field studies of primates. G feels that this evidence justifies our looking for unknown inner phenomena in the nonhumans, and I agree. But insofar as he has fallen into the trap of mentalism, his plea is likely to be rejected. Whatever the inner life of other animals, it is going to be different from that of man. Species do not evolve in parallel but divergently.

As a minor point, G uses the word "institution" in a sense that departs from its usual meaning with respect to humans. As used by sociologists, human institutions such as religion, government, and so forth, are supra-organizations based on verbal language and culture, and for which it would be impossible to find analogues in nonhuman societies.

It is obvious that this field of inquiry lends itself to controversy. By its nature, it rests on subjective inferences concerning human nature. If what I have said about the creative nature of the human internal world is correct, it follows that subjective inferences will vary from one individual to another. Finally, I am in favor of fantasy (suitably controlled) in science. It has always been fascinating to speculate concerning the inner world of other animals, and I hope these papers will start us on the way to something more than speculation. And, whatever we find about the nonhumans, this kind of research will lead us to know more about ourselves.

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Does mind matter? The December 5, 1978 *San Diego Union* ran the following episode of the comic strip "Tucker" by Joe Martin. Two men are seated facing one another. One has an animal (probably a chimpanzee) on his lap. Behind the man with the chimpanzee stands a third man, looking on. The following conversation occurs:

Man with chimpanzee: I've spent 10 years trying to prove that man could communicate with the apes, only to find their level of intelligence so hopelessly below humans that I must abandon research and seek new work.

Second seated man: Gee, that's too bad.

Chimpanzee: Grrbnit.

Onlooker: (looking at his watch) Almost 10:30.

How shall we interpret the onlooker's remark? Was he Natural Man, understanding the chimpanzee's utterance where the foolish scientist (probably a positivist behaviorist) had lost the natural ability to do so? Or was he engaging in "rich interpretation" (Brown, 1973) of the chimpanzee's vocal noises, imputing to the chimpanzee his own state of mind (perhaps it was time for his mid-morning coffee break)?

But then, do we need to impute a state of mind to the onlooker? Harzem and Miles (1978), in a recent discussion of philosophical issues in behaviorism, remind us of Wittgenstein's "polar principle" – "Since concepts distinguish, they divide material in at least a two-fold way. Thus if one characterizes something as being 'X' one is thereby excluding the thesis that it is 'not-X.' . . . The polar principle, then, invites us to ask the question, 'As opposed to what?' "

What would it mean to say that the chimpanzee, or the human onlooker, does not have a theory of mind? Theory of mind as opposed to what? What would it mean to say that the chimpanzee, and the human onlooker, do not have minds at all? Minds as opposed to what?

I am enormously impressed with the experiments that P&W and SR&B describe in their papers. They have uncovered, or instilled (as the case may be), remarkably complex behavior in their chimpanzees. The work that these investigators have been doing over the past ten years has revealed an affinity between ape and human that was hardly suspected earlier. But, as a thoroughly indoctrinated positivist behaviorist, what I do not understand is the necessity for ascribing a mind, or a theory of mind, either to human or to chimpanzee. How would our understanding of brain and behavioral processes be different if we did not do so?

Reading G's paper, I think I glimpse the key to my puzzlement. G, and P&W, appear to be using mind, awareness, theory of mind, mental states, and the like, as causal terms. But surely mind, if it refers to anything, simply names the complex functioning of an organism with a complex brain, so complex that subtle relations among stimulus events in the environment are able to trigger correspondingly complex (brain-mediated) behavior? I do not doubt that Sarah, viewing the trainer's videotaped predicament, selects the photograph that depicts the correct solution. I do not doubt that Austin and Sherman have learned the value of sharing the loot that speaker-listener cooperation has enabled them to obtain. The information-processing mechanisms of the chimpanzee brain must be on a scale of complexity comparable to the human's. To express one's appreciation of such behavior by awarding it the epithet of mind is at least excusable. What does not seem to me excusable is the intimation that talk of mental states or awareness *explains* the chimpanzee's or the human's information-processing mechanisms, or how those mechanisms govern overt behavior. Contemporary psychology appears to want to call back the ghost in the machine, as contemporary society longs to call back God. I understand that a *belief* in spirits of one or another sort may affect both one's everyday behavior and one's scientific behavior, but I cannot believe that *spirits* do. If they do, then what we need is a theology of chimpanzee and human, with P&W and G showing us the way to Revelation. For myself, I prefer to go on studying and cataloguing the functioning of complicated organisms in interaction with their complicated environments, and to admit that I haven't a clue how they do it. My faith is that the answer lies in the brain, not the mind.

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EDITORIAL NOTE

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What about their internal languages? 1. *Fun, but so what?* All three papers are, of course, fascinating sources of information about some of the things chimpanzees and bees can do. It is comforting to have rigorous laboratory observations to reinforce and augment what every dog-owner or circus-goer knows about the rich mental life of other animals. No doubt anecdotal evidence cannot be as important for the advance of science as papers that resound with phrases such as "the altered performance was again accompanied by a noticeable orientation of the attentional response" (SR&B). Behaviourists will surely cower and tremble before P&W's ingenious use of the antibehaviourist beliefs of apes! And, finally, we must welcome G's use of philosophy to push away some behaviourist barriers to scientific insight, even if both mentalists and behaviourists prove unable to provide theories with deep explanatory power.

Yet, despite their virtues, and the evident satisfaction with which these authors put down their intellectual rivals, I find something sadly lacking: an awareness of deep problems and a search for deep explanations. I'll try to enlarge on this.

2. *Deep science and shallow science.* Looking back over the history of science we can distinguish three major types of advance:

1. the collection of facts expressible in previously available language,
2. the extension of our thinking powers by the creation of new concepts, taxonomies, symbolisms and inference techniques, and
3. the construction of generative explanatory theories about underlying mechanisms, a task that has much in common with engineering design.

The collection of facts (including laws and generalisations) is intrinsically interesting, and also important for the other two processes, since facts determine what it is that our theories need to explain and help to show up inadequacies in our descriptive and explanatory resources, and they may even be of great practical value. But facts alone give us no new understanding, no new insight into underlying mechanisms, no new ways of thinking about old phenomena.

Are the authors of these papers merely concerned to collect facts? Clearly not: they are also deeply concerned to learn the extent of man's uniqueness in the animal world, to refute behaviourism, and to replace anecdote with experimental rigour. But what do they have to say to someone who doesn't care whether humans are unique, who believes that behaviourism is either an irrefutible collection of tautologies or a dead horse, and who is already deeply impressed by the abilities of cats, dogs, chimps, and other animals, but who constantly wonders: How do they do it?

This is the sort of question which is at the heart of all science (and philosophy), namely: "How is this possible?" For example: How is it possible for a chimp to interpret flat pictures as representing three-dimensional scenes involving agents with purposes (P&W)? How is it possible for a chimp to learn to fish for juice with a sponge on a string (SR&B)? How is it possible for a bee to find its way to a specific location (G)? How is it possible for a chimp to pull a blanket with just the right force to retrieve a ball? How is it possible for a chimp to learn that after being shown a videotape she is to select a photograph from a box left by the experimenter before he departs, then place the picture alongside the television set, and finally ring a bell to recall the experimenter (P&W)? How can a chimp realise that tapping another's hand is an adequate method of getting the latter to relinquish the straw through which *he* is sucking juice (SR&B figure 6e)? How does the other chimp know that this is the intention? How can they learn to push buttons? How can they remember where to look for the effects? How can they learn associations? How can they use them? How can they combine previously learnt actions into larger wholes (SR&B)? How can they use so-called iconic symbolism (SR&B)? How can they form beliefs? How can they react to unintended cues from experimenters? How can they form beliefs about the intentions or beliefs of others (all three papers)? How can they have likes or dislikes (P&W)?

