Sir Oran Haut-ton finds his voice

Apes, Language, and the Human Mind
by Sue Savage-Rumbaugh, Stuart G. Shanker and Talbot J. Taylor
Oxford University Press: 1998. 288 pp. £22.50, $29.95

Robert Seyfarth

In the first edition of the *Systema Naturae* (1735), Linnaeus arranged animals into groups based exclusively on anatomical features, lumping humans and nonhuman primates together. This sparked a strong reaction, particularly from the followers of René Descartes, who believed that humans were unique because they possessed minds and language. In later editions, Linnaeus responded by separating humans from other animals and basing his classification not just on anatomy but also on temperament, character, type of garments worn (if any), and forms of government.

In the nineteenth century, Cartesian dualism found an outlet in satirical accounts of apes who, if properly trained, could become respectable members of human society. In one story, an orang-utan named Sir Oran Haut-ton gains respect because he rescues a maiden in distress without taking advantage of her. Lacking language, however, he is not considered truly human. None the less, Sir Oran is elected to parliament, where his lack of speech becomes an asset because it gives him the reputation of a powerful but cautious thinker.

The authors of the present volume believe that Cartesian dualism is alive and well, particularly among those who refuse to accept Sue Savage-Rumbaugh's claim that Kanzi, a bonobo (or pygmy chimpanzee), has learned language. These critics, the authors claim, are wrong and their errors have major consequences: "When the implications of the Kanzi research have been fully assimilated, the way we look at, understand, and represent the relationships between language, cognition, and behavior will no longer be the same."

The first of the book's four chapters is a narrative account of Kanzi's infancy and youth. The second reviews Descartes's theories and argues that modern Cartesians — called 'bifurcationists' — dominate all branches of cognitive science. In chapter three, the authors describe what must be done "to place ape and human studies on an unbiased (that is, scientific) footing". In chapter four, they discuss the new perspectives that will emerge as a result.

Unlike subjects in earlier ape-language studies, Kanzi was not explicitly taught a linguistic form of communication. Instead, he 'picked up' the use of lexigrams on a computer keyboard while Savage-Rumbaugh and her colleagues were trying (with some difficulty) to teach these lexigrams to Kanzi's mother. This is an important point because it means that Kanzi's ultimate performance cannot be dismissed as simply the result of training. In fact, more than any other captive ape, Kanzi seems to have learned his system of communication in ways that are similar to those used by children. Kanzi's communication is also unusual because he interacts with people in at least three ways: by using his keyboard, through hand gestures such as pointing, and by responding to what people say. Kanzi's linguistic skills are most strikingly displayed when he responds correctly to unusual commands such as "Put the toothbrush in the lemonade".

Many scientists, however, remain unconvinced. Not, the authors believe, because they have examined the data objectively, but instead because they share a pervasive Cartesian bias against the application of mentalistic terms to apes and the use of linguistic terms to describe their communication. Psychologists studying animal learning, for instance, accept that an animal's behaviour can be guided by mental representations like memories, but refuse to say that 'meaning' or 'reference' play any role in the language of captive apes. This is inconsistent.

Similarly, the authors argue, the biased views of bifurcationist linguists allow them no room *in principle* for Savage-Rumbaugh's idea of 'primitive linguistic skills'. When she claims that Kanzi understands simple sentences, they reply that to understand a sentence one's brain must first parse it, and to parse it one must have an internalized generative grammar with all of its attendant parameters, rules, lexical categories, dictionary entries and so on. In other words, to prove that Kanzi has a partial language one must prove that he has it all; otherwise he doesn't have any.

To get around this methodological cul-de-sac, the authors advocate an entirely different approach to Savage-Rumbaugh's results. Instead of analysing each claim — that Kanzi understands action-object relations, for instance — separately, as a kind of 'propositional atom', results "must be seen… as emergent properties of the lived story that is partially recounted in [Savage-Rumbaugh's] narrative". If the discourse works, we should accept claims about its underlying elements. If Kanzi responds to a sentence the way a human would, and if a human's response to Kanzi's sentence doesn't shock or surprise him, then sentence comprehension

Sex and politics in the furred world

Screaming, hooting , scheming and arguments — no, it's not the House of Commons, but all part of the power struggles of the chimpanzee colony at Arnhem Zoo in The Netherlands. The revised edition of Frans de Waal's *Chimpanzee Politics* (John Hopkins University Press, £20.50) expands and updates the story of the colony, now more than 25 years old, and reveals that the social gap between humans and chimpanzees is surprisingly small. Small wonder that US politician Newt Gingrich put the first edition on his reading list for members of Congress.

must have taken place. And (the authors quote Wittgenstein) "to understand a sentence means to understand a language".

Are the critics of ape-language research biased? I don't think so. In fact, the critical accounts written by Steven Pinker, Joel Wallman, Michael Tomasello and others have helped to elucidate the complexities of language and to clarify which linguistic elements are missing from Kanzi's impressive communication (for the most recent debate see E. Kako's forthcoming "Elements of syntax in the systems of three language-trained animals", *Anim. Learn. Behav.*, in the press).

Are cognitive scientists hopelessly wedded to Cartesian dualism? It is certainly true that practitioners in this new field do occasionally act as if humans are the only organisms with brains — one review, for example, *Foundations of Cognitive Science*, edited by M. I. Posner (MIT Press, 1989), contains no mention of animals at all — but this bias is changing. Most cognitive scientists now recognize that theories of how brains work must take account of ants that navigate across vast tracts of desert, birds that recover thousands of previously stored seeds, monkeys that recognize other animals' social relationships, as well as Kanzi.

