

I Introduction

My first inkling of cultural primatology came in January, 1975, when Caroline Tutin and I went to the Mahale Mountains of western Tanzania. At that point, I had 15 months of field experience studying the wild chimpanzees of Gombe and thought I knew the species. On the first day out at Kasoje, this was proven wrong. We saw the chimpanzees of K-group doing the grooming hand-clasp, something neither we nor anyone else had ever seen at Gombe, in thousands of hours of observation (see Figure 1.1). We were dumbfounded by its elegant symmetry. However, upon returning to camp, when we mentioned the discovery to our host, Professor Junichiro Itani, he was unimpressed. Did not all chimpanzees do this?

At that point, I realised that there was no such creature as The Chimpanzee, if by that was meant behavioural uniformity across the species *Pan troglodytes*. Instead, there was behavioural diversity across chimpanzees, apparently at the level of populations. So, how to explain this variation? This book describes my attempts to answer that question, and many other related ones, over the last 25 years.

As an ethologist, I knew about within-species variation in animal behaviour. One of my fellow postgraduate students at Oxford, Michael Norton-Griffiths, had done elegant cross-fostering experiments on oystercatchers, showing that parental foraging and food-processing techniques were passed on from parent to offspring (Norton-Griffiths, 1967). But that was diet, and diet is influenced readily by the availability, abundance, or distribution of food items. In contrast, the grooming hand-clasp at Mahale was a social behavioural pattern, apparently arbitrary in form and independent of any obvious environmental constraints; it is essentially a mutual gesture.

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FIGURE 1.1 Adult female (left) and adult male of K group, Mahale, engage in grooming hand-clasp. Note infant in her lap, asleep but still clasping hair of his mother's side.

As a primatologist, I had admired the pioneering studies of Japanese field workers, studying the Japanese monkeys, especially on Koshima (Kawamura, 1959). Individuals invented new techniques of food processing or of thermoregulation, and these were taken up by fellow troop members. The scientists termed this 'protoculture' (Itani & Nishimura, 1973). But each of the behavioural patterns, whether sweet-potato washing or hot-spring soaking, was instigated by humans; it was not spontaneously shown by the monkeys (Matsuzawa, 2003). In contrast, the grooming hand-clasp of the Mahale chimpanzees had nothing to do with humans, being instead an expression of ape sociability.

Finally, as a comparative psychologist, I knew of the hefty published literature on song-learning in passerine birds. Complementary studies in field and laboratory of its nature and nurture had established that traditions of vocal learning exist, passed on from generation to generation (Slater, 1986). In some cases this led to regional dialects, and sometimes these vocal variations were shaped by interaction with others, even with non-singers (West *et al.*, 2003). But these same songbirds showed no other impressive feats of social learning; they were essentially one-trick ponies with a very good trick. The hand-clasping chimpanzees, on the other hand, showed many other examples of behavioural diversity, from tool use to courtship.

These, and all the other explanations for phenotypic variation that were available then from the natural sciences, did not seem enough to explain what the chimpanzees were doing. We needed to look further afield, and the obvious alternative was the social sciences. Enthusiastic but naïve, we wrote up our Mahale findings and submitted them to a prestigious journal, *Man*, the Journal of the Royal Anthropological Institute of Great Britain and Ireland. Thanks to the intellectual generosity of its editor, Peter Loizos, the article was published (McGrew & Tutin, 1978). Apart from one damning response (Washburn & Benedict, 1979 (see below)), our discovery sank without trace. The late 1970s was not a time to champion cultural primatology, at least in the West.

