of the central cycle of !Kung stories. Further genres (songs, prayers, dialogues, and so forth), which hold at least as much interest as this one for the student of folklore, will be dealt with elsewhere.

It can be seen readily from this brief survey, however, that a collection of tales from living San presents material of very great interest to anthropology. In many cases social and ecological conditions dealt with in the stories are still observable in the present-day environment surrounding the storytellers. Structural analyses of the tales surely will help us to comprehend a few more of the cultural categories which have remained puzzling. Close attention to the tale variants may help us to know what degree of creative imagination is permissible within the bounds of !Kung tradition. And enjoyment of the stories for themselves may involve us personally in some of the less tangible aspects of !Kung life, things we rarely speak of but nevertheless strive to understand.

!Kung Knowledge of Animal Behavior

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(or: The Proper Study of Mankind Is Animals)

Nicolas Blurton Jones and Melvin J. Konner

Dobe man bringing home a porcupine

The investigation reported here concerns !Kung knowledge of animal behavior (ethnoethology) and their methods of acquiring and organizing this knowledge. This chapter compares their data and methodology with the data and methodology of modern ethologists, as described for instance by Tinbergen (1963). The investigation came into being in the field as a result, in part, of a chance coincidence. An interest in animal behavior led to a question from Blurton Jones which aroused a lively response from some !Kung. This gave Konner the idea of investigating their interest further. The investigation had three other points of origin. (1) Levi-Strauss (1962) has argued that the competence of the mind of "savages" in particular fields is best evaluated by collaboration of anthropologists and "western" experts in the subject. (2) Washburn and Lancaster (1968), Laughlin (1968), and others have argued that the long period in human evolution during which man lived as a hunter and gatherer may be expected to have included a selection pressure on the human brain, such that man became interested in animal behavior and competent in finding out about it. Laughlin's observations on the Aleuts suggests that they indeed have great knowledge and interest in comparative anatomy and comparative behavior, and we might expect comparable knowledge among the !Kung. (3) Although it is commonplace among anthropologists to argue that *Homo sapiens,* of whatever race and culture, shows a uniformly high level of intellect, an opposite view is deeply ingrained in the mind of the layman and, as far as we can judge, in the minds of serious academic writers on the history of science and the achievements of western man. Variations in the use to which this intellect is put occur even within cultures, and this paper could be regarded as an attempt to see which variants in our culture compare most closely to the !Kung within the specific area of animal behavior. While we refer to some aspects of !Kung hunting procedure, we make no attempt to treat this subject systematically.

Methods and Procedures

In August and September 1970 Blurton Jones visited Melvin Konner and Marjorie Shostak during their field work. Besides investigating topics of mutual interest in child behavior, Blurton Jones and Konner held a series of discussion groups on animal behavior with five or six !Kung men at each group. In all we held six seminars in three villages, and they lasted two to three hours during the evenings. Before the meeting we would think of a general line of

questioning, and during the meeting Blurton Jones would raise a question in animal behavior which Konner would translate to the !Kung. The !Kung, in turn, would then discuss the matter, one or more individuals might volunteer information. Konner would translate back to Blurton Jones, and both authors would enter replies in their notebooks. One seminar was completely tape-recorded. Once under way the discussions proceeded at a good pace, and it was notable that the participants found the exercise interesting and

Telling the hunt

showed little sign of tiring of the topic. The atmosphere was more like a lively seminar than an interview. As a precaution against misunderstanding, a !Kung man experienced in working with anthropologists and particularly with Konner and Shostak, participated in each meeting and, where necessary, retranslated from the idiosyncratic language of the older men into a more familiar !Kung.

In our questioning we concentrated on establishing how much the !Kung knew about animal behavior, with a view to checking this against existing knowledge of western scientists. Incidental to this, many hints came out about how they know and find out about behavior, and, to some extent, how they explain it. Consequently we can begin to make a comparison with modern ethology, a western science of the behavior of animals. In practice, the comparison with knowledge of western scientists is rendered quite as difficult as the comparison of methodology since the !Kung appear to know a good deal more about many subjects than do the scientists. Because of this we often cross-examined them on data that was new to us, and, in doing so, revealed interesting features of their methods for finding out about behavior and their attitudes toward observation and toward the nature of facts.

Comparisons with findings of western scientists was our main check against "tall stories." Blurton Jones's knowledge provided an immediate stimulus to cross-examine on any statement which contradicted or extended the better-known scientific findings. Some !Kung observations which we refused to believe were later proved correct when subsequently checked with ethologists who have worked in Africa.

Objectivity of Observations

It became evident fairly early in the study that the !Kung were very careful to discriminate data from theory and interpretation, and, even more so, to discriminate observed data from hearsay. But as data, along with directly observed behavior, goes behavior deduced from tracks. This seems to be regarded with confidence comparable to behavior that they have actually watched animals performing, but they always do distinguish the two data sources. A further distinction is made between, on the one hand, behavior that they have seen or reconstructed from tracks and, on the other hand, behavior that they think may happen, or that they have heard somebody say they have seen.

The features of the discussion which led us to believe that they

discriminate between observation and hearsay are of several kinds: (1) They admit ignorance very readily. Often after a question there would be a long silence, or a series of "I don't knows" from each participant. This was distinct from a response to an unclearly phrased or unclearly pronounced question, when always some attempt was made to get a repetition of the question and to find out what was being asked. Some remarks from the notes on the seminars support this view: (a) One man said that he had heard of people who have seen kudu fighting, but he himself never has. (b) When asked whether newborn buffaloes (*Syncerus caffer*) stayed with their mothers or were hidden, one man replied that because buffaloes are so dangerous, he had not looked to see where the babies were, "Since buffaloes kill you, you don't go after them." (c) And at another village where they have on occasion gone after buffaloes, when asked whether baby lions' eyes were open at birth, they laughed and said, "If you go over there and look, won't you be dead?"

