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More on Tool Use Among Primates

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that the course of cultural evolution, like that of bio-evolution, is strewn with the wreckage of ingeniously functional dinosaurs. This does not deter us from inquiry into the functional relationships which account for the adaptive radiations of extinct forms. Low-energy agrarian cultures are undoubtedly doomed, not because they are negative-functioned, but because there is selection in favor of more efficient techno-environmental and techno-economic arrangements. Bennett renders a disservice by trying to cram my article into a synchronic functionalist mould. He is baffled by my suggestion that the "inefficiencies of the system" are "part of its functioning." Why is this so difficult to comprehend? In comparison with "tribal" cultures, the ecosystem of contemporary India is a radical evolutionary advance. (How else shall we describe the huge population?) In comparison with high-energy industrial systems, on the other hand, the whole ecosystem seems drastically inadequate in terms of the survival and well-being of its human components.

The CA☆ discussion which followed my article emphasized the possibility that the ecosystem in question is deteriorating. It seems obvious that the efficiency of the whole system is in decline as a result of population pressure. There are too many people and hence too many cattle. The wretchedness of both the human and cattle populations intensifies our expectation that evolutionary modifications in the traditional ecosystem are about to take place. This does not mean, however, that the system as it stands, even with all of its consequences in terms of hunger, disease, and suffering, has ceased to be functionally superior to the more primitive systems from which it emerged.

I do not subscribe to the theory that cultural evolution occurs only through the advent of otherwise insoluble crises. Most evolutionary modifications consist of the replacement of functioning features by better-functioning ones. If Bennett shares this view of evolution, then he must take account of the remaining strengths of the positive-functioned aspects of the Indian cattle complex. Applied anthropologists have the professional obligation to be certain that their intended innovations are functionally superior to what they are trying to replace. It would be convenient if the cattle complex could be regarded as a product of silly superstitions and ignorant mismanagement. Under such circumstances anything that would work would be better than nothing. Indeed, many "experts" seem to think that this is the case. My article refutes that point of view. Although I certainly did not intend to become involved with the question of development priorities, I can see how the article might be useful in that connection. It might help to create an understanding of the vast scope and intricate nature of the modifications which are required if the survival and well-being of the Indian people is to be advanced in fact as well as in theory.

To my dismay, Bennett declares that "the program suggested by Harris' findings would seek small but significant modifications in the existing regime to make cattle more directly related to cash agrarian economy." Since my article was not concerned with cultural change (but rather with the relation between ideology and ecosystem), any such "suggestion" is strictly Bennett's responsibility. But since he makes applied anthropology the fulcrum of his critique, I wish to disown his proposal whether or not my article suggested it to him. His program strikes me as just the kind of desultory tinkering—a result of too much expertise and too little general theory—which is associated with the recently confirmed increase in the gap between the developed and underdeveloped nations. Bennett says that my paper "illustrates some of the dangers of applying microcosm theory to macrocosms." Let us ignore the erroneous association between ecosystems, tribal studies, and microcosms. (The concept of ecosystem is not a product of tribal studies, and studies of ecosystems are usually rather macrocosmic compared with studies in which a single species or population is the focus.) The "experts," it seems, are interested, not in the kind of overview which is contained in my article, but in the local variation in cattle management. How can we expect anything more than local results from local measures? What my article should have suggested, if anything, is that it is too late for patchwork. A whole new ecosystem is needed. India, contrary to Bennett's advice, needs both steel and more food. It will get neither, it seems to me, if its population continues to expand and our aid program continues to operate without a general theory of cultural evolution.

More on Tool-Use Among Primates[1]

by S. L. Washburn☆ *and*
Phyllis C. Jay☆

Berkeley, Calif., U.S.A. 16 vii 66

It is unfortunate that Kortlandt's comments (CA 7:215-16) on Hall's paper, "Tool-using performances as indicators of behavioral adaptability" (CA 4:479-94) were published without alteration after Hall's death. Surely Kortlandt would have preferred to put his criticisms in a general form rather than address questions to the dead. We will never know how Hall might have answered, but, since there are important evolutionary issues involved and since we think that Hall was correct, we would like to reply to Kortlandt.

