Chapter 5
Origins Theories in the 1980s

> We must begin by stating the first presupposition of all human existence, and therefore of all history, namely, that men must be in a position to live in order to be able to 'make history'. But life involves before everything else eating and drinking, a habitation, clothing, and many other things. The first historical act, is, therefore, the production of material life itself. This is indeed a historical act, a fundamental condition of all history, which today, as thousands of years ago, must be accomplished every day and every hour merely in order to sustain human life.
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> Karl Marx and Friedrich Engels, *The German Ideology* (1846)

Until the 1960s, no section of the scientific community was devoting itself in any consistent way to unravelling the mysteries of human social origins. Like Freud before him, Lévi-Strauss with his peculiarly sex-oriented theory was an isolated figure; he was interested in neither archaeology nor evolutionary biology, and despite his immense influence worked very much on his own. His social anthropological colleagues had eschewed 'origins' for fifty years, for reasons which were discussed in Chapter 1. Meanwhile, although scientific books and papers on human origins were still being published, they did not even claim to deal with the social aspects but examined bones and stones with the aim of arranging them within chronologies and typologies.

All this began to change in the 1960s and early 1970s. The popular books of writers such as Konrad Lorenz, Robert Ardrey and Desmond Morris were arousing public controversy with their emphasis on the positive aspects of aggression, territoriality, uninhibited sexuality and other 'natural' tendencies in us all (Chapter 1). More important, however, were other developments. Extremely exciting new discoveries of fossil hominids were being made at Olduvai Gorge and at other sites in East Africa. Field studies of primates in the wild had begun revolutionising our understanding of the biological background to human physiological and social evolution. Fuelled by revolutionary new dating techniques, archaeology was in a ferment of revolt against what were claimed to be tenacious traditions of gentlemanly insularity and amateurishness governing site excavations and their interpretation. Finally, the rise of anti-racism, feminism and vigorous 'new left' Marxist political currents meant that the focus of social anthropological interest was dramatically changing. A new generation of anthropologists was anxious to make a clean break with the discipline's colonial traditions and isolation and make links with anti-imperialist struggles, with the rest of the scientific community and with the concerns of the wider world.

Social accountability and cross-disciplinary fertilisation were the catchwords of the time, and the study of human origins was the topic which, more than any other, seemed to provide the focus for such a coming together. Soon, young anthropologists, archaeologists, primatologists and others were committing themselves on a new and higher level to a project which had been left unattended for over half a century: an onslaught on the still unresolved challenge of explaining human physical and social origins. Seminars, conferences and papers began linking social anthropology with archaeology, primatology and other disciplines in ways which would have seemed unthinkable or even outrageous a few years previously.

1966 saw the most ambitious and significant event in this connection – the 'Man the Hunter' conference (Lee and DeVore 1968). This was devoted to the study of the hunting-and-gathering way of life which – the participants broadly agreed – had dominated human life for about 99 per cent of our period of existence on this planet. One of the main organisers – Richard Lee – was a Marxist who, with Eleanor Leacock and a small number of others, had kept alive the spirit of Lewis Henry Morgan within social anthropology in the United States. Lee had worked among the !Kung; in his view these gentle people practised a version of that 'primitive communism' which for millennia had been inseparable from humanity's once universal hunter-gatherer lifestyle (Lee 1988).

Another main organiser and coeditor (with Lee) of the symposium, Irven DeVore, was also a social anthropologist, but an unusual one. Sherwood Washburn, doyen of modern primatological field studies, had encouraged him to study the social behaviour of baboons explicitly as a model for the behaviour of protohominids. By the time of the 'Man the Hunter' conference, DeVore was already one of the main authorities on primate social life, and was in a unique position to compare and contrast this with information on human hunter-gatherers.

The 'Man the Hunter' conference was a turning point. Lévi-Strauss was present, and archaeologists mingled with social anthropologists, palaeontologists, primatologists and many others in an attempt to place the hunter-gatherer way of life in perspective as an evolutionary adaptation of immense antiquity and significance in determining the whole nature of human existence. The conference marked a radical shift in American social anthropological theorising – a virtual abandonment of Boasian cultural particularism (see Chapter 1) in favour of various forms of ecological functionalism. It also became impossible any longer for anyone to treat human evolution as the study of just 'bones and stones'. The conference made it not only legitimate but mandatory to examine early hominid fossils and artefacts in the light of ethnographic knowledge and to try to construct appropriate models of early human behaviour and social organisation.

The new school of archaeology led by Lewis Binford helped to validate this approach: Binford insisted that forms of social patterning were encoded in the material remains which archaeologists could excavate and analyse. In his view, there was no hope of deciphering these remains, however, without conducting ethnographic fieldwork among contemporary hunter-gatherers specifically to answer questions posed by the digging, without much other comparative information beyond the traditional confines of archaeology, and without sufficiently careful, controlled and thorough techniques of excavation and analysis (Binford 1983).

It was in this period, and in part through the 'Man the Hunter' conference itself, that palaeoanthropology in its modern sense was born.

However, the outcome of the conference was not all positive. Firstly, very little consciousness of gender was displayed. The title 'Man the Hunter' to many participants meant just that – the topic was not humans of both sexes, but quite literally 'man'. It would be a few years before the impact of feminism made itself felt (Fedigan 1986).

Secondly, the new realisation of the significance of hunter-gatherer lifestyles led to a rather oversimplified view of human evolution and origins. It was assumed that the evolution of humans *was* the evolution of the hunter-gatherer adaptation. Perhaps the most often-cited passage from the conference was this:

> in contrast to carnivores, human hunting . . . is based on a division of labour and is a social and technical adaptation quite different from that of other mammals. Human hunting is made possible by tools, but it is far more than a technique or even a variety of techniques. It is a way of life, and the success of this adaptation (in its total social, technical, and psychological dimensions) has dominated the course of human evolution for hundreds of thousands of years. In a very real sense our intellect, interests, emotions, and basic social life – all are evolutionary products of the success of the hunting adaptation. (Washburn and Lancaster 1968: 293)

In the following years, consequently, wherever ancient 'human' fossils were found, it was inferred that the hominids concerned must have been 'hunters' or 'hunter-gatherers' in roughly the sense in which the !Kung, the Hadza or the Australian Aborigines are. Since the latest, very exciting hominid fossil finds at Olduvai Gorge and elsewhere were being dated by the revolutionary new potassium-argon technique to two or more million years ago, the results were peculiar.

'One of the practical consequences for palaeoanthropology', as Robert Foley (1988: 207) comments, 'has been a model of human evolution that is essentially gradualistic and unilinear'. The important elements of a hunter-gatherer way of life – food sharing, hunting, a division of labour, a home base and so on – were thought to be identifiable very early in the fossil and archaeological record. In this period, it was almost as if specialists were in a race to see who could find evidence for human life, in a social as well as a physical sense, earlier than anyone else.

One result was that the differences between various hominid taxa took on the appearance of minor anatomical variations, rather than functionally and adaptively significant features. Hunting and gathering was hunting and gathering; this basic mode of production had remained in all essentials the same for countless millennia. Since all the various hominids were supposedly doing the same thing, it was not at all clear why *Homo habilis* differed from *Homo erectus*, nor why the Neanderthals and modern humans differed from one another at all. This problem will be seen to afflict most of the theories of social origins which have been put forward over the past ten to twenty years.

What is the current state of thinking on human social origins? To help put into perspective the synthesis which this book will propose, this chapter provides a representative set of position statements. Many date from around 1981, a year of intense debate on the topic of origins in academic journals. Although all of these theories are now past history, many of their insights remain valid, and any new theory will have to build on the advances they have made.

Most of the theories featured here focus on one problem – that of *getting males to provide food for females and offspring*.

To appreciate the significance of this, it is necessary to recall our earlier discussion of totemism and the hunter's own-kill taboo – and then note that in no primate species can a pregnant or nursing female in any way depend for her food supplies on a male. Although highly co-operative hunting (Boesch and Boesch 1989; Boesch 1990) and meat-eating may occur, nothing resembling 'totemic' food avoidances can be found. Primate meat-eaters, in fact, differ from culturally organised human hunters in the following respects:

1. When a baboon or chimpanzee kills an animal for food, the killer typically eats – or attempts to eat – the meat.

2. Sometimes one animal does the killing whilst another does the eating. But such an event is *never* a deliberate act of exchange. Chimpanzee

males who have collaborated in a hunt, and are very probably siblings or close kin, certainly *do* often share their spoils. But for a whole carcass to pass from one hunter to another would be unusual *unless* the first animal had seized more than it could possibly monopolise and was mobbed and robbed despite its efforts. Only the crudest behaviourist could claim that since the meat in fact changes hands, all this 'amounts to the same thing' as hunter/gatherer-type sharing or exchange. The most that we can speak of, probably, is 'tolerated scrounging' (Isaac 1978) or 'tolerated theft' (Blurton Jones 1984).

3. When baboons or chimpanzees make a kill, there is no delay in starting to eat. Consumption begins on the spot – indeed, it may even precede the kill. Strum (1981: 263) observes that when baboons (at Gilgil, Kenya) start eating, the victim is typically still alive. There are no signs of even the most rudimentary or prefigurative inhibitions or taboos delaying consumption until a predetermined destination or 'home base' has been reached.

4. If, following a kill, portions of the victim are carried away, the reason is (typically) the reverse of that motivating such transport among human hunter-gatherers. Far from carrying away the meat for others to consume, the animal will typically be scampering off with a portion up into a tree (Suzuki 1975: 262–6) or into the distance to escape from others' demands.

5. Although it has been claimed that chimpanzees have a 'rudimentary' sexual division of labour – with females specialising in termite-fishing, for example, while males hunt (McGrew 1979, 1981: 58) – the fact is that the two sexes do not exchange with one another their respective products. Since the members of each gender group are on a nutritional level entirely self-sufficient, it is unclear in what sense even a 'rudimentary' sex division of labour may be said to exist.

In this light, it seems extraordinary that proponents of gradual evolution in palaeoanthropology should have succeeded in drawing simplistic parallels between *primate* meat-sharing and the patterns of meat distribution characteristic of modern *human* hunters and gatherers.

Following the discovery of meat-eating by primates in the wild (Goodall 1986; Strum 1987), it was argued almost throughout the 1970s and 1980s that primate hunting – which can produce very impressive levels of synchrony and co-operation in the actual *hunt itself* (Boesch and Boesch 1990; Boesch 1990) – 'naturally' leads to orderly and co-operative food sharing, and that primate field studies illustrate how easy it was for human hunter-gatherer norms of distribution to evolve.

The feminist writer Frances Dahlberg (1981: 7–8), for example, asserted that hunter-gatherer type food-sharing is nothing extraordinary: it evolved 'among chimpanzees, contemporary human foragers, and certainly ancient hominids as well'. In her view, 'the sociobiological concept of kin selection' explains this. Individuals who share food within groups which include close kin are 'rewarded by gene representation in following generations'. Dahlberg claimed that the results were particularly evident among chimpanzees:

> Adult food sharing among chimpanzees does not involve aggression; adults beg for meat, they don't grab it. The successful chimpanzee hunter shares with other males and occasionally with an adult female, especially one who is in estrus.

In similar vein, the primatologist Hladik (1975: 26) claimed that chimpanzee 'hunting and meat-eating behaviour could be compared with what is known about primitive human tribes of hunter-gatherers', whilst a colleague insisted that chimpanzee hunting 'blurs the line dividing human and non-human behaviour' (Harding 1975: 256).

Statements of this kind went almost unquestioned until late in the 1980s. They were, in effect, assertions that our ancestors had to cross no rubicon – accomplish no revolution – to establish the human hunter-gatherer configuration. It was claimed that we can see its rudiments already among meat-eating baboons and chimpanzees. To put such statements into perspective, and before turning to our survey of recent origins theories, we may usefully review some of the most celebrated case studies of primate meat-eating in the wild.

The Problem of the Hunting Ape

Jane Goodall (1986: 299) describes how Gombe chimpanzees rush up to a successful hunter and furiously struggle to seize a share of his kill. In the chaotic scramble which follows, the forest becomes filled with screams, barks, waa-barks and pant-hoots. Almost always, it is eventually a tough male who emerges victorious in any such fight; he runs off, attempting to monopolise 'his' meat.

'Meat is a highly coveted food', Goodall comments on her chimpanzee subjects, 'and often there is intense aggressive competition around a kill.' Those without meat attack those in possession, possessors counter-attack, and dominant but meatless animals attack empty-handed companions if these are of lower rank or are seen as competitors in the general conflict. 'Begging' does occur and is sometimes successful, but in any large group, there is no doubt that sheer physical struggle is a more important factor in determining how much – if anything – each animal receives.

One writer (Suzuki 1975: 262–6) describes some Budongo Forest chimpanzees who had just killed a subadult blue duiker. Ten minutes after the victim was first heard screaming,

> I found the four big males in a tree crying and struggling with one another for the spoils of the duiker. . . . Several furious struggles . . . took place between the four animals; these were followed by silent periods of eating the meat.

The victim was killed through being bitten, mauled and eventually torn limb from limb in the struggles of each chimpanzee to obtain a share; the primatologist could 'hear at thirty metres the sound of tearing meat and bone'.

As an evolutionary gradualist, anxious to find parallels with the sharing-behaviour of human hunter-gatherers, the reporter who observed all this (Suzuki 1975) confidently interpreted it 'as a case of cooperative working for the division of the spoils'.

The pattern is still more competitive among baboons. 'The young antelopes that baboons sometimes kill', writes Kummer,

> are almost exclusively eaten by the adult males, and fighting over such prey is frequent. The inability of baboons to share food is a behavioural characteristic that probably prevents them from shifting to hunting as a way of life. (1971: 59)

The whole situation places females at a severe disadvantage. 'In baboons and chimpanzees', notes Harding (1975: 253), 'the killing of small animals appears to be an activity carried on only by adults and almost exclusively by males.' Much the same applies to bonobos (Susman 1987: 82). Since the killers are also likely to be among the main eaters, and since the eating begins on the spot, the result is a foregone conclusion. Even should a female manage to make a kill, she will typically be robbed of it by some aggressive male very soon.

This last point needs to be emphasised. Whatever the force of 'the sociobiological concept of kin-selection' (Dahlberg 1981: 7–8), it does not mean that male chimpanzees in a typical group spontaneously give each other meat or provide meat for females. *Far from bringing meat for females to eat, the usual pattern is for males to rob females of whatever meat they may have been lucky enough to obtain.*

Among Gombe chimpanzees, even the toughest female cannot count on holding a piece of meat for long. 'Gigi' was one whose ability to defend herself against males was quite remarkable:

> On a number of occasions she maintained possession of her meat despite determined assaults by adult males. Once, for example, she caught a large juvenile colobus [monkey] when it fell or jumped to the ground during a mixed-party hunt. Satan instantly leaped down, chased after Gigi, and attacked her vigorously as she crouched over the prey. She managed to escape and rushed up a tree with her prey. (Goodall 1986: 307)

Satan – a particularly strong male – followed her up the tree and attacked her. Both chimpanzees fell ten metres to the ground. Gigi ran off, chased by Satan, who was then momentarily distracted – some other chimpanzees had made a second kill nearby. However, he was still empty-handed eight minutes later; again, he chased Gigi up a tree; again, both fell heavily to the ground. A third chimpanzee, 'Goblin', then attacked Satan, allowing Gigi to escape back up into some branches. Another male, 'Sherry', charged up and grabbed the prey. Gigi did not let go; 'both pulled, screaming loudly'. Satan charged back, attacked Sherry and inadvertently allowed Gigi once more to escape up a tree with her piece of meat. Satan raced after her; Gigi crashed to the ground, and Sherry – waiting below – managed to grab the prey and tear off a large part. Finally, Satan robbed Sherry, allowing Gigi, 'for a while at any rate', to eat a little of her hard-won spoils.

Given this kind of 'sharing', in which physical struggle far outweighs the importance of communicative subtleties such as 'begging', it is no surprise to find that among common chimpanzees (Hladik 1975: 26; Goodall 1986: 301–12), bonobos (Susman 1987: 81–2) and baboons (Harding 1975: 249; Strum 1981: 276), meat-eating is largely monopolized by the more powerful males.

It is true that females can gain meat by sexual means. A common female chimpanzee tactic is to present her rump to a meat-possessing male; the more alluring this sexual offering, the better her chances of gaining something. As Jane Goodall (1986: 484) observes, the bodily contortions involved in this kind of 'sharing' can be remarkable:

> When a female in estrus is begging meat from a male at Gombe, it is not at all unusual to see the male, carcass clutched in one hand, pause in his feeding to mate her – after which she is usually allowed to share his prey. I have even seen females, during copulation, reach back and take food from the mouth of the male.

Note that the female 'may' be allowed a share in the meat – but only *after* she has paid in the currency of her own flesh. Note also that because offerings of meat and other privileges are bestowed on females who display oestrus swellings, 'the swollen state has been prolonged well beyond the biological need for female receptivity and attractiveness to the males around the time of ovulation' (Goodall 1986:484). As Goodall puts it, the female chimpanzee's swelling 'in a way, serves as a sexual bargaining point. . . '. There are some costs – for example, being fought over by rival males and perhaps severely wounded – but the benefits evidently outweigh these.

Much baboon evidence illustrates a similar logic, but here in particular we see how it is not only females, but males who are faced with certain difficulties as well:

> Sumner in consort with Peggy was a classic case. Peggy stared fixedly at a nearby carcass as Sumner copulated with her. When he was done, she

> determinedly circled back to the carcass – which was surrounded by males – while again and again Sumner tried to chase her in the opposite direction. (Strum 1987: 131).

Sumner's problem in this case was that 'his' female would gladly trade sex with any male who could tempt her with meat. His only hope of stopping such liaisons was to physically drive her the other way.

As Shirley Strum (1981: 269) strikingly points out, the whole situation can subject a successful male to stressfully conflicting pressures and temptations:

> For example, when a male was in sexual consort with a receptive female and then conflict occurred between maintaining proximity to the female and eating meat, the male chose to continue consortship. At times the male appeared to be deliberating, looking back and forth between the meat and the female, but finally chose to follow the female.

Thirty-five times in one year, Strum observed dominant males apparently torn between meat and sex, reluctant to decide between the two but eventually abandoning the meat. Resolution of such conflicts ranged from males 'entirely ignoring meat-eating opportunities' to their simply allowing associated females to keep and eat the carcass. 'Even males with very high predatory scores chose estrous females over meat.'

In the context of hominid evolution, any such logic would have played havoc with males' freedom to hunt. It is simply not possible to guard or chase a female and chase a prey animal at the same time; a dominant male is in a strong position provided he does not have to be in two places at once. 'The trouble with that system', as Lovejoy (cited in Johanson and Edey 1981: 338) puts it,

> is that the alpha male's authority is enforced only by his presence. If he goes down to the river for a drink, he loses it. Some other watchful fellow is always hanging around. By the time the alpha male gets back, his chance for having any offspring may be gone.

If a similar system existed among early hominids', Strum (1981: 299) comments, 'a major change in reproductive strategies would have been necessary before males could give predation the priority it needed as a prelude to further division of labour between the sexes.'

All the evidence indicates that such a change would have been immensely difficult to achieve.

Origins theories of the 1980s

With this primatological background in mind, we are now in a position to look at our sample of origins models, almost all of which focus on the issues we have just discussed.

1. Glynn Isaac

Glynn Ll. Isaac (1971) 'The diet of early man: aspects of archaeological evidence from lower and middle Pleistocene sites in Africa'

Glynn Ll. Isaac and Diana Crader (1981) 'To what extent were early hominids carnivorous? An archaeological perspective'

Glynn Ll. Isaac (1983), 'Aspects of human evolution'

Isaac was a brilliant palaeoarchaeologist who worked under Louis Leakey in 1961, and became responsible, before his premature death in 1985, for interpreting several of the most crucial East African sites linking hominid activities with animal remains. His 1971 paper was one of the opening contributions in what has become an on-going multidisciplinary attempt to relate specific hominid-related archaeological sites with models of foraging strategy.

Of the East African sites which provide our entire fund of information on early hominid life, Olduvai Gorge contains by far the largest body of evidence. The various excavations at Olduvai are in geological deposits spanning an extremely long period – from about 1.8 million years ago to 600,000 years ago. The older deposits, known as 'Bed I', are among the best preserved, providing evidence of activities beside a lake whose margins were gradually receding.