(2) They argue about generalizations based on scant data and will disagree but will try to reach an answer. (a) On the subject of newborn buffaloes: one man suggested that buffaloes are like cows, so would be unlikely to hide their babies; someone disagreed with this suggestion, and a discussion ensued about what would really happen with the buffalo. The discussion produced more observations; and, eventually, once the problems of measuring time were resolved, agreement was reached on the fact that the newborn buffalo follows the mother from very early in it's life. (b) Someone suggested that lions spot and follow the tracks of their prey, and that they know which animal they are following, whereupon others disagreed as to whether the lion knows which track belongs to which prey. Although this is clearly not a field in which it would be easy to obtain a correct answer, the fact that a guess at this is not acceptable is some evidence of a distinction between fact and fiction. (It certainly contrasts with the impression one has of 19th-century British gamekeepers, or the sort of countrymen among whom Gilbert White (1789) had to attempt to discern the truth about the English countryside.) (c) After a disconcertingly complete demonstration of the behavior of the honey guide (*Indicator spp.*) a man suggested that the honey guide sometimes leads leopards to honey. This suggestion was then qualified by the objective statement that if you are following the bird you sometimes see a leopard. The statement was taken up immediately and negated by someone who claimed that the bird leads people to leopards, not leopards to the honey. It would seem to us very likely that, as the behavior of the honey guide is probably

based in part on a mobbing response to large animals, it may indeed lead people to leopards by the same rather fortuitous way in which it leads them to honey. In the same discussion the men said they did not know whether the honey guide leads the honey badger (*Mellivora capensis*) to honey, although the suggestion that this is so is widespread in the literature, and the !Kung knew that the honey badger eats honey. In another discussion there was a striking rejoinder by an elderly man that his colleagues should speak only if they have seen things happen. This was provoked by a speculation that children could be killed by fires.

(3) They are able to report new data and do so without pressure. This again is markedly different from the average pet owner, or even many countrymen, who must be really forced into saying what they have seen. (a) We asked if lions ever eat elephants; this provoked laughter until one elderly man said that they sometimes take baby elephants, an observation which gained him many amazed and admiring looks. He then proceeded to describe how he had seen the body of a dead elephant baby, and the body of a dead lion, and sets of tracks which had suggested to him that the lion had killed the baby elephant, and the mother elephant had come and killed the lion.[1] (b) During the questioning about kudu (*Tragelaphus strepsiceros*) fighting, a young man described how he came across two males with their horns interlocked, pushing at each other, and then added that he shot them, they separated and died. Another man imitated the sound of kudu fighting and described this as something to listen for when stalking them for a kill. Someone else did an accurate imitation of the ungulate "flehmen" face, when describing the courtship of eland. The frequent imitations, both accurate in sound and convincing though not necessarily morphologically accurate in gesture, formed a large part of the descriptions (as, indeed, in many ethological discussions!). In fact many of them seem to take great delight in lengthy, detailed, and very gripping, even to the non-!Kung speaker, descriptions of events they had seen. The nonverbal arts of the story teller are very much in evidence, but as far as we can see they did not take licence with the facts. These descriptions also often include considerable detail, as illustrated, for example, by a description of the method by which a leopard kills an animal: the leopard sees the animal and, semiconcealed, crawls slowly toward it until it is lying down four to five yards away; then it springs and grabs the prey at the throat—its arms over the victim's shoulders and legs around its waist. Then winding its tail around the back legs of the animal (they say the leopard's tail is very strong), the leopard bites the prey in the throat.

(4) They will disbelieve each other and on occasion seem to expect skepticism of each other. For example, when somebody said that he had heard that elephants bury their babies up to their neck in the sand, everybody laughed uproariously at his gullibility. A man who described once having seen tracks of ten leopards together at one gemsbok (*Oryx gazella*) kill said that he went back and brought people out to see the tracks because otherwise they would not have believed him. When they came to inspect the tracks themselves, they confirmed that he was correct in his interpretation.

(5) Their response to being asked how they know a particular fact is never defensive; it typically leads to a long and careful description of the observations or of the tracking evidence. For instance, we challenged a description of the hunting conduct of a pair of lions. A man had described how the lions approached together to a certain distance and then split up. One advanced directly a short way and then lay down to wait, while the other encircled the prey and then pounced on it, whereupon the waiting animal rushed up and joined in the attack. We questioned them on the evidence for the timing of the relative acts, and this question was met by careful description of the tracking evidence since nobody in this particular group had seen such an event. The tracking evidence for the paths taken by the two animals is clear enough; the evidence for relative timing of the attacks is that the subsequent tracks of the animal who lay down are not those of a lion stalking near to the prey nor of one about to leap at its prey, but were the tracks of a lion running leisurely in an erect posture.

We were anxious to follow up a description which we obtained in two separate villages from unrelated people of the way lions go about eating an animal they have killed. In particular, we were told that they do not eat the intestines but remove and bury them. This was such a surprise to us that we cross-examined them closely, only to find them obstinate in this view. There were two men who claimed to have watched lions doing this, and we found it hard simply to disbelieve them. We were also told, but now with some impatience, that people use this knowledge to get intestines which they, unlike lions, eat. We found people in both villages who had gone to the site after the lions had moved away and had dug up the intestines to take home.