Kortlandt objects that Hall judged the evidence Kortlandt and his collaborators had collected inadequate and did not accept the evolutionary model that they proposed (Kortlandt and Kooij 1963). In supporting Hall's position we will consider first the nature of tool-using, then Kortlandt's evidence, and finally Kortlandt's evolutionary "dehumanization" hypothesis.

Interest in human evolution and the way modern man uses tools has distorted the way in which manipulation of objects by non-human primates is described and evaluated. We agree with Hall's assessment of the evidence: —that object-using to a very limited extent occurs in a variety of animals and is not necessarily a mark of particular intelligence. Oakley (1954) reviewed much of the evidence for tool-use and came to the conclusion that it is *skill,* not merely use, which characterizes man. In spite of the fact that all primates have hands and feet adapted for grasping and manipulation and use the hands a great deal in feeding, the chimpanzee is the only primate aside from man in which any substantial item of the diet is obtained by the use of a tool (Goodall 1965).

Object manipulation by non-human primates is limited almost exclusively to agonistic displays, and it only causes confusion to label the object used in the display a "tool." For example, *in addition to* the gestures, postures, running, vocalizations, etc., of display, the gorilla may pull grass or other vegetation and throw it about (Schaller 1963); chimpanzees may pull off branches and wave them or pick up rocks and fling them (Goodall 1965); orangs drop branches (Schaller 1961); and many species of

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monkeys will jump on or shake branches. In a display the point seems to be to appear as large (hair erection), fierce (facial gesture and posture), and noisy as possible. A handy branch or, more rarely, a stone, may be used to enhance the effect. The use of the object as a part of display is very different from anything that we normally think of as tool-use. It is the confusion of the object-in-display category with tools, in the sense of weapons, which confuses the thinking on tool-use in monkeys and apes.

It is not useful to use a category like "throwing" without a careful statement of the situation in which the throwing occurred. In display, throwing is part of a sequence of integrated behaviors, and intepretation depends on understanding the function of the sequence as a whole. The only descriptions of agonistic object-use which permit functional analysis are those that give the whole display and the conditions which elicited it. Few such descriptions exist. (The situation is precisely comparable to what happened in social anthropology with the introduction of the functional point of view of Radcliffe-Brown and Malinowski; the earlier accounts did not contain the answers to the questions.)

If the majority of what has been called "tool-use" in non-human primates is use of an object in agonistic display, then it is easy to see why tales of throwing and clubbing are almost useless as a source of data. The results from a questionnaire would only be useful if the questions were framed by someone appreciative of the necessity of functional analysis and answered by those who had a substantial understanding of the whole context of the behaviors they described. Hall and Kortlandt disagreed completely on what constitutes useful information on tool-use. We agree with the position taken by Hall.

Kortlandt's reliance on information obtained from zoos raises the question of what constitutes useful evidence on tool-use. Behaviors evolved under free-ranging conditions and the adaptive meaning of behaviors can be appreciated only in their natural setting. A cage can hardly be regarded as a natural setting, and we have never seen a zoo cage that provided the kind of objects which may be used in displays under natural conditions. Though many chimpanzees and gorillas have been observed in zoos, in neither species was the nature of events in the agonistic display seen until they were observed in the wild. For example, chest-beating was described as an isolated action rather than as part of a sequence of behaviors. Behavior in a zoo shows that an animal is capable of doing certain things, but it gives virtually no information on meaning or frequencies of normal behaviors. For example, since throwing objects is part of the normal agonistic displays of chimpanzees, it is not surprising that this behavior occurs in zoos. If it is rewarded, the behavior may become frequent, and, since feces are usually the only object available in the cage, some chimpanzees become efficient throwers of feces. It would be a mistake to conclude from this, however, that feces-throwing is a part of the normal display of free-ranging chimpanzees. The frequency with which sticks are used in agonistic display can be increased by making sticks easily available, as Kortlandt did. Under certain circumstances chimpanzees will throw sand, if that is all that is available. None of these situations can be used as evidence that chimpanzees of times long past used feces, sticks, or sand more than contemporary chimpanzees; they cannot provide support for the theory that the ancestral apes were more human in their behaviors than the contemporary ones. In each of these situations, man has so limited the environment and arranged the rewards that a kind, or frequency, of behavior appears which is very different from anything observed under natural conditions. A parallel example is that while rhesus monkeys can be trained, as in India, to dance in a bipedal position, this does not mean that the ancestors of rhesus monkeys were dancing bipeds!