In his 1971 paper, Isaac argued that the Bed I deposits indicate that humanity's distinctive hunter-gatherer lifestyle – featuring a sexual division of labour and reliance on a home base – stretches back some 2 million years to the early Pleistocene or late Pliocene (p. 281). He describes 'concentrated patches of bone' and comments:

> It seems certain that hominids were the prime agency creating these concentrations. The sites document a behaviour complex that is fundamentally human: tool manufacture, a partly carnivorous diet achieved by hunting and/or scavenging, and the practice of bringing meat back to a home base for sharing amongst the members of a social group.

In common with most other participants in the 'Man the Hunter' symposium, Isaac linked the origins of bipedalism with the first appearance of the 'home base'. The assumption was that walking upright evolved because it freed the hands for carrying things to and from the base.

Isaac in particular argued that (1) food-carrying using baskets or other containers and (2) the occupation of home bases as places where food was shared and consumed are the two distinctive practices by which we can distinguish humans from other primates. Chimpanzees simply eat as they go, consuming what they find on the spot. When they have eaten enough for their own private needs, they stop collecting. Hence they have no need for bags or other carrying aids. By contrast, human hunter-gatherers find food and instead of immediately eating it, carry it (or some of it) to another place for others to eat. This 'other place' is the home base, which may be quite some distance from the foraging-area. Repeated carrying of food to such specific locations results in the localised accumulation of food refuse, and it is this 'which has made archaeological study of prehistoric life possible' (Isaac 1971: 279).

One of the Bed I Oldowan sites, known as FLK 'Zinj', has for long been a focus of particular interest because several hominid bone fragments were found in its various levels, including the remains of a robust early hominid known initially as 'Zinjanthropus'. This 'Zinj' level, dated to about 1.8 million years ago, has preserved by far the densest concentration of archaeological materials from any Bed I level, including many pebble tools and tens of thousands of tiny splinters of bone (Potts 1988: 18, 29). An even earlier lakeside site, known as DK-3, has been dated to about 1.9 million years, and included a 4-metre in diameter roughly circular jumble of rocks frequently interpreted as the earliest known evidence of a man-made structure (but see Potts 1988: 257–8).

In his earlier papers, Isaac (1969, 1971) argued that the presumed hominids responsible for such debris patterns were hunters whose prey included large game animals. Later (Isaac and Crader 1981: 103n), he pleaded guilty to having overstressed this model, admitting that there was little real evidence for it. His more cautious formulation was that the hominids at this early stage 'were opportunistic scavenger/hunters and that, given the simplicity of the technology of the time, the flesh of medium and large prey was probably obtained more by scavenging than by hunting' (Isaac and Crader 1981: 86).

However, Isaac continued to insist that even at this very early stage, hominids were engaging in 'active food-sharing'. He envisaged small social groups occupying temporary base camps from which individuals or subgroups travelled over a home range each day foraging. Food surplus to the gatherers' needs was brought back and shared. This was quite different from so-called 'sharing' as practised by chimpanzees:

> We distinguish *active food sharing* from the kind of behavior reported for chimpanzees. . . . 'Sharing' is in part a misleading label for what has been filmed and reported among chimpanzees; that would be better designated as 'tolerated scrounging'. (Isaac and Crader 1981: 103n)

In Isaac's view, real food-*sharing* is a deliberate bringing of food for others to enjoy, and is a form of economic life characteristically and uniquely human. It is this which was occurring almost 2 million years ago by the lakeside at Olduvai.

Discussion

Isaac's interpretations quickly met with many detailed objections. One of the first into the fray was Lewis Binford, who poured scorn on the notion that Plio-Pleistocene hominids were departing daily from a home base, using food-carrying bags or baskets, the males conscientiously foraging or hunting in order to provide meat for the females and young waiting back at home. He pointed out that the actual evidence for any of this was non-existent. His view was that while a few of the bones showed signs of hominid activity, the hominids (rather diminutive ape-like creatures) must have been cautious and opportunistic foragers and scavengers, not hunters, and there is no proof at all that the concentrations of bone represent living floors or 'home bases'. Binford (1983: 59) in fact argued that the Bed I Oldowan hominids did not hunt and had only very small components of meat in their diet. 'The signs', as he put it, 'are clear. Earliest man, far from appearing as a mighty hunter of beasts, seems to have been the most marginal of scavengers.'

It soon became agreed that Isaac's interpretations had underestimated the dangers which would have been presented by ferocious competing carnivores around this East African lakeside. Noting that 'an odorous collection of food remains would rapidly attract other carnivores', two specialists with much experience of predator and other mammalian behaviour in Africa (Schaller and Lowther 1969: 335) had earlier pointed out that not until defensive arrangements had been made secure would it have made sense for hominids to bring carcasses to the places where infants and young were being cared for. Binford (1983), Potts (1984a: 136; 1988: 259–60) and Shipman (1983, 1986) endorsed this view, and it is now widely agreed that since Oldowans were small and vulnerable creatures, apparently lacking fire, they could not have afforded to sleep or rear young anywhere near large, fly infested carcasses smelling of blood – and least of all risk sleeping by a lakeside teeming with prey where lions and other nocturnal predators habitually made their kills.

Binford (1983: 68) wrote that the lakeside Oldowan so-called 'working areas' were probably places where non-hominid predators habitually attacked drinking prey; hominids may occasionally have gone to such places to scavenge animal remains which had been left behind, using stones to smash the bones.

Early in the 1980s, Richard Potts re-evaluated the Olduvai evidence and concluded that none of the so-called 'living floors' represented a home base. The famous stone circle at the DK site, he argued, was probably produced by the roots of a large tree, and was certainly not a shelter or 'home' (Potts 1988: 257–8). He argued that the 'working areas' were points where stone tools were cached – tools which hominid hunter/scavengers left at lakeside sites and at other points about the landscape. Whenever an animal of convenient size was killed or a dead one found, it would have been dragged to the nearest cache of ready-made tools so that it could be cut up. Something approaching caching of stone tools – for example, stone hammers used to open palm-oil nuts – has been observed among West African chimpanzees. Despite the absence of real logistic planning, they may at least remember where they dropped a particularly useful stone the last time they used it (Boesch and Boesch 1983). If caching were what the Oldowan hominids were doing, it was really only a modest advance on what chimpanzees can do, and certainly implies neither home bases nor a hunter-gatherer-like sexual division of labour (Potts 1984a, 1984b, 1986, 1987, 1988; Potts and Shipman 1981; Potts and Walker 1981).

On the question of meat-eating, Binford (1981, 1983, 1984) carried out a statistical analysis of the patterns of animal bone damage and loss at the FLK 'Zinj' and other Olduvai sites, concluding that the patterns were more often consistent with animal predators' gnawing and crunching than with human butchering. In fact, the animal bones have been minutely investigated for signs of possible hominid hunting or meat-eating, using a scanning electron microscope. One research team (Bunn 1981; 1983; Bunn and Kroll 1986) described a series of very fine linear grooves on bone surfaces, which most specialists now agree represent cut-marks made with knife-like stone flakes when hominids detached meat from the bones. Another find was of a bone on which a cut mark made with a stone edge can be seen to cross an underlying carnivore tooth mark. It has been inferred that the carnivore had the bone before the presumed hominid did, the implication being that the hominids scavenged meat remains that other predators had already discarded (Shipman 1983, 1984; Binford *et al.* 1988). More recently, *percussion* marks on bones have been added to the list of scratches, cut-marks and other possible diagnostics of early hominid behaviour at Olduvai Gorge (Blumenschine and Selvaggio 1988).

In his earlier writings Binford probably overstated his case, but although his was perhaps an 'extreme' position, denying that the Oldowan hominids were hunting even small game or doing anything impressive at all, virtually all archaeologists now agree with most of his critique. The consensus is that a sexual division of labour associated with even a temporary home-base arrangement did not emerge until considerably later than the Pliocene or early Pleistocene (see Brain 1981; Gowlett 1984; Shipman 1983, 1984, 1986; Potts 1984a, 1984b, 1988).

2. Tanner

Nancy Makepiece Tanner (1981), *On Becoming Human.*

Nancy Makepiece Tanner (1987), 'Gathering by females: the chimpanzee model revisited and the gathering hypothesis'.

Nancy Tanner, of the University of California at Santa Cruz, put forward a more feminist perspective on human origins. Her book (Tanner 1981) was in two parts. Firstly, she reviewed the chimpanzee studies to date, and outlined her reasons for taking chimpanzee patterns of social behaviour as a reasonably accurate model for early human life. The second part of her book attempted to relate this 'chimpanzee' model to the palaeontological and archaeological evidence for human evolution.

Tanner argued that as our early ancestors – australopithecines – moved out from the forest into more open savanna environments, it was the females who were most under pressure to innovate in the food quest, since these had responsibility to feed their offspring. Their response was to develop the digging stick, along with other tools used for plant foraging, while males continued to forage for themselves in more traditional ways.

Tanner envisaged a matrifocal group of a few females with offspring, including older juvenile males, as the central social unit. Adult males were at first peripherally involved, but became gradually 'incorporated' during the Pliocene as hominisation proceeded. Tanner did not see males as particularly necessary for protection against predators, since female chimpanzees seem to be as good at driving threats away as males are. Big game hunting did not enter into this picture at all, although the author accepted that small animals would have been part of the diet, and there may have been some scavenging.

This situation, according to Tanner, would have selected for intelligence and resourcefulness on the part of females, and also for stamina and carrying ability.

Changes in sexual behaviour would have occurred. Whereas chimpanzee females, like many other primates who have evolved in 'multi-male' settings, advertise the fertile period with large sexual swellings around their rumps, in hominids these would have disappeared. Tanner saw this as a simple and mechanical consequence of bipedalism: as the female stood upright, her vaginal area would no longer have been visible from behind, and so ape-like 'oestrous swellings' would no longer have served any signalling function (1981: 209). We will discuss oestrus loss in greater detail in Chapter 6.

Still writing of the time when bipedalism was evolving, Tanner stressed: 'It was the mothers who had reason to collect, carry, and share plant food; at this time males were likely still foragers, eating available food as they went' (p. 141). However, over time, males would have found that they were more likely to be granted sexual favours if they learned to travel with females and share food with them.

The females would have tended to select as their sexual partners not aggressive, dominant males but the more friendly, co-operative types, who would have been relatively lightly built and with progressively smaller canines. As Tanner puts it: 'Perhaps early hominid females preferred males who used their mouths to kiss, rather than the ones who bared sharp teeth' (p. 210).

'Females and males', in this picture, 'might become sexual friends who sometimes travelled together, and finding a temporary sex partner could easily occur in the larger groups that camped near water, along river beds and lakes' (p. 209). In these larger groups, just as in the smaller ones, there were few sexual conflicts, jealousies, fights or problems.

In his review of origins theories, Graham Richards (1987: 166) paraphrases Tanner's argument succinctly:

> for Tanner males are helpful occasional visitors to the matrifocal group; on entering this busy domestic world they had better make themselves useful and behave, for the females exercise a high ethical standard in evaluating them and choose to mate accordingly.

Only on the eve of the emergence of *Homo*, in Tanner's view, did hunting appear, playing a minor but increasing role in hominid food-acquisition. Tanner says very little about the later stages of evolution, her argument being that the basically human configuration was already in place from earliest times (Tanner 1981, 1987).

Discussion

Tanner's book was a healthy corrective to the male-centred bias of most previous palaeoanthropologists. With all the emphasis on 'Man the Tool-Maker', 'Man the Hunter' and so on, the female of the species had barely been noticed before. As Tanner (1981: xiii) put it:

> In exploring the roles of members of my own sex along with the roles of males in early human social life, this model seeks to correct what has been both a ludicrous and a tragic omission in evolutionary reconstructions.

But although she played down early hominid hunting whereas Isaac overestimated its role, her thesis suffered from many of the defects of Isaac's. Tanner's was a *gradualist* picture, based, apparently, on the assumption that evolving society presented its members with few difficulties. Hominids from very earliest times were essentially decent and even 'human' in their basic lifestyle. Males were mostly 'helpful'. Not a lot had to happen to make these distant ancestors (australopithecines) fully human in the sense in which modern hunter-gatherers are.

Too often, Tanner used a verbal formulation to hide a difficult problem. Take a passage such as the following:

> Overall, females apparently were choosing males who were sociable, cooperative, willing to share, and protective. In general, then, sexual intercourse would not be disruptive of either ongoing group interaction or organizational flexibility. (1981: 210)

But *why* was sexual intercourse suddenly 'not . . . disruptive'? Can it really have been because the males chosen by females were genetically more 'sociable'? And if that were the case, why were hominid females so discriminating whereas baboon and other primate females continued 'choosing' males of a different kind? Tanner offers no plausible explanation. Gathering as such cannot have been the cause. Not just humans but all primate females, after all, practice 'the gathering adaptation' in one form or another, even if most do not use digging-sticks.

Tanner's theory is essentially about the supposedly more co-operative genetic constitution of protohumans, particularly in relation to their sex lives. Yet it is hard to believe that through female selection – operative to this effect in the case of the early hominids but no other species – men became *genetically* 'sociable . . . protective' and so on. If what is at issue is male genetic 'nature', many feminists would surely ask whether there is any evidence that human males ever *did* become as nice as this! The fact is that sex can be immensely disruptive of social harmony, not only for all known primates, but for humans in most cultural contexts, too – including, of course, our own. Evolutionary selection pressures as such seem to have done little to render sex non-disruptive or males sexually tolerant in a genetic sense, and we would therefore seem to require a totally different kind of explanation in order to understand how the problem of sexual conflict was dealt with in the course of human evolution.

In short, while Tanner's book helped to change the whole tone of discussions on human origins, focusing attention on females as no previous contribution had ever done, its underlying theory was simply not adequate. Most of the book was about gentle and co-operative chimpanzees, and the basic argument was that only the most minor of changes from a chimpanzee lifestyle were required in order to set the hominids along the road towards cultural humanity.

This will not do, for two reasons. Firstly, chimpanzees are not intrinsically gentle and co-operative, as used to be imagined twenty years ago, but often murderously aggressive, infanticidal and cannibalistic (Bygott 1972; Teleki 1975: 169–72; Goodall 1986: 488–534). It all depends on circumstances, not genes, just as it does with us.

Secondly, a theory which says that the problems were in essence solved already, before culture, by primates such as chimpanzees, is really not a theory. It does not explain why culture as such – with its taboos, its rituals, its symbolic systems, complex kinship systems, grammatical systems and so on – ever became needed at all.

3. Lovejoy

Owen Lovejoy (1981), 'The origin of man'

Lovejoy, Professor of Anthropology at Kent State University, Ohio, centred not on ecological or technological changes, and not on the development of hunting, but on reproductive factors. His view was that human evolution required above all intensive parenting, and that the most essential prerequisite of this was male involvement in getting food for females and young.

Lovejoy was primarily concerned to find the ultimate factor at the very start of hominid evolution which began requiring that most distinctive of hominid anatomical traits – our adaptation to upright gait. In this context, he dismissed various previous theories. Hominids, he wrote, did not begin walking on two legs to hold or use tools, to hunt game or to escape from predators once the protection of the forest had been abandoned – all such factors postdate the earliest evidence for bipedalism in the fossil record. Walking upright, Lovejoy emphasised, arose extremely early in the course of hominid evolution – millions of years before the emergence of stone tools or hunting. The earliest evidence for it is a series of footprints in the mud found at Laetoli in Tanzania, dating to about 4 million years ago (Johanson and White 1980). What was it which, from such very early stages of hominid evolution, necessitated this peculiar and (for mammals) unprecedented primary mode of locomotion?

Lovejoy argued that far back in the Miocene, before our ancestors had even begun leaving their (presumed) original forest environment, the basic social, sexual and reproductive patterns which were to determine the course of all subsequent evolution had already been laid down. Man's 'unique sexual and reproductive behaviour' had already been established. Sexual competition between males was minimised or even eliminated at a very early stage through an arrangement which made it possible for every male to have exactly one sexual partner – no more, no less. According to this scenario, conflict was minimised and our species made human by monogamy and the nuclear family. Lovejoy argued that it was in the course of adapting so as to be able to bring provisions exclusively to his mate and offspring that the monogamous hominid male began walking on two legs.

In presenting his model, Lovejoy sought to explain (a) why males began systematically provisioning females and (b) why this necessitated monogamous pair-bonding and a strictly 'nuclear' form of family.

In approaching the first question, Lovejoy spotlit a problem which he thought would have been faced by the ape-like Miocene ancestors of the hominids. This was an extraordinarily slow rate of reproduction.

The evolution of the primate order as a whole – from lower to increasingly 'higher' forms, with larger and larger brains – is achieved only at some cost. This is borne mainly by the female of the species, who must go through an increasingly prolonged pregnancy and must nurse her offspring for longer and longer periods of time. As primates become more intelligent, so they require more nurturing and learning before they are capable of surviving on their own at all. To obtain this nurturing, they slow down their biological clocks, as if to give themselves more time. In other words, there is a progressive prolongation of gestation, infancy and all other life phases.

Lemurs are at the lower end of the scale, with a fast clock. Following conception, they are quickly born, quickly mature and usually die before they are 20 years of age. With macaques, the whole process is slowed down; birth, maturity and death are all delayed. Gibbons delay everything further, and chimpanzees delay each life stage further still, often remaining reproductively active until about 40 years of age. Humans have delayed the attainment of each stage furthest of all. Gestation lasts 38 weeks (compared with 34 for chimpanzees, 18 for lemurs), childhood dependency continues for a decade and more, female reproductive life lasts until around fifty, and female life expectancy extends (uniquely for any primate) many years even beyond child-bearing age.

The extreme and prolonged dependency of their offspring poses a particular problem for female chimpanzees. Even at the age of five or six years, a young chimpanzee may still be getting rides on his mother's back while she is foraging for food. Admittedly, the burden borne by female chimpanzees is less than that of human mothers, but it has to be remembered that the chimp mother has to do all her foraging for herself, with no economic support from others. No woman in a human hunter-gatherer context is forced to be so self-reliant.

The combination of intense mothering and foraging burdens makes it impossible for a chimpanzee to give birth to several infants in quick succession. Any chimp mother who did this would have to neglect many of her offspring. Field studies at Gombe in Tanzania in fact show the average period between successive births to be 5.6 years. A chimpanzee female does not reach sexual maturity until she is about 10 years old; if she is to reproduce herself and her mate – that is, if a stable population level is to be maintained – she must therefore survive to an age of 21 years. Within any given population, various factors – accidents, predation, infection and so on – tend to lower the average life expectancy of all individuals, and in the case of chimpanzees, there is very little tolerance in the system. If chimpanzees were to enter a new, more dangerous, environment, how could they avoid increased infant mortality or avoid average female life expectancy from being pushed below the critical figure of 21 years?

Lovejoy envisaged that the ape-like ancestors of the hominids faced some such problem. Conditions in East Africa in the period when bipedalism was evolving, he wrote, involved increased seasonality and the development of diversified mosaics – that is, a variegated landscape of patches of woodland, grassland, rivers, riverbanks and so on. Hominids in this context would need to be omnivorous and capable of exploiting a range of different types of environment; they would also need to boost their rate of reproduction to cope with occasional harsh conditions or severe seasons.

Crucial factors contributing to infant mortality in chimpanzees include inadequate mothering and (even in the case of 'good' mothers) injuries caused by falling off the mother's back. Many of these problems stem from the fact that the mother has to keep moving from place to place as she looks for food; Lovejoy (p. 344) saw this as a significant cause of infant mortality and 'the most important restriction on primate birth spacing'.

Hominids during the late Miocene (according to Lovejoy) may have had at least as slow a rate of gestation and maturation as chimpanzees. Unless they were to regress to smaller brains and faster clocks, there would have been only two theoretically possible ways of producing more surviving offspring. One would have been to reduce the interval between one birth and the next; the other would have been to reduce infant mortality in some way. Yet both would have posed immense problems – demanding more intense and vigilant mothering, distributed among yet more offspring, on the part of females who were already heavily burdened.

There was only one radical solution: a completely new distribution of parenting responsibilities between the sexes, involving an end to the primate male's ancient freedom from the responsibilities of parenthood. The hominid female had to be released from the need to be perpetually on the move in search of food. She had to be allowed to rest, to choose a safe sleeping and living area in which to care for her offspring, to stop having to carry infants around over long distances – and devote the energy thereby saved to intensified mothering. This meant that the male had to enter the picture and actually start providing food. His privileged status as a member of the leisured sex would have to be brought to an end.