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Table 1.1 *Levels of analysis for cross-cultural comparisons of chimpanzees. Note hierarchical embeddedness descending from left to right.*

Level	Example
Species	<i>Pan troglodytes</i> versus <i>P. paniscus</i>
Subspecies	<i>P. t. troglodytes</i> versus <i>P. t. schweinfurthii</i> versus <i>P. t. verus</i>
Population	Mahale (Tanzania) versus Kibale (Uganda)
Community	Kanyawara versus Ngogo communities at (Kibale)
Clan	'F' versus 'G' lineages at (Gombe)
Individual	Frodo versus Freud, in 'F' lineage

My overall point is a simple one: faced with a dataset of ape behavioural diversity that demanded explanation, we were forced to invoke *both* the natural and social sciences for help. Thus, we had to try to straddle an intellectual divide in which one side believed that evolutionary theory explained everything, and the other that it explained nothing. Even now, 25 years later, biocultural anthropologists still catch flak from both sides of the divide (Cronk, 1999).

LEVELS OF STUDY

Consider behavioural diversity (see Table 1.1). To explain it requires clarity about which levels of analysis are being compared, and lack of this can lead to confusion, or worse. Comparison of apples with oranges yields a confusing fruit salad, so it is worth setting out this embedded structure now, as it will come up again. The top and bottom levels of the six-level schema in Table 1.1 are the easiest to tackle.

Comparing closely related *species* such as the chimpanzee and the bonobo is necessary to reconstruct their phylogeny or to infer the behavioural repertoire of a common ancestor. But differences between species are not likely to be cultural, since by definition they have

different genotypes that could account for the variation, and occupy different ecological niches.

At the other extreme, comparing *individuals* within a species can be revealing, especially with regard to issues of ontogeny or personality. Much of psychology depends on this level of comparison. For chimpanzees at Gombe, the fact that, of the two brothers, Frodo acts like a bully and Freud appears relaxed in outlook, is fascinating, but one would be most likely to ascribe these individual differences to upbringing or character, not to culture.

The four intermediate levels each present a different set of issues with regard to explaining behavioural diversity in some kind of cultural terms as follows.

- (1) At the *subspecies* level, chimpanzees are divided into three recognised geographical races across Africa: central, eastern and western. There is a purported fourth subspecies, found between the central and the west, in Cameroon and Nigeria, but it was described only in genotypic terms (Gonder *et al.*, 1997) and is only now being studied in nature (Sommer *et al.*, 2003). Comparison on the broadest, species-wide scale can be done at the subspecific level, from the mountains of Tanzania, to the lowland forests of the Congo basin, to the savannas of Senegal. But the three-way split is not really equal: the western chimpanzees differ genetically from the others far more than eastern and central differ between themselves (Morin *et al.*, 1994). These subspecies are real, in that gene flow between them is now prevented by zoogeographical barriers, especially rivers. Only the western chimpanzees use percussive technology to crack open nuts, and the boundary between crackers and non-crackers seems to be the Sassandra-N'Zo river in Ivory Coast (Marchesi *et al.*, 1995).
- (2) Within a subspecies are its constituent *populations*, which amount to demes. These were probably larger in area and numbers until recently, when human activities, especially deforestation, have fragmented them. Not that long ago, all of western Uganda was probably a single megapopulation of chimpanzees; now it is a string

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of beads of various-sized blocks of forest. Most of the analyses invoked in this book are at the level of population, making use of the well-known study sites of Gombe, Mahale, Tai, Bossou, etc. (A guide to study sites of chimpanzees is given in Chapter 6.) If there were to be a Chimpanzee Relations Area File to match the Human Relations Area File (that venerable repository of ethnographic data) then it would be at this level.