In summary, we think that Hall was right to restrict the evidence he used to those accounts that described tool-use in context under natural conditions, and we believe that Kortlandt's confusion comes from reliance on questionnaires, behavior in captivity, and the accounts of untrained observers.

Quite aside from the Hall-Kortlandt controversy, viewing object-use-in-agonistic-display as a behavioral category helps to resolve two kinds of questions. The first question is why tool-use, which seems so obviously adaptive, did not evolve in other lines of primates. Almost all of what has been called tool-use is either an artifact of conditions in zoo or laboratory or is object-in-display. The function of the object-in-display is to increase the effect of the display; since actually hitting is usually not the objective, there is no selection for more accurate aiming, or for more suitable branches—in short, no selection for an effective tool. The second question is how human tool-using evolved. Recognition of the display function of objects suggests that the answer may involve both this use and the use of objects for economic purposes *and* from display (Washburn, CA 4:492). The kind of display given by chimpanzees might lead, over many thousands of years, to both use of clubs and accurate throwing of stones, based on repeated experiences that actual hitting was even more effective than display alone. This theory gets around the problem mentioned by Mech (CA 7:200) that a weapon used ineffectively would be worse than none at all. Agonistic displays are effective even if the object used in the display does not hit. The object is only a part of the behavior sequence and may actually be an unimportant part. The use of objects in display, particularly the kind seen in chimpanzees, is the general kind of repetitive situation in which small changes may be rewarded that is most likely to lead to evolution. This is why we find Goodall's rich accounts of the behavior of chimpanzees so interesting. Far from being dehumanized, chimpanzees give us the closest parallels to the way of life of our ancestors.

There is one final point which we think must be stressed, although neither Hall nor Kortlandt did so. Detailed similarity in a behavior, such as throwing, is only possible if the underlying structures are similar. The action in man and chimpanzee looks similar because the anatomy of the arms and trunk, and especially that of the shoulder (Grand 1964) which allows overhand throwing, is the same. Overhand throwing is impossible for most primates because they are quadrupedal. It was accounts of overhand throwing in primates in which it is anatomically impossible that first made us very suspicious of the usefulness of casual accounts of throwing. From a structural point of view, underhand scooping and throwing is very easy for the knuckle-walking chimpanzee and gorilla, being only a minor change from the usual swing of the limb in walking. In a quadrupedal monkey, such as a baboon, the hand is placed on the object palm down and the object is tossed forward in an action very close to normal locomotion; scooping underhand is difficult and overhand throwing impossible. Progress in understanding manipulation of objects by primates will come from the detailed analysis of what the animals do, from study of the underlying structures that make the actions possible, and from seeing these actions performed under natural conditions. Both experiments and field observations are needed, and we are a long way from understanding even such an apparently simple category as tool-use.

Reply

by Adriaan Kortlandt☆

Amsterdam, Netherlands. 30 IX 66

Washburn and Jay's reply to my comments (CA 7:215-16) on Hall's paper on tool-use in primates (CA 4:479-94) gives me an opportunity to express not only my regret that I was unable adequately to adapt my comments after Hall's death, but also, more in particular, my very deep regret on the loss of this outstanding colleague. He and I were always friends. I am convinced, therefore, that in the scientific discussion between him and me no personal elements were involved, and I would regret it if anybody took my comments in a personal way. Certainly Hall would not have done so.