Lovejoy argued that this was the breakthrough on the basis of which hominid evolution set off on its distinctive course. Quite unlike other primates, the earliest hominid females stayed at or near a 'home base' while males ranged further afield. As each female with her offspring remained near a fixed base, her male consort would go out periodically in order to bring back food. The reduction in female mobility was an immense gain, reducing the accident rate during travel, maximising female familiarity with the core area, reducing exposure to predators, and allowing intensification of parenting behaviour (p. 345).

But why monogamy? Lovejoy saw this as the only solution to the chaotic problems of sexual conflict which any other system would have involved. For example, how else could a male depart periodically from his sexual partner, free of the anxiety that some rival male might take advantage of his absence? According to Lovejoy (p. 345), only monogamy and a one-to-one sex ratio could provide a solution: each male would then be sexually satisfied, competition for mates would no longer disrupt everything, and the male who went away to forage would not risk losing his mate.

Other considerations (according to Lovejoy) point in the same direction. In a polygamous harem system, the female population is attached to only a small proportion of the males potentially available. Within each harem unit, in other words, the sex ratio is two or more females to every male. Any such system would have obvious drawbacks for females in need of male-derived food. On the one hand, much of the energy of the dominant males would be wasted on the constant fights needed to keep control over each harem. On the other hand, the remaining males – the losers in the competition for mates – would be excluded from the breeding system, unattached and therefore not used by the females as a source of food. These groups of 'bachelor' males would roam about, unmated and in a sense 'wasted' as potential food-getters ('an untapped-pool' of reproductive energy: p. 346). Emerging protohuman females, in Lovejoy's argument, needed the services not just of a fraction of the adult male population, but the totality. Each female needed a whole male all to herself. Monogamy satisfies this condition, and also guarantees paternity to males.

Because of this, Lovejoy saw the matrifocal basic unit of non-human primates as of little relevance to human origins. In his view, it gave way at an early stage to pair-bonding. As he wrote: 'there would be a gradual replacement of the matrifocal group by a "bifocal" one – the primitive nuclear family' (pp. 347–8). The nuclear family was described as a 'prodigious adaptation' central to the success of early hominids, and firmly established in the Miocene (that is, 5 million and more years ago).

The new system would have made it possible and adaptive for each male to provide food strictly for his monogamous partner and his own genetic offspring, and no others. Whereas chimpanzees, when they find food, utter a 'food-call', inviting others to come and share the find, human males would not have done this. 'In the proposed system', in Lovejoy's words,

> selection would not favor this behavior: instead, selection would favor a behavior that would benefit only the male's own reproductive unit. The simple alternative to the food call would involve collecting the available food item or items and returning them to the mate and offspring. (p. 345)

Human males would have been careful to keep the food to themselves and to their own nuclear family.

The better each male was at provisioning his mate and her offspring, the more likely were his genes to be immortalised. Since monogamy meant that each male was assured that all his food-getting efforts enhanced the survival prospects of his own offspring and no one else's, such behaviour was powerfully selected for. Lovejoy (p. 345) was at pains to point out that this would have nothing to do with 'reciprocal altruism', since 'it would only benefit the biological offspring of the male carrying out the provisioning and thus would be under powerful, direct selection'. Lovejoy explained bipedalism as arising from males' need to carry food to their mates and offspring.

Meanwhile, the females who were in sexual terms most willing and desirable would have been those best able to motivate their mates to provide for them. This led to attractive breasts, buttocks, skin and so on – females being quite markedly differentiated from males in such terms – and also to 'continual sexual receptivity'. Evolving hominid females, well cared for and not obliged to travel and forage so much, could now give birth to increasing numbers of increasingly dependent, slow-maturing offspring. The effect of intensified parenting, protracted learning within the nuclear family, and enhanced sibling relationships (resulting from more offspring of a similar age being brought up within each family) enhanced each child's chances of survival in the world.

Discussion

'The Origin of Man' was in its time an authoritative article which quickly became a favourite following its publication in the prestigious journal, *Science*, in 1981.

It focused immediately on what is still recognised as the basic problem: the primate male's traditional unwillingness or inability to provide food for his mate and offspring. Lovejoy's theory highlighted the immense burdens of motherhood imposed on the evolving primate female, and saw human evolution in terms of this figure's emancipation from some of the difficulties involved in combining foraging with child care. Institutionalising the 'home base' was seen as the key condition of this emancipation.

Moreover, Lovejoy showed awareness of the problems which would have been posed by inter-male sexual competition in any 'harem' type of mating system. He pointed out that if females were to take maximum advantage of the provisioning services of males, and if inter-male sex fights were to be minimised, then the male population as a whole must have been brought into the mating system and a one-to-one sex ratio established.

But while Lovejoy's selection of problem areas was perceptive and often convincing, his scenario has in the end fared no better than its rivals. The most devastating fault was one which also demolished the other theories, and has been touched on briefly already in our discussion of Isaac. Lovejoy's dates were seriously wrong. Studies of tooth-eruption schedules in immature fossil australopithecines have shown that their maturation rates were not significantly different from those of living chimpanzees (Bromage and Dean 1985). Whatever it was, therefore, which lead to the emergence of bipedalism, it certainly had nothing to do with the reproductive factors Lovejoy envisaged. Linked with this, there is no archaeological evidence for the emergence of a home base arrangement even in the Pliocene, let alone the Miocene. Even in the Middle Pleistocene, there is little firm evidence, and there is now a virtual consensus that a clearly demarcated home base and sexual division of labour did not appear until very late – possibly as late as the arrival of anatomically modern humans. This is much too late for it to have anything to do with bipedalism, large brains or any other specifically hominid as opposed to pongid anatomical traits.

Beyond this, however, Lovejoy's argument was quickly criticised on various grounds.

Females and bipedalism

Lovejoy's theory is a good example of what the feminist writer Fedigan (1986: 29) describes as the 'coat-tails' theory of human evolution: traits are selected for in males, and then females evolve by clinging to the males' coat-tails. Lovejoy attributes the evolution of upright gait entirely to the food-carrying activities of males. Females in this model are given little to do except feed, reward their partners sexually, give birth and nurture their young. The fact that they, too, walk upright is not accounted for, except to the extent that their shared genetic inheritance makes the females of any species tend to 'keep up' with males as a matter of course.

A more serious problem concerns the very antiquity of bipedalism. In the 1960s, writers on evolution almost invariably saw human origins as a single complex process involving such elements as hunting, tool use, food sharing, the emergence of a sexual division of labour – and bipedalism. All these developments were supposed to have been directly and simultaneously interrelated, in the sense that no single element could fully evolve without the others. In particular, bipedalism could not have evolved prior to the making and carrying of tools, because until tools began to be made, there was no need to free the hands 'in order' to hold and use them. All this seemed plausible enough in those years, when it was thought that fully evolved bipedalism was a late development, emerging at about the same time as stone tool-making. However, now that the Laetoli and other finds push back the origins of upright walking to 4 or perhaps even 5 million years ago, we have two choices. Either we conclude that upright walking evolved independently, long before systematic stone tool use, hunting or other characteristically 'human' activities. Or we are forced to say that the basically 'human' way of life began immensely far back in the past – long before anyone had previously thought.

Unfortunately, Lovejoy took the second course. He acknowledged that hunting and tool use could have had nothing to do with the origins of bipedalism. But he still tried to save the old paradigm by arguing that the 'essentially human' lifestyle – involving intense parenting at a home base, made possible by a sexual division of labour within a primordial 'nuclear family' – indeed stretches back 4 or 5 million years, far enough into the past to qualify as an explanation for bipedalism. More obviously even than Isaac, Tanner or Hill (see next section), Lovejoy fell into the trap of telescoping the various quite distinct, widely separated phases of human evolution into one decisive 'moment' which – because bipedalism had to be included – was necessarily thrust back into the Miocene.

In reality, as Richards (1987: 193–204) points out in his survey of origins theories, it is seeming increasingly likely that we need a non-social, non-'human' and extremely simple *physical* explanation for bipedalism (see Chapter 7). We need an explanation rooted in an understanding that what we are trying to explain, at this stage, is not 'human life' in a social or political sense at all, but the initial evolutionary divergence of one particular zoological species – the hominids – from the ancestral pongid (ape-like) stock. All attempts to explain this divergence by reference to a 'uniquely human way of life' are a retrospective imposition of our own preoccupations on to the lives of creatures whose priorities were rooted in their own times, not ours. Such arguments are part of an old paradigm which must be abandoned in its entirety.

Lovejoy and monogamy

The old idea that early humans were monogamous continues to have its proponents. 'However,' as the feminist sociobiologist Sarah Blaffer Hrdy (1981: 175) put it when Lovejoy was writing, 'taking this position now necessitates a certain anthropocentrism and special pleading'.

Among non-human primates, monogamy produces not advanced forms of sociability but a very elementary, simple and sparse social life, with little variety or political complexity to select for novel forms of self-awareness or intelligence. Compared with other primates, those which are monogamous appear to eat lower-quality diets, have an inferior ability to perceive social relationships and have minimal levels of role differentiation (Kinzey 1987: 109). Moreover, monogamous primates are known to be 'behaviorally more conservative, and ecologically more restricted' than their non-monogamous counterparts (Kinzey 1987: 105). The behaviour of gibbons, for example, is stereotyped, with little regional variation.

Among non-human primates, in fact, a monogamous mating system appears to have the least long-term adaptive value, and it has been argued that this may apply to humans, too. In a powerful contribution on the whole subject, Kinzey (1987: 106) writes:

> The lack of social networks is the major disadvantage of monogamy per se. Promiscuity does not normally occur in *any* human society, but polygyny and polyandry taken together are much more frequent than monogamy. They encompass a greater extension of social networks than monogamy; they have greater long term adaptability, and consequently they are more common. Probably the majority of cultures in the world practice some form of extended family in which the living group contains more than a single pair and their children.

The palaeontological evidence, such as it is, does not seem to fit the monogamy theory either. It seems that there may have been pronounced sexual dimorphism among early hominids (Johanson and White 1979), and it was not all of the 'epigamic' kind which Lovejoy described in his article – the female having large breasts and buttocks, the male a prominent penis, and so on. These 'soft-tissue' characteristics which Lovejoy envisaged do not fossilise and so we lack evidence either way; what does fossilise is bone, and where male skeletons are to a marked extent larger and heavier than females, as seems to be the case with the australopithecines, then some kind of polygamous mating system with inter-male competition seems likely (Foley 1987: 171).

We can agree with Lovejoy that if the birth interval were to be reduced whilst the period of childhood dependency were lengthened, then mothers would need additional social support. But why assume a totally isolated Miocene or Pliocene female, utterly dependent on support from 'her' male, when all the indications are that these evolving ape-like hominids would have been highly sociable animals, living in groups? Might there not have been close female-to-female kinship bonds within such groups – bonds which could have been drawn upon by intelligent mothers in times of need? Hrdy (1981: 98, 217) documents multi-parenting ('allo-parenting', as it is termed) among non-human primates, showing that a variety of related females may assist the mother in caring for her infant, sometimes freeing her for unencumbered foraging. Could not evolving hominid females have formed quite extensive coalitions, and might they not also have tempted various males to give them support, perhaps even deliberately confusing issues of paternity in order to get as many males as possible to offer protection to their offspring?

In discussing such features as the physiology of the human clitoris, Hrdy (p. 176) argues that human females have been 'biologically endowed with a lusty primate sexuality', their ancestors displaying 'an aggressive readiness to engage in both reproductive and nonreproductive liaisons with multiple, but selected, males.' Her belief (Hrdy 1981: 153–8) is that many unexplained features of female sexual physiology and anatomy may have evolved in the service of a deliberate female strategy of confusing paternity! The idea is that females manipulate their sexual relations with a succession of different males, sometimes several simultaneously, so as to make a large number respond positively on the basis that her offspring just *might* be their own! This certainly seems to be common among numerous primates, and it is an idea supported by others besides Hrdy – for example, Hill and Kaplan (1988: 280), who rely in part on their social anthropological fieldwork among the Ache. In all this, Lovejoy's line of reasoning is precisely reversed.

We know (see previous chapter) that neither monogamy nor polygamy is rooted in any simple way in a species' genes. All primate males – including humans – are or would be mainly polygamous in some situations, primarily monogamous in others. Human males today are not particularly monogamous, and it is far from certain that females are very different, although mothers with heavy child-care responsibilities may have fewer opportunities or inclinations to prioritise their sex lives in the way some males in many cultures can afford to do.

In any event, as Hrdy (1981: 179) points out, men in patriarchal cultures have never had much confidence in women's instinctive monogamy, and have invented chastity belts, clitoridectomy and draconian penalties in their attempts to impose 'fidelity' on their wives, meanwhile practising rather different standards themselves. 'Whole chapters of human history', Hrdy (p. 179) writes, 'could be read as an effort to contain the promiscuity of women'. In no human culture does monogamy appear to be sustainable without powerful cultural, religious, legal and other sanctions.

Meanwhile, hunter-gatherers such as the Aborigines of Western Arnhem Land, Australia, openly celebrate sexual freedom, each woman on ceremonial occasions taking advantage of her traditional right to enjoy extra-marital sex – including, sometimes, relations with a string of different lovers in a night (Berndt and Berndt 1951). Similarly, Eskimos during their prolonged winter ceremonies traditionally engaged in sacred orgies which approached very close to complete 'sexual communism' (Mauss 1979: 60, 68). !Kung San women in the Kalahari desert increase their sexual activity, with lovers as well as husbands, particularly at mid-cycle, around the period of ovulation (Worthman 1978, cited by Hrdy 1981: 139). According to Malinowski (1932: 221), marital and extra-marital love-making games and celebrations among the Trobriand Islanders reached their climax each month at around full moon.

Erotic festivals involving sexual 'licence' form a recurrent pattern in hunter-gatherer and other ethnographies from all parts of the world. Even groups stereotypically conceived as rather sexually restrained or even prudish – such as the Hopi Indians of New Mexico – are known to have had the wildest 'secret dances' dubbed 'vulgar and wicked' by the Spanish authorities as well as by later government and religious officials (Eggan *et al.* 1979). Beyond this, moreover, it is simply the case that the nuclear family is not even recognised as a unit, terminologically or conceptually, in many non-western cultures; so-called 'extended' forms of kinship with 'classificatory' terminologies are almost universal, with sibling bonds usually accorded greater symbolic or ritual value than the (usually) all too fragile bonds uniting husband with wife.

Despite its different assumptions, Lovejoy's theory suffers from many of the defects of Tanner's. We know that in the long run human males were, as Lovejoy says, drawn into a form of behaviour unknown among other higher primates – systematic provisioning of their offspring and sexual partners. This human pattern, then, is one which certainly has to be explained. But it is not at all clear why we should introduce the male as a 'naturally' co-operative parent and food exchanger at the very beginning of the hominid line, unless it is to salvage the now discredited idea that bipedalism emerged contemporaneously with the evolution of 'culture'. Few specialists nowadays still argue this case, but if this idea is abandoned, then Lovejoy's theory implies that symbolic language and culture were simply not necessary in order to draw the male into performing his provisioning role. The implication is that it was all a matter of natural selection operating upon male genetic characteristics. This continued until there had evolved a male who was 'naturally' monogamous, and who 'naturally' devoted his time to finding food for his offspring and mate. The argument that such a monogamous male exists – even in the present, let alone 4–5 million years ago – is surely not conclusively proven.

Lovejoy placed 'human social and reproductive life' far back into the dawn of hominid existence, and elevates it to the status of prime mover. In his model, social life does not evolve out of its own material conditions. It does not evolve out of the transition to bipedalism, the emergence of tool use, changing ecological circumstances, movement into new environments, increasing reliance on meat etc. etc. Rather, it is established at the very outset, in the form of 'the nuclear family', at a time when the earliest hominids occupy an ecological niche similar to that of modern forest-dwelling chimpanzees. Lovejoy wrote in the concluding lines of his paper that his model 'implies that the nuclear family and human sexual behavior may have their ultimate origin long before the dawn of the Pleistocene' (p. 348).

In fact, he was referring to the Miocene. In this view, a sexual division of labour operating within the boundaries of the nuclear family is the one constant feature of the whole of human evolution, a 'prodigious achievement' central to the success of the very earliest hominids and the context within which all subsequent advances have been achieved. Given all that we know about monogamy in both primates and contemporary hunter-gatherers, given the time scales, and given the complete absence of any archaeological evidence for a home base until at least the Middle Pleistocene – it seems unlikely.

4. Hill

Kim Hill (1982), 'Hunting and human evolution'

Kim Hill (1982), a sociobiologist at Emory University, Atlanta, set out to reaffirm the hunting hypothesis in opposition to Tanner and other supporters of Woman the Gatherer. Hill's model assumed an early male monopoly of hunting, but was designed to explain how, nonetheless, females could have gained access to at least some of the meat.

Hill (1982: 533) envisaged a very early population in a game-rich environment where returns from predation were particularly high. Carnivorous males, it was argued, would then be able to satisfy their hunger in a few hours, whereas females – denied access to meat – would still need to forage all day. Males would then have much free time, and new strategies might evolve in an attempt to use this to increase fitness:

> One strategy that might be very successful for males would be to continue hunting during the day, and provide females with food resources in an attempt to increase the possibility of copulation with receptive females. The pattern of males hunting while females continued to forage primarily for plant items, would be the beginnings of sexual division of labour.

In Hill's model, promiscuous mating was assumed. On the one hand, males competed against one another on a direct behavioural level for meat and sex. On the other, females competed against one another sexually, the most desirable and constantly available attracting the best hunters. Noting that among both chimpanzees and baboons, females displaying oestrus signals receive more meat than non-oestrus females, Hill argued that an accentuation of this kind of selection pressure would have led to the continuous 'shamming' of oestrus among constantly receptive human females. In short, it was argued that the primate pattern of oestrus soliciting (see Chapter 6) would only have needed accentuating and systematising for something like the human hunter-gatherer pattern to have evolved.

Males, according to this model, did not at first fully provision females but consumed most of the meat they obtained themselves (Hill 1982: 537). As pregnancy and child care limited female mobility, an increasing reliance on meat food ensured that the males as a whole were in a stronger bargaining position than the females. An implication was that the females, badly needing meat which only males could provide, were prepared to do almost anything to get it. Females who offered copulations to males could induce them to fetch meat for them. The more continuously females could copulate and display oestrus signals, the more meat they got. They therefore eventually adapted so as to be able to display oestrus signals – both real and 'sham' – all the time. Meanwhile, bipedalism evolved as males ran to and fro using tools and fetching meat, those males best at running or walking upright being able to carry most in their hands and therefore enjoying most reproductive success.

At the end of this paper, Hill (1982: 540) summarised the theory under eight points:

> 1. A sub-population of Miocene apes found itself in a region where easy prey made it logical to specialise in hunting.
>
> 2. Unencumbered by offspring, males were better at this than females. With time on their hands, males hunted beyond their own needs, bringing meat to oestrus females so as to 'trade' it with them for sex.
>
> 3. As the males used artificial weapons in the hunt, the size of their canines decreased, since these were no longer needed and hampered the chewing of meat.
>
> 4. Because sexiness was useful for getting meat, females developed longer and longer periods of sham 'oestrus', obscuring the real moment of ovulation and evolving towards the human condition of continuous sexual receptivity.
>
> 5. Sexual competition between males was high, with some males very poor at hunting or gaining mates while others were extremely successful. Those males who could best carry meat to females reproduced best. Since meat-carrying and tool-use required hands which were freed from locomotory functions, bipedalism in males evolved.

6. Once tools could be carried, it made sense to rely on them more. Tool-making led to changes in the shape of the hand.

7. Tool-use and predation – especially upon other primates – developed intelligence.

8. Thanks to male provisioning, there was a decline in infant mortality, leading to increased mean longevity, increased juvenile dependency and therefore an increased need for grandmothers to assist with child-care. For this reason, females started living on beyond their reproductive years.

Richards (1987: 166–7) aptly and inevitably dubbed all this the 'prostitution' theory of human origins. He summarised the differences between the Tanner and Hill models as follows:

> While both see food-acquisition as central in selecting for bipedalism, for Hill it operates on the male, for Tanner on the female. For Hill the sexual deal is sex-for-meat, for Tanner it is sex-perhaps-for-good-behaviour (bringing meat being good behaviour of course).

Discussion

Hill's article cannot easily be dismissed. Indeed, this book will outline a theory closely related to Hill's, with a similar emphasis on the 'sex for meat' principle of exchange.