Of course, all these, and myriad other questions suggested by the papers, may be asked about human beings too! But when the authors begin to broach such questions, for instance as G does at several points, they hardly seem able to go beyond antibehaviourist incantations that reiterate what has always been obvious to anyone with common sense: that animals have experiences, beliefs, hopes, fears, doubts, surprises, intentions, plans, and so on. As if we already knew how to explain these things, and the only problem was to collect more examples.

3. *Am I being unfair?* One cannot do everything, least of all in a short scientific paper, even a speculative one, like G's. Surely a scientist is entitled to choose an area of research and pursue it? Surely it is unreasonable for me to criticise these authors for not pursuing the questions that interest me? Perhaps, but I suspect that it is not merely a difference of interest that is at issue.

Although the questions I have formulated are still a long way from being answered, one thing seems clear from the few serious attempts that have been made to gain some insight into these matters: *all the abilities in question seem to depend on internal processes in which symbols of some kind are constructed, stored and manipulated: they use inner languages.*

For some first crude attempts at theorising about such internal processes see Miller et al. (1960), and more recent work in artificial intelligence reported in Sussman (1975), Winston (1975, 1977), Boden (1977), and Lindsay and Norman (1972). What sorts of internal symbolisms are required? How are they used? How are they acquired? Are very different kinds used for different purposes? How are they stored? How did they evolve? These are all basically unanswered questions. But if it is even remotely plausible that in order to perceive, learn, find their way around some terrain, form and execute intentions, and so forth, animals must make use of internal symbolisms, then surely one might expect discussions of the ability of apes to use *overt* languages (sign language, push-button language, gesture, or whatever) to be related to speculations about their *inner* linguistic competence?

The essence of language is often thought to be its use in communication. What I am saying is that there is a more fundamental class of uses – for storing information and procedures, and for making inferences, forming plans, guiding actions, and so forth. In every way this is more basic: it evolves earlier, it develops earlier in individuals, and it is a prerequisite for the overt use of language for communication.

This inner symbolic competence is clearly quite profound even in relatively unintelligent animals, to judge from the enormous difficulties artificial intelligence workers have experienced in their efforts to simulate apparently commonplace abilities. (I am not talking about physiological processes. Studying the physiology of a computer can tell you very little about the programs that run on it, since these may be radically altered without physiological change.) Could it not be the case that by theorising about such (mostly unconscious) inner symbol-manipulating processes (going far beyond traditional mentalists in precision and detail), we might be able to form a framework to guide research into the overt linguistic abilities of apes, and possibly other creatures? And then we shall not be dependent for our scientific motivation on a concern about the uniqueness of human beings, or semantic quibbles about the essence of language! Unless work on the behaviour of animals is placed in the context of attempts to theorise about underlying mechanisms, it is little more than ethological rubber-necking (often done very effectively in TV documentaries).

4. *On experimenting in the dark.* Insofar as claims are being made about controlled experiments, we have a right to ask "How do the experiments *work?*" A scientist should not be satisfied with an experiment using some complicated piece of apparatus whose behaviour he could not explain. Yet all the experiments described in these papers require the animals to deploy very complex cognitive skills: perceptual skills, learning skills, problem-solving skills, memory skills. Without the use of these skills, the animals would not be able to acquire or display the other skills that are explicitly being studied, such as the ability to use symbols in a cooperative situation, or the ability to think about somebody else's predicament. Why are the researchers content to study the latter skills without any theory of the mechanisms underlying the skills that are part of their experimental set-up? Without such understanding, the observed behaviour is subject to radical ambiguities of interpretation. How can we tell to what extent the experimentalists' descriptions are naively anthropomorphic?

I suspect that many experimenters are as unaware of the need for explanations of the kinds requested above as the child who feels no need for an explanation of why unsupported apples move downwards. For instance SR&B assume that "deferred imitation" might explain some of Washoe's behaviour, and P&W suggest that "physical matching" might be an explanation of some of Sarah's problem solving.

5. *Conclusion.* Of course I cannot now give explanations, and that is the main reason why my criticism is so unfair. But I have a strong suspicion that in the long run we shall all learn more if we spend a little less time collecting new curiosities and a little more time pondering the deeper questions. The latter are harder and don't generate publications so easily, but the questions are important and we need more good young scientists trained to think about them. The best method I know of is to explore attempts to design *working* systems that display the abilities we are trying to understand. Later, when we have a better idea of what the important theoretical problems are, we'll need to supplement this kind of research with more empirical studies (cf. Pylyshyn 1978).

Finally, an ethical comment. The discoveries reported in the papers by SR&B and P&W show that at least some apes have a profound potential which they cannot realise without human intervention. (It is not clear how far the use of computerised equipment is essential.) This is no different from the situation with humans: without the benefit of an elaborate culture a human child will not develop the ability to talk, to play or enjoy music, to solve mathematical problems, to puzzle about the workings of the human mind. It is now widely accepted that people have a *right* to the kind of education and social opportunities that will enable them to realise their potential – not all their potential of course, for instance not the potential to become vicious, which possibly lurks in all of us. Whether they have the right or not, it is clear that for the vast majority of human children the opportunities for development just are not available – and often the right is not recognised. But insofar as they have the right, it would seem that similar reasons exist for ascribing such a right to other animals. Where this argument ends I cannot tell, but at least it should be borne in mind by all who are interested in finding out just how much apes can be helped to become human.

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What's the matter with mind? The vehemence with which criticism has been directed toward *Griffin's* proposal that animals have a mental life and experience something akin to what we call "awareness" is simultaneously reasonable and puzzling. Understandably, behaviorism evolved as a historical antidote to the excesses of structuralist introspection in human psychology and to the excesses in attribution of mental qualities such as "the appreciation of music" to birds, the use of "tactics and strategems" to insects, and "heroism and patriotism" to dogs (Lindsay, 1888) common in the last century. Given the behaviorists' decision to study laboratory rats and pigeons in a highly restricted laboratory environment, evidence of behaviors that would allow one to intuit the existence of something akin to mental states in animals would be rare indeed.

However, if one considers that the study of mental states has again become a legitimate topic in psychology (masquerading as human information processing or cognitive psychology) or if one observes animals other than laboratory rats in natural environments or even in greatly enriched laboratory environments, it is puzzling that G's arguments should have provoked so much controversy [see Haugeland: "The Plausibility of Cognitivism" *BBS* 1(2) 1978).]

In a sense there is nothing really new about "cognitive ethology. Some mental processes of animals have been extensively studied over the past three decades. For example, psychophysical functions have been gathered in a large number of species in several sensory modalities, and a psychophysical function is a statement about the relationship between the physical world and the mental world [see Wasserman & Kong: Absolute Timing of Mental Activities" *BBS* 2(1) 1979]. Short-term and long-term memory processes have been extensively studied in animals using many of the same techniques used by cognitive psychologists to elucidate the structure of the human mind (Medin, Roberts, and Davis, 1976 *op. cit.* G). Even such mental states as "expectancies," "desires," and "intentions" can be studied objectively in animals following paradigms devised by Irwin (1971). The intellectual processing capacities of some animals have been shown to differ only in degree from those of human beings by a large variety of studies ranging from Köhler's to those of present-day chimpanzee researchers. Thus, there should be no difficulty for even the behaviorist to accept that certain aspects of the animal mind can be effectively studied.