It is difficult for me to reply to Washburn and Jay's argument that

> the results of a questionnaire [i.e., Kortlandt and Kooij's (1963) questionnaire] would only be useful if the questions were framed by someone appreciative of the necessity of functional analysis and answered by those who had a substantial understanding of the whole context of the behaviors they described.

I would like to know exactly *what* questions Washburn and Jay would frame, and precisely on *which* points they disagree with the type of functional analysis we have applied. Such comments would stimulate future research. (The general considerations given by Washburn and Jay in their present comment by and large run parallel to our argument.) Finally, it would be instructive to know *which* people, or categories of people, they would discard as sources of evidence. I admit that this is an important issue. When I am interviewing people on animal behavior, I often ask them to act the animal they describe. Their ability to do so gives an indication of their observational acuity. Other checks are more difficult to paraphrase. Busy people involved in administrative work are often bad observers, even if they have "a substantial understanding of the whole context of the behaviors they describe." The reports of several such people had to be discarded from our enquiry results. I therefore disagree with Washburn and Jay's implicit view that sophistication guarantees reliable observation and lack of training implies unreliable observation. Too often animal psychologists of some repute are bad observers. What is relevant is not what training the observer has had, but whether or not his account is correct and precise enough for the purpose for which it is intended.

For the rest, I would like to stress again that we used a *combination* of questionnaires, personal interviews, reliability checks of respondents and interviewers, plain observation, straightforward experimentation, film registration of motor patterns, etc., both in zoos and in the wild (in contrast to Hall, who used *only* field data from the *literature,* some of it unreliable); and that we applied both functional and motivational analysis, because we felt that any less multifaceted approach would yield less dependable results. The enquiry method was included because "the major advantage . . . is that one can exploit the collective knowledge gathered by hundreds of people during their lifetimes," in spite of "its great disadvantage . . . that the quality of the reports differs greatly and one must evaluate carefully the trustworthiness of the respondents" (Kortlandt and Kooij 1963:63). It is only for reasons of discretion (and, if I remember correctly, at the request of the editor) that we did not disclose in complete detail how we checked the reliability of our respondents and interviewees. I cannot see why naturalistic field observations *only*, as advocated by Washburn and Jay, should yield more reliable results than the *combination* of enquiries, naturalistic observation, and experimentation, *both* in the wild and in zoos. On the contrary, I think that the a priori exclusion of any particular kind of data may lead to biased conclusions.

Similarly, in my opinion, it is irrelevant to argue against my experimental approach on the grounds that I made sticks available, or more sticks available, for the intimidation display by chimpanzees. In the wild, there are alaways some sticks available. Moreover, in the wild, intimidating chimpanzees do break off branches and small trees, and do use these as intimidation tools, as I had observed already several times during the 1960 fieldwork. The essential issue is not the methodological question of whether or not one is allowed to make sticks (more) available. In actual research it is facts, rather than methodology, that count. Methodology is only talking about the techniques to obtain facts, but the ultimate aim remains the facts, not the talking. The facts are, briefly:

1) Chimpanzees in captivity use sticks and other objects as agonistic tools against small animals very rarely, even if they are readily available; and chimpanzees in the wild have never been observed to do so, even if plenty of sticks were at hand. (In all my experiments with living and dead reptiles, birds, small and medium-sized mammals this has never occurred.)

2) Both in captivity and in the wild, chimpanzees do use sticks and other objects occasionally as intimidation tools against conspecifics and, in areas in the wild where they are not too shy of man, against humans.

3) An extremely fierce use of intimidation tools and a much higher performance level were elicited when chimpanzees, in experiments both in captivity and in the wild, were confronted with a stuffed leopard and, in captivity, a living leopard.