However, like Isaac's and Tanner's models, Hill's telescoped bipedalism and the basic social changes involved in becoming human into a single complex occurring in the late Miocene or early Pliocene. Hill saw the 'sex for meat' scenario as nothing radically novel, but as the extension of a tendency characteristic of baboons and chimpanzees – the tendency of oestrous or sham-oestrous females to invite copulations in exchange for male-procured provisions.

The article provided an intriguing explanation for male bipedalism and for the male contributions to food-carrying, tool-making and the sexual division of labour. It did not seem quite so convincing in explaining the role of females in all of this. The main qualities apparently required of females were that they should reproduce, prove good mothers and be continuously receptive to the most successful hunter-males.

Hill's theory suffers from a number of problems. We will not dwell on 'oestrus shamming' here, since oestrus loss and the evolution of the human female reproductive cycle will form the subject matter of Chapter 6. Suffice it to note that human females do *not* accentuate, extend or 'sham' oestrus at all but, on the contrary, have dampened down primate oestrus signals to the point at which they have completely disappeared. If there is one thing about human females which needs to be explained, it is not that they act as if they were in oestrus all the time – which would mean that they were forever unable to say 'no' – but that quite unlike any other primate, women can say 'no' at any time whatsoever, even when ovulating. Competitive prostitution would not seem to be a very good explanation for this.

Beyond this, however, there are other problems. Lovejoy's theory posited monogamy as an answer to the problems presented by inter-male sexual conflict and the consequent 'wasting' of males who, potentially, might have been used by females to provision them. Hill assumed promiscuity and polygamy, but unfortunately simply failed to address the associated problems that Lovejoy had drawn to our attention.

Although Hill spoke of a sexual division of labour, it is far from clear how competitive sexual soliciting could have produced any such result. A sexual division of labour implies that males bring food to females and offspring. But given Hill's premises, a male hunter in possession of meat would have had very little incentive to carry or drag his catch to a pregnant or nursing female waiting for him at some distant point. In the absence of either monogamy or generalised inter-female solidarity, there would always have been a certain number of 'free' females chasing after the best hunters so as to be first on the spot when a kill was made or when meat was being butchered at a tool cache or processing site. It seems strange that Hill overlooked this logical consequence of this 'free competition' model, because it would have had severe reproductive consequences of precisely the kind Lovejoy had envisaged. Young and/or non-pregnant females would have had a competitive advantage, whereas females burdened with offspring would have been the least mobile, the least readily available and the last to get meat, even though their needs would have been greatest.

A further problem is that public oestrus shamming implies continuous public sexual interest and the incitement of competition between males. The picture conjured up is not one in which co-operatively organised males are left free of sexual cares and worries – left free to make planning decisions on where to find distant game, how to track it, or how to invest in building up a detailed shared knowledge of the surrounding area. Rather, with competition making every male afraid to leave 'his' females unguarded, the picture is one of insecure and anxiously competitive males trying to snatch game as quickly and continuously as possible from the immediate surroundings. In this situation, an array of conflicts and contradictions can be envisaged. The moment one male had left 'his' female to go hunting, would not some rival have taken advantage of his absence? When a male had killed an animal, would not fights have broken out over the spoils? And if females were only interested in meat, without caring who hunted it or where it came from, would not males who robbed their companions often do as well as or even better than genuine hunters?

It is not that there would have been no answers to such problems. It is simply that any long-term stable answers – evolutionary solutions involving the establishment of a home base and genuine sexual division of labour – would have necessitated extensive coalition forming and gender solidarity, taking us beyond Hill's scenario into a very different one not based on 'prostitution'. We will return to these issues in a moment. In the meantime, let us examine a more recent variation on the 'prostitution' theme.

5 Parker

Sue Taylor Parker (1987) 'A sexual selection model for hominid evolution'

Parker's model is very like Hill's, showing little if any feminist influence. Parker's is a sophisticated, well-documented Plio-Pleistocene sex-for-meat scenario, explaining bipedalism as 'a male adaptation for nuptial feeding of females' (p. 235). The origin of 'higher intelligence' and 'language' in *Homo sapiens* is attributed to 'male competition through technology and rule production to control resources and females' (p. 235). Parker places greater emphasis than Hill on gathering and scavenging as opposed to hunting, at least in the early stages of evolution. Moreover, she differs from Hill in recognising that early hominid females could not have motivated males to bring food to them while they waited behind at a fixed home base. In Parker's scenario, the picture conjured up is that of females having to chase after males in order to be the first present when kills were made or meat butchered at a processing site. But if anything, this is an even more explicit and uncompromising 'prostitution' model than Hill's.

Parker unfolds her scenario as follows. Plio-Pleistocene hominid males, she writes (p. 243), would go out foraging at a distance and use the food – for example, roots extracted with digging-tools – for sexual purposes: 'Through courtship or nuptial feeding of estrus females, males could entice females to go away with them on "safaris" or honeymoons where competing males were not a threat.'

Later, Parker (p. 244) continues, the coveted foods would have included increasing amounts of meat. Brains taken from hunted animals are particularly valued by chimpanzees, but are difficult to extract through hard skulls which first have to be smashed (Teleki 1973: 144). Hominid males who used rocks or hammers for the purpose may have helped solve this problem, discovering a particularly useful enticement for attracting females.

Still later, according to Parker (1987: 245–6), males would have dragged whole carcasses of animals, not to a 'home base', but to special sites dotted about the landscape:

> A male subsistence strategy of bringing carcasses of scavenged prey to special sites where processing tools were stored (Potts, 1984b) would have paid off reproductively by attracting females to locations where they could be guarded at least temporarily.

By adapting to walk on two legs instead of four, writes Parker (p. 243), males would have been able to get, transport, defend and display such coveted foods:

> This pattern could have arisen naturally through female choice of males who responded to their begging for favored food; presumably males who were able to get more preferred foods unavailable to females, e.g., meat, would have been preferred by females.

Like Hill, Parker assumes promiscuity; 'nuptial feeding' implies not long-term parental investment but each male's short-term provisioning of numerous females in exchange for casual sex (p. 244). Like Hill, Parker also sees male dominance as having intensified owing to the male monopoly on meat (pp. 243, 246).

What counter-strategies would females have evolved? Parker accepts that females of all species generally 'prefer not to be controlled', and would have attempted to maintain their own freedom, particularly where choice of sexual partner was concerned. In her view, increasingly sexy and intelligent protohuman females would have played off males against one another in order to pursue their own ends. They would have incited inter-male fighting 'by using one male as a foil to get the attention of another', males battling with one another with increasing intensity for 'control of females through provision of meat' (1987: 246–7).

Discussion

Parker's article was informative and well researched, and at the time of publication was a state of the art expression of sociobiological and neo-Darwinian thought on human origins. Nonetheless, her scenario conjures up a picture of sexual chaos on precisely the scale necessary to prevent human culture from emerging. We are told, for example, that at food distribution points there would have been intense 'aggressive competition among males', adding to 'the value of using aimed missiles in combat'. Put bluntly, this 'nuptial' picture is one of males hurling stones or spears at one another in fights for temporary control over females at butchering sites, with females actively inciting males to intensify the violence! Parker seems not to have considered whether it would have been in the genetic interests of females with increasingly vulnerable offspring to collude in such fights.

We may leave aside the many problematical aspects of all this and concentrate on one issue. Regardless of how much or how little inter-male violence we assume, the system Parker envisages is one of ruthless competitive sexual selection, placing a very high premium on the ability of females to become fully mobile as they chase after meat-possessing, highly mobile males. Now, it seems undeniable that such a system would favour sexually available, non-pregnant females at the expense of burdened mothers. But this means that females with large-brained, slow-maturing offspring would be discriminated against. The best-provisioned female meat-eaters would be those fastest at presenting themselves sexually at kill sites or butchering sites. Females would have to compete with one another in racing to such sites as kills were made; they would also have to compete in appearing sexually tempting to the males.

The problems with all this are considerable. Pregnancy, breast-feeding and other reproductive responsibilities would all interfere with both sexual availability and mobility. The 'losers' in such a competition would probably include the best mothers, the 'winners' the worst. Certainly, Parker explains no better than Hill how any such system could generate mechanisms to ensure that females with increasingly dependent offspring had priority of access to meat. And as females who prioritised meat-eating had to scamper about chasing males and therefore had fewer or smaller-brained surviving offspring, we might imagine that in each generation, the females most abundant in the population would be those whose genetic constitution best enabled them to *avoid* becoming too reliant on meat.

Yet the core objection to both Hill's and Parker's scenarios is a still more fundamental one. It concerns the manner in which both models conceptualise 'sex-for-meat' exchange. As noted earlier, in the form in which it is presented, this kind of 'trading' does seem to resemble 'prostitution'.

Now, the anthropomorphic description of primate oestrus-soliciting as 'prostitution' is not new. Solly Zuckerman popularised the usage in his pioneering book, *The Social Life of Monkeys and Apes*, published in the 1930s (Zuckermann 1932: 233). After a discussion of primate sex-for-food and sex-for-status exchanges, the author commented that if a particular response of a sexual nature 'is always followed by the acquisition of some social or material advantage', then 'it is legitimate, for purposes of description, to refer to the response as a form of sexual prostitution'.

The question we must now ask, however, is whether 'prostitution' – as Zuckerman's words might imply – is something intrinsic to all forms of sexual bargaining for economic gain. Is it an inevitable consequence of the fact that nursing females needing male support may have nothing to 'sell' but their bodies – or are totally different forms of sex-economic exchange actually possible?

Zuckerman wrote many years ago, and there is nowadays no need to give any particular weight to his formulations. However, the difficulty is that few contemporary palaeoanthropologists apparently feel any need to draw a distinction between primate sex-for-meat exchanges and human hunter-gatherer practices such as 'bride-service'. Indeed, as in Sue Parker's case, the two may be explicitly linked. Parker (1987: 244) writes of her 'nuptial feeding' model: 'This scenario has the virtue of connecting chimpanzee behavior with modern ethnographic behavior [e.g., meat for sex as described by Siskind (1973a)]. . . . '

By 'chimpanzee behavior', Parker means the oestrus-displaying and 'presenting' behaviour of female chimpanzees as they beg for meat from males.

Hill (1982: 533) notes a similar pattern among both chimpanzees and baboons, and continues:

> The widespread reports in ethnographic literature [e.g., ... Siskind 1973a] that human males frequently trade meat for sexual access or that good hunters obtain more wives suggest that this is the optimal solution for 'hunting apes' to increase their fitness.

Again, a direct primate–human parallel is drawn. In each case, in other words, the writer draws a parallel between primate females who competitively present their rumps to meat-possessing males, and hunter-gatherer women who make marital relations dependent on their menfolk's hunting success.

The 'sex-for-meat' concept will by now be familiar to the reader; it is central to the theoretical construct of a sex strike and therefore to the whole argument of this book. More specifically, we encountered sex-for-meat exchange in our discussion of bride-service in hunter-gatherer societies, and concluded that it lay at the root of men's 'avoidance' of their own kills (Chapters 3 and 4). Hunters normatively avoid eating their kills precisely because the whole point of hunting is to surrender the meat so as to earn goodwill from their spouses and/or in-laws and thereby qualify for marital relations.

In responding to Hill and Parker, we need to think very carefully about all this. It seems important to determine precisely what is the relationship between hunter-gatherer bride-service and what Zuckerman long ago termed primate 'prostitution'.

Although the topic has not been exhaustively debated, it seems that the majority of social anthropologists would deny any simple parallel between these two. Certainly, application of the term 'prostitution' seems problematical in the human case. 'The exchange of something for sexual favors is not considered prostitution', writes one cultural anthropologist (Witherspoon 1975: 25), referring to the viewpoint of the Navaho Indians. 'On the contrary, sexual relations without exchange are considered immoral.'

This last point seems vital: amongst almost all hunters and gatherers, as well as in more developed tribal cultures, it is actually considered wrong for a woman to have sex *without* extracting some material gift from her spouse or lover. To this we can add that wives with their kin rather than husbands or lovers are in the forefront in enforcing this rule, and that there is a sound economic basis for it. For a woman to offer sex 'free' would be for her to let down her sisters and her kin. It would undercut their sexual bargaining power, and consequently they would collectively react.

Regarding the situation among the Trobriand Islanders – among whom, as usual, men have to 'pay' for sex – Malinowski comments:

> This rule is by no means logical or self-evident. Considering the great freedom of women and their equality with men in all matters, especially

> that of sex, considering also that the natives fully realise that women are as inclined to intercourse as men, one would expect the sexual relation to be regarded as an exchange of services in itself reciprocal. But custom, arbitrary and inconsequent here as elsewhere, decrees that it is a service from women to men, and men have to pay. (1932: 269)

Malinowski does not link this obligation to pay with 'prostitution', but neither does he repudiate such a parallel. Witherspoon (1975: 25), in his discussion of a similar situation among the Navaho, does tackle the issue and explicitly warns that we must be careful not to impose western concepts of morality which might lead us to see 'prostitution' wherever we find apparent 'payment' for sex. It is tempting to agree. But assuming that the 'prostitution' label is rejected on more than purely moral or political grounds, what exactly is the scientific basis for distinguishing between 'prostitution' and so-called 'moral' patterns, when in both cases females grant sexual favours in exchange for material benefits?

Different observers may arrive at different conclusions, but if we use solidarity as a touchstone, some new and perhaps more satisfactory insights can be obtained. Instead of concerning ourselves in the abstract with whether women engage in sex with an eye on economics, we can look at the concrete social effects and ask: do women's demands for 'payment', under the specific concrete circumstances, enhance social solidarity – or undermine it? No one has ever argued that the 'prostitute', in the contemporary European sense of this term, is an active agent of social solidarity. By allowing men to 'buy' sexual access to her body with money, prostitution in fact allows men to play off one category of women against another; in the eyes of the female community as such, it is this which undercuts all women's bargaining power in relation to 'their' men.

At the end of Chapter 4, we concluded that an element of female gender solidarity, implying a measure of collective sexual self-control, was an important mechanism through which women in many traditional cultures help sustain the own-kill rule. By this means, in other words, women ensure that they receive gifts of meat from men. An alternative strategy which may be combined with gender-solidarity is for women to maintain strong links with brothers or other kin. In either case, by not being too 'loose' sexually – in other words, by maintaining solidarity and the right to say 'no' – women help ensure that hunters do not take them for granted but instead have to work for their marital rights, surrendering their game and (usually) carrying the meat all the way to a home base where women can process it without having to travel far.

Although women must be attractive and capable of enjoying sex for this logic to work, it is equally true that an essential ingredient is an element of sexual negativity – sexual resistance. It is this second element which no sociobiological model of human evolution has as yet properly taken into account. Almost all contributions from sociobiology in the 1980s stressed 'continuous receptivity' and the evolution of female 'sexiness', yet seemingly forgot that none of this would be tolerable to any female unless her increasing ability to signal 'yes' became matched by an equal and opposite capacity to signal 'no'. Having lost her hormonal cyclical period of anoestrus or incapacity for sex, she urgently needed to be able to signal 'no' with at least equal effectiveness in other ways. In the typical hunter-gatherer bride-service configuration, as we saw in Chapter 4, this second capacity is evidently central. Solidarity enters in at this point because if women are to be effective in signalling 'no', they cannot afford too much inter-female rivalry and competition. If one woman were to signal 'no' only to find another beside her signalling 'yes' to the same man, her bargaining position would be completely undermined. Put crudely, women need each other's support in controlling the supply of sex.

A central argument of this book is that far from constituting 'prostitution', such collective control over sex lies at the root of all sexual 'morality'. Janet Siskind (1973b: 235) shows an understanding of this when she perceptively points out: 'If women are to be the incentive for male hunting efforts, they must be scarce. . . '. One way for women to become scarce, as Siskind continues, is 'to limit sexual access by social rules of sexual morality. . . '

Now, in conceptualising the evolutionary origins of this, we do not need to envisage anything symbolically sophisticated: all that would be required, as an absolute minimum, would be the capacity of female coalitions or groups of kin to inhibit unwanted male sexual advances. The mere fact that such resistance were collective – supported in principle by women in general, without breaches – would give it the embryonic status of a 'rule'.

At this point it seems appropriate to bring the Sharanahua directly into our discussion, partly because the reader will be familiar with them from Chapter 4, partly because both Hill and Parker explicitly appeal to them in support of their scenarios.

Two points stand out. Firstly, gender solidarity among the Sharanahua is pronounced. What Siskind (1973a: 109) terms the 'combination of same-sex solidarity and antagonism to the other sex' permeates the social structure.

Secondly, by any conventional definition, a 'prostitute' among the Sharanahua would be a woman for whom solidarity was not a priority. If the 'price' offered to her were high enough, she would break ranks with the other women of her community, offering her body for personal gain regardless of the collective female consensus. If the Sharanahua women as a whole decided on a sex strike, motivated by collective dissatisfaction with the hunting performance of their menfolk, the prostitute would be a strike-breaker. She would be the one to offer her favours on an individual basis to whichever male(s) could provide her with enough meat. For example, she might be prepared to meet one or a few men privately outside the village – perhaps far out in the bush – to exchange sex for economic benefits, cheating the sex-striking women back 'at home'. Moreover, if solidarity between the men and hence their capacity for co-operation in the hunt were in part founded on their respect for the women's sex strike, the prostitute would be a threat in another respect, too. She would tend to undermine this male collective resolve, appealing against solidarity to the private sexual self-interest of males. In all these respects, the 'prostitute' would stand in opposition to the bulk of her gender group.

To equate the normal Sharanahua 'sex-for-meat' logic either with primate oestrous soliciting or with human prostitution seems in this light an extraordinary confusion of opposites. One strategy involves prioritising gender solidarity under all circumstances; the other involves dispensing with solidarity in pursuit of competitive personal gain. In this context – and bearing in mind the previous arguments of this book – we can adapt the words of Durkheim (1961[1925]: 59) in linking 'morality' quite simply with solidarity: 'Moral goals, then, are those the object of which is *society*. To act morally is to act in terms of the collective interest.'

No matter how much this classic formulation can be queried or refined (see, for example, Ingold 1986: 222–92), at the simplest, most elementary level, human sexual 'morality' can have no other basis. Durkheim himself may not have been thinking particularly of gender solidarity, but his principle is all we need. The 'moral' hunter-gatherer woman is the one who keeps in step with her sisters, her kin and/or her gender group, on occasion refusing sex unless or until the male(s) in her life can be induced to behave acceptably, for example by providing meat. The 'immoral' woman, by contrast, is the selfish one, who exploits her body's attractions in competition with other women, using sex for her own personal gain at the expense of her sisters, undermining gender solidarity and thereby weakening the position of her gender group as a whole.

On this basis and no other can we decide upon the morality of a sexual act, or decide upon whether or not to call it 'prostitution'. Whether material benefits are involved is entirely secondary: what matters is whether, in pursuit of any benefits, solidarity is enhanced or undermined. Admittedly, no social system is ever a manifestation of either pure solidarity or pure competition – both tendencies will always be present to some degree. But in any stable system, one logic or the other will prevail. It is the basic argument of this book that only one logical thread, carried through to its conclusion, leads us towards central-place foraging, a home base, sexual morality and a genuinely human lifestyle. The other thread is a competitive, primate-style 'prostitution' pathway, leading social life in wholly non-cultural directions. Hill's and Parker's models of human origins pursue only this second pathway, and provide us with an interesting object lesson for that very reason.

Zuckerman (1932: 233) described primate 'prostitution' as 'mainly an effect of the system of dominance upon which sub-human primate societies are based'. This is an important insight. As Pateman (1988:189–218) has eloquently re-emphasised, females can systematically prostitute themselves to males only if the overall social structure is one of male dominance, resting as this does upon divisions and rivalries between females who then have to compete to gain privileges from members of the dominant sex.

We saw in Chapter 4 that the only effective answer to male dominance is female solidarity. Neither Hill nor Parker says anything about female gender solidarity or collective *resistance* to male dominance or exploitation. Their two related models in fact imply a massive shift or even 'counter-revolution' away from some of the basic primate patterns discussed in Chapter 4. It was noted there that female primates tend to determine social structure by arranging themselves spatially in accordance with their own foraging requirements, leaving the males to map themselves secondarily on to the female-defined distribution pattern. In the origins scenarios of Hill and Parker, however, protohuman females can no longer obtain their own food. The space over which females forage no longer has the same value, and female decision-making no longer has the same power in determining overall social structure. Males now monopolise access to the basic economic resources. It is they whose foraging strategy becomes primary, and the females have to 'map' themselves on to this pattern, adjusting their sexual behaviour to match the new male-defined economic realities. Female 'prostitution' – in this case as, perhaps, in all cases – is an expression of female economic dependency and weakness.