Direct observations were also convincing for the immense amount of detail that was given, a point which we will return to when discussing the reasons for !Kung interest in animals. A further incredible elaboration of lions' fastidious feeding habits was also followed up: and although we cannot fault the !Kung's answers, at the same time

we scarcely can bring outselves to believe the descriptions they gave us. They said that if during its careful dissection of the intestines from its prey, the lion breaks the intestines and lets their contents spill onto the carcass, it then will not eat the meat and often, indeed, will leave the entire carcass. They simply answered our general challenge by saying that if the lion has gone away, one is likely to see feces on the carcass; but, on the other hand, if the lion has removed intestines and is still there and one frightens it away, one never finds feces on the meat. Up to and excepting the abandonment of soiled carcasses, all these observations have been confirmed by ethologists' reports, either from the Serengeti in Tanzania (Schaller 1972, p. 271) or from the Wankie Reserve in Rhodesia (Douglas-Hamilton, personal communication to Blurton Jones, 1972), and by naturalists' reports from elsewhere (Guggisberg 1961; Stevenson-Hamilton 1954).

During descriptions of the calf-raising practices of various ungulates one man explained how the data for kudu could be obtained easily if one tracks the mother until finding the remnants of the birth. Then one observes the two pairs of tracks going together for a short time, which, in kudu, then divide in two directions. If one follows the baby tracks, one can find the baby hiding there.

(6) The only reports which went against our argument that the !Kung were quite reasonably objective in their reports of behavior are as follows: (a) A man said that the kudu infant is always hidden by the mother, and then said the mother hides it and goes off to eat until she has enough milk, whereupon she returns to feed the baby. The basic facts behind this description seem to be very clear, and this is, indeed, one common way in which ungulates care for their infants. However the confusion of observation of spaced feeding with the causal suggestion that she eats all this time to produce enough milk is different from most of the !Kung remarks and uncharacteristic of their usual distinction between observation and interpretation. (b) When somebody said that he had heard that elephants bury their babies up to their necks in the sand, although it led to general laughter, it did result in one man, again uncharacteristically, saying that he had seen this. On cross-examination it turned out that he had seen a pile of sand and a lot of tracks of elephants with babies, whereupon he wisely had given in to the !Kung view of elephants with infants and had run away without stopping to examine the pile of sand. (c) The discussion mentioned above of whether buffalo calf-raising resembled that of cows was also an example of !Kung nonobjectivity in that the person who suggested this possibility was at least speculating, though we felt that he regarded this as speculation and

not as a definitive statement on buffalo calf-raising. Indeed the examples which we quoted as giving rise to argument also seem to imply a failure in objectivity in the person who made the statement (see part 2 above).

We conclude from this summary of !Kung observational method that their efforts resemble the methods of modern-day western ethology; as regards (1) attention to detail, (2) distinguishing data from hearsay, and (3) general freedom from inference. In these respects their observations are superior to those of naturalists such as Gilbert White and Aristotle, and very sophisticated indeed when compared with the legions of animal behaviorists among western hunters, gamekeepers, and pet owners.

Explanations of Behavior

The general impression gained from these seminars is that !Kung are not particularly interested in explanations about behavior or theories about behavior, although this may have been influenced by our questioning; we focused on data and changed the line of questioning at certain points in a discussion to produce facts.

It was not possible to discriminate completely explanations from what were merely methods of reconstructing behavior from raw observations. For instance, the discussion of why one may see leopards while following a honey guide jointly concerns explaining this coincidence and reconstructing behavior of the honey guide; in fact, the behavior of the honey guide explains the likelihood of meeting leopards in this situation. Konner's observations of !Kung discussions during tracking indicate that they can generate hypotheses at a great rate, but these concerned behavior and condition of the animal that was giving rise to the spoor—that is, they are explaining the spoor. This is different from explaining why animals behave in ways in which they are found to behave, although in a tracking situation the theorizing consists of inducing the behavior from which it is possible to deduce the injury to the hunted animal, as well as the likely time until its death.

However, we may look at the seminars with a view to seeing traces of the distinction that ethologists make between causation of behavior, and functions, effects, or survival value of behavior. Ethologists hold these to be distinct kinds of subjects, distinct questions about why an animal behaves in the way it does, although many people (both laymen and nonzoologist students of behavior) are unaware of these possible distinctions. We may look at the !Kung's statements

both to see whether they make this distinction and to see whether, within these particular fields, they show signs of explanations comparable with western biologists' explanations.

In the area of causation or motivation of behavior the !Kung seem to be very similar to the English laymen in that their motivation explanations mostly boil down to anthropomorphic statements. These usually can be reduced to the statement that an animal does something because it wants to, which is really no more than saying that it does it. When asked why lions' favorite prey was wildebeest (*Connochaetes taurinus*), the !Kung answered that it was because the meat tasted good. We asked whether people found it good; some said that it did, others that it did not, but they held to the suggestion that it tasted good to lions.

Other statements about motivation did not concern the goal. For instance, wild dogs (*Lycaon pictus*) are too frightened to attack people unless there are many dogs together, but in a group they are unafraid because there are many of them, just like people. This similarity to people was also mentioned when they told us that sometimes dogs kill lions, even adult lions, by ganging up on them (as spotted hyena (*Crocuta crocuta*) sometimes do: Kruuk 1972). It was explained that the dogs were not afraid because there were a lot of them, and that even people were not afraid if there were a lot of them. A similar kind of statement about motivation concerns animals who do or do not "have anger;" lions, leopards, and wild dogs were described by one man as the animals that have anger and therefore would take children if one lets them wander about. (Before contemporary scientists feel superior to these inadequate kinds of explanation, we might do well to consider the various forms of drive theory still prevalent in the behavioral sciences; see Hinde 1959).