Moreover, current views of animals' and children's ability to recognize the mental states of others are by no means as clear cut and Cartesian as the authors suggest. They make no mention, for example, of recent studies by Dare Baldwin, Simon Baron-Cohen, Daniel Povinelli, Tomasello and others that show that mental state attribution is not simply an ability that is either present or absent. Even in children, an awareness of others' mental states emerges gradually, progressing from early manifestations of social referencing by infants to the comprehension of false beliefs by four-year-olds.

Also not mentioned are studies by Tomasello showing that in general chimpanzees raised by humans perform cognitive tasks better than chimpanzees raised by chimpanzees, regardless of whether the animals have received language training. This reminds us that a complete understanding of the relation between language and cognition cannot in principle come from Kanzi or the ape-language projects alone.

Finally, there is an irony in the authors' view that full acceptance of their results is blocked by the Cartesian dualism of their critics. They demonstrate clearly that dualists come in many forms, but neglect to mention the group of Cartesians to which they themselves belong. For years, psychologists and linguists have assumed that the best way to explore an ape's mind and its capacity for language is to bring it into human society. In the nineteenth century, this view inspired imaginary, satirical accounts; in the twentieth, it drove the French to establish a model village in French Guinea as a training ground to civilize apes (local women would be nurses and guides), the British to plan a colony to make chimpanzees human, and Americans to teach apes language. Few people would ever apply the same assumption to humans — we naturally study the minds and languages of people in other cultures in their own habitats and on their own terms. Viewed in this light, the ape-language studies of Savage-Rumbaugh and others are, in their methods if not their conclusions, classically Cartesian. From them we have learned an extraordinary amount about the cognitive and communicative skills of apes immersed in human society. What they tell us about the mind and communication of apes in ape society remains to be seen. □

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Trials and errors

The Baltimore Case: A Trial of Politics, Science and Character
by Daniel J. Kevles
Norton: 1998. 509 pp. $29.95, £21

Jon Turney

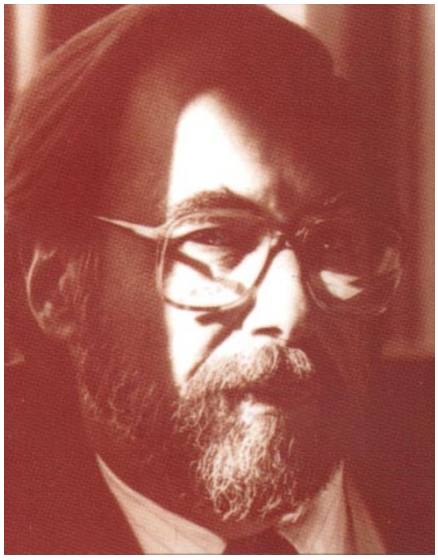
Few of us, I dare say, had the stamina to follow the Baltimore affair properly. A rich stew of supposedly fishy results, personal vendettas, enormous egos and the murky politics of leaks, smears and intimidation, it simmered for ten years. True, it had spice. An American Nobel laureate in biology was hauled before pugnacious Congressmen to rebut charges of scientific fraud in a paper he had co-authored. The US secret service pored over laboratory notebooks from no less an institution than the Massachusetts Institute of Technology (MIT). But the charges turned on the interpretation of fairly arcane aspects of the expression of immunity in transgenic mice. And, by the time the multiple investigations, and investigations of the investigators, came to a halt, it was hard to feel any the wiser.

So Daniel Kevles performs a signal service by applying his historian's acumen to the mountain of documents accumulated by all those investigations, and to his own detailed interviews with the participants, to give us an orderly narrative of the whole affair. He did so, he says, because it seemed to him likely to "throw some light on science in late-twentieth-century American society".

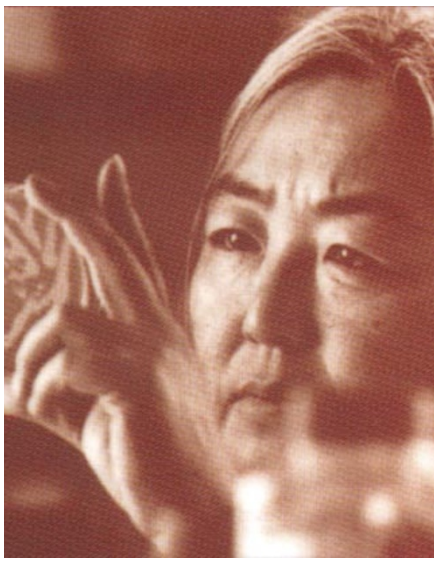
But he clearly got caught up, as historians of contemporary affairs will, in the detail of the events, to the extent that he published a lengthy article in *The New Yorker* exonerating the accused scientists some weeks before a panel of the US National Institutes of Health came to the same conclusion. The result is a book that is likely to be judged, as one of the blurb writers suggests, the definitive account of the affair, but not, perhaps, a complete consideration of its wider significance.

So what was it all about? The first thing is that it should more properly be known as the Imanishi-Kari affair. David Baltimore's co-worker, Thereza Imanishi-Kari, was an expert in cellular immunology, and was accused of faking results published in *Cell* in a paper co-authored with Baltimore and four other biologists. In fact, the accusations grew stronger over time, but that was the final allegation. The results — and the scientists — were then subject to an extraordinary series of investigations: by Tufts University, Imanishi-Kari's new employers; by MIT; by the self-appointed 'fraud-busters' at the National Institutes of Health, Walter Stewart and Ned Feder; by a congressional committee; and by a succession of NIH panels and reviews.

The first two found no case to answer.



Baltimore: refused to be cowed by investigations based on shoddy evidence.



Imanishi-Kari: faced accusations from within her own laboratory.