One thing is certain. If this really were the prevailing logic within a population of evolving hominids, the 'home base' institution with its accompanying female-defined parenting priorities would not evolve. The centre of gravity would be male-defined hunting space or processing space, and females would have to revolve around this, offspring or no offspring. Whenever game was caught, females would be in a race with one another to get to the kill site or butchering site, since those first to arrive would have the best prospects of obtaining a share. This is the exact inverse of the human hunter-gatherer pattern, in which *despite* the importance of hunting, women can and do centre their lives around a home base to which males laden with game are forced to return, and in which despite physical and reproductive distinctions between women, solidarity ensures that all share in the provisions which the opposite sex brings home.

Mobility, Group Size and Home Bases

It will have become apparent by now that the core concept central to all the models which we have surveyed is that of the 'home base'. Before we can finally put these theories into perspective, we need a better understanding of what this really entails.

Although in technological terms it rests on many factors including the domestication of fire (Oakley 1958; Wymer 1982; James 1989), what has all too often been overlooked is that the home base as an institution is equally rooted in a fact of sexual politics – the fact that human females do not 'chase after' males out hunting in the bush. In effect, hunter-gatherer women stand their own ground. Even if they gather over a wide area, they do so usually quite separately from men, typically in all-female groups. Their activities and solidarity may give them considerable autonomy and power. Female status among hunters and gatherers varies widely according to conditions (Hayden *et al.* 1986), but whatever the precise mode of foraging, women almost invariably organise their lives around 'their own' space, whose focal point is the hearth and campsite.

For all models of human origins, the concept of the 'home base' has always been and still remains central. As DeVore (1965: 33) put it several decades ago, no monkey or ape has such a base; when a baboon troop leaves its sleeping place in the morning, all the troop members must move together. There is no assurance that the troop will return to the same sleeping place in the evening, and every individual, even though sick or injured, must keep up with the others or risk permanent separation from the troop. Moreover, because the whole troop moves together, DeVore continued, it is not possible for baboons to hunt other animals effectively:

> Even more important, the absence of a home base makes it impossible for males to go in one direction in search of game while females and juveniles disperse to gather vegetable foods – a system of food-getting which seems universal among hunter-gatherers.

Although hardly anyone now talks about home bases in the Pliocene or Early Pleistocene, its significance for modern hunter-gatherers and for our understanding of the transition to symbolic culture remains undiminished. Its particular importance in the present context is that it tells us something about the role of early women.

For all hunters and gatherers, the home base is the central focus of activity. Its precise location usually selected mainly by the women, it is a known, predetermined point on the landscape to which all the members of a given group, however widely scattered, can consistently return and meet. It is the place to which provisions are brought, and at which food is prepared and consumed. 'The home base', as Potts (1988: 249) puts it in his book devoted largely to this topic,

> is also the place where group members sleep, make tools, and perform other maintenance activities. It is the primary spatial arena of social activity: the exchange of stories and information, the redistribution of

> food, the rearing and protection of young, and the reciprocal exchange of resources or services.

A division of labour along gender lines is integral to the home base concept. It is at the base that marital sex normally occurs and that two categories of resources, 'meat' and 'gathered foods', can be systematically exchanged between two well-defined and usually rather rigidly demarcated groups – men on the one hand, women on the other.

Some feminists, for example Fedigan (1986), have rejected on ideological grounds the linkage of women in so many origins models with 'home'. This reaction is understandable in view of the manner in which anthropologists have tended to allow nuclear-family, western images of passive domesticity to pervade the concept. But perhaps this whole topic can be evaluated in a different way once it is realised that the 'home base' for hunter-gatherers has little to do with 'home' in its western cultural sense as a privatised space peripheral to the centres of social and political power. In terms of the evolution of human culture, to be centrally involved in establishing the base camp was to be in a pivotal position, in effect carving out and defending a collective space which was to become the controlling centre of all politics, all social solidarity and all economic exchange.

If we take into account the transient camps of tropical hunter-gatherers, it becomes clear that a home base is not necessarily a single permanent location. It may be occupied for only a few days. In most cases, the degree of permanence of the camp varies according to the season. But regardless of whether a camp is occupied for months, weeks or only days, the basic point is that the camp exists as a space which has been marked out as a distinct sphere and set apart from the foraging trail as such. A chimpanzee sleeping site is just one point in a chain of points along a foraging trail. A home base, for the duration of its occupation, is the beginning and end of all such trails.

Almost universally, this distinct social sphere is particularly associated with femininity. Once established, its value is that it exempts many members of the group – such as the young, the sick and the elderly – from having to move on each time an expedition to explore distant foraging grounds is mounted. Women in particular can base themselves in the same defended, watered, sheltered and pivotal spot for days or even weeks before moving on, specialist hunting parties or other foraging groups continually being sent out and welcomed back with supplies. This is the essence of what Lewis Binford (1980) terms 'logistic mobility', which he contrasts with the 'residential mobility' (that is, the constant shifting of sleeping sites) of primates and pre-cultural hominids.

Why can humans occupy the same camp for days or even weeks continuously, whereas primate groups, such as chimpanzees, have to keep moving their sleeping sites on a day-to-day basis? The reasons are complex, but an important dimension is the fact that the hunter-gatherer group does not simply forage in the area immediately adjacent to its sleeping site. There is therefore no need to move on once immediately local resources have been used up. Instead – as Lewis Binford (1983) has vividly described in the case of the caribou-hunting Nunamiut Eskimo – a constellation of far-flung localities can be combed for food whilst retaining the same base camp. Certain members of the group can forage at great distances, leaving other members, particularly immature offspring and their mothers, behind. When necessary, the distant foragers – typically hunters – may even stay out overnight, after which time, if successful, they return to the base-camp laden with food (Binford 1983: 130). Because an extremely wide area may be combed for food, there is no need for the base camp as such to be constantly moved.

All primate foragers, by contrast, experience a conflict between being residentially stable on the one hand, and being sociable on the other. The two are simply not compatible. As group size increases, so the foraging group as a whole – males and females – has to become more continuously mobile. This is because primates have to eat as they go, each individual relying essentially on whatever food it can find for itself in the space adjacent to its own body. The more individuals there are in each foraging group, the smaller is the available body space, the greater the internal competition for food, and the sooner the temporarily occupied area is 'eaten out'. The larger the group, in other words, the briefer must be its stay at any one place (Dunbar 1988: 305–22).

A consequence is that in proportion as primates are sociable, so they are compelled to devote more energy to moving around, the females having to bring their immature offspring with them (for a full discussion see Dunbar 1988: 292–322).

In any discussion of the evolution of hominid patterns of foraging and group living, this logic must be taken into account. The connection between large brains, slow maturation rates and added burdens of motherhood has been discussed already in this chapter. As brain size and childhood dependency increased, evolving protohuman mothers would have been confronted with increasingly severe infant-transport problems. But the difficulty is that on this logic, the evolving protohuman female whose intensifying burdens tempted her to cut down on travel would have had to forage in a relatively isolated way, keeping other females at a distance from each feeding spot as it was discovered. Depending on the local availability of food, this would have made it difficult for groups of females to do something which is commonplace in modern human camps of hunters and gatherers – support one another in child care and other domestic tasks. It would certainly have placed strains on close links with all relatives, including grandparents; the more relatives present in each foraging group, the less easily could it afford to stay

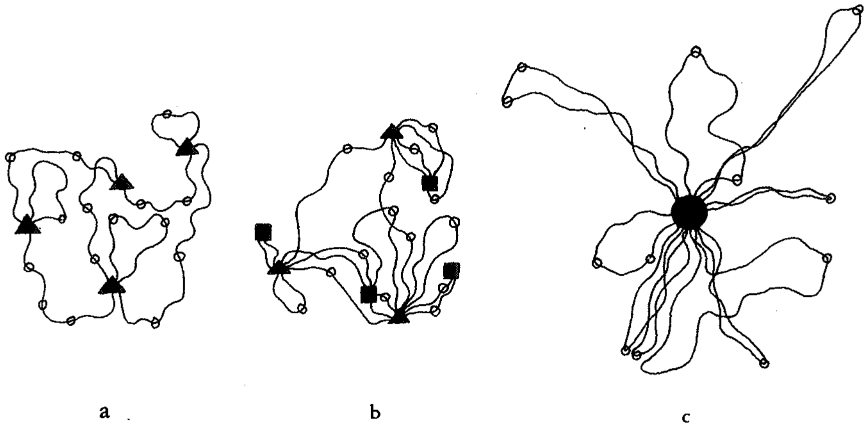


Figure 3 Foraging and spatial patterning. ▲ = sleeping sites. ■ = areas for resource-processing, typically centred on tool caches. ○ = points along the trail at which food is obtained.

- a A common primate pattern: sleeping sites are distributed at intervals along the foraging trail.
- b Hypothesised early *Homo* pattern. Focal points now include stone tool caches as well as sleeping sites.
- c Modern hunter-gatherer 'logistic' pattern. Sleeping, tool storage, food processing, child care, information exchange and all activities except actual food procurement can be focused on a single semi-permanent base camp.

for a period in any one place.

By the standards of these primate-derived considerations, human hunter-gatherer females have achieved something quite remarkable – combining intensified group life (and almost always some significant sharing of child care and/or other household burdens) with facilities for resting and nursing offspring at a home base. Despite living in large social groups, human females do not have to travel across the landscape from dawn to dusk in search of food, carrying their offspring with them and ending each day at a new location. They have in this sense broken through the whole system of constraints governing primate social evolution. Group-living human females can afford to centre their lives at and around a home base area largely because, as we saw in Chapter 4, they have adopted a strategy of 'standing their ground' and making the opposite sex do some or much of the necessary foraging for them. The problem, of course, is to explain how this breakthrough could have been achieved (figure 3).

The 1980s in Retrospect

Isaac, Tanner, Lovejoy, Hill and Parker share one assumption in common. This is that our task is to explain the origin of the hunter-gatherer way of life by means of a single all-embracing theory which covers a vast timespan since it must also explain bipedalism and the slow, supposedly culturally inspired divergence of the hominids from the pongids. It is this simplifying gradualist assumption underlying all the theories which the latest fossil and genetic evidence has undermined, leaving the field in a state of some confusion and uncertainty.

The key issue is the approximate date of emergence of a recognisable 'hunter-gatherer' social configuration centred on the institution of a 'home base'. No one now disputes that the early hominid tool-makers ate meat. What matters is how significant this component of their diet really was, and at what stage there emerged anything resembling the contemporary hunter-gatherer pattern in which females control the domestic area and gather while males go out hunting at a distance and bring food 'home'.

The truth must reside somewhere between two 'extreme' possibilities. The first of these extremes is Isaac's view, supported with varying emphases and varying views on the relevance of hunting by Tanner, Lovejoy, Hill and (in part) Parker. This is an 'early' scenario. Human evolution was always gradualistic and unilinear. The sexual division of labour stretches back into the distant Plio-Pleistocene (or even the Miocene) because it was something which hominids as a species just 'did', perhaps in connection with their evolving bipedalism and relatively large brains. If there was any 'human revolution', within the paradigm of these writers, the expression refers not to an unprecedented and momentous Late Pleistocene political transformation, but to an essentially zoological process of accelerated genetic evolution which got under way two or more million years ago, when hominids were emphatically no more than one animal species among others. Such a way of looking at matters would imply that the characteristically 'human' system, however novel, was not dependent on fully symbolic culture for its success; it would be something which chimpanzees, for example, might have managed if only they had been subject to slightly different selection pressures – if only they could have walked upright more easily or displayed a little more intelligence, generosity and/or dexterity.

The contrasting possibility is a 'late' scenario. It would seem to imply a process of conflict, struggle, set-backs, local extinctions – and occasional bursts of explosive evolution once radical solutions to pressing problems had been found. This would be in keeping with Lewis Binford's recurrent theme that for change to occur, 'the system . . . must be under stress in some way, must face some problem' (1983: 222). In this context, we would need to know: What was the zoologically 'insoluble' problem whose ultimate solution was language, the incest taboo, the 'home base' arrangement, a sexual division of labour, ritual, art and, in short, symbolic culture?

Acceptance of the late scenario would imply that establishing a home base arrangement with concomitant sexual division of labour was delayed – seemingly endlessly – because it was something profoundly difficult to achieve and sustain. It did not simply 'evolve', immediately, whenever environmental pressures made it in our eyes theoretically 'optimal'. Instead, because of the difficulties inherited from the past, evolution was held back. For a million or more years, hominids – including even the large-brained and skilled tool-maker, *Homo erectus* – remained unable to make what with hindsight we see as the 'necessary' breakthrough. As a consequence, population levels remained fairly low, technological development reached a plateau and then stopped, and the various hominid species or subspecies stayed locked within a relatively narrow band of habitable ecozones within Africa, Europe and Asia.

In this book, it will be shown that something approaching the second scenario now looks more likely. Even so intelligent a creature as *Homo erectus* seems to have got stuck in a rut for about a million years, the process of advance being 'quite clearly constrained', as Clive Gamble (1986b: 6) puts it, 'by factors other than simply technological competence'. This 'late' scenario would envisage certain ancient and deeply rooted socio-sexual and political constraints restricting what even the most competent, intelligent, 'sociable' hominids could achieve. The theoretically or retrospectively 'optimal' system of establishing a sexual division of labour/home base was not attained. Although to us it seems logical, for evolving hominids themselves it was not optimal for the simple reason that it was not even possible – its material preconditions had not evolved. It required not just bipedalism, tool-making, basket-making technology, larger brains or good-naturedness, but a massive social and sexual revolution culminating in the firm establishment of collectively agreed moral regulations and symbolic culture in something like the form in which it governs the lives of hunter-gatherers to this day. The neural, anatomical, physiological, technological, ecological and other preconditions which had to be met before all this could work were numerous, and it took two million or more years from the first manufacture of stone tools before they were all in place simultaneously in the case of any one population.

An effort of the imagination is needed if we are to comprehend what was at stake. The fact that contemporary hunter-gatherers manage the sexual division of labour easily should not blind us to the difficulties – contemporary humans, after all, are the beneficiaries of millennia-old established rule systems and traditions which are the products of the human revolution and did not exist prior to it. Demarcating the 'home base' area from a much wider foraging range was not just a conceptual or technical challenge. It presupposed on the part of females a powerful capacity for solidarity and a resistance to any attempts to make them move from their chosen 'home' whilst hunting was in progress. It presupposed on the part of males a respect for this resistance, and sufficient self-control to avoid either rape or temptations to eat their own kills on the spot. It meant being able to separate the act of *production* systematically and regularly from a postponed act of *consumption*. Above all, it presupposed that males could travel long distances away from their female sexual partners for periods of time, *realistically able to dismiss the worry that rival males might take sexual advantage of their absence.* To the extent that any primate legacy of male behaviourally competitive sexuality still prevailed, all this would have presented a vast set of challenges.

If such considerations were valid, we might expect evidence for a firm sexual division of labour – that is, a clearly demarcated home base area implying the complete liberation of hunters to forage at great distances – only late in the archaeological record. Evidence for it would be bound up with the first firm evidence for other dimensions of mental and social collectivity such as ritual, religion and conventionalised symbolic art. That would mean that even archaic *Homo sapiens* – accomplished tool-maker, fire-user, hunter and possessor of a massive brain – never quite found a stable and effective solution to the sexual and political problems involved, leaving the final perfection of this arrangement to await anatomically modern humans possessing fully symbolic culture. If there was a 'human revolution', its final successful consummation came remarkably late.

Conclusion: Economics, Meat and 'Higher Purposes'

Before we leave the origins theories of the 1980s, it seems worth tying up some threads with a final thought. It is often pointed out that a sizeable contribution to the diets of many modern hunter-gatherers is food obtained while actually foraging. This is the so-called 'snack factor'. Foods are often eaten immediately upon obtaining them, instead of being shared at campsites (Hayden 1981: 419).

It is a fact, however, that such foods tend to be of vegetable products rather than meat, and of smaller categories of game rather than large ones. In general, the larger the category of prey animal, the more powerful are the inhibitions against eating one's own kill (Chapter 3). Moreover, it is certainly not the non-tabooed types of food – berries, nuts and so on – which occupy a central place in traditional symbolic or ideological systems. Vital as gathered foods may be – just as air is vital, or water – they are likely to be 'free'. All members of the social group tend to have equal access to them, and taboos and avoidances are less imperiously required. It seems reasonable to suppose that dependence on such necessities has always been a feature of human and pre-human life, yet for that very reason – *because* gathering was so vital whether before, during or after the transition to culture – it cannot qualify as the new factor precipitating the establishment of culture in its modern sense. This new factor must have been women's success in harnessing the hunting capabilities of men.

The baboon and chimpanzee evidence surveyed earlier in this chapter indicates that initially, meat would have been 'free' like other foods, except for the fact that its distribution would have been particularly 'unfair'. Young offspring and their mothers would have been in effect penalised, but to begin with, no one would have been in a position to impose collective norms of 'avoidance' or 'respect' ensuring meat's equitable circulation and exchange.

But if this were so, then hominid life would still have been in a pre-economic phase. Until collectively imposed norms of sharing and exchange began to make their presence felt, the realm of 'economic' life in the human sense would still have been non-existent. As the early economic anthropologist Thurnwald (1932: xi) put it:

> The devouring of newly killed beasts. . . . certainly cannot be called economics. More than this is implied in the term. If there ever was a time when man, or his ancestor, lived from moment to moment on what he killed or caught, it was a time without economics.

Or as the archaeologist Robert Braidwood (1957: 122) observed, a man who followed animals 'just to kill them to eat' would be 'living just like an animal himself'.

An implication is that whatever the importance of 'foraging', to be human is to go beyond this – it is to engage in relations of 'production', a position linked historically with the names of Marx and Engels:

> Men can be distinguished from animals by consciousness, by religion, or by anything else one likes. They themselves begin to distinguish themselves from animals as soon as they begin to *produce* their means of subsistence. . . . (1947[1846]: 7)

In Marx's terms, it is the dimension of systematic social *exchange* which defines food procurement as 'production'. Marx writes: 'although isolated labour (its material conditions presupposed) can also create use-values, it can create neither wealth nor culture' (1951[1875]: 2,18) The isolated individual, outside society, feeds himself on what he finds. 'In society, however', Marx continues,

> the relation of the producer to his product, as soon as it is completed, is an outward one, and the return of the product to the individual depends on his relations to other individuals. Nor does the direct appropriation of the product constitute his purpose, when he produces in society. Between the producer and the product distribution steps in, determining by social laws his share in the world of products; that is to say, distribution steps in between production and consumption. (1971b[1859]: 27–8)

Returning, now, to our discussion in Chapters 3 and 4, we may say that if hunters in fully human cultures 'produce', there is a direct connection with the existence of the own-kill rule. Adapting Marx's words, we may say that to the extent that the hunter avoids eating his own kill, 'the relation of the producer to his product, as soon as it is completed, is an outward one. . . ', so that 'the direct appropriation of the product' is not production's purpose.

We have seen that in human hunter-gatherer cultures, hunters do not kill game in order to eat the meat. In fact, they often consume very little of it.

Where psychological motivations are concerned, men hunt less to eat than to win self-esteem and to be perceived as generous and skilled hunters, particularly in female eyes. That is, they hunt for complex reasons connected with their self-esteem, their sexuality and their general social status and prestige. These 'higher' purposes can be pursued successfully only to the extent that each hunter can avoid being so greedy and short-sighted as to consume his own kills – a fault which would be seen as something akin to incest (Chapter 3). Only to the extent that individuals 'respect' their own produce (procreative and economic) can the realm of human life, with its cycles of circulation and exchange, come into being. Because primates, by contrast, start to eat food immediately as it is found or as soon as it is physically possible to do so, circulation cannot develop and economic life cannot even begin to arise.