Some explanations seem to be directly and ethnocentrically anthropomorphic. For example we found two pieces of behavior that were explained in terms of "withholding," a serious infraction against !Kung morality, and one for which great temptations may arise. When asked why lions should bury intestines which they are not going to eat, people answered that they did not know, but added that perhaps the lions were withholding meat from the vultures. One man also felt it was dangerous to take these buried intestines in case the lion felt that they were being "withheld" and took vengeance. (He claimed a dramatic instance of this.) Leopards, although they leave the intestines for the vultures to eat, wedge the ribs of their prey in a tree; but the !Kung say that they never return to eat them. When we asked why they hang things in trees, a man then said,

"perhaps, I don't know, but they may be withholding from the brown hyena" (*Hyaena brunnea*). Another anthropomorphic drive explanation was the description of the baby eland (*Taurotragus oryx*) as lazy because it can be seen apart from its mother at an early age.

Moving on from the motivational field we come to a number of explanations that confuse, to greater or lesser degrees, motivation with function. Being told that leopards do not eat intestines we asked why; and someone, again saying first that he did not know, conjectured that perhaps they were like people in that they did not like eating feces, but unlike people in that they had no hands and could not get the feces out of the intestines. This is, on the face of it, a very reasonable, proposed, short-term justification for why leopards do not eat intestines. But to biologists the absence of hands is most interesting, not as a physical limitation of the leopard's behavior, but as a result of adaptation, or at least as something adaptively compatable with its behavior, especially feeding habits. This our !Kung informant had not taken into account.

That he is not alone among modern man becomes more evident when we discuss some explanations for features of ungulate calf-raising practices. The consensus of opinion seemed to be that on the first day a newborn buffalo calf is left by its mother, but subsequently it follows her except when the mother has to go far away to get water. When we asked why the mother leaves the baby when she goes for water, we were told that the baby can not walk because his feet are still soft. This may or may not be a correct explanation of the immediate causation of the separation, but it implies an error of explanation in terms of survival value which can be found readily in contemporary literature on mammalian development. Some contemporary writers, differentiating caching species from following species, explain this difference by reference to an assumed early state of development at birth of caching species. They assume that the young of such species are unable to walk, or, at least, to walk well enough to follow their mothers (Widdowson 1970). Apart from the clear incompatability of this interpretation with actual observations of the young of caching species (e.g., Walther 1969; and !Kung observations of kudu), this explanation is naive from the evolutionary point of view, since they assume the young of caching species must be born underdeveloped. Yet upon actual observation these writers would find that the underdeveloped state is in no way an explanation of the caching adaptation, and that even the reverse might be the case. Thus it seems that although the !Kung have no clear idea

about evolution or the survival values of behavior, they are not as far behind some of their post-Darwinian contemporaries in this field as one might have expected.

The !Kung do seem to have some rudimentary views on ecology. The group sizes of kudu were held to depend entirely on the number of kudu in an area, and to be extremely variable with respect to season and locality. They argued that game tends to avoid areas where there are cows and people, although they explain this by their being frightened by cows rather than in terms of the lack of grass where cows graze. Indeed in the case of the kudu they may be justified because this species predominantly feeds on leaves (as the !Kung told us) and is one game species which has increased even during the recovery of cattle from the Rinderpest epidemic. It is also held that there used to be a good deal of game in the area, but there are fewer left. No very clear reason is given for this, and they could be referring to the particular area in which a village has not existed for a very long time. It seems to us more likely that any decrease in game is due to an increase in the range of cattle rather than to an increase in the number of people. The cattle denude the ground surface in an area of some miles' radius from each waterhole, which makes access to water difficult for the game—as does the fencing of the wells.

In the conversation that gave rise to these seminars we asked at one stage why the kudu had horns of the kind it has, and why it should have horns at all. Answers ranged from explanations of the shape in terms of the horns of combatant males interlocking (which coincides strikingly with recent biological work) to the statement that God gave him beautiful horns because he wanted the male kudu to be beautiful and to be different from female kudu so that he knew he was a male. (We were also given demonstrations and descriptions of the way that the kudu "cleans" its horns by scraping them in the sand.) God also enters into the discussions at other times, when, for example, he refuses to allow anyone to eat monkeys. And the old people say that monkey meat is bad so one does not eat it, and if one does, one dies. The word translated as "created" was used quite commonly in the discussions of resemblances and differences between animals.

Some discussions, like many a modern western ethology seminar, were rescued by a participant making the important point of discourse embodied in the following quote: "You were talking about its color; we are talking about its meat; if you want to talk about color, then this animal is different." (This remark incidentally capped a highly convincing demonstration that the !Kung were able to

use a number of different classifications of animals and to move from one classification to another readily, an ability that is supposed to be little developed in "primitive" people. See Bruner et al. 1966.) In discussions of how carnivores hunt, and whether they do it like people, one man remarked that most carnivores hunt at night because they have noses and do not need to see far. (This is another reverse survival value explanation.) The lion's seeing with its nose was described during an account of a lion's hunt a little while later in the same seminar, and another man then added that a lion uses both its nose and its eyes.

The paucity of explanations alongside great richness of data about animal behavior makes an interesting contrast with the situation in animal behavior research in the west some sixty years ago when the literature was abundantly full of theory and empty of data. Our impression is that this has been true of animal behavior and psychology in other historical periods. The contrast of theory and data also can be seen in the difference between ethology and psychology; the latter has a greater concern for large scale theories and for testing hypotheses and a much smaller concern for amassing data (a lesser faith in human "inductive" ability?). Is it possible that there is in practice some incompatibility between a turn of mind geared to theorizing and a turn of mind geared to recording and discovering facts? What the !Kung seem really good at is working out what happens, not in explaining it or theorizing about it.