What then is the problem? G is proposing that animals may experience awareness of self and of others, that they have intentions, and that they may experience emotions such as empathy, joy, or fear. There seems to be no *a priori* reason for rejecting these attributes of mind for nonhuman animals, while accepting the psychophysical and intellectual similarities of human and nonhuman animals. Yet questions of awareness, emotion, intentions, or consciousness seem to raise the behaviorist's hackles more than questions of perception, learning skills, tool use, and so forth. Perhaps it is the behaviorist's traditional subjects, white rats and pigeons, that impose blinders. After all, even if I accept the rat's perceptual or operant behavior as a model of similar processes in human beings, do I dare attribute consciousness, awareness, or empathy to a creature like that!

However, even casual observation of animals in more natural environments suggests, at least intuitively, some of the mental states that G argues exist in animals. Consider the following anecdotes: (1) On warm days a cat is put outside early in the morning. As the days become colder in the fall the cat disappears from sight somewhere between 8:15 and 8:45 A.M. If anyone searches for her, it is unlikely that she will be found in the same location on successive days. (2) An infant pygmy marmoset has been pestering its adolescent sibling for several minutes. The adolescent begins to move and emits calls that generally accompany movement to a distant part of the home range. The infant follows. The adolescent stops en route. The infant passes by and continues on to the other location. The adolescent ceases calling and returns to its original location without the infant. (3) Pygmy marmosets almost always give a call when approaching the feeding tree. Other feeding animals move away. Occasionally the animal fails to call and arrives at the tree unannounced. Invariably a brief fight ensues. (C. Snowdon and A. Hodun, unpublished observations.)

One can argue that each of these anecdotes provides presumptive evidence for qualities such as awareness of self, awareness of others, intentionality, and in the second case even deception. Mere anecdotes, however, fail to prove the existence of such mental states. In fact, claims of mental states for these animals may be subject to the same mistakes Lindsay and others made in the last century. Nevertheless, contrary to the argument of SR&B, anecdotal data and intuitions should not be rejected out of hand. The overwhelming weight of many such anecdotal observations has motivated the search for the existence of mental states in nonhuman animals (and not just in chimpanzees and honey bees), and subsequently, attempts to demonstrate formally that such states exist. (It is interesting to note that no primatologist with whom we have discussed G's proposals for animal awareness has objected to them. In fact the typical response is "So what else is new?")

How does one formally demonstrate mental states in nonhuman animals? G proposes that the chimpanzees who have learned an artificial communication system are the best subjects for confirming such mental states. On the other hand, the papers by *Savage-Rumbaugh et al.* and by P&W indicate that the artificial communication systems taught their chimpanzees may not be that useful for understanding the mind of the chimp. SR&B make much of the fact that chimps can exchange information and material through a symbolic communication system. Rather than illustrating notions of chimpanzee awareness, consciousness, and so forth, the studies seem rather to indicate that chimpanzees can be remarkably clever in obtaining food. In fact, obtaining food underlies all the communication that goes on in the Yerkes laboratory. All the conversations reported for Lana revolved around her obtaining food; otherwise she seemed uninterested in conversing (Rumbaugh, 1976 *op. cit.* SR&B). Likewise, Sherman and Austin only learn the names of objects that are useful for obtaining food. Given this concreteness about using symbols only when they will serve to obtain food, it is difficult to imagine a Yerkes chimp ever being able to learn the lexigrams needed to tell us about mental states. It would be interesting to know whether chimpanzees can acquire and use symbols that could express self-awareness, empathy, prevarication, and the like, or whether being able to communicate about mental states abstractly is one of the few points of uniqueness remaining to human beings. However, a symbolic communication system may not be necessary for the inference of mental states.

The most interesting point in the SR&B paper is the observation that the animals share food with each other. Since there is little or no evidence that chimpanzees ever share food in the wild, it is significant to find that the chimpanzee who obtains the food shares it with the one who provides the tool for obtaining the food. Unfortunately, no information is presented on the acquisition of food-sharing behavior. Did food sharing occur on the first trials or did one chimpanzee have to learn to share food with the other in order to have the other continue to provide tools? If the chimpanzee knew on the first occasion that he would have to share food with the tool provider to obtain continued cooperation, then that would be evidence that the chimpanzee has a theory of mind. It would be interesting to know whether tool exchange would still occur if food exchange were prohibited. So long as each chimpanzee had equal opportunity as tool provider and tool receiver, they would still receive equivalent amounts of food. However, the animals would have to develop a strategy based on anticipating future food availability and future dependence upon the other animal in order to be motivated to provide immediate assistance to the other animal in obtaining food. Such behavior would demonstrate a very sophisticated theory of mind. It is a pity that SR&B have restricted so much of their presentation to a critique of "linguistic" chimpanzees and a defense of their own methods. They have missed the fact that the issue has moved beyond one of linguistic competence to the broader issue of the structure of the chimpanzee mind.

Premack & Woodruff show that mental states in chimpanzees can be studied effectively. Such studies do not require a symbolic communication system nor do they require a dependence upon food reinforcement in order to demonstrate mental states. The studies of P&W are much more like those one would perform with human beings, and because a symbolic communication system is not necessary, their methods can also be applied to the study of mind in other species. On the basis of what P&W present, and on the gleanings from the SR&B experiments, there is evidence not only that our intuitions about mental states existing in nonhuman animals are likely to be accurate, but also that we can study such states in a rigorous fashion without repeating the errors of Lindsay and his followers.

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by **Stephen P. Stich***

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Cognition and content in nonhuman species. "What, if anything, do animals think about?" This is the question with which *Griffin* begins his paper. The thesis of my commentary is that G's question is badly cast, for it conflates a pair of questions that are best kept apart. His question suggests that if animals think at all, then they must think *about something.* And this, in turn, suggests that if we cannot say what an animal thinks or believes, then it is inappropriate to say what an animal thinks or believes, then it is inappropriate to say that the animal has thoughts or beliefs at all. On my view, the cognitive ethologist would do well to separate the question of *whether* animals think from the question of *what* they think, and thus leave open the possibility that certain species of animals are cognitive systems, though we cannot, in principle, say what it is they think. Put in another way, what I am urging is that we distinguish the question of whether an animal is a *cognitive system* [see Haugeland: "The Plausibility of Cognitivism" *BBS* 1(2) 1978] from the question of whether its cognitive states have an expressible *content,* and if so, what content they have. Let me elaborate on both these questions.

1. Are animals cognitive systems?

Our everyday vocabulary about mental states and processes gains much of its meaning from the role it plays in an informal and largely tacit theory, a folk psychology, which we regularly invoke to explain our own behavior and that of our fellows. Thus consider the following dialogue:

A: "Why did Nixon keep the Watergate tapes?"

B: "Because he believed that he could never be forced to surrender them, and he wanted to use them to write his memoirs. If he had believed that he would be ordered to turn them over, he surely would have burned them." Here a certain action or bit of behavior (Nixon's keeping the tapes) is explained by citing a belief (that he could never be forced to surrender them) and a want or desire (to use them to write his memoirs). Also, a counterfactual prediction is offered: had he had a different belief, he would have behaved differently.

The folk psychology of beliefs and desires has been the subject of considerable philosophical scrutiny (Dennett, 1969, 1978; Fodor, 1975; Goldman, 1970; Harman, 1973; Lewis, 1974; Stich, 1978a, 1978b, 1979a). The theory postulates two quite different types of psychological states, beliefs, and desires, with normal subjects having large numbers of each. It also postulates several additional mechanisms and systems which interact with a subject's store of beliefs and desires. They include the following:

i. the perceptual system(s), which serve to monitor the external and internal environment and to insert and delete beliefs from the belief store as the perceptual input changes;

ii. the inference mechanism, which generates new beliefs from old;

iii. the "practical reasoning" mechanism, which generates new desires from old desires in conjunction with beliefs (as when my desire to hear Solti conduct and my belief that he will be conducting at the Kennedy Center next Saturday generate the desire to go to the Kennedy Center next Saturday);

iv. the desire-interpreting mechanism(s), which convert certain desires (like the desire to move my left leg forward) into appropriate bodily motions.