Were it not for the own-kill norm, then, it might seem legitimate to equate human hunting activities with primate predatory behaviour. Once the explanatory value of the 'own-kill' concept is recognised, we can no longer afford to blur conceptual boundaries in this way. We have seen in earlier chapters that human hunters typically 'respect' or 'avoid' their own kills, at least on some symbolic level even if not always in more literal ways. Even relatively 'selfish' hunters in most cultures offer up animal 'sacrifices' to the spirits, or they take care to avoid 'totemic' flesh, or they 'respectfully' hunt creatures puzzlingly defined as 'kin'. As Leslie White (1949) put it long ago, humans can even distinguish 'Holy Water' from 'water' – a litmus test of symbolic capacities if ever there was one. All this is a basic condition of true economics; and it indicates something which is by primate standards extraordinary. Since non-human primates show no signs of the necessary self-restraint, civic consciousness or ability to observe ritual avoidances, we must conclude that before the hominisation process was completed – bringing with it the establishment of 'economics' for the first time – a truly revolutionary restructuring of primate behavioural norms had to be achieved.

Chapter 6
Solidarity and Cycles

> Revolution is necessary not only because the *ruling* class cannot be overthrown in any other way, but also because only in a revolution can *the class which overthrows it* rid itself of the accumulated rubbish of the past and become capable of reconstructing society.
>
> Karl Marx and Friedrich Engels, *The German Ideology* (1846)

It has been a consistent implication of my argument that for this restructuring to be accomplished, women had to take the power. As members of the oppressed sex, they had to develop their coalitions, curb internal rivalries, stand by one another regardless of reproductive condition or personal circumstance – and compel males to bring meat on pain of exclusion from sex.

There was no way that males could adequately transform their behaviour whilst females were still colluding in their own oppression. As we will now begin to see, the evolving human female therefore had to define her own space, enforce respect for this, inhibit male exploitation and rape, signal her inviolability in her own blood and – in effect – seize power in the emerging institution of the 'home'. In this chapter, I will focus on a preliminary condition of this process – ovarian synchrony and the evolution on that basis of a specific and very unusual kind of reproductive physiology and anatomy.

Biologists have long been puzzled by the evolution of the human female menstrual cycle. E. O. Wilson (1975: 547–8) saw it as an example of 'extraordinary evolution'; menstruation, he wrote, has been intensified whilst the 'estrus, or period of female "heat", has been replaced by virtually continuous sexual activity. . .'. Primatologists Washburn and Hamburg (1972: 277) voiced what was until recently a consensus in noting that oestrus loss rendered human females 'quite different' from any other primate.

Strictly speaking, lack of oestrus is now seen as a feature shared by all the higher primates, in that sexual intercourse among them is not rigidly confined to the female's fertile moments. Nonetheless, the earlier writers were not entirely mistaken. In all primates, sexual motivation fluctuates cyclically, reaching a peak during ovulation, which may be announced with a public signal. With the exception of women, all primates also exhibit cyclical vulvular swellings of some kind (Dixson 1983). While in some species these are hardly noticeable, in others they are pronounced and/or accompanied by striking changes in sexual skin colouring, special scent emissions and so on (Zuckerman 1932; Rowell 1972; Dixson 1983). Such displays – which are related in an obvious way to the oestrus signals of other mammals – may be regarded as uncontrollable, involuntary 'yes' signals sent out by primate females at around the time when they are most likely to conceive. No mature male in the vicinity can resist the temptations of a female in such a state. In what follows, the term 'oestrus' will be used in a loose way to refer to this female condition which characterises many primates but which humans have completely lost.

Mating Systems and Ovarian Cyclicity in Primates

Chimpanzees ovulate for one day out of the thirty-six of their cycle, but prominently inflate the area around their genitals for about ten days beforehand. The female's sexual fervour intensifies as her swellings rise. As she approaches peak oestrus, she becomes, typically, surrounded by numerous males. In the early stages of full swelling, these take turns to copulate, one after the other, showing little aggression or competitiveness. Towards the moment of actual egg release, however, she becomes increasingly monopolised by a single dominant male who is likely to succeed in fertilising her (Goodall 1986: 450–1).

Throughout the remainder of her cycle, such a female does not arouse males or show much interest in sex (Graham 1981; Tutin and McGrew 1973; Tutin and McGinnis 1981; Goodall 1986: 443–87). Whereas oestrous females typically follow around after highly mobile males, anoestrous ones tend to stay behind in the company of other females, although they are often rather solitary (Wrangham 1979).

Among baboons, cycle lengths and other details differ, but there is the same tendency to reach a peak of excitability – 'gadfly madness', as Hrdy (1981: 156) puts it – as sexual swellings rise.

In any species, the female's sexual physiology is shaped by selection pressures specific to her usual mating system. This means that the modern reproductive physiology of womankind – which cannot have changed much over the past few tens of millennia – should tell us something about the mating system on the basis of which she evolved.

Female primates monopolised in stable one-male harems, such as gelada baboons, tend to evolve shorter periods of receptivity and less striking swellings than do chimpanzees and others adapted to multi-male systems (Dunbar 1988: 153; Dixson 1983). To understand why, primatologists have attempted to work out the costs and benefits of extended receptivity and/or pronounced swelling under different social conditions.

Flaunting one's attractions during oestrus may prompt males to grant special favours, but it also invites harassment from resentful same-sex companions, as happens among geladas (Dunbar 1980a). The risk can be minimised by trying to signal to the alpha male only on those days when other females are hormonally least interested in sex. We might say that if there is a shortage of males and nothing radical can be done about this, the females might as well ration themselves to avoid too much harassment from one another. Asynchrony, relatively modest signalling and receptive periods of only a few days – the gelada pattern – will tend to result.

On the other hand, it may not be that the shortage of males is an immutable fact of life. It may be possible to break out from the 'harem', or invite other males to come in. One-male monopolies over large harems tend to be enforceable only with difficulty. 'Illicit' matings with outsiders occur, and in the case of some species in some ecological contexts, these become the norm.

To the extent that a one-male monopoly proves untenable, the mating system which replaces it can in theory evolve towards (a) monogamous pairing, (b) polyandry or (c) a promiscuous 'multi-male' system. Monogamy is not found among terrestrial, savanna-dwelling primates (Hrdy 1981: 36), whilst polyandry is found only among tree-dwelling marmosets and tamarins (Goldizen 1987). For ground-living primates, the basic alternative to a 'one-male' harem system is a system of promiscuous 'multi-male' units.

Instead of competing to obtain once-for-all control over a whole group of females, males in multi-male contexts are faced with the fact that they can never permanently 'win' their females. There are various alternatives, including the formation of special long-term friendships with particular females, but in general fidelity is not to be expected and males have to compete more or less continuously for each copulation as and when a female comes into oestrus (Dunbar 1988: 176).

In this situation, since there is no shortage of males, females come under less mutual pressure to avoid reproductively unnecessary sex, so that there is less need for females to display their receptivity only briefly and in sequence. If sexual relations with multiple male partners enable females to gain added male support, receptivity will extend markedly beyond each female's fertile days.

In itself, this would not explain prominent oestrus displays. Indeed, it might be thought that where males were easy to get, there should be no need for females to enter into fiercely competitive sexual self-advertising. In primate multi-male contexts, however, while the greater availability of males initially reduces inter-female competition for them, the evolution of extended receptivity by the same token builds up the competitive pressure once more, even though this pressure now acts in a different way. When a number of females are receptive simultaneously in a multi-male context, they compete not for males as such – after all, any receptive female can get those – but for the most desirable males at the most crucial moments. In fact, as they approach ovulation, females in such situations compete vigorously for access to the most dominant males. Faced simultaneously with a number of 'yes'-signalling females, each such targeted male makes his choice on the basis of the surrounding females' explicit sexual 'advertising'. Females adapted to such pressures display, as Alexander and Noonan (1979: 446) put it, 'the most dramatic advertisements of sexual receptivity, the most obvious and intense sexual competitiveness, and . . . the most striking cases of receptivity outside the ovulation period'. It then becomes rather difficult for a male to distinguish true ovulation from what might be termed 'sham oestrus', but selection pressures naturally favour those males who are best at doing this.

The Puzzle of the Human Female

We have seen that non-arboreal, non-forest-dwelling primate mating systems are stretched out between two opposite conceptual poles – so-called 'one-male' harem systems on the one hand, 'multi-male' systems on the other. Concrete mating arrangements approximate towards one pole or the other, with females evolving the forms of their cycles and reproductive physiologies accordingly.

Now, the problem is that the human female seems to belong to neither pole; nor does she fit in at any point in between. She could not have evolved in a harem system – for that would have endowed her with strictly limited, cyclical, hormonally governed periods of receptivity. But neither could she have evolved in a 'multi-male' system based on promiscuity and female competition for insemination by dominant males. We can infer this because, lacking either oestrus or sham oestrus, the human female appears ill equipped to compete with her sisters in the requisite way.

The human female is in principle sexually receptive, regardless of fertility, throughout the whole of her cycle. Her interest in sex, despite possible slight peaks at ovulation and/or menstruation (Udry and Morris 1977; Adams *et al.* 1978), never becomes as overwhelming as it is for primates in peak receptivity and remains essentially unchanged. Conversely, she is equally able to refuse sex at any time: at no point during her cycle is she the slave of her hormonal state.

In fact, the human female does not signal 'yes' with her genitals at all.

Instead of being externally marked as a public display, ovulation has evolved in the reverse direction, to the point at which the moment of maximum fertility has become effectively concealed. In neither appearance nor behaviour is it possible to determine a human female's fertile period. Far from males in her presence being made publicly aware of her ovulation, the human female's special condition is kept so close a secret that unless she is unusually aware of her own physiology she will not even know the moment herself.

Theories of Oestrus Loss

In the human case, direct, blatant sexual competitiveness within a promiscuous mating arena has evidently been subject to strong negative selection. This is supported by two primatological observations. Firstly, monogamous primates, most notably gibbons, show extended receptivity and concealed ovulation, although not in so extreme a form as in the human case. Secondly, concealment combined with very extended receptivity characterises tamarins and marmosets, whose mating systems are versions of 'cooperative polyandry', each female consorting with more than one male partner (Goldizen 1987: 39–40). Neither monogamy nor polyandry necessarily implies interfemale solidarity; both, however, minimise situations in which females are in direct sexual competition for successive copulations with the same male. In searching for the type of mating system responsible for the human female condition, we can therefore rule out polygamy. We are drawn instead towards monogamy, polyandry – or some unsuspected pattern resting on inter-female solidarity on a level unknown among non-human primates.

The theme of a 'sex strike' was broached in Chapter 4; it is now time to consider its possible relevance. If female gender solidarity were increasingly being prioritised as a means of upholding collective sexual bargaining power, what kind of mating system and associated physiology would females evolve? Can we imagine an oestrus-governed female easily observing the discipline of a sex strike under pressure from her companions? Or would selection pressures in this context favour those females more liberated from their cycles – those better able to signal 'yes' or 'no' at any time, dependent not on hormones but on the requirements of inter-female political calculation and solidarity?

Although answers at this stage must remain tentative, merely to ask this question is to begin to glimpse the possibilities in a new way. As will be shown more fully towards the end of this chapter, the concept of gender solidarity allows us to begin to unravel some of the more difficult theoretical problems. Perhaps surprisingly, however, this solution has only recently been suspected. Meanwhile, numerous very different theories to explain the human female condition have been at the centre of scholarly debate.

Alexander and Noonan: the 'female deception' theory

Probably the most popular has been the 'female deception and self-deception' theory, which in its best-known form was put forward by the American sociobiologists Richard Alexander and Katherine Noonan (1979).

These authors set out from the crucial fact – as central to sociobiology as the class struggle is to Marxism – that females and males may have radically different reproductive interests. Typically, the female needs to get 'her' male to stay with her once she is pregnant; the male needs, on the contrary, to inseminate as many females as he can. In the course of hominid evolution, the argument runs, it was the female of the species who eventually won this battle:

> We suggest that concealment of ovulation evolved in humans because it enabled females to force desirable males into consort relationships long enough to reduce their likelihood of success in seeking other matings, and simultaneously raised the male's confidence of paternity by failing to inform other, potentially competing males of the timing of ovulation. (Alexander and Noonan 1979: 443)

The crucial idea here is that by losing their public signals, females succeeded in 'tricking' males, denying them any means of knowing when to impregnate them. For males, the only sure answer was to have sex with one and the same female throughout her cycle, whilst keeping all rivals away; nothing else could provide confidence in paternity.

The authors stress that had females betrayed the slightest sign of the moment of ovulation, their whole strategy would have collapsed – males would have abandoned their mates the moment they felt sure they had fertilised them. But it is difficult, they continue, to keep something from your partner if you are conscious of it yourself. The best deceivers are the self-deceived. Consequently, the evolving human female had to suppress even her own awareness of the vital moment. 'Concealed ovulation', the authors conclude, 'we view as a particularly powerful and instructive case of deception of others, linked with self-deception and made more effective by it.'

This theory is an ingenious one and, as will be seen, needs only to be enhanced in the context of a rather different model of early human mating-systems (Turke 1984) to appear very convincing. Before noting some criticisms, we may go on to consider another version of the same theory.

Stoddart: odours and the sexual division of labour

D. Michael Stoddart (1986) of the University of Tasmania links vaginal odours and their reduction with the evolution of hunting. He suggests that extended hunting trips combined with 'gregariousness' brought problems for the originally monogamous protohuman couple – particularly since, in the early stages, the would-be faithful female was unwittingly giving off sexually irresistible odours to all about her each time she ovulated. The problem was

> that if some males left the home camp on hunting trips which might last for several days at a stretch . . . bonded females remaining at the home camp would not infrequently produce odorous ovulation advertising signals while their mates were absent and while other males were present as guards.

As the nearby stay-at-home males ('guards') were aroused by such odours, sex would unavoidably have occurred whilst the dutiful but unsuspecting hunters were absent. This (according to Stoddart) would eventually have undermined the hunter males' confidence in paternity, discouraging them from investing care in their partners' offspring. To avoid this outcome, females had to stop involuntarily soliciting sex at the wrong times. Olfactory sexual signals consequently had to be suppressed.

It will be noted that Stoddart's model assumes a kind of 'sex strike' hypothesis, to use the terminology of Chapter 4. Females have to build up the sexual confidence of hunters by, in effect, 'promising' not to have sex with anyone else while they are away. The assumption is that ovulatory odours were inhibited to enable females to do this.

Benshoof and Thornhill: the 'cuckoldry' theory

But there have been criticisms of the notion that oestrus loss evolved to confer certainty of paternity. Responding not to Stoddart but to earlier versions of the oestrus-loss-sustains-monogamy theory, Benshoof and Thornhill (1979) contest the notion that concealed ovulation could have helped at all in increasing a male's confidence in his paternity. On the contrary, they say, the early human male's ignorance of his mate's condition would have caused intense problems. Each male would have needed to guard his female against covetous neighbours, never knowing whether he should join the hunting party and risk being cuckolded or stay at home on the off-chance that this would be her fertile week.

No system, these authors argue, could be more poorly designed to guarantee paternity than the system of concealed ovulation. The system means that no male has any idea when his partner might conceive. Taken in isolation, this actually *decreases* any male's ability to link his intercourse with the pregnancy of his partner. Indeed, the whole point of Alexander and Noonan's argument is that this is so: it is said that *because* of the newly accentuated male paternity uncertainty, counter-measures – in particular, the maintenance of round-the-clock vigilance over the female sexual partner – were required. But if paternity assurance were the deciding factor and monogamy the consequent mating system, humans would surely not have created such problems in the first place. They would have evolved the pattern common to other monogamous primates: a short, well-defined oestrus with very little advertisement of the fact. Under this system, the female's monogamous partner knows perfectly well when she is in heat, whereas rival males are kept ignorant because they are kept at a distance and the signals are not sufficiently public. Then the female's mate need only guard her for the few vital days; after that, he can go hunting secure in the knowledge that however unfaithful she might be, he will be the father of any baby she has (Shaw and Darling 1985: 82–3, citing Benshoof and Thornhill 1979). Again, it will be noted that this is a kind of 'sex strike' hypothesis, at least in the sense that the female does not engage in relevant or genetically threatening sex whilst her hunting partner is away.

Benshoof and Thornhill argue that this initially happened: protowomen were in a limited sense 'monogamous' in that, despite their many possible 'affairs', they had *fertile* sex only when their chosen partner was at home. Reducing their public sexual signalling to ward off unwanted males, they signalled just sufficiently to let the favoured partner know the correct moment to inseminate.

The authors acknowledge that this would still not explain the actual human condition which we find – complete oestrus loss, in which even the favoured partner has little if any idea when his partner is ovulating. To explain this 'later' development, the two researchers follow a complex course, arguing that when couples began living in large social groups (it is assumed that they did not do this before), the females found themselves surrounded by a wide choice of males. It then became in their interests to deceive their partners, getting impregnated by males who were in genetic terms the 'best', regardless of whether these gave help in provisioning or child-care. So human females had sex with their 'faithful' partners for most of the cycle, but during ovulation sneaked off to get pregnant by the best obtainable mate. Concealed ovulation made this possible, since the 'faithful' partner had no way of knowing that his sexual intercourse was not fertile; assuming the offspring to be his, he provided the support and child-care that the mother required. This has been termed the 'cuckoldry' theory of oestrus loss (Shaw and Darling 1985: 84).

The fatal flaw in all this, however, is that women would not have known of the correct moment to 'sneak off' and get themselves pregnant by an illicit lover. Despite all their good intentions, they would have kept getting pregnant accidentally by their faithful partner, with whom they spent so much time. 'Although some women think they can tell when they are ovulating', as Shaw and Darling (1985: 84) put it, 'the vast majority most decidedly cannot, and even with our current technological ability to measure basal body temperature and to sample and categorise cervical mucus, the time of ovulation is notoriously difficult to pinpoint'. Benshoof and Thornhill try to fall back on the 'self-deception' argument, but it is difficult to understand how women could have cuckolded their partners at ovulation without knowing it – impelled by some inner hormonal force of which they were unaware. 'Surely', as Shaw and Darling (1985: 84) have written, Benshoof and Thornhill's proposed system 'would function infinitely better if the female herself knew when an egg was ready and confined her *affaires de coeur* only to that time'.

Nancy Burley: the anti-birth-control theory

Starting out from the fact that the moment of egg release is concealed from women themselves, yet another frequently cited hypothesis holds that such concealment was necessary – to stop culturally motivated women avoiding motherhood completely. Given half a chance, argues Nancy Burley (1979), women, who for cultural reasons are in a unique position to understand in advance the pain and dangers of childbirth and the burdens of child-care, would simply never get pregnant. If they knew when they ovulated, they would simply avoid sex at that time. Concealed ovulation has evolved to prevent women from practising such a disastrous form of birth control.

Burley cites much evidence that abortion/infanticide in traditional cultures must be practised by women secretly, husbands being overwhelmingly unsympathetic. But – even assuming earliest cultures knew of the connection between sex and pregnancy, which is to say the least uncertain (Montagu 1974) – is it true that, left to themselves, culturally aware women would avoid motherhood completely? Surely not. Birth control as a positive measure to improve parenting quality would come far closer to most women's genetic interests *and* cultural ideals than the desire to escape motherhood altogether – certainly in pre-industrial cultural contexts, and surely in our own as well. And if this is taken to be the case, ovulation concealment becomes a mystery once more. For women's self-knowledge of the moment of ovulation would be an immense benefit in terms of conscious fertility control. As Burley (1979: 841) herself acknowledges, the woman who was aware of her cycle would seem to possess all kinds of advantages in comparison with her more ignorant sister. She would be more free to have sexual affairs with multiple lovers, without having to worry about pregnancy at an inopportune time. But on the other hand, she could also get pregnant more quickly when it suited her. In other words, she could consciously organise optimal spacing between births, without having to resort to the extraordinarily wasteful and emotionally harrowing techniques of abortion and infanticide.

In this light, it remains difficult to understand why ovulation should have become concealed in the human case to so remarkably complete an extent. Concealment is a form of ignorance. From whichever standpoint it is examined, this particular form of ignorance would seem at first sight to be a handicap imposed upon both sexes rather than an asset.

Menstruation

Despite oestrus loss, hormonally controlled sexual signals are *not* entirely missing from the human female menstrual cycle. On the contrary, menstruation in the human case has been *accentuated* as an external display. It is at menstruation rather than ovulation that the human female experiences her behaviour as hormonally influenced to a certain degree. Although this is not unique among primates – rhesus monkeys display behavioural changes mainly around menstruation (Rowell 1963) – it remains an unusual phenomenon, which any full theory must explain.

