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