There is much more that might be said about the various components of our folk-psychological theory. For the present, however, these brief remarks, along with figure 1, will, I hope, suffice to indicate something of the gross architecture of the theory.

Now I have claimed that folk psychology is the intuitive theory used by the ordinary man when offering common-sense explanations of the behavior of his fellows. But, of course, there is no guarantee that this folk theory is a *true* theory, or even a first approximation to a true theory, of the explanation of behavior. Indeed, it is just on this point that behaviorists and cognitivists divide. The behaviorist urges that there is no need to postulate anything that much resembles the elaborate and interacting systems of folk psychology, since behavior can be explained by postulating much more modest mechanisms intervening between stimulus and behavior. Cognitivists, by contrast, are inclined to take folk psychology seriously, at least as a first approximation. Thus the cognitivists' models will postulate beliefs (or memories or cognitive maps or schemata) and desires (or plans or goal structures) along with perceptual and inferential mechanisms which interact more or less along the lines sketched in figure 1. As I propose to use the term *cognitive system,* the claim that a given subject is a cognitive system is equivalent to the claim that a theory that accounts for the subject's behavior will be cast along the lines of figure 1. A thoroughgoing behaviorist would claim that neither animals nor people are cognitive systems. However, there is no *a priori* reason to think that the same model is applicable to all species. So it would be perfectly consistent to be a behaviorist about paramecia and a cognitivist about chimpanzees. As G stresses, the behaviorist model is wildly implausible for animals like chimpanzees. The complexity and sophistication of the behavior recounted in the *Savage-Rumbaugh et al.* and *Premack & Woodruff* papers serve to underscore the point. G is surely right that much of the allure of a thoroughgoing behaviorism is rooted in the suspicion that the alternative is some form of Cartesian dualism which postulates a ghostly extramaterial realm [see Puccetti & Dykes: "Sensory Cortex and the Mind-Brain Problem" *BBS* 1(3) 1978]. But the suspicion is quite unfounded. To say that an animal is a cognitive system, and

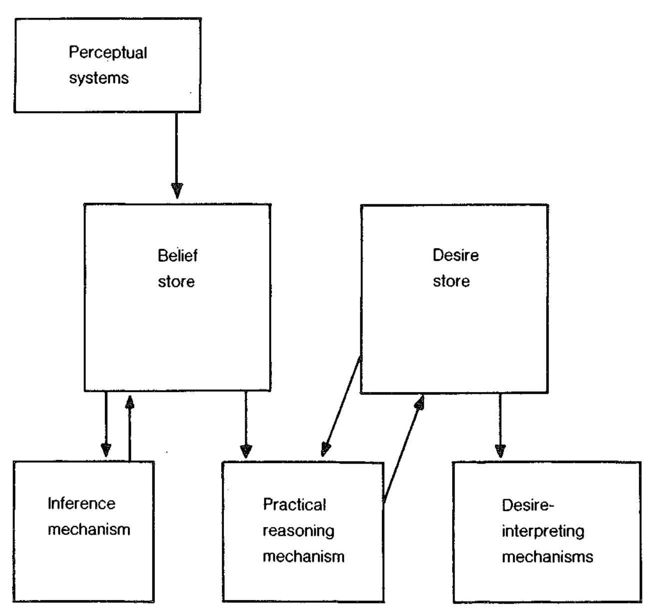

Figure 1 (Stich). Cognition and content in nonhuman species.

thus that it has beliefs and desires, makes inferences, and so forth, is to say simply that the mechanisms responsible for the animal's behavior are organized (more or less) as indicated in figure 1. And this is a claim that might be true of a system as unghostly as a computer-controlled robot.

Much of G's paper is a polemic against behaviorism and in favor of cognitivism. In light of the lingering virulence of behaviorism in this area, the polemic is quite appropriate. However, when the issue is cast as a debate between cognitivists and behaviorists, it is easy to lose sight of a fundamentally important point. *Cognitivism and behaviorism are not the only two alternatives.* It is perfectly possible that the best models for the mechanisms underlying behavior in higher animals will not at all resemble the layout of figure 1, but will nonetheless postulate elaborate interacting mechanisms that are anathema to behaviorists. Should this turn out to be the case, then we should, I think, conclude that animals (or people for that matter) do not think, do not have beliefs, do not have desires, and so forth. For terms like "think," "believe," and "desire," gain their meaning from the role they play in folk psychology. If folk psychology is false, these terms will have no proper application. An analogy may be helpful here. Terms like "witch" and "cast a spell" gain their meaning from the role they play in a folk theory that we long ago discarded as false, and so we believe that there are no witches and no one can cast spells (cf. Rorty, 1965).

Before turning to the question of content, we should note that the various components of a cognitive model are inextricably interrelated. We can, of course, choose to *study* one or another component while attempting to hold the rest of the system more or less constant. This is the strategy of those cognitive theorists who focus on human memory, for example, or on human inference. However, it is not a memory alone that results in a subject answering a question in a certain way. Rather, it is a memory plus (at least) a number of additional beliefs about what the experimeter wants, and a desire to report honestly what one remembers. While it is a perfectly sensible, indeed inevitable, research strategy to study one component of a cognitive model while keeping the rest constant, it is quite another matter to suppose that one or another component might sensibly be said to exist without the rest. It simply makes no sense to speak of a system that, for example, has a full complement of desires, but has no beliefs, is incapable of practical reasoning, and so on. For to be a desire just *is* to be a state that interacts with the rest of the components of the cognitive model in the appropriate way. Thus I suspect that it is incoherent to suggest, as P&W do, that on the chimpanzee's theory of mind people and other chimpanzees might be thought of as having motivational states but not belief states.

2. Do animal cognitive states have expressible content, and if so what content do they have?

This second question presupposes an affirmative answer to the first, since if the members of a given species are not cognitive systems at all, then they simply do not have cognitive states such as beliefs, desires, and thoughts. But if we grant that a model along the lines sketched in figure 1 is appropriate for a given species, then we can go on to ask about the content of the animal's belief and desire states. That is, we can attempt to characterize the animal's beliefs in the way we typically characterize the beliefs of our fellow men and women. For example, I might recount some of the beliefs of a human friend by saying: "She believes that someone is frying bacon in the kitchen;" or: "She believes that Los Angeles is east of Reno." Generally, when we attribute beliefs to a fellow human, we do so by finding an appropriate sentence to replace "p" in: "S believes that p." The sentence selected must express the content of the belief being attributed; often it is the sentence the subject herself would use to express the belief. Similarly, if P&W's chimpanzee Sarah is a cognitive system, then we might try to specify the content of her beliefs by finding suitable sentences to replace "p" in "Sarah believes that p."

However, as a number of philosophers have noted (Dennett, 1969); Davidson, 1975; Stich, 1979a, 1979b), the possibility of giving this sort of characterization of a subject's beliefs *presupposes that the subject's store of beliefs is appropriately similar to our own.* Analogously, in order to translate what a subject says into our language requires that the subject's store of beliefs be appropriately similar to our own (cf. Quine, 1960). To see the point vividly, imagine a human subject who is suffering from degenerative senility. Gradually the subject's store of beliefs and memories slips away. In one such case familiar to me, the subject, at an advanced stage of the disease, would regularly respond to the request, "Tell me what happened to McKinley," by saying, "McKinley was assassinated." However, when the subject was asked questions about assassination it became clear that she had quite forgotten what the word meant. For example, if asked whether people are dead after being assassinated, she claimed she did not know. When coming from this subject, the words "McKinley was assassinated" can no longer be interpreted as meaning that McKinley was assassinated. Nor can the belief state underlying the pronouncement be identified as the belief that McKinley was assassinated. There are analogous, though less depressing, problems in interpreting the words and attributing content to the beliefs of small children.