A woman loses considerably more blood during menstruation than does any other primate. This shedding of blood, although small, represents a significant loss – a loss which has to be made good by additional food intake, particularly of iron. The adaptive advantage of this has not yet been explained.

Although there is no biological imperative to avoid sex during this period, in traditional human cultural contexts, menstruation in fact signals 'no' (Chapter 11). It would seem that in proportion as she *lost* her long periods of hormonally determined *non*-receptivity, the evolving human female was obliged to compensate with some other powerful means of signalling 'no'. Among contemporary hunter-gathers, menstrual taboos are particularly intense in northerly latitudes, where meat dependence tends to be heaviest (Kitahara 1982). In this context, Stoddart's (1986) theory that a loss of ovulation odours underpinned the sexual division of labour finds it complement in the mirror-image theory that an *accentuation* of negatively interpreted *menstrual* odours can achieve the same result, keeping the sexes apart during those periods when men need to concentrate on the hunt (Dobkin de Rios 1976; Dobkin de Rios and Hayden 1985; Testart 1985, 1986). In other words, the elimination of oestrus odours and accentuation of menstrual odours may be head and tail of the same coin. The bearing of all this on the concept of a culture-generating monthly periodic 'sex strike' will be explored in Chapters 9 to 11.

In western contexts, of course, menstrual taboos have to an extent been relaxed, but compensatory constructs such as 'premenstrual syndrome' (Dalton 1977, 1979; Lever 1981) may lead to not substantially different results. At least one feminist strand of thought (Martin 1988) insists that the 'once-a-month-witches' (Donelson and Gullahorn 1977) who strain their marriages under the banner of this syndrome are not sick – merely intolerant of marital or other stress which may seem more acceptable at other times. In the premenstrual period, in other words, women are less able to tolerate society's and their partners' pressures and demands. There is an enhanced bodily self-awareness, a lower tolerance threshold – and so a stronger tendency to rebel. Western culture-specific constructs such as 'PMS' may on this analysis represent attempts to come to terms with such realities, given that traditional menstrual taboos no longer perform that protective, gender-segregating function.

There are doubtless some counterparts to all this among primates. But with the exception of rhesus monkeys (Manson 1986: 26), primate females give an impression of being behaviourally governed by hormones not so much at menstruation as at the opposite point – during and around ovulation. As Shaw and Darling (1985: 58) put it:

> although monkeys menstruate, it is an insignificant event, overshadowed by a more important event taking place at a different time during the cycle – the periodic sexual heat, which happens at the time of ovulation.

When she is not due to ovulate, the primate female may simply withdraw from sex or from consortship with males, without having to struggle against expectations to the contrary. We might say that she does not have to struggle to assert her periodic 'right to strike' – because her physiological periodic anoestrus does it for her.

For reasons which have yet to be explained, in any event, the evolving human female concealed ovulation and its associated odours almost completely, extended her receptivity as never before, and accentuated both the odours and visible manifestations of the menstrual flow. Taking these features together, the human configuration appears not just different from the usual primate pattern, but its inverse. *Whereas the basic primate pattern is to deliver a periodic 'yes' signal against a background of continuous sexual 'no', humans emit a periodic 'no' signal against a background of continuous 'yes'.* This reversal indicates something of the nature and scale of the sexual revolution central to the process of becoming human.

Synchrony: Seasonal and Menstrual

Among mammals generally, breeding seasonality is a form of reproductive synchrony which has long been recognised. In this case, it is the sun which is the ultimate determinant, producing the seasonal cycle and with it a definite schedule of resource availability. By contrast, menstrual synchrony implies, etymologically at least, a link with the moon.

Primates do not show the strict breeding seasonality of most other mammals. Nonetheless, all primate reproductive cycles vary seasonally because they are sensitive to light and other environmental cues. Among macaques, geladas and patas monkeys, for example, the onset of reproductive activity is triggered by a seasonal flush of vegetation produced by rain (Dunbar 1988: 65, citing Fa 1986; Dunbar 1980b; Rowell and Richards 1979). The reproductive cycles of gibbons are phase-locked with seasonal variations in available fruits (Dunbar 1988: 281, citing Chivers and Raemakers 1980).

When births all occur at a definite time of year, it is because giving birth at this time is optimal for the mothers concerned. Birthing at the commencement of the lean season – when the offspring would certainly die – would be a waste of time and energy. The restricted time window for giving birth then presupposes an earlier restricted season for fertile matings – and hence for roughly synchronised ovulations at that time.

In many cases, however, reproductive synchrony can be an effect less of environmental than of social factors. In such cases, births may show an even annual distribution for the population as a whole, yet marked synchrony within particular local groups (Dunbar 1988: 65). Among patas monkeys (Rowell 1978) and geladas (Dunbar 1980b), the cause is often the sudden appearance of a new male who takes over a harem, prompting the females to come quickly into oestrus together. Another factor may involve lactation. If a group of breast-feeding langurs (Hrdy 1977) or yellow baboons (Altmann *et al.* 1978) all wean their infants at the same time, then they will soon resume cycling together. Wallis (1985, citing Goodall 1983) notes that wild female chimpanzees at Gombe resume postpartum cycles near the end of the dry season – which corresponds to the period during which the peak number of oestrous swellings is detected. She suggests that after weaning their infants, females schedule their first oestrous swellings using cues from other females, who may not have been mothers in the recent period. This is interesting because it shows how cycling and non-cycling, pregnant and non-pregnant females can in the long run become synchronised to the same rhythm.

Macaques are seasonal breeders regardless of the habitat occupied, whilst monkeys such as langurs and howlers emphasise or minimise seasonality according to local conditions (Rudran 1973, Jones 1985). The seasonal transition from a non-reproductive to a reproductive condition can result in synchronised matings followed by a batch of synchronised births. Even in non-seasonal breeders, synchronised cycles can often be inferred by keeping a tally of births from year to year. If females tend to give birth once every two years, any synchrony between their cycles will lead to one year with very few births, one year with numerous births, the next with few again, and so on. Evidence for such year-to-year birth oscillations suggests that females in many non-seasonal species locally synchronise for social or other reasons (Dunbar 1988: 65).

Humans are even less seasonal than other primates. Yet we are certainly not immune to traces of reproductive seasonality. Finnish birth records show a peak of conceptions and of twins, triplets etc. in summer and a trough in winter (Daly and Wilson 1983: 339, citing Timonen and Carpen 1968). The higher the latitude – and hence the greater the contrast between summer and winter day lengths – the greater the effect. 'The obvious conclusion', Daly and Wilson (1983: 339) comment, 'is that light exposure has at least some influence on reproductive function in our own species'.

We may now turn from primate breeding seasonality – whose condition is only that births are restricted to within a few weeks of any year – to the stricter phase-locking effect of menstrual synchrony, which implies the synchronisation of menstrual onsets accurately to within days.

Little is known of menstrual synchrony in primates, but it seems to be rare – partly because many primates in the wild cycle only for a brief period every few years before getting pregnant again, and so fail to experience the necessary series of successive menstrual onsets. Among chimpanzees, the potential for synchrony is evidently present: an experimental study found significantly synchronised oestrus onsets among female chimpanzees caged together and spending social time with one another (Wallis 1985). But in the wild, adult female chimpanzees tend to forage in isolation, and so synchrony stemming from close association would not be expected to occur even among unmated females.

Within any group of primate females, the way their reproductive and/or menstrual cycles relate to one another may be a sensitive barometer of power relations between the animals. If one female is dominant over another and in conflict with her, a phenomenon known as 'suppression' tends to occur. Among captive marmosets, for example, when a dominant female ovulates, this itself seems to inhibit the ovulation of her female subordinates (Hrdy 1981: 44, citing Hearne). Far from the ovulation of one female attracting or helping to trigger that of companions, in other words, one female's ovulation occurs at her rival's expense.

It is not known what would happen if a subordinate female in such a relationship did begin to breed. Hrdy asks:

> Would the dominant female drive her away? Or perhaps murder the subordinate's offspring, as wild dog and chimpanzee females are known to do? In either of these events, it would behove the subordinate to defer reproduction – which, after all, is a costly and risky enterprise – until she has a territory of her own. (1981: 44)

Among savanna baboons, geladas and many other monkeys, the presence of a dominant female may induce delays in maturation, inhibition of ovulation or even spontaneous abortion by subordinates (Hrdy 1981: 99, 106). Dunbar (1988: 69) comments that even very low rates of physical aggression can induce reproductive suppression; the subordinate's *perceived* self-status within the group may be enough to produce the effect, providing it is reinforced by attacks at least occasionally. There is probably no better example of just how subtle and complex competition between females can be, Hrdy notes, than the effects of one animal upon the ovulatory cycle of another.

Menstrual Synchrony in Humans

Since the 1970s, medical science has begun to acknowledge what countless women must already have known for generations – that when women who are friends associate closely with one another, their cycles begin to synchronise. Remarkably, in contemporary western cultures, most of the male sex even today remains unaware of this potentiality, despite its being 'common knowledge' (Kiltie 1982, citing McClintock 1971, Weideger 1976) among women themselves.

The effect was first scientifically documented in 1971, in a paper in *Nature* by Martha McClintock (1971). Having noted that social grouping can influence the balance of the endocrine system, she went on:

> Menstrual synchrony is often reported by all-female living groups and by mothers, daughters and sisters who are living together. For example, the distribution of onsets of seven female lifeguards was scattered at the beginning of the summer, but after 3 months spent together the onset of all seven cycles fell within a 4 day period.

McClintock herself worked with 135 young residents of an American women's college. Each was asked to record the onset of her periods. At the start of term, new entrants were cycling on different schedules, whereas by the end of the year, friends' and roommates' onsets were occurring within a few days of one another, friendship rather than proximity being apparently the most important factor.

Comparable findings were soon confirmed (Quadagno *et al.* 1981; Skandhan *et al.* 1979; Graham and McGrew 1980). Admittedly, a study by Laura Jarett (1984) of 144 mainly Catholic subjects at two all-women colleges produced less clear-cut results: 86 women did not significantly synchronise with their roommates. But here the mean cycle length of the total sample was 35 days, probably because of the women's Catholic-regulated, relatively sex-negative, all-female environment. Studies have shown that networks of female friends who also enjoy regular intercourse with men have shorter, more regular cycles and consequently synchronise more easily than women who have female friends but are celibate (McClintock 1971; Cutler *et al.* 1979a).

In seeking to isolate the olfactory or other communicative mechanisms responsible for menstrual synchrony, one research team (Russell *et al.* 1980) took sweat from the armpits of a volunteer who had discovered that she could 'drive' a friend's cycle into correspondence with her own. Eight women were exposed nasally over four months to a solution containing this volunteer's sweat, while a control group were given a neutral solution. Those in the control showed no change, whereas four of the five women in the experimental group were soon beginning their periods within a day of the donor's.

Unfortunately, the sample used here was small. Moreover, the technician applying the sweat solutions was herself the volunteer who had produced the sweat, opening up the possibility that something else about her presence was influencing the subjects (Doty 1981). Others attempted to repeat the experiment under more rigorous conditions. Conclusive positive results were at first claimed (Preti *et al.* 1986), but the statistical treatment of the subjects' menstrual calendars was in turn devastatingly criticised (Wilson 1987), leaving our understanding in a state of some confusion. We know that menstrual synchrony occurs. The mechanisms causing it and its possible functions are not clear.

We have seen that amongst competing female primates in dominance/ subordinance relationships, cycles may clash with and suppress one another rather than synchronise. This has general theoretical implications.

In discussing menstrual synchrony amongst humans, Richard Kiltie (1982), a zoologist from the University of Florida, asks us to consider the case of a group of co-wives in a polygamous household unit. Even if several of these females were close friends and synchronised, he writes, they would always be at the mercy of any female prepared to 'cheat' by ovulating and soliciting sex during her companions' non-fertile period. The 'cheat', on this analysis, would have a clear field; whenever her companions were sexually unavailable, she would be signalling her receptivity. At such times – as the only female available out of the whole harem – her chances of extended relations with the polygamous male would be unfairly high. Given the usual Darwinian genetic assumptions, it is difficult to see how evolution could avoid selecting for 'cheating' in this context. Synchrony could never establish itself.

Kiltie's (1982) view is therefore that human menstrual synchrony may be 'an evolutionary vestige' of something which had significance at some earlier stage in evolution – perhaps when hominid females bred more seasonally. Alternatively, he suggests, synchrony could be a trait that never had an adaptive effect in its own right, being merely a side-effect of some other adaptation. Certain physiological cycles of men – for example, cycles in basal body temperature and hormone levels – tend to become synchronised with the menstrual cycles of their wives (Kiltie 1982: 417, citing Doehring *et al.* 1975; Persky *et al.* 1977; Henderson 1976; Vollman 1977). When two men cohabit, their cycles also tend to synchronise (Henderson 1976).

Kiltie (1982) suggests that like such phenomena, the potentiality for menstrual synchrony is probably 'epiphenomenal', being of little adaptive significance. In support of this, he observes that hunter-gatherer women may only rarely experience a sufficient number of successive menstrual onsets for synchrony to become established, since like other mammals including non-human primates, they tend to spend most of their reproductive lives either pregnant or breast-feeding (Short 1976). Kiltie acknowledges, however, that synchrony ought to be strong within groups of closely associated adolescent females, who experience frequent anovulatory cycles and hence continuous menstrual cycles for several years following first menstruation (Nag 1962; Short 1976).

In any group of humans, normal cycle length can vary quite widely, ranging from about 21 to 40 days. This variation is confined to the preovulatory (follicular) phase; some women take scarcely a week to build up a uterine lining, while others may take up to three weeks. It is this part of the cycle which is sensitive to outside influences. Stresses and emotional disturbances can bring forward or delay ovulation to a marked degree; extreme and sustained anxiety can delay or suppress ovulation indefinitely. In contrast, the postovulatory (luteal) phase takes two weeks in almost all women. Once ovulation has occurred, in other words, menstruation is likely to occur two weeks later, whatever the emotional situation (Bailey and Marshall 1970; Presser 1974; Vollman 1977). It seems, then, that synchrony is at the most basic level not menstrual but ovulatory synchrony (McClintock 1978). This means (to anticipate) that if emotion-structuring cultural rituals were to have an effect in inducing continuous menstrual synchrony, they would have to act first and foremost upon the preovulatory phase.

Despite individual variation, repeated statistical studies consistently show that the average human female menstrual cycle length is 29.5 days (Gunn *et al.* 1937; McClintock 1971; Vollman 1977; Cutler *et al.* 1980). The average duration of pregnancy is 265.78 or 265.79 days, counting from conception to birth (Menaker and Menaker 1959). As Menaker and Menaker (1959) point out, this is nine times the menstrual cycle length (9 multiplied by 29.5 gives 265.5).

The fact that women have a 29.5-day average menstrual cycle length and a precisely ninefold gestation length suggests that generalised reproductive synchrony – a single rhythm involving all women equally, regardless of whether they are pregnant, lactating or cycling – may have been adaptive at some evolutionary stage. Not only arithmetic but astronomy seems supportive in this connection: there are unmistakable suggestions of a correspondence between human reproductive periodicity in general and the 29.5-day cycle length of the moon (see Chapters 7 and 10). No other primate shows so close a correlation between menstrual cycle length and the lunar month, nor between any whole-number multiple of the menstrual rhythm and the length of gestation.

What is certain is that human females, no less than other primates, are sensitive to time and possess within their bodies the means to schedule ovulation, conception, birth and other reproductive events quite accurately. Where it pays her to do so, the human female can to a significant degree shape the profile of her reproductive life to a pattern communicated through a variety of external cues, including those provided by neighbouring females of her own species.

Human Origins, Concealment and Synchrony

With all this in mind, we are now in a position to examine a final theory of human origins which was put forward in the 1980s. It has been left to the present chapter because it converges closely with the solution which this book will propose.

Paul Turke (1984) 'Effects of ovulatory concealment and synchrony on protohominid mating systems and parental roles.'

Turke is a sociobiologist at the University of Michigan, and has collaborated with Kim Hill and others in extending sociobiological methodologies into the traditional terrain of social anthropology (Betzig *et al.* 1988).

In working towards a new synthesis, Turke draws on the findings of Hrdy, Lovejoy, Hill and others (Chapter 5). His starting point is the observation that human males provide far greater provisioning services for their mates than do other primate males. He assumes that early females must have played a role in establishing this situation, and sets out to investigate how they did it. By what means, in other words, did evolving human females succeed in making males 'earn their keep'?

In proposing an answer, Turke sets out to explain the following features of human female reproductive physiology:

(a) concealed ovulation
(b) continuous but discriminating sexual receptivity
(c) the potentiality for menstrual synchrony.

The author takes it that 'the earliest protohominids' (wisely, no dates are suggested) lived in chimpanzee-like multi-male troops, the females advertising ovulation and receptivity with conspicuous swellings. The females did not synchronise their cycles. Sex relations were promiscuous, with perhaps occasional temporarily exclusive consortships. General promiscuity meant that males had low confidence in paternity, and did not invest much in their offspring.

This situation began to change in a human-like direction as scrub and grasslands encroached on these creatures' former forest habitat. Increasingly, predators posed a danger, individuals sought safety in numbers, groups became large, competition between neighbouring groups intensified and foraging strategies became more complex.

It will be remembered from Chapter 4 that in this situation, Dunbar (1988) envisages the formation of female coalitions, these becoming the stable core of early hominid social groups as they moved into open terrain. This suggestion dovetails closely with Turke's model, which assumes that adult reproductively active females – those who were not already pregnant or breast-feeding – became sufficiently close to one another for their cycles to begin to synchronise.

Turke assumes increased infant dependency, slower maturation and substantially increased burdens on mothers. In this context, he asks: How did females compel males to provide more child-care help? His answer is that females began to extend their oestrus signals, so that the signals from one female would be more likely to overlap with those of her sisters. They simultaneously dampened down the gaudiness of their signals, reducing the distinction in appearance between one part of the cycle and the rest. Finally, they began to synchronise their ovulatory cycles within each local unit.

In explaining how all this would have strengthened females in their efforts to secure extra help from males, Turke asks us to imagine two young females who have just begun cycling. Slight genetic variation has rendered the oestrus signals of one more dampened and more extended than those of her companion.

During each period of receptivity, Turke points out, the more gaudily displaying female should attract the more high-ranking male (dominant males, under the old system, monopolise the females most obviously ovulating). The more modest female should obtain a more lowly male partner. However, he would be more likely than others to give his mate his continuous time. This would be partly because his lower status would restrict his mating opportunities elsewhere, partly because the longer duration of his partner's receptive period would force him to stay with her longer – and partly because she herself would be less likely to entice high-ranking males to come in and disrupt their togetherness. As a result, the lowly male would have a higher than average confidence of paternity in any resulting offspring, making it particularly worth his while, genetically, to invest his parental care in them.

Turke proceeds to ask what would happen if this modest female also began tracking her more provocative sister's hormonal status, synchronising her periods of ovulation with hers. Using modern human females as a reference, he suggests that a statistically significant degree of synchrony would exist after four cycles, the process of convergence beginning after just one cycle. Suppose that at the start of the process, one female ovulated when the other was menstruating. This would mean that the lowly male became threatened at precisely the moment his partner was ovulating, for the other female in the system would be in a relatively unattractive condition, tempting 'her' male to look elsewhere for a mating opportunity. Now suppose that the two females began to synchronise, their moments of ovulation converging more and more closely. As synchrony developed with each cycle, Turke argues, the modest female's male partner would find the sexual competition at the moment of ovulation less intense. This would be simply because at the crucial moment, each female, simultaneously with her female companion, would draw to herself a male who might otherwise be on the lookout for a mating opportunity elsewhere. Each male's prospects of mating with 'the other' female at the moment of ovulation would by the same token be reduced (Turke 1984; citing Knowlton 1979).

Turke (1984) suggests that female hamadryas baboons, who synchronise markedly within local units, exemplify this logic to a certain extent. They in effect co-operate in order to prevent harem owners from monopolising more than about two females per harem. It is as if monogamy were a desirable ideal, but that having only one other female in one's harem were the next best thing. Synchrony in this context helps keep the alpha male under some control. Predictably, Turke notes, hamadryas dominant males rarely attempt to philander with intratroop females, harems are stable and uniformly small, and the protection and care males afford to their mates and mates' offspring is substantial and well documented. In other words, Turke sees hamadryas baboons as taking some steps along the road hypothetically travelled by female protohominids in the course of becoming human.