4) In the wild, *forest-dwelling* chimpanzees used sticks, tree-trunks, etc. *only* as *intimidation* tools against animated and non-animated stuffed leopards; their motor performances were relatively poor; they rarely selected for big sticks; and the leopard was never hit with an object. (Six trials with large bands of chimpanzees were conducted.) Conversely, in a trial in captivity with a group of three adult *savanna-dwelling* chimpanzees which had been captured at an almost adult age (and so must have known leopards as predators in the wild) and which were kept under semi-wild condtions, the apes selected only *very big* sticks, used them mostly as *true fighting weapons* with almost human motor patterns, and hit the stuffed leopard four times with the sticks, ending up with a tremendous "consummatory blow." Furthermore, the only case in which a highly reputed fieldworker (Millot of Paris) is reported to have been hit by a throwing chimpanzee in the wild was reported from a savanna habitat.

5) Circumstantial evidence based upon vocalizations suggests that savanna-dwelling chimpanzees in the wild do attack live wild leopards, at least if they are in a band.

6) Half-grown chimpanzees born in a zoo, which had rarely or never shown any throwing or clubbing activity and which had never seen a large Feline, almost immediately started to use intimidation tools when a leopard or half-grown tiger was shown to them, but their aim and performance level was extremely poor. A really good aim and a high performance level in throwing and clubbing is achieved in zoos only if the apes have plenty of space. Thus the response as such is instinctive, but the aiming has to be learned.

7) Motivation analysis, both in zoos and in the wild, demonstrated that all these behaviors occur only when the apes are in a strongly ambivalent state of conflict between fear and agression. Analysis of the motor patterns indicated that throwing, clubbing, incipient weapon-use, etc. are extensions or derivatives of the general intimidation displays which are typical of primates.

All these points demonstrate that the zoo evidence and the wildlife evidence parallel one another. This justifies us, for the time being, in extrapolating from points (4) and (6) to predict that savanna-dwelling chimpanzees in their natural habitat, when they are in a large band, will use large sticks, tree-trunks, etc., as true and effective fighting weapons against leopards, whereas forest-dwelling chimpanzees will use any sticks at hand as intimidation tools only. Such a prediction allows us to set up a working hypothesis and to undertake to test the hypothesis experimentally under wildlife conditions. (Two of my collaborators are at present on their way to Africa to try and conduct the crucial experiment, and, if possible, also to obtain comparative data on this issue for different subspecies in the same type of habitat.)

By means of the above summary of the present state of our research on agonistic tool use in chimpanzees, I have attempted to demonstrate how immensely important zoo observations and experimentation can be in stimulating and evaluating fieldwork, *and conversely*. It is in this respect that Washburn and Jay seem to disagree with me most fundamentally. To them, zoo data appear to have no value at all and should, therefore, be ignored. They seem to be unaware that Kortlandt and Kooij's paper was just one step in a long-term research program that includes not only behavioral, but also ecological, fieldwork and palaeontological aspects (Kortlandt 1965, 1966*a*, 1966*b*, n.d.; de Bournonville n.d.). In such a comprehensive program no kind of data can be neglected.

For example, it was circumstantial evidence from zoos that caused me to suspect as early as 1957 that our contemporary savanna-dwelling chimpanzees might be, and their ancestors might have been, to some extent, carnivores—an assumption later confirmed by Goodall (1963) and perhaps corroborated by Pei (1957). Similarly, it was the evidence of "savanna-adapted" behavior in zoo chimpanzees and in the wild that induced de Bournonville and me to look for chimpanzees in areas hundreds of miles north of the rain forest belt—a search which (through the kind help of the Services des Eaux et Forêts of the countries involved) led to the discovery of scattered and relict populations at the northern edge of the *Isoberlinia*-Sudanian vegetation belt of savannas and dry forests, in the south of Senegal and Mali, the north of the Ivory Coast, and the northwest of the Central African Republic