Now it might be thought that these problems are in principle solvable, and that with sufficient effort we could find a sentence that appropriately expressed the content of the child's belief or the meaning of the senile patient's sentence. But that, I would urge, is not the case. There simply is no sentence in English that can be used to capture the content of the child's or the senile patient's belief (see Stich, 1979b). However, the impossibility of finding a content sentence that does justice to these beliefs does not mean we cannot talk about these beliefs. Still less does it mean that the child and the senile patient have no beliefs, that they are not cognitive systems. It means only that we cannot use expressions of the form "S believes that p" in talking about these subjects' beliefs. The alternative is to describe the beliefs as developmental psychologists and students of child language do, by describing the sensory input that leads to their formation, the sorts of inferences that are and are not drawn from them, how they interact with motivational states, and so forth. My point, then, is that we can treat subjects as cognitive systems and study their beliefs and inferences fruitfully, even though we cannot express the content of those beliefs.

Finally, let me indicate how these observations are relevant to G's paper. G is much concerned with how we are to interpret, translate, or decode the communicative behavior of animals. If the view I have been urging is correct, then this talk of interpreting communicative behavior is systematically ambiguous. On one reading, an interpretation may be an account of the circumstances under which a given bit of communicative behavior is produced, the effects it has on its audience, and even, perhaps, the inferential relations among the beliefs and desires that underlie the behavior. On the other reading, to give an interpretation is to translate the communicative behavior into our language, to give some English sentence whose content is the same. Now when the communicative behavior at issue is the behavior of insects, the latter sort of interpretation is almost certainly impossible. The cognitive differences between people and bees are much too great for there to be any sentence of ours that captures the content of a bee's beliefs or desires. But the fact that we cannot express the content of a bee's beliefs loses much of its sting when we realize it does not mean that bees are not cognitive systems.

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EDITORIAL NOTE

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by **Shimon Ullman***

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Mental representations and mental experiences [G]. As a result of a fundamental distinction he fails to draw, valid arguments are mixed with invalid ones in

G's advocacy of "cognitive ethology." In a nutshell, the distinction is between *mental representation* on the one hand and *mental experiences* on the other.

I shall use the term "mental," or "symbolic representation" as roughly equivalent in meaning to G's "mental images of objects, events, or relationships." A system can justifiably be said to have symbolic representations of a certain domain if certain events within the system can be consistently interpreted as having meanings in that domain (see Ullman, 1979). If, for example, certain voltage patterns inside a computer can be consistently interpreted as having a meaning in the domain of arithmetic, the computer can be said to employ a symbolic representation of arithmetic entities. In the same sense, the brain might be said to employ symbolic representations (which in this case are also called "mental representations") of (for example) physical objects or events [see also: Haugeland: "The Plausibility of Cognitivism" *BBS* 1(2) 1978].

Humans also have what G calls "mental experiences," "awareness," or "conscious experience." Symbolic representations and mental experiences should not be confounded. There is nothing in the former that necessarily entails the latter. Computer systems provide one example of this distinction. They often employ elaborate symbolic representations. They also have, to a limited extent, visual and linguistic capabilities. These capacities do not imply, however, that they enjoy any sort of mental experience.

Symbolic representations within a system are amenable, at least to some degree, to empirical investigation. Chomsky's theory of syntax, for instance, aims at revealing the mental representations of syntactic structures and the way these structures are manipulated. Marr and Nishihara (1978) provide another example of investigating mental representations, in this case visual recognition of three-dimensional shapes.

Mental experiences, unlike symbolic representations, probably cannot, in principle, be shown to exist (see also Sherrington, 1940; Schrodinger, 1958; Penfield, 1975). In this I agree with Cambell and Blake (1977 *op. cit.,* G) and with Krebs (1977 *op. cit.,* G) (G's Section 7). In refuting their position G confuses mental representations with mental experiences. This same confusion is reflected in claims such as: "Consciousness itself is a representational system" (John, quoted in G's Section 2); "It seems possible . . . to detect and examine any mental experience or conscious intention that animals might have" (G's Section 2); or "The study of consciousness is a legitimate branch of natural science" (Taylor, quoted in G's Section 5).

Embedded in this confusion are two fallacies frequently encountered in discussions of conscious experience. The first is the fallacy of identifying self-awareness with the representation of the self (see e.g., Dawkins, 1976). Gallup's studies (G's Section 7) suggest that the chimpanzee has some sort of a representation of its own body, and this is taken by G as an indication of self-awareness. A similar view is reflected in John's definition of consciousness as "information about the information in the system" (G's Section 2). There is nothing mysterious or very special about representing in a system certain aspects of the system itself. Computer programs have been written (e.g., MYCIN, a program that analyzes bacterial infections) that can reason about their own behavior. They possess "information about the information in the system" without having self-awareness or mental experience of any kind.

The second fallacy is to equate the capacity for planning future activities with conscious intentions. Again, if the ability "to form a plan, and make a decision – to adopt a plan" (Longuet-Higgins, quoted in G's Section 2) were indeed a test for conscious mind, computing machines should long have qualified as conscious intentional beings.

Since he does not draw a distinction between symbolic representations and mental experiences, G maintains that the exclusion of mental experiences from the realm of empirical investigations amounts to behaviorism. This accusation is not justified. In fact, mental experiences lie outside the realm of contemporary cognitive psychology as well. The difference between the two approaches is that the behaviorist denies the existence of symbolic representations, or at least the usefulness of the concept, while in cognitive psychology mental representations are of key importance.

G advocates the use of communication in investigating the mental experiences and mental representations in animals. The point that the study of communication in the animal kingdom might be of value is well taken. Less clear, however, is why communication should have a special status. One of the arguments raised by G is that communication provides a direct window to other minds: "It provides our best evidence about the mental experiences of our fellow men." Communication, like any other behavior, cannot provide direct evidence for mental experiences. Our best evidence for mental experiences in other minds is perhaps an isomorphism argument: If they are built the same way, they probably share similar properties. In this limited sense, empirical studies may provide indirect and inconclusive evidence for mental experience. Accordingly, the communicative act cannot in itself provide a proof for the existence of mental experience. Computers can communicate intelligently with one another and with humans without being endowed with experiences similar to humans. An extra caution is required when drawing conclusions on the basis of communicative behavior, since humans have a compelling tendency to attribute to the communicating agent false humanlike properties (see for example the reactions reported by Weizenbaum to his simple-minded "conversing" program ELIZA, (Weizenbaum, 1976).

Communication is certainly central to the study of mental representations in humans. What makes it particularly instrumental is the richness of information conveyed through human language. G's hope that communication with animals (especially outside the ape family) will be rich enough for them to report to us directly "about mental experience, awareness, intentions, and the like" [Section 7] seems somewhat optimistic at present.

In conclusion, the argument that studies of communicative behavior, along with other studies, might be of help in exploring the symbolic representations employed by certain animals seems plausible. I do not see, however, why they should enjoy the special status G argues for, nor can I accept his claims regarding their power in exploring mental experiences in the animal kingdom.