Why !Kung Study Behavior

The obvious answer to the question, that any people who hunt animals must know enough about them to catch them, can take several forms. In terms of natural selection it would seem to be irrefutable. But in terms of the resulting motivation the answer is not quite so clear. We would like to suggest four lines of evidence bearing on the motivation of !Kung interest in behavior. (1) First is the question of which species they know most about, whether they only know about their prey species and perhaps their possible predators or competitors, and know little and care little about other species. It is difficult for us to be confident about our conclusions on this question because our interests limited the issues we questioned. However we do have some idea of which animals the !Kung know most about.

(2) We have evidence that they sometimes observe animals more than is necessary for the purpose of the hunt in which they are in-

volved. For example, one man described courtship of a pair of gemsbok in great detail, adding that he was so involved in watching them that he forgot about shooting them, and they went out of sight before he was able to.

(3) Often during the seminars people would begin to discuss some point among themselves and recount observations to each other. This added to our very clear impression that they found the topic interesting for its own sake, and found the seminars highly absorbing. A strange feature of these discussions was that the participants seemed to gain a lot of new information, or at least heard about observations and generalizations concerning behavior which were quite new to them. This implied to us that the !Kung might not of their own accord discuss animal behavior very much, but what they do is to report at length and dramatically individual excursions and hunts.

(4) This gives rise to our fourth line of evidence about the reasons for knowledge of behavior. This is the immense amount of detail that they remember and, therefore, see when they are watching an animal. For example, we have mentioned their descriptions of the way a lion eats an animal it has killed. Admittedly this is a situation where if a !Kung sees it he can do little except watch; and he is interested in watching and waiting until he decides the time is right for lighting a fire to chase the lion away. But he need not observe or recall any details of this situation other than to note the responses of the lion to other lions or vultures or other creatures about, or to gain a very general impression of how much the animal has eaten. The amount of detail observed and remembered and the evident delight in recounting these observations suggest to us that natural selection has arranged for a greater interest in animal behavior than that aroused by the practicalities of any specific hunt. This provides a system in which a large store of information is accumulated and communicated, and which may or may not turn out to be of use in hunting. The motivation of these activities seems only indirectly related to hunting. Stories are told not because someone wants to go hunt a particular animal but because people are gathered around the fire, and someone has been on a hunt, or needs to entertain a visitor. This indirect adult communication of important information seems comparable to the indirect way young men acquire information about animals and technology, which appears to be quite simply a matter of watching and listening to other people and then trying for one's self. There is almost no direct teaching. Indeed Konner[2] witnessed an enlightening argument between some younger men who hunt very little and some

older and more active men. The inactive young men accused the older men of having neglected to teach them hunting. The older men countered that this was not something that one taught anybody, it was something that one just did. "You teach yourself"—a very common phrase among the !Kung—would be applicable here.

The evidence concerning the phylogenetic extent of their knowledge is difficult to evaluate. Most of our questioning was about ungulates because of Blurton Jones's interest in their maternal behavior, and much of the rest was about carnivores because of the !Kung's interest for survival and our interest in establishing whether carnivores were in any way a threat to children. They know a considerable amount about both these groups. And they appear to know more about lions from whom they scavenge than about other carnivores who seem to be of less importance as a source of food, or perhaps less difficult to chase away. They appear to know rather little about monkeys, probably because the only monkey found anywhere nearby was a vervet (*Cercopithecus aethiops*). It is perhaps relevant to remark that although they know about baboons and have occasionally seen them, most of the remarks about baboons came from one man's observations of a captive baboon that some white men had once had some miles away. But he was able to imitate and describe in enormous detail feeding behavior of this captive baboon and the foods it ate.

The seminars were occasionally interrupted if one of the participants got hold of our field guide to mammals. Once they were used to looking at the pictures (which took only a minute or two), these were a source of endless fascination; and, as far as we could see, any species was interesting. However, much of the fascination with the book was perhaps fascination in learning to see the pictures and a delight in being able to teach friends to do this. Another favorite evening occupation was to look at the Konners' color slides of familiar people or places. Although we asked very little about birds, unless sometimes trying to collect !Kung names for those illustrated in our books, their knowledge appeared to be extensive. An indicated above, they do describe accurately the mobbing behavior and inadvertent leading behavior of the honey guide, although without apparent grasp of the motivational issues involved. However, Konner did hold one seminar on bird behavior, with the help of Peter Jones, the Oxford-trained, Botswana government ornithologist. There was only one such seminar, and it focused on the behavior of passerines, especially quelea (*Quelea quelea*). But it did not generally inspire a level of confidence in !Kung knowledge comparable to what we were

accustomed in discussions of mammalian behavior. Still, one anecdate is noteworthy.

Subsequent to the bird behavior seminar Konner, Shostak, and Jones were traveling with two !Kung men by Land Rover. Knowing of Jones's interest in quelea (he had been retained by the government to explore possible solutions to the serious quelea pest problem), the two men pointed out a low stand of thorn bushes which, at a distance, looked like any other but which, on close examination, proved to have been stripped of leaves on the distal few inches of their branches. The men said that this had been done by quelea, which were in the habit of preparing bushes in this way and then returning after a few days to rest on the ends of the branches. This observation, which was unknown to Jones, and which proved to be correct, enabled him subsequently to improve greatly the efficiency of his investigation and to collect at an early stage of the nesting cycle specimens previously inaccessible to him (Peter Jones, personal communication).

We asked nothing about snakes, but we were told that one we disturbed had to be killed, because although harmless, or at least not poisonous, "it climbs up your legs and goes into your anus. . ." This is in all likelihood a myth about the relatively unknown, comparable to the giant baboon that lurks in the bush waiting for unsuspecting women. However the snake in question fled up a small bush, and it is quite conceivable that it sometimes mistakes peoples' legs for trees, thus giving some substance to the legend.