- (3) Within a population are the actual *social units* of which individuals are members. For chimpanzees, these are called variously 'communities' or (unit)groups. These are not troops of constant association – as in many monkeys – nor are they families – as in lesser apes – but units that rarely (if ever) are all together in one place at one time. Instead, they fuse or fission into parties of varying composition: all-male parties for patrolling, nursery aggregations of mothers and dependent offspring, consorting mating pairs, etc. Yet the members are bound up in a social unit, whether it is males co-operating in territorial defence or females emigrating to breed. In one case, the population is only one community (Bossou), in another only two communities probably survive (Gombe), and in some populations, there are enough communities that we cannot be sure of their number (Mahale). Unfortunately, for the sake of comparison, little is published on neighbouring groups, or what is known is uneven or not coincident in time. Whenever possible in this book, comparisons will be drawn at this cultural level, e.g. K-group versus M-group at Mahale, and Kanyawara community versus Ngogo community at Kibale.
- (4) Finally, there are kin-based subdivisions within the unit group. These *clans* seem to be matrilineal, but our knowledge of patrilineality was constrained until recently by lack of knowledge of paternity (Morin *et al.*, 1994; Vigilant, 2002). Almost nothing is known of chimpanzee cultural life at this level, though speculation has been provocative, e.g. Goodall (1986b) told of the F-lineage's style of mothering, passed down through three generations from Flo to Fifi to Fanni at Gombe. Clan influences clearly are there to be

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investigated: the alpha male position at Gombe has been held for most of the last 20 years by members of either the F or G lineages. By the time this appears in print, the balance may well have tipped again, with Gimble ousting Frodo.

If apes were humans, any of the four levels of subspecies, population, community, or lineage would be fair game for study by social scientists, especially sociocultural anthropologists.

HUMAN UNIQUENESS

But apes are not humans. That simple fact could stop cultural primatology in its tracks. If only humans are cultural, then social scientists need look no further than our species for subject matter. To address this point requires a consensual definition of the phenomenon, which is a subject for the next chapter. Assume for the moment that culture somehow can be defined in a way that encompasses what humans do and are, but also can be applied operationally to other species. Even if this possibility is granted in principle, there are several strong objections to considering it in practice.

One objection is the obvious point that *Homo sapiens* is unique. This is true, but by definition so is every other species (Foley, 1987). Species uniqueness does not prevent us from using the comparative method to undertake (for example) immunology, or to elucidate a phenomenon like lactation, in our closest living relations. A blood transfusion from a human could save a chimpanzee's life, or vice versa, but only with the right combination of A–B–O blood types. When we test the efficacy of vaccines or the effects of drugs on cognitive performance in chimpanzees, we do so for the very reason that we and they are fellow hominoids sharing many key traits.

But surely (some say) humans are behaviourally, cognitively, and emotionally unique, and that is what determines whether or not a creature is cultural. We are the only species known to pilot aircraft, invent calculus, and celebrate St Valentine's Day. Fair enough, but most humans, either as individuals or as societies, do none of these.

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That makes them no less human, any more than prelinguistic infants, nonlinguistic autists, or postlinguistic elders are denied their humanity because they fail the language competence test.

The true measure of human species uniqueness is to know which (if any) of its *universal* traits are *qualitatively* different (in kind) from the universal traits of chimpanzees. Quantitative differences (in degree) are not enough. We do not deny human status to societies that have the abacus but not the computer, or that treat illness and injuries but lack the germ theory of disease. Examination of a few candidate traits will show the difficulties in pinpointing human cultural uniqueness.

Bipedal locomotion is a form of behaviour that is universal to humans; we all walk and run (Hunt, 1994). At first glance, upright bipedality also seems unique among primates, too, as great apes are habitual quadrupeds. But a second glance shows that apes can assume bipedal stance or gaits if the context is apt (Hunt, 1992), and in exceptional cases, a wild ape can become even habitually bipedal (Bauer, 1977). A black-and-white difference starts to look grey.

Or take mathematical ability as one aspect of cognition of which humans justifiably are proud. Even the simplest foraging societies have some system of numbers, even in nonliteracy. No one has yet seen a wild chimpanzee count on her fingers or sort sets of items by number. If this happened, we would be entitled to infer numeracy. Meanwhile, studies of captive chimpanzees put into situations where numerical ability yields a payoff show that they can do arithmetic (Boysen & Hallberg, 2000; Biro & Matsuzawa, 2001). More recently, American coots in nature have shown the ability to count their eggs in the nest, to guard against brood parasites who slip in their alien eggs (Lyon, 2003). Thus, an apparent qualitative difference now looks more quantitative.