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EDITORIAL NOTE

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by **Bernard Weiner and Susan Landes**

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A cognitive psychology for infrahumans. Since the turn of the century, American psychology has been guided by Morgan's canon – "in no case is an animal activity to be interpreted as an outcome of the exercise of a higher psychical activity, if it can be fairly interpreted as an outcome of an exercise which stands lower in the psychological scale" (Morgan, 1896). This canon came about as a revolt against the typically unsubstantiated claims of members of the anecdotal school of psychology, who gathered stories illustrating the supposed amazing intellectual capacities in infrahumans. Hence, while this school of psychologists contended that moths fly into fire because they are curious, the newer psychologists, guided by the cautions of Morgan, countered that such behavior was more probably due to a built-in, unlearned mechanism, akin to a tropism.

The behaviorists coupled Morgan's Canon with Darwin's continuity postulate to develop their unique approach to human behavior. They reasoned that if the behavior of infrahumans can be explained with mechanistic principles, and if there is an infrahuman-human continuity, then human behavior can also be explained without appealing to higher-order processes. It thus came about that psychology was said to have lost its mind!

But all of this is old history. Mechanism as the basic approach to the understanding of human behavior has been laid to rest; in the last twenty-five years we have witnessed the rebirth of cognitive psychology. But it is a cognitive psychology different from that which was championed by the anecdotal school. This cognitive psychology is influenced by information theory and computer models and analogues; it is a cognitive psychology with the methodological sophistication introduced by the behaviorists, wary of the errors of the early introspectionists; it is a cognitive psychology that addresses the problems of personality, motivation, and social psychology.

The papers in the present series are part of this larger cognitive movement. It now seems, for example, that chimps have a theory of the mind and that behaviors of infrahumans are not only instigated by associations between sign stimuli and releasers. The argument now is that if human behavior is guided by higher-order processes, and if there is a human-infrahuman continuity, then the behavior of infrahumans also is influenced by these mental processes. The circle is thus completed. This is also a return to our common-sense and everyday observations. But, as the authors in this issue so creatively show, it is the experimental data that force us to take very seriously the cognitive explanations and inferences.

The shifting methodology. The theoretical movement from mechanism back to cognition has been accompanied by some methodological alterations. There is a trend in both infrahuman and human research to study complex behavior in its natural habitat, in field settings, where the experimenter does not intrude upon the normal flow of events but rather observes, records, and then interprets. The anthropological field research with infrahumans has broadened our appreciation of the complexity of other species. These observations have called attention to some of the limitations of, for example, making inferences from captive lower animals or, for that matter, from human subjects in an experimental room. Thus, the field research movement served as a precursor for the work of, for example, P&W.

But, as P&W demonstrate, we also have to craft the environment so that theories about the chimp's theories can be tested. For certain questions to be answered, environments must often be created; this may be crucial in theoretical advancements when the limits of the mind are under investigation. One might guess that these limits are in great part determined by the boundaries of our experimental sophistication. In sum, the field research and the current laboratory methodology are now complementary (a point that we will come back to later in this discussion).

Cognition, emotion, and motivation: New research directions. It has long been recognized that the three aspects of psychology – cognition, emotion, and motivation – are closely intertwined, so that these distinctions often are hard to maintain. If it is now accepted that infrahumans engage in higher-order cognitive behavior, then it follows that our conceptions of their emotional and motivational lives must also change. For example, current research in the area of affect not only examines the biologically-rooted emotions, but also cognitively mediated feeling-states. Considering the chimp Sarah, for example, one might ask: If she observes a good person doing a bad deed (or vice versa), will she exhibit surprise? If she does something that her peers cannot do, will she be proud? If she does not try to help a colleague in distress, is she capable of experiencing guilt or shame? If she tries to help but fails, will she display or experience a difference emotional reaction? These questions seem to be amenable to the experimental magic of P&W. Descartes, caught within the mechanistic framework for infrahumans, wrote that "animals eat without pleasure and cry without pain." Let us broaden the research, away from cognitions per se and toward their dynamic psychological implications and consequences.

A similar argument can be made about the study of motivation. For example, it appears that Sarah may desire to maintain cognitive consistency or a balanced cognitive state, with good things happening to good people and bad things to bad people. This is in accord with the suggestion of Lawrence and Festinger (1962) that infrahumans as well as humans experience adverse psychological reactions when their cognitions are not in harmony. Do Sarah, and other members of her species or additional species, exhibit a broad array of psychogenic motivations? To date, the experimental studies of infrahuman motivation have been largely confined to viscerogenic need states such as hunger and thirst, with an occasional recognition that infrahumans are also curious and act to increase stimulation. But do they attempt to do tasks well (need for achievement), to help others (altruism), to maintain friendships (need for affiliation), to have an impact upon others (need for power)? Some of our observations appear to implicate these sources of motivation, but without evidence of the sort that P&W have been able to provide regarding the mediating role of cognitions. Let us move in this motivational direction to develop a more complete comparative psychology.

The evolution of cognition: The use of mental processes for a comparative taxonomy. Darwin's theory of evolution through natural selection is the organizing principle of biology. One of its principal postulates is that the process of evolution is gradual and continuous; evolution does not consist of a series of sudden changes or saltations. It was not, however, until the 1920s and the advent of population genetics that the gradual origin of complex types and new species became widely accepted. Genetics revealed how new species might emerge through a process of gradual evolution.

Traditionally, comparative anatomy has been the discipline most concerned with evolutionary arguments. However, the idea that sufficient morphological evidence could tell us everything about the relations between species has proven incorrect. The work reviewed and offered in this issue concerning the cognitive capacities of infrahuman primates may provide another avenue to the evolutionary history of our order. These new clues could lead us to alter our prior ideas or, conversely, to have even greater confidence in some conclusions already reached. For example, the newer data on the cognitive capabilities of apes reaffirm the conclusions from both comparative anatomy and primate field studies. The field studies of the 1960s and 1970s have shown that African apes have complex and highly personal social relationships enduring over lifetimes, tool use, elaborate gestural communication, and even cooperative hunting. In a sybiosis between field and laboratory work, SR&B have demonstrated the extent of these capacities. The very fact that the ape does things in the laboratory that are not observed or cannot be performed in the field may be a key to the understanding of the evolution of the mind.

In sum, a cognitive ethology should provide further data about humans' evolutionary relations to other animals. The imaginative experiments thus far reported enhance Darwin's view of gradualism and continuity in evolution. The research supports the thesis that humans share a common ancestry with modern apes and that there probably was a long period during which selective pressures shaped a basic adaptive complex before the pongid-hominid lines diverged. Experiments with other taxa are needed to elaborate this point, but it seems clear that human and ape have some broadly similar cognitive abilities not shared by monkeys and other infrahumans.

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Of apes and hemispheres. In his "Prospects for a Cognitive Ethology," D. R. Griffin calls for a research program on nonhuman cognition that would center on internal representation of self and world, on intentionality, and on awareness. Language is a natural source of evidence for intentions and awareness, but can one adduce nonlinguistic evidence for their existence? Can intentions occur in the absence of language? This is the perennial question of the relation of language to thought, and it is central to SR&B's and P&W's papers. SR&B's paper is really about the cognitive basis of language acquisition in apes, whereas P&W's paper is about nonlinguistic evidence in apes for cognitive structures that have simple human linguistic labels, such as "know," "believe," "doubt," and "like." The study of the relation of language to thought through cases of unusual dissociation between linguistic and cognitive abilities is in the tradition of experimental psychology. Apes and the human right hemisphere share the status of models of cognition without a well-developed expressive language, and I will therefore comment on the three papers from the perspective of my own work on language and cognition in the surgically separated right hemisphere (RH) of split-brain patients.

The lure of continuity [G]. I read G's focus on intentionality and awareness in nonhuman cognition as a welcome heuristic for exploring the possible consequences of internal representation in animals rather than as a literal or dogmatic insistence that, say, honeybees possess intentions and plans. This heuristic is useful precisely to the extent that it leads to the formulation of behavioral tests for the occurrence of internal representation of self, of other conspecifics, of the world, and of intentionality. This can lead to interesting experiments and alternative interpretations of existing data. Implicit here is the assumption that the cognitive behavior of nonhuman species is more complex than is commonly believed.