Instances which provide evidence of the seriousness of their interest in behavior occurred when one night around midnight the seminar was joined by four additional men who sat quietly behind the participants, listening with concentration and without fidgeting, talking, or yawning despite the lateness. Also, during seminars people occasionally volunteered new information that we had not asked about, and on one occasion a man volunteered new information about lion hunting behavior and then moved on spontaneously to hunting behavior of wild dogs.

Evidence that knowledge of behavior is closely related to its applied value comes from many statements made during the seminars. In discussing fighting of kudu, one man described the sound one hears of their horns crashing, and how, if one hears the sound, one can approach to shoot them. The same man, a very enthusiastic and busy hunter also described how one tracks infant kudu, showing that it sleeps away from its mother, and that one can follow it to where it is hidden, and kill it by hitting it. In telling us how wildebeest

infants follow their mothers soon after birth, they said that one can not catch the infants because they follow their mothers so soon.[3] But in the same seminar people described the way in which the kudu mother returns and calls the fawn, which then runs out to join her to feed, and how the mother never goes to the place where the infant sleeps, a degree of detail which seems hardly necessary if one is simply trying to shoot mother or baby. Someone also described going again and again to the same place to examine sets of tracks of a mother which had been visiting an area repeatedly for several weeks. He found tracks going back and forth and failing to understand this, returned continually until he found tracks of the same animal with an infant. He then realized what was happening and, after this, tracked the pair and killed the infant. This whole procedure apparently took two months, and it seems hard to believe that the mystery of these repetitive tracks was not as great a motivation to make the man persist in his studies as was the slender possibility of catching up with the animal. Whichever his motivation, the man was clearly utilizing a long process to reconstruct from repeated observations of the tracks the behavior and the causal situation of that behavior.

The following observation indicates the practical value of knowing in detail the hunting behavior of competing predators. A !Kung man described how he and a lion were pursuing the same giraffe, or rather stalking it. The lion was the first to charge and climbed up the giraffe while fighting at it, but the !Kung and his companion frightened the lion away and shot an arrow into the giraffe. The giraffe ran off, and the man followed it for the rest of the day while it was still living. In the meantime, the lion was nowhere to be seen. The man went home, came out the next day to find the giraffe dead, and took the meat. During all this time the lion had failed to reach it. This may be a dramatic instance of the difference in the size of the home range of lions and people. !Kung hunts may cover enormous distances, but we have no information as to how much distance they cover geographically since their prey often turn back and go over the same ground again. But one man reported shooting a buffalo (which in itself is rather unusual), whereupon it ran away. What was unusual was that he shot it *before* it fled and claimed to have tracked it the next day for twenty miles, still failing to catch up with it. It must be noted that the !Kung method of killing requires extensive tracking *after* the first wound is made with the poisoned arrow, while the poison is taking effect. (Lion hunting procedure typically requires tracking, if any, only before the attack.)

We presume that much information is gained from descriptions of hunts and observations of others, but, as mentioned above, it seems to be mainly a matter of listening to people story-telling, and not a more highly ordered system of information transmission.

Whether the !Kung interest in animal behavior is of ultimate practical value in every case seems to us, finally, beside the point. The point is that evolution has produced in them an inquisitive turn of mind which leads them to explore problems and accumulate knowledge beyond what it is most immediately necessary for them to know. This turn of mind evidently proved more adaptive than a severely pragmatic approach, because evolution retained it.

In one seminar the !Kung listed four mental qualities essential in hunting: knowledge (*chi!ā*), sense (*kxai ≠n*), cleverness (*/xudi*), and alertness (*chiho*). Konner's observations of conversation during tracking reflect the sort of mental process that selection for hunting has retained. In effect, there is a set of problems to be solved by the hunters over the course of several hours or days, and these problems re-present themselves continually: Where is the animal now? Which way is it going, and how fast? Is it likely to stop or to reverse direction? Where and how seriously is it wounded? How long will it live? Answering these questions requires adducing evidence concerning time of year; time of day; heat; wind direction; terrain; depth, shape, and displacement of tracks; condition of feces; condition and displacement of grass, twigs, and shrubs along the spoor; amount, position, and color of blood on the ground, grass, and bushes; and the store of knowledge concerning the behavior of different prey species, especially when under attack, which we have demonstrated.

Some of this evidence is utilized in a simple way. For example, only gemsbok among antelope can be successfully hunted with dogs, because only they will consistently stand and fight the dogs, as opposed to fleeing. But most items of fact must be integrated in a complex way with all the other rapidly changing variables of the hunt. Typically, in the course of following an animal, a working hypothesis as to his position or condition will be advanced and then tested continually against the spoor. For example, Konner accompanied a man returning from an unsuccessful kudu hunt. It was early afternoon. They began following a gemsbok spoor which, the man said, was made the same morning. After about twenty minutes the man stopped and said, "No, it was made last night," and abandoned the spoor. Asked what made him change his mind, he indicated a single gemsbok hoofprint with a mouse track inside it, that is, superimposed on it. Since mice are nocturnal, the gemsbok print must have been left during the night.

If two or more men are hunting together, they will discuss, within the obvious noise restriction, the evidence bearing on the working hypotheses, and argue in a way not dissimilar to the discussions in the seminars. Konner observed a zebra hunt in which the working hypothesis, that the zebra was wounded high on the body, had to be abandoned when a man showed that grass, which had been bloodied near its high tip, had first been bent to the ground by the passing animal, bloodied by its foot, and then returned to the upright position after the animal passed. Thus the hypothesis of a wound in the foot was still sufficient to account for the data.