As for emotions, we can but infer them on the basis of behaviour, whether by vocalisation, facial expression, gesture or posture, in terms of what resonates with us by similarity. Tickle a chimpanzee, and she laughs; startle a chimpanzee, and he grimaces; threaten a chimpanzee, and she lashes out; groom a chimpanzee, and he sprawls relaxed. All

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of these signals of feelings are recognised readily by the average person. More dramatically, when we see an orphaned ape with her dead mother, her demeanor or 'body language' is one that, if seen in a human child, would be interpreted as grief.

So, are there any qualitatively unique traits shown by humans that could be used to deny culture to other species? The determined sceptic will fix on language. No one has ever heard a wild ape speak. In practice, it can be said that what we know about one another as humans comes from verbal report. You talk and I listen. Most of socio-cultural anthropology, and therefore most of what we know about human culture, is based on the speech of native informants. Once translated, transcribed, deconstructed, etc., the recorded texts reveal the meanings that underlie and permeate the human condition.

What nonsense! Speech is behaviour too, just like another observable action. Words are voluntary puffs of air, and so they need not reflect reality in the slightest. Large-brained, intelligent creatures practice deception, and one of the easiest ways of doing so is by telling lies. (Ask yourself honestly, when the truth matters, do you pay more attention to the words or to the accompanying non-verbal signals?) Why should an anthropologist believe an informant's words to be true, any more so than any other human being seeking to learn something from a companion in everyday life? Yet the corpus of ethnography, at least with regard to ascription of meaning, is based on this tenuous premise. The situation actually is even more vexed: given a creature intelligent enough to be prone to self-deception, then even the most honestly intended verbal report may be false. We do not know if apes are self-deceivers, but humans are.

So, is language a methodological curse or blessing for cultural anthropology? Clearly, if words are predictive of action, then like any other signal, they can be valid and reliable data. ('When we paint our faces blue, we are ready to go into battle.') On the other hand, words that refer to mental or emotional states never can be verified. ('Because I dreamt last night of battle, today I will paint my face blue.') An extreme view is that language is useful in studying culture only when it is corroborated by other acts. ('Back off, or I'll smash your blue

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face in!') A less extreme view is that language alerts us to attend more to some things than to others. ('We only paint our faces blue when the moon is full.') But many verbal statements will always be obscure ('We paint our faces blue when the gods of war are angry.')

One possible insight into the problem would be to videotape people without their knowledge, thus guaranteeing a record of their spontaneous behaviour. Then, two sets of naïve viewers would scrutinise these 'candid camera' tapes, seeking to understand what was going on. One set would have the sound turned on, the other off. To what extent would the latter data be impoverished? By 10, 50, 90 per cent? So far as I know, the experiment has never been done, but the videotaped material is available (Eibl-Eibesfeldt, 1989).

Are observers of non-linguistic chimpanzee behaviour in the same position as video viewers with the sound turned off? Yes, in many ways, which means that their data are sparse but probably valid. No cultural primatologist will secure an interview with a wild ape, but by the same token, no observer of apes will get lied to. (Actually, it is not quite so pat. As apes have been shown to practice nonverbal deception among themselves (Byrne & Whiten, 1988), they also may do so to human researchers.)

The upshot of all of this is that cultural primatologists studying nonlinguistic apes and cultural anthropologists studying linguistic humans both have costs and benefits with which to cope. In the end, both draw inferences, and the usefulness of the resulting knowledge is a function of how good they are at inferring. In either case, they can do ethnography, even if their methods differ. Sometimes, the results will readily be comparable, and so the accuracy of inferences will be high, e.g. if both humans and apes use stones as hammers to crack nuts, then we compare not only their artefacts, but also their ways of acquiring and using them to solve a simple problem: how to extract a kernel from a shell?

Other times, the inference will be tricky. Suppose humans and apes live in the same forest, and both decline to eat blue duiker? We ask the humans 'Why?', and they tell us that consumption of the