I cannot understand why Washburn and Jay show such a lack of appreciation of zoo data and experimentation. Perhaps as anthropologists they are interested primarily in naturalistic descriptions and evolutionary aspects, whereas the scope of interest of students of animal behavior includes also motivational research, ontogeny of behavior patterns, the "innate vs. learned" problem, etc. The design of DeVore's book (1965) and particularly its last chapter (by Washburn and Hamburg) clearly indicate the restricted focus of interest of such anthropology-centered work. Perhaps another reason is that most American zoos are not as good as most European ones. In the United States, even a good zoo still tends to be considered as some sort of circus entertainment, rather than as a scientific institution. Whatever the case, it may be helpful to enumerate some of the advantages that a good zoo can offer:

1) In a zoo, one has an opportunity to get familiar with the ways and expressions of the animals at close distance and to learn to estimate ages before fieldwork starts. Both Goodall and I profited much from such introductory work.

2) In a zoo, one can study the emotional expressions of related animal species from a comparative point of view, analyze the underlying motivations, and measure their meaning and effect in social intercourse. The work by van Hooff (n.d.) provides a good example.

3) In zoos, all animals belonging to one and the same order are normally kept in the same type of cages or compounds. Consequently, the differences in behavior observed between different families, genera, and species must be attributed as a rule primarily to differences in phylogenetic adaptation to different habitats. This is why zoo people who know many species from everyday observation are often more aware of ecological habitat factors than fieldworkers who have studied only one, or very few, species in only one, or very few, localities. For example, the fact that, in captivity, most apes and monkeys belonging to predominantly terrestrial species (including chimpanzees!) are panicked as a rule by the sight of a snake, whereas the predominantly or exclusively arboreal species show curiosity, though some caution, towards it virtually proves that the risk of a snake bite is very much greater on the ground than in a tree, in spite of the fact that the tree snakes include the most poisonous species (Antonius 1938-39; Kortlandt, unpublished). Incidentally, the spider monkeys are an exception and therefore suggest that some South American arboreal snakes are more likely to bite than the Old World ones. This example shows how certain ecological data can be collected in a zoo within a quarter of an hour or so, and at no cost, whereas their collection in the wild would require many, many years and an enormous sum of money. Furthermore, in the wild it would be virtually impossible to determine to what extent the differences in behavior beween, for example, the chimpanzee and the orangutan may be attributed to genotypic or to phenotypic factors, because the vegetational and "physiognomic" characters of their habitats are quite different.

4) In good zoos and laboratories, the animals can be kept under controlled conditions, and records of their individual life histories are available. Consequently, one can study many aspects which can virtually never be studied in the wild. For example, the experiments mentioned above, in which half-grown apes which had never, or hardly ever, shown any agonistic tool-use, were confronted with a leopard for the first time, are absolutely inconceivable under wildlife conditions.

5) In poorly furnished zoo environments, one may observe ecologically irrelevant behaviors (e.g., vacuum activities) under conditions that throw new light on the function of such behaviors under natural conditions. Conversely, in well-equipped compounds, the behavior catalogue may be richer than in the wild. For example, in Hamadryas baboons, Kummer and Kurt (1965) observed in the wild only two behavior patterns which they had not seen in captivity, but saw in captivity nine behavior patterns that had not been observed in the wild. "The main tendency of the zoo colony was the enrichment of social behavior" (Kummer and Kurt 1965:14).

All these points obviously refute Washburn and Jay's position on the inadequate of zoo evidence. Furthermore, in Hamadryas baboons at least, "the composition of one-male-groups described by Zuckerman and observed in the Zürich Zoo was a replica of wild groups, down to the smallest detail," and "relative frequencies of behavior categories in the sex-age-classes were mostly the same in the zoo and in the wild" (Kummer and Kurt 1965:14). All this does not imply, of course, that fieldwork is unnecessary. It does imply, however, that fieldwork should be complemented by zoo and lab work, and conversely, to compensate for the inadequacies and invalidities of each.