Such an intellective process is familiar to us from detective stories and indeed also from science itself. Evidently it is a basic feature of human mental life. It would be surprising indeed if repeated activation of hypotheses, trying them out against new data, integrating them with previously known facts, and rejecting ones which do not stand up, were habits of mind peculiar to western scientists and detectives. !Kung behavior indicates that, on the contrary, the very way of life for which the human brain evolved required them. That they are brought to impressive fruition by the technology of scientists and the leisure of novelists should not be allowed to persuade us that we invented them. Man is the only hunting mammal with so rudimentary a sense of smell, that he could only have come to successful hunting through intellectual evolution.

Nonscientific !Kung Beliefs about Animals

In order to avoid leaving the incorrect impression that all !Kung beliefs about animals are arrived at through strict induction, we mention briefly several nonrational beliefs:

Myths and the myth cycle: Stories are told (Biesele, Chapter 13), of an ancient time in which the identity of various animal species is closely enmeshed with that of mythic-heroic human figures. Some stories have the heroes turning into animals when they get into situations in which they need the animal's characteristics. These are told to account for the origin of some species (for example, the antbear, *Orycteropus afer*).

Baboon rapist: Women are warned not to walk in the bush alone at night lest they be attacked by a giant mythic baboon of remarkable sexual appetites. This possibility does not seem to dissuade them, though other, more realistic ones do at times.

Bird possession: Infants are sometimes said to be "possessed" (an unsatisfactory translation) by predatory birds which they see while sleeping. A parent recognizes that the infant has seen the bird because

it clenches its fists at that moment in its sleep, like the bird closing its talons. After this an elaborate ritual must be performed daily to prevent the child's death, and some infant deaths are attributed to such possessions.

Others: As mentioned above, some harmless snakes are said to run up people's legs and enter their anuses. Millipedes are for some reason treated with utter revulsion and never touched under any circumstances, because of their alleged smell. A large caterpillar seen only in the rainy season is said to cause malaria.

Summary: A number of nonrational beliefs about animals may be enumerated, but these seem to play a small role in day-to-day !Kung life and in their interactions with animals. Bird possession is the only one people treat quite seriously. In other words, such beliefs do not interfere with the study of animal behavior. They seem to exist in a domain of the mind quite separate from ethno-ethological knowledge.

Conclusions

We regard our material as showing in summary:

(1) That !Kung have an advanced ability to observe and assemble facts about behavior and to discriminate facts from hearsay and interpretation. In this ability they surpass lay observers and many professionals in western society.

(2) Their explanations of animal behavior are, in contrast, not very notable. But it is important to remember that the faults in their interpretations that we have pointed out (theories of motivation which are tautologous, teleological, "survival value" explanations) are commonly found in western man, even among western scientists.

(3) Their motivation for acquiring knowledge about what animals do goes far beyond the immediate, momentary needs of hunting (beyond what is needed for successful hunting of the animal that they are observing). We suggest that this level of interest nonetheless may be of adaptive value. Knowledge is acquired when not needed, when the pressure is off, but it may well be useful at another time; or, collecting it may in some instances be "vacuum activity" in the strict ethological sense. In any case it is clear that the habits of mind involved will have been strongly selected for.

(4) There seems to be relatively little transmission of information from one man to another, even from old to young.

Perhaps verbal transmission of information is indirect, through people telling the story of their day's excursion as opposed to direct lecturing of old by young. Thus, as with (3) above, knowledge may

be acquired mainly "out of context," in the relaxed social setting of the early evening, but it is then available when needed. One wonders if the trade-off for the rather patchy nature of the knowledge transmitted is a greater efficiency in the "filing" and retrieval of information stored in a system of the subject's own construction. This system is put to use when the subject wants to listen and when the story teller's art gives many pegs on which to hang the information, and is quite different from one where he would try to store in his head someone else's data filed on that person's system.

The explanation for the fact that knowledge gained "informally" is assimilated more easily and rapidly than knowledge gained under pressure or direct instruction lies somewhere common both to that psychological suggestion itself and to the fact that it usually *is* acquired this way in !Kung society. We have to ask why knowledge is acquired this way, and the answer to that may be also the answer to "why does memory work that way?" One suggestion, itself raising further questions, is in the adverse reaction many people have to direct instruction. Not only can they be intimidated and confused (Holt 1969), but Lee (1969b) and Gould (1969) indicate that !Kung and Yiwara Australian aborigines can be irritated by and can disapprove of people who tell other people what to do or in any way set themselves above anyone else. This presumably (and the people think so too) relates to very basic features of their society and its ecology such as food sharing. Since it is highly probable that successful exploitation of the social hunting niche depends on extensive food sharing, this is a powerful force among the selection pressures on hunter-gatherer behavior. It is not, perhaps, far-fetched to suggest that this force may have been strong enough for long enough to set constraints on the way that information was best transmitted from person to person and acquired by individuals. However, this is highly speculative, and we would claim to have demonstrated little beyond the importance of reexamining our ideas on the function of old people as teachers or libraries (we suggest they are not reference libraries but are dramatized documentary television) and of examining closely the ways that information about subsistence is acquired and transmitted in hunter-gatherer societies.

In the philosophy of science it is usually supposed that the purpose of a theory is to predict events in the future or in novel situations (though there are heretics among biologists who will comment that theories are a cover for ignorance, that theories and explanations always turn into descriptions when you really understand them). One might have thought then that there would be great survival value for

the !Kung to have powerful theories about animal behavior. The perfect theory would allow one to predict even more of the behavior of every animal in every situation, and perhaps to contrive situations which maximize hunting success. But the !Kung with whom we talked did not seem to be great theorists. They simply loved to know about what animals do. There are several possible answers to this apparent paradox:

(1) There cannot be any grand universal theories of behavior; the nature of the data, primarily the diversity of species, forces on !Kung and biologist alike a greater respect for facts and for the diversity of life than for attempts to explain them in a simple way.