What behavioral criteria could distinguish an intentional honeybee from an automaton bee in the Lindauer report? Or a hard-wired instinct for self-preservation from existential fear of death? Perhaps the best evidence would be the animal communicating its spontaneous introspections, but that is precisely the kind of data that is missing. Otherwise one posits intentions and awareness only when it becomes unparsimonious to account for the range of observable behaviors in terms of special-purpose mechanisms. Behavioral evidence for intentions includes planning or internal representation of delayed future outcomes, choice between similarly attractive alternatives, and an affective posture of preference, hope, and belief in the feasibility of the outcome. It is a main task of a cognitive ethology to articulate simple behavioral tests for planning, choice, and belief that rely on behaviors that are within the animal's cognitive repertoire.

For example, it may be possible to test an ape's competence for planning (and its sense of past) by having it reconstruct sequences of pictures so as to represent a significant novel future (or past) event or by letting it show preference for one sequence over others. To show belief one needs to show, among other things, that the animal follows a previous expectation even when it is in conflict with otherwise apparent fact. Awareness is perhaps best demonstrated by recognition of self, and this can be easily tested if the animal can again recognize pictorial representations of personally significant real events and a symbol of self. This could, but need not, be a picture or mirror image of the animal and clearly requires special training in the ape, though not in the human RH.

By now it should be clear that I do not subscribe to the position that mental experiences can only be detected and analyzed through the use of language and introspective reports. It is also misleading of G to argue that there is "no qualitative dichotomy, but rather a quantitative difference in complexity of signals and range of intentions that separate animal communication from human language." Rather, range and complexity seem to me to be precisely what (so far) make human language unique and what qualify a system of communication as language. Consequently, G's call for a cognitive ethology can be strengthened since it need not depend on the actual versatility of animal communication systems. In other words, nonverbal tests of complex cognitions are possible. In fact, any nonlinguistic procedure used to train and test the ability of the ape to acquire symbols for "future" or "yesterday" could be used to assess directly the acquisition of the underlying concept of time. It is this underlying cognitive structure rather than its linguistic expression that is important here. I have used such nonverbal paradigms to study RH mentation and found that its cognitive and linguistic abilities develop and operate fairly independently of each other (Zaidel 1978b).

How would one develop a systematic program for designing experiments on language and thought in a communicating species? There are at least two classes of possible paradigms. The first may be described as "data or hypothesis-generating experiments," such as observation of natural, spontaneous behavior. This is G's approach to communication. Similarly, factor analysis of rich data bases can generate hypotheses about patterns in the data. Here as elsewhere the observer's interpretation is necessarily affected by his world view, but he can nonetheless observe an infinity of potential patterns in the data. Nonverbal projective (e.g. constructional) clinical tests are also designed to generate hypotheses about the subject's personality structure, but none of the ones available (such as the Luschner Colot Test, the Szondi Test, Make-a-Picture-Story World Test) has adequate validity or reliability.

A second class of paradigms is the common "hypothesis-testing experiments." One example of this is the forced choice, multiple choice, conceptual matching-to-sample paradigm, or simply "the multiple choice paradigm." Here a stimulus is presented or a problem is posed, and the subject has to solve it by pointing to a picture that best corresponds to the answer. I have used this paradigm extensively with the disconnected RH since no speech responses are required. For similar reasons P & W use it now with Sarah. Of course, hypotheses-testing paradigms no longer tap the animal's natural performance repertoire and focus instead on limits of competence under specific training conditions. Experimental psychology is especially guilty of incorrectly inferring the structure of natural information processing from unnatural and contrived limit-case experiments, and of treating rare skills, such as abstract mathematical reasoning, and common abilities, such as language, as theoretically equivalent.

Do apes have language [SR&B]? In the absence of spontaneous linguistic introspection in apes it may still be possible to teach chimps an artificial language system which can be used subsequently to communicate about ape mentation. This is what SR&B are trying to do. Although they claim to provide a methodologically conservative demonstration of linguistic communication between two chimps, I think they have actually demonstrated some cognitive prerequisites for symbol use in these nonhuman primates. SR&B's experimental paradigm seems too narrow to qualify as linguistic communication. They have trained two chimps to perform a complex sequence of acts in a specific context in order to obtain food. There is no evidence of spontaneous extension of the social exchange to novel cases. Even when the apes were trained to request and provide tools, the dyadic exchange did not occur spontaneously, and communicative intent had to be taught first in a precise context. Thus this example of alleged linguistic communication fails the crucial tests of novelty and range of application. How context-independent do we require linguistic communication to be? We want it flexible enough to apply to a new partner without special training yet not so nondiscriminating that it applies to an inanimate model or to a clearly uncooperative mate.

However, it is not important whether or not we call the social exchange between Sherman and Austin linguistic, for SR&B's paper presents an important account of the interplay between complex cognition and symbol use in chimps. The exchange task is undeniably complex: It involves generalization, notably about role reversal and the interchangeability of food type and location; it also involves the manipulability of objects and other individuals; and it involves social interaction mediated by linquistic symbols. The greatest insight comes from the cognitive constraints on the acquisition of the task. These were already apparent in the preliminary naming task, where in order to avoid confusion, no more than three items could be used in any session. It was found that the apes could not learn to name some objects, so tool requests had to be taught in the highly specific functional context of obtaining particular foods from particular sites. Similarly, tool naming was harder than tool requesting. It is clear that the chimp acquires the symbol for a particular word in the context of a specific function which it finds difficult to divorce from the word itself. Use of more than two objects, changing the examplars, and intersession delay all caused performance breakdown in the object-naming task. From these data it may follow that verbs should be easier for the ape to acquire than proper and abstract nouns.

SR&B's data suggest that the cognitive bases for word meanings are very different in the chimps and in the disconnected RHs. The RH lexicon is clearly more abstract and context-independent. In fact, although the RH can often signal the meaning of a verb by pointing to a picture describing the action, it may nevertheless be unable to perform the same action. Thus tool use and the imitation of action do not seem to form the basis of word acquisition in the RH. Furthermore, SR&B cite cases of spontaneous iconic gesturing as an adjunct to the abstract keyboard symbols. In contrast, even though it has access to some writing and other meaning-carrying symbols, the RH rarely if ever initiates expressive communication. The RH can manipulate linguistic symbols semantically (e.g., by matching printed synonyms or antonyms), but its linguistic competence is inherently receptive.

When language fails [P&W]. In academic circles I have often encountered a certain "mystique of reticence," whereby verbal reticence in social interaction is interpreted as judiciousness, even profundity. Perhaps P&W were so lured by Sarah's persistent reticence after some thirteen years of training in using an artificial language, that they were moved to anthropomorphize her and even try to show with nonverbal tests that she is a philospher (although philosphers never seem guilty of reticence). Weary of waiting for Sarah to express her latent philosophical concerns spontaneously, P&W undertook to elicit her cognitive and philosophical competence by ingenious applications of the multiple-choice paradigm.

The paradigm is of the hypothesis-testing variety, but it nevertheless allows incredible freedom in the formulation of hypotheses. Most commonly the choices are pictures, and the relation between them and the stimulus or problem can be highly associative and thus arbitrarily abstract. The foils can be carefully selected to constitute precisely the contrasts of direct interest and to allow for the analysis of false positive errors. No speech or manual construction, only pointing or show of preference, is necessary for a response, so the paradigm is ideal for testing brain-damaged patients with cognitive deficit, children with congenital language disability, and the disconnected RH. I have used this paradigm to obtain comprehensive linguistic and cognitive profiles for the nondominant hemisphere, including tests of phonology, syntax, semantics, memory, intelligence, and personality (Zaidel 1978a, b).