With regard to some of Washburn and Jay's other points I may be quite brief:

I think it is premature to state that "the chimpanzee is the only primate aside from man in which any substantial item of the diet is obtained by the use of a tool." Zoo evidence suggests that wild capuchin monkeys may depend on tools to crack certain types of nuts. Incidentally, it is worth remembering that such a modest creature as the ant lion (*Myrmeleon* *formicaleo*) depends almost entirely on "tools" to get his food.

I cannot see what we will gain by conceptually and nomenclaturely dichotomizing the use of "intimidation objects" and of "fighting tools." In actual behavior there is a continuous gradation between them, and the motivation is about the same, both in the chimpanzee and in humans. Washburn and Jay seem to imply that I have confused the two; I can only refer to the original papers.

Neither their data nor their arguments are relevant to the dehumanization hypothesis of the evolution of the African apes, for Washburn and Jay by-pass the facts and grounds upon which this hypothesis is based. Apart from this, a hypothesis is an attempt to order the *available* evidence with a view to finding appropriate ways to obtain decisive evidence.

Contrary to what Washburn and Jay state, Kortlandt and Kooij *did* consider that the movement patterns in throwing depend on underlying anatomical structures. We stated (1963:76) that the difference in style of movement between chimpanzee and baboon, "can at most only partially be explained by anatomical differences."

I agree with Washburn and Jay that we are still "a long way from understanding even such an apparently simple category as tool use." That is why I think that research on this subject should continue and be extended, along *all* routes available, without excluding any particular kind of approach.

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On Soviet Views of Totemism

by J. L. FISCHER☆

New Orleans, La., U.S.A. 30 IX 66

S. A. Tokarev's review of Soviet treatment of totemism (CA 7:185-88) was of great interest. I find myself in agreement with a number of his conclusions, especially (1) that it is useful to distinguish totemism from various kinds of magical rites directed primarily toward useful or dangerous properties of natural species, since many totem species are neither useful nor dangerous; (2) that the totemic classification of natural species and objects is, in Engels' words, "a fantastic reflection in the human mind" of kin relations; and (3) that there are formal and functional similarities and even remote historical connections between totemism and contemporary religions. I am also inclined to agree with the view that there may have been a universal totemic stage of human society, although this is harder to demonstrate since the direct evidence has been irretrievably lost and since there is considerable variation in the social organization and totemistic practices of historically observed hunting and gathering peoples. In any case, clan totemism was at one time much more widespread than it is today.

Tokarev does not deal at length in his review with certain questions which seem important to me. One of these is, why should totemism be a *fantastic* reflection of social relations rather than a *realistic* reflection? Other related questions are, granted that totemism is a fantastic reflection of kin relations, in what ways does it distort an objective image of these, and why, and how does this distorted image become accepted and established in the life history of individual members of the society? A simple appeal to the power of tradition does not seem sufficient, since without some active force supporting the distortion the tradition should vanish in a few generations.

The American anthropologist Edward Sapir once remarked, "It is strange how little ethnology has concerned itself with the intimate genetic problem of the acquirement of culture by the child" (Mandelbaum 1951:595). One non-anthropologist who did concern himself with this problem in a certain fashion was the psychiatrist Sigmund Freud, especially in his work *Totem and Taboo* (1952). Freud noticed the spontaneous occurrence in many modern children of behavior, fantasies, phobias, and dreams about animals which closely resembled the totemistic beliefs and practices which he had read about in the ethnographic literature. He further concluded that the symptoms of these children involving animals originated in the emotional conflicts of the children concerning their family members, principally their parents. The reason the conflicts were not expressed overtly was that it would be too frightening for the child to face the full extent of his resentment of his parents and his demands upon them in view of his necessary dependence on their good will and care. Children therefore constructed animals as secret or unconscious representatives of the parents and displaced much of the conflict onto these animal symbols. Totemism, Freud postulated, arose out of a similar social-emotional conflict which formerly extended into adult life in