(2) The antipredator behavior of many species includes a highly adaptive random component—it is in part genuinely unpredictable (Driver and Humphries 1970; Humphries and Driver 1970). (But !Kung probably know when it becomes unpredictable and in what parameters it is unpredictable, for example, being ready for gemsbok at bay to charge or to run again.)

(3) The theory of behavior they use, an introspective, anthropomorphic interpretation, is adequate or even better than adequate. (We know cases where they emphasize a similarity to people—"Wild dogs are like people, if there are a few they are afraid, if there are many they are not afraid." So they are clearly aware that some animals react like people in some respects, but that others do not.)

Biologists do have one grand universal theory, the theory of evolution by natural selection. The !Kung do not have this theory, as far as we know. We should probe further about where animals come from and why they differ, before being totally confident about this. Besides the practical knowledge of animals that we have discussed, the !Kung have a rich mythology about animals, including stories of a mythical remote past. These were never referred to in our seminars, though on other occasions Konner discussed these matters with people, and Biesele (Chapter 13) has made a careful study of them. The two areas seem to be completely different compartments of intellectual life, and the existence of creation myths need not exclude an evolutionary theory. Indeed, the origin myths do accommodate biological change, holding as they do that all animals evolved from people; and the concept of adaptation does figure in them since the transformation often occurs when the animal's human progenitor has gotten himself, through mischief or stupidity, into a situation where he really needs some key adaptive feature of the animal. For example, the antbear "evolves" when its human ancestor tries to escape some pursuers by fleeing underground. In a short while his

hands turn into antbear claws, more suitable for digging, a Lamarckian sort of change through adaptation. However, the references to God giving the male kudu horns is an indication of putting God and his motives in precisely the logical position of the theory of evolution by natural selection.

Answer (1) above seems to us the most likely. This is a subject that has been discussed fairly thoroughly in the biological literature (Lehrman 1953; Hinde 1966, or 2nd edition) with the conclusion that the diversity of animals is such as to make the possibility of a general theory of motivation and behavioral mechanism highly unlikely. It seems as if the !Kung would do best simply to know a lot about each animal. Though this would not preclude a pay-off for predicting from a well-known species to a little-known species through some kind of taxonomy, they are clearly aware of and act according to *species* difference. "Look out, it's a gemsbok not a kudu, you know." (Gemsbok charge, kudu do not.)

It is tempting to suggest that the history of "grand unifying theories of behavior" in animal behavior has a parallel, if not actually more than one, in the possibility that if one's main concern is animal behavior, one's main problem is variety and the initial strategy is to acquire factual knowledge; whereas if one's main concern is human behavior, one's main strategy is to apply an introspection-based theory. The latter may work very well on humans, but it cannot be extended far into the variety of animals.

In discussion of our results we are, perhaps, handicapped by being practicing scientists rather than full-time students of the history of science. Nonetheless we should attempt now to evaluate two of the points we raised in the introduction. Man's evolution in a hunter-gatherer niche should have meant that there was selection pressure on the human brain such that man became interested in animal behavior and competent about finding out about it. We can say that the !Kung are clearly interested in animals and animal behavior; the extent of their enthusiasm is hard to convey. They are also clearly very competent in finding out about animal behavior *from nature*—from the animals themselves and their tracks. In contrast they seem to acquire less from each other than we might expect. They distinguish sharply between observed behavior and hearsay and interpretation. In this respect they have an ability and an approach which is also one of the basic features of the scientific method and which has most sharply distinguished science from other intellectual pursuits. They are able to and they believe it important to distinguish reports of observation from other kinds of statement.

This brings us to the second point: the narrow perspective of some writers on the history of science and the intellectual achievements of urban man. We must concede that the !Kung show no use of mathematics, and on the whole do little experimentation (also true of the majority of western man). But it seems equally clear that the !Kung have, and use in their profession, some of the intellectual requirements of modern science.

Every contributor to this volume could hammer his or her own nails into the coffin of western man's dramatizations of his intellectual rise from the Stone Age. We found our animal behavior seminars chastening at many levels. The sheer volume of knowledge is breathtaking. They laughed to hear that there are people who think that the spotted hyena only scavenges; they know that lions sometimes scavenge from hyena kills; and so on and on. The accuracy of observation, the patience, and the experiences of wildlife they have had and appreciate are enviable. The sheer, elegant logic of deductions from tracks would satiate the most avid crossword fan or reader of detective stories. The objectivity is also enviable to scientists who believe that they can identify it and that the progress of science is totally dependent upon it. Even the poor theorization of our !Kung left one uneasy; their "errors," the errors of "Stone Age savages," are exactly those still made today by many highly educated western scientists (tautological theories of motivation, inadequate application of natural selection theory). We have gained little or nothing in ability or intellectual brilliance since the Stone Age; our gains have all been in the accumulation of records of our intellectual achievements. We climb on each other's backs; we know more and understand more, but our intellects are no better. It is an error to equate the documented history of intellectual achievement with a history of intellect. It is an error to assume that changes in about 7,000 years of urban civilization represent a final stage in a progress which can be extrapolated downwards into our preurban past. Just as primitive life no longer can be characterized as nasty, brutish, and short, no longer can it be characterized as stupid, ignorant, or superstition-dominated.

Sharing, Talking, and Giving: 15

Relief of Social Tensions among the !Kung

Lorna Marshall

People sitting close together