However, the multiple-choice paradigm is not universally applicable to all subjects. It has cognitive prerequisites which we cannot assume a priori to obtain for nonhumans or RHs. Thus a subject has to share the problem context with the test designer, and he has to be able to recognize two-dimensional pictures of three-dimensional objects as well as the meaning of complex pictures. Both seem to be the case for RH but the former not for naive apes. Similarly, the paradigm depends on the subject's ability reliably to search, remember, and select from alternatives. In the case of the RH this seems limited to choice sets with about four to six items, but what about Sarah? A systematic use of multiple-choice pictures calls for a metric of picture perception and of pictorial associations similar to that available in verbal learning.

P&W describe two exciting variations of the multiple choice paradigm. Instead of using still pictures or drawings they use dynamic video scenes; they also incorporate a recursive condition into the procedure (pictures of subjects taking the test) which makes it possible to evaluate second-order cognitions or metacognitive relations. P&W's use of these techniques demonstrates that evidence about intentionality and awareness can be elicited without expressive communication. Yet the paradigm lends itself to testing binary dichotomies (e.g., does Sarah regard an adult chimp as less likely to make errors than an adult gorilla?) rather than affective gradients or degrees of cognitions. The latter would require some pretraining, such as teaching symbols for comparative terms or teaching rating responses on a generic intensity scale represented, say, by a horizontal series of color chips arranged in order of increasing saturation.

It seems plausible to tap Sarah's sense of physical reality by asking her, as P&W do, to identify the solution to the problem of a hose improperly attached to a faucet. But in what sense (except association) could she realize that plugging in a fan will make it work? Surely there are more direct ways of studying Sarah's sense of causality and reality, such as by using pictorial multiple choices of Piagetian tasks. P&W assert that they are not primarily interested in the animal's grasp of physical relations, but rather in using that grasp to establish, say, that the chimpanzee has an abstract concept of "problem." But the animal's conception of physical reality and causality are probably central to its concepts of error, ambiguity, and conflict which, in turn, are essential ingredients of "problem." Sarah's ability to solve diverse problems tells us nothing about whether she has a conceptual category "problem" in mind which she applies to all these problems. Are P&W justified in attributing their own human interpretation to Sarah without specifying behavioral criteria for acquiring the concept "problem?"

P&W may be attracted by philosophical questions and ingenious, logically complex paradigms at the expense of simpler answers to more direct cognitive questions. Consider the embedded video tape paradigm. It is clearly useful for studying second-order cognitions such as opinions or feelings about opinions or feelings, and especially for eliciting reactions to the whole experimental paradigm itself (video of ape judge observing an actor). But is the embedded video tape necessary for distinguishing a "smart" from a "stupid" observer? This could be tested by making the observer O be the participant P rather than its judge. Surely the question of whether Sarah thinks that O can solve a problem should be answered before the question of whether she thinks that O would correctly judge a video tape solution to the problem.

But these are minor gripes. P&W outline an exciting program for studying a wide range of cognitive issues focusing, notably, on knowledge, belief, and empathy. Empathy – using oneself as a partial model for another – is important because it is a good example of an affective heuristic serving a cognitive function. Empathy is the basis for shared context that makes it possible for Sarah to recognize the meaning of the stimuli and problems. But empathy need not entail complete identification with an actor or problem solver. Thus I can empathize with Sarah without equating our respective cognitive abilities, or without even liking her. In particular I can empathize with Sarah *and* have an opinion about the limitations of her knowledge at the same time. I believe the whole distinction between motivation and knowledge tends to be drawn too sharply in the sense that, internally, inferences about knowledge in humans may usually incorporate inferences about belief. In other words, we associate feelings with particular cognitions in order to facilitate their processing and change.

Is the alleged cognitive and receptive linguistic superiority of the disconnected human RH over the ape a consequence of the much richer cognitive experience of the RH as part of normal development? We don't know much about the learning process in the normal RH, but the disconnected RH seems uniquely unresponsive to standard behavioral learning paradigms and to error correction (Zaidel, 1978b). It is distinguished by what it has not learned, in spite of years of potential experience (e.g., to speak). Indeed, Chomsky's anti-Skinnerian argument has emphasized the inadequacy of a learning theoretic account of language acquisition and the need to postulate an innate biological capacity for language. On the other hand, it is generally acknowledged that human competence for language can only be realized after exposure to normal communication and social interaction (cf. feral children). The question then becomes, what environment would constitute a comparable natural catalyst for the upper limits of ape language?

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EDITORIAL NOTE
*Received too late for a response from G, P&W, or SR&B. See Continuing Commentary.

Author's Response

by **D. R. Griffin**

Helpful "talk" on what to "do"

I welcome these thoughtful and significant contributions to the development of cognitive ethology. I am so sympathetic to the general ideas expressed by many commentators that it is pointless to say so at great length; and I will leave to SR&B and P&W specific questions about languagelike behavior learned by captive apes (see also Ristan and Robbins, in press). To paraphrase **Beck,** the "talkers" have lots of constructive and promising suggestions for future "doers." These will help us to *inquire* whether particular kinds of mental experiences are likely to be occuring in various animals and to control our impulse to *assert* whatever we may believe about these matters. As Karl Popper put it succinctly (Popper and Eccles, 1977), "Let our theories fight it out . . . let our theories die in our stead."

One theme runs through several commentaries, especially those of **Beck, Candland, R.T. Davis, Hebb, Limber,** and **Rachlin:** Comparative psychologists have actually been studying the minds of animals for several decades through experiments on discriminative learning, problem solving, cross-modal transfer, perceptual constancies, perception of barriers and detours, learning sets, and the like. Of course, understanding these topics is directly relevant to a cognitive ethology, and it is helpful to be assured that so many behavioral scientists have really been studying animal thinking all along. But only very rarely (for example, in Hulse *et al.,* 1978) have recent students of these kinds of problems attempted to relate their findings to mental experiences of the animals concerned. Unwary students and general readers can perhaps be forgiven for interpreting what they hear and read to mean that all nonhuman animals are what Malcolm (1973) called "thoughtless brutes." The recent revival of interest in cognitive ethology will be a valuable development even if it accomplishes no more than the general recognition that behavioral scientists are indeed studying the minds of animals as well as their behavior.

It is important to keep firmly in mind the distinction between awareness and responsiveness. Granted that detecting awareness in another species is difficult, let us tentatively assume some measure of mental continuity and begin by considering the human case. Clearly we can be aware of some stimulation or relationship without responding to it in any detectable fashion. Conversely, we respond to many kinds of stimulation without being aware that we are doing so [cf. Roland: "Sensory Feedback to the Cerebal Cortex During Voluntary Movement in Man" *BBS* 1(1) 1978]. We are convinced that this is true of other people primarily because they tell us about current or past awareness of particular objects and events. Can we hope to obtain comparable evidence about awareness in other species?

Behavior, no matter how complex and adaptive, can always be interpreted without postulating awareness, but the plausibility of such an interpretation varies enormously. What kinds of animal behavior provide the strongest evidence of awareness? Appropriate communicative behavior is clearly *one* of the strongest potential sources of such evidence. This becomes most convincing when it involves communication about internal representations rather than about concurrent stimulation, not because awareness of ongoing events is unimportant, but because communicative behavior, which has the property of displacement, seems more likely to be accompanied by awareness.

As empirical scientists we can all concur in **Beck**'s preference for "doers" over "talkers." He warns us against "chimpomorphism," or