Turke's conclusion is that it would be in the interests of any overburdened protohuman female to gain a male for herself and extract maximum help from him by synchronising as far as possible with her female companions. If females did this, then the old male strategy of competing to maximise the number of females inseminated would be thwarted, while higher than average confidence of paternity would add to the rewards of male investment in existing offspring (Turke 1984; Knowlton 1979). If the environmental/ecological conditions envisaged – movement into open territory, greater predation danger and so on – were intensifying group life and putting a premium on parental care, then the result should be a spread of ovulatory synchrony and concealment through the population (Turke 1984: 36).

Although evolution towards one-to-one relatively stable coupling is envisaged, Turke's model differs from Lovejoy's in that females are closely associated with one another and act together in constraining their male partners to behave appropriately. Turke sees this as involving a reversal of hominid female sexual preferences: females who synchronise begin to seek out lower-ranking males. They bring previously excluded males into the system, where they become of value in assisting with child-care and provisioning. This implies a political process is which the status of dominant polygamous males is subverted in favour of previously lower-ranking males more likely to meet changing female requirements.

We can now fit the jigsaw-piece supplied by Turke alongside a complementary item encountered early on in this chapter. We noted then that if a large group of females are condemned to share only one male between them, then they should lessen the costs of competition by avoiding one another's sexual space. They should sharply demarcate their time into clearly segmented portions, using unmistakable but brief oestrus signals for this purpose. Then each female could carefully avoid impinging on the sexual time of her sisters, each one's ovulation perhaps suppressing that of her sisters. Maximised asynchrony would of course be the result. Turke's argument represents the opposite side of the coin: if the females in a harem can break out and gain access to a male each, then they should logically lessen internal competition and tie their mates more continuously to themselves precisely by blurring all cyclical distinctions to the maximum possible extent, whilst simultaneously synchronising with one another. Such female oneness across time and space would confront philandering males with an all-or-nothing choice, preventing them from picking and choosing between different females or between the fertile and infertile moments of any one female.

It is worth adding that human females' permanently enlarged breasts – unlike the small, wrinkled ones of chimpanzees which enlarge only specifically for lactation – can perhaps be regarded as adaptive in the same context. Human breasts develop at an early stage in a young woman's life, often long before she actually begins lactating. Thereafter they remain full-looking at all times – giving little clue as to real reproductive status. They are therefore in a sense 'egalitarian'. They help to make pregnant, lactating and cycling females all seem equally maternal, or alternatively, equally sexually inviting. They would therefore have helped females to maintain synchrony in the emission of sexual signals.

In fact, enlarged breasts can be regarded as sending out a 'no' signal, at least on one level. Enlarged breasts mimic lactation. To that extent, the signal emitted is that the female concerned is anovulatory. Yet in humans – and humans alone – this signal (like ovulation concealment) has become confusing and deceptive. With her large breasts, a female who is really fertile in effect signals *as if she were breast-feeding and therefore unlikely to conceive*. A primate or protohuman male interested only in achieving immediate fertilisation ought to be discouraged by this (although, of course, a male with a longer-term parental interest in supporting this particular female and her offspring need not be). The ability to emit precisely such a highly selective 'no' signal would be very much in the interests of females who needed to persuade certain types of males to leave them sexually alone (Ffitch 1987). And the pattern would fit in neatly with Turke's ovarian synchrony model. Like ovulation concealment and ovarian synchrony, in other words, lactation-mimicking breast enlargement can be understood as an adaptation through which females prevent males from picking and choosing between them on a short-term basis in accordance with varying prospects for fertile intercourse. Although there are probably additional reasons for breast enlargement in non-lactating human females, possibly involving the storage of body fat, this extension of Turke's model may help us to understand what has long been regarded as an extremely puzzling problem (Caro 1987).

Turke's theory rests on no palaeontological or other direct evidence for reproductive synchrony in hominid evolution. The hypothesis is not buttressed with findings from archaeology or from the study of contemporary hunter-gatherers. The various archaeologically testable staples of other theories of human origins – hunting, tool use, the home base, a sexual division of labour – are not mentioned. Synchrony is inferred, rather, on purely theoretical grounds, drawing on studies of (a) shrimps (Knowlton

1979), (b) non-human primates and (c) women in contemporary western college dormitories.

The value of Turke's model is, however, that it introduces a new concept. Alone among the models discussed so far, it implies a form of inter-female solidarity ('reciprocal altruism' to use the sociobiological term) as a factor contributing to evolution in a human-like direction. We can now see that females who blur/extend their signals competitively (the chimp pattern), or who sharpen signal definition and restrict receptivity to avoid harassment (geladas), are evolving in wholly non-human directions. To this we might add that those whose monogamy is premised on the complete spatial isolation of females from one another – the gibbon pattern – are if anything still further removed from the evolving human norm. The human revolution was pioneered by females who combined pair-bonding with intensified gender solidarity, so that sexual attachments were not at the expense of wider forms of connectedness. The influx of males, extended receptivity, the added time available for love-making and the consequent reduced competition meant that associated females could develop relations of sisterly trust, achieving harmony not through sexual self-restriction or mutual avoidance but through reciprocally upholding one another's sexual success.

In Turke's model, females do not exactly go on 'sex strike' together. But ovulation concealment implies the same logic at a deeper level. What is collectively withdrawn from males is not sex as such, but knowledge of which female is ovulating when. Females use synchrony to provide leverage against dominant polygamous males, involving all available males in providing parental care. By not competing against one another in emitting sexual 'yes' signals, females in each group are in effect refraining from undercutting one another in sexual competition for each other's males. We might at this point adapt Marxist terminology and view the females as expressing a form of 'class solidarity' (although of course 'gender solidarity' is the correct term). Each female is asserting her own reproductive interests, but she is doing so in ways which simultaneously serve the interests of her sisters.

Conclusion: Synchrony and Revolution

If evolving human females at a crucial stage synchronised their cycles in the manner Turke supposes, this must have been within the context of powerful inter-female coalitions, the conditions of which were touched on in Chapter 4.

Robin Dunbar's (1988: 319–20) view is that as the ancestral hominids emerged from their former forest environment into more open territory, the females would have tended to cluster together in larger groups, seeking safety in numbers. Within the female groups, coalitions would have been formed, as subordinate females suffering from harassment attempted to find allies. At first glance, as Dunbar points out, it might seem that all this could have enabled dominant males to monopolise quite large groups of females.

But for all species in which harems occur, there is a limit to the harem size a male can manage. In large harems, rebellions tend to break out, the most subordinate females becoming dissatisfied first. 'As harem size increases', in Dunbar's words, 'more and more of the females begin to suffer from reproductive suppression and are consequently more willing to desert their harem male' (1988: 167).

It is not just the number of females in any harem that matters, Dunbar notes, but their cyclical state. A male might be able to manage, say, five females – provided never more than two came into oestrus at any one time. Should all five start displaying their receptivity together, their demands would swamp him and he would risk losing them to rival males. In this context there is always a threshold number of *receptive* harem females beyond which an alpha male will be unable to cope. As Dunbar puts it: 'Once the probability of co-cycling females rises above this critical threshold, the harem-holder will be unable to prevent other males entering his group and mating with his females' (1988: 141). So by synchronising, the females in a harem could theoretically organise a 'revolution', enticing in new males and thereby propelling themselves into a different social order – a different mating system – for a while.

Among various primates, such revolutions have actually been observed. In the case of two well-studied groups of redtail monkeys and blue monkeys, for example, 'there were rare occasions when up to six females were cycling together. During these periods, the harem-holder was unable to prevent other males from joining the group and mating with the females.' Once the females had ceased cycling, however, the extra males left the group, which reverted once more to being a conventional one-male harem (Dunbar 1988: 141, citing Cords 1984, Tsingalia and Rowell 1984). It is therefore no surprise to find that among primates generally, multi-male groups tend to be found where reproductive synchrony is high – for example, where there is a short breeding season each year – whereas one-male units tend to occur in year-round intermittent breeders (Ridley 1986).

In this light, we can discern two strategic options open to females in a harem system which they experience as restrictive. One is to accept the system. This may be the best course if ecological constraints make attachment to a single polygamous male an optimal solution to problems of foraging, communal defence and so on within the environment. If there is no long-term viable way of breaking out or of bringing other males in, then the dominant male's position is secure, in which case inter-female competition for his favours or services is imposed on all inescapably. In that event, Kiltie's (1982) arguments against the possibility of establishing synchrony would apply. To synchronise would be misguided, since even if a few co-operative females managed to entrain their cycles, any female wishing to get ahead in life would simply cheat, ovulating out of step so an to gain as unfair advantage over her companions.

The alternative, which certain ecological conditions might favour, would be to resist the whole system and change it – either by setting a lower limit on harem size, or by making even the smallest kind of harem ungovernable. The latter solution, of course, is the more radical. If the harem-holder is frail or vulnerable, and if previously excluded males can be reached at his expense, then dissatisfied females might synchronise as part of a break-out plan, the aim being to link up with previously marginalised males. In that event, the 'cheat' could be simply outflanked – left to get on with seeking favours from the once-dominant male whilst everyone else ignored him and broke away to liaise with other males at the peak moment of synchronised ovulation. Dunbar (1988: 148) points out that it would probably be the more subordinate females – those suffering most from reproductive suppression – who would have the greatest interest in subverting any system based on single-male monopolies. It is they who would be most inclined to incite outsider males to come in and form liaisons with them, synchronising with coalition allies in order to do so. 'Even if only some females synchronise their cycles', Dunbar comments, 'they may be able to attract enough males into the group to meet their needs'.

Where homind evolution is concerned, we have no direct evidence that a link-up between oppressed females and previously excluded males along the lines suggested by Paul Turke occurred, nor that female ovarian synchrony played a part in achieving this. Nonetheless, on purely theoretical grounds, we may begin to detect quite a revolutionary potential in such synchrony – this biological capacity which human females nowadays possess, and which they have doubtless possessed throughout the span of hominid evolution.

At the very least, taking synchrony into account extends our understanding by widening our view of the possibilities available to evolving female hominids. Whether synchrony occurred in the period referred to by Turke, we do not know. What we do know is that to the extent that it did occur within any population at any time, it would have tended to subvert male attempts to monopolise large harems of females. Dominant males, on this analysis, would have maintained their power only where they could operate a policy of 'divide and rule'. Where their cycles were randomised, females could be dealt with one by one and thereby managed and controlled. Synchrony, by contrast, would have been a manifestation of inter-female solidarity; its achievement would have granted females a special kind of power, enabling them to escape being privatised by dominant males either monogamously or in harems.

Chapter 7
The Shores of Eden

> The jealousy of the male, representing both tie and limits of the family, brings the animal family into conflict with the horde. The horde, the higher social form, is rendered impossible here, loosened there, or dissolved altogether during the mating season; at best, its continued development is hindered by the jealousy of the male. . . . Mutual toleration among the adult males, freedom from jealousy, was, however, the first condition for the building of those large and enduring groups in the midst of which alone the transition from animal to man could be achieved.
>
> Friedrich Engels, *The Origin of the Family, Private Property and the State* (1884)

Paul Turke's ovarian synchrony model not only helps explain the human menstrual cycle. It also sheds unexpected light on some central problems of human biosocial and cultural origins. It enables us to appreciate that the final, culture-inaugurating phase of the human revolution – the phase during which the male sex was at last forced to abandoned its former sexual competitiveness and assist females in accordance with new, solidarity-based rules – was in a profound sense 'nothing new'. Underlying it was a logic of intensifying sexual synchrony and control which had roots deep in the past, when the basic features of modern human anatomy and physiology were being determined.

Synchrony and the Fossil Record

Early hominid fossils are found from end to end of the East African Rift Valley – an immense geological system of volcanic mountain ranges, valleys and interconnected estuaries, rivers and lakes stretching from the Gulf of Aden almost to the southern Cape. Such fossils are found in no other part of the world. A possible implication of Turke's model would be that conditions unique to the Rift Valley enabled females to synchronise on a scale not possible in other locations. This in turn would lead us to ask what these special conditions may have been.

We can at once eliminate Paul Turke's own suggestion – that ovulatory synchrony became established when a forest-dwelling ape for the first time began moving out into open savanna territory. This scenario is now as outdated as others which locate the decisive events leading to a modern lifestyle as far back as in the Pliocene or early Pleistocene (Chapter 5). Even if survival in the savanna were possible at such early times, dense aggregations (such as those of baboons) would not have been. Whilst many savanna-dwelling primates can eat virtually anything, human digestive systems are highly selective (Milton 1984; Stahl 1984). To a chimpanzee-brained, ape-like creature whose hunting capabilities were relatively undeveloped or associated basically with males, movement into semi-arid savanna would have meant entering a vegetationally impoverished habitat, incapable of facilitating synchrony through sustaining large groups of females all foraging in the same vicinity. Even the idea that more meat would have been available in the savanna is mistaken. The most recent primate evidence totally undermines the assumption of any necessary connection between movement into open savanna and the beginnings of co-operative hunting. Contrary to the preconceptions of the savanna theorists, it has been demonstrated that dense rain-forest-dwelling chimpanzees (such as those in the Taï National Park in the Ivory Coast) eat much more meat and hunt much more co-operatively than do their savanna-woodland or savanna-dwelling counterparts. It is the rain-forest chimps who are most likely to plan their hunts in advance and set out seeking for prey, organising an elaborate ambush, instead of just chasing an animal when one happens to be encountered (Boesch and Boesch 1989; Boesch 1990).

Robert Blumenschine (1987) has become known for his careful observations on predation and the fate of carcasses in the present-day Serengeti. His conclusion is that if early hominids were in part scavengers – as most analysts now assume – they could not have been savanna-dwellers. The open savanna would have been well populated with hyenas. These superbly equipped animals would easily have out-competed hominids within any scavenging niche. On the other hand, hyenas tend to avoid the dense woodlands which surround rivers and freshwater lakes, despite the fact that carcasses may be abundant in such places. Feline predators make overnight kills of zebras and adult wildebeest close to the waterfronts, leaving plenty of marrow-rich bones and other high-quality foods along the wooded shores. Leopards have a habit of storing their partially eaten kills – usually gazelles and other small ungulates – high in trees, and hominids would have climbed up to steal these (Cavallo and Blumenschine 1989). For Blumenschine, in short, 'riparian woodlands' – a habitat of trees bordering lakes, rivers and estuaries – are the most likely dry-season setting in which scavenging early hominids could have survived.

In the case of all evolving hominids, ovarian synchrony, where it existed at all, must have depended on the ability to maintain area-intensive foraging patterns. Females could only associate closely with one another where there was enough food to sustain their togetherness within each small patch of temporarily occupied territory. The moment the climate cooled, turned dry or for some other reason reduced the primary productivity of the local terrain, more space would have been required per foraging individual. Under these conditions, group life of any kind – and with it, synchrony – would have become more difficult to sustain. If we concur with Turke that the precursors of the hominids must have been forest-dwellers, and that their divergence from the apes was based on a move into some other habitat, then the synchrony scenario means that an alternative to open or semi-arid savanna must be found.

Turke's model would lead us to link variation in consistency of ovulatory synchrony with key aspects of hominid evolutionary diversity. We cannot know whether any single population of fossil hominids was synchronising in accordance with Turke's model. But given the lack of evidence for substantially delayed maturation, neoteny or encephalisation in the Pliocene, it seems unlikely that his model can be applied to the early stages of evolution which he himself envisages.

Over the period of hominid evolution as a whole, the presence or absence of synchrony between co-resident females would doubtless have been uneven. Females in some localities would have synchronised weakly or not at all, while in other areas they phase-locked more consistently. Opportunities for female gender solidarity and synchrony would have been influenced by local resources and associated patterns of foraging – in other words, by ecologically determined patterns of female aggregation or dispersal. If synchrony were a condition of long-term genetic approximation towards anatomical modernity, this variable pattern would in turn have meant that while certain hominids were evolving in 'modern' directions, others were adapting in divergent ways. In any event, we can reject the notion that Plio-Pleistocene hominids were under any obligation to advance unilineally towards modernity. Each of the various different species and sub-species was following its own trajectory, most of which led in directions rather different from the road which we can retrospectively trace as leading to ourselves (Foley 1987, 1988).

Turke's model would imply, then, not that all females behaved in the same way; only that those which synchronised best were the ones to evolve in the most anatomically modern directions. The most consistent synchronisers would have secured the most male support, and in this context would have been the most effective mothers. The added support would have enabled mothers to become more slow-moving, more attached to sheltered, well-watered spots or to camp-fires affording warmth – and consequently more able to prioritise child care. Relatively premature babies would have survived better, leading to increased neoteny and hence the possibility of larger brains (Turke 1984).

At the opposite extreme, on Turke's logic, failure to synchronise would have been associated with higher levels of sexual conflict, philandering, monopolisation by alpha males – and the exclusion of subordinate males from the breeding system. The adaptive pressures of mating systems of this kind tend to select for physical fighting capacities in males – 'brawn' rather than 'brain'. One result is high levels of sexual dimorphism. Another is minimised male involvement in parenting. In this light, the model might attribute some of the less gracile or less human-like products of evolution (for example, the robust australopithecines, or some of the more robust and/or dimorphic species of *Homo*) with the probability that as radiating hominids colonised what were by previous standards impoverished or arid areas, female ovarian synchrony became difficult to maintain. Groups of foraging, largely self-sufficient females would have had to space themselves out in the search for food – perhaps in small 'family'-type groups which aggregated only infrequently if at all. The possibilities for widespread female gender solidarity would therefore have been weak or non-existent, and in most situations this may have allowed males to monopolise individuals or small groups, setting up boundaries dividing one breeding unit from another. It is tempting to speculate that in the more barren or more marginal environments occupied by *Homo erectus* or archaic *Homo sapiens*, patterns of this kind became established. Such developments would in turn have set up locally specific selection pressures driving hominid evolution in non-modern directions.

According to Richard Wrangham (1987: 68), the common ancestor of apes and hominids had hostile, male-dominated intergroup relations, polygamy, and a social system which allowed for few alliance bonds between females. In the view of Robert Foley (1987: 171), this situation did not change fundamentally even when the hominid/ape evolutionary divergence occurred. Foley argues that 'the early hominids possessed a social organisation not dissimilar to that of other terrestrial primates – large group size and competition between males for access to females'. He suggests (p. 172) that like geladas and other baboons, 'early hominids may have had a single-male, polygynous reproductive system'. *Australopithecus afarensis* does seem to have been heavily sexually dimorphic, the males having rather large canines (Johanson and White 1979), and it is generally true that a high level of dimorphism tends to correlate with a harem system, or at least with strong sexual competition between males. More controversially, however, Foley (1988: 219) extends his model into the Late Pleistocene, arguing that humans during the last ice age practised 'a harem system of polygynous mating' or, in any event, 'a system of patrilineal control and organisation of females'. It is, of course, a view diametrically at variance with that advocated here.

It is true that a move into open territory – at least among many primates – can lead to the setting up of one-male dominated 'harems'. However, we have also noted an apparent incompatibility between such mating systems and the evolution of large brains. This would make a harem model perhaps consistent with the cranial anatomy of *Australopithecus afarensis*, but certainly quite inconsistent with that of evolving *Homo*. It would seem that any kind of harem monopolisation would have been particularly incompatible with the evolution of large brains under seasonal or other conditions of climatic stress. Any fall in the primary productivity of the local terrain would have added to child-burdened females' need for foraging assistance from mobile males – at the very moment when these same factors were excluding many of these males from the breeding system. As a result, selection pressures would have acted against mothers who produced slow-maturing, ultra-dependent, large-brained offspring. Endlessly moving, autonomously foraging, hard-pressed mothers would have been under pressure to produce smaller-brained babies – precocious survivors with plenty of stamina and brawn. If with the hominid digestive system it was impossible to survive in the harsher areas without immense intelligence and hence large brains, the result would have been extinction or retreat into richer habitats.

In fact, we know that in all periods prior to the Middle and Later Pleistocene, hominids were restricted to relatively resource-rich and predictable ecosystems. *Homo erectus* tested and in places extended the limits of such constraints, but did not transcend them. The ecosystems exploited by the hand-axe makers of Africa and Eurasia were quite diverse – grassland or woodland savanna, the steppe, montane grasslands and the sea coast, among others. But they avoided cold regions with seasonally sparse vegetational cover or resources. Even the Neanderthals were forced to retreat from severe cold, remaining restricted within ecological zones able to sustain their relatively area-intensive foraging patterns. It was only symbolic culture-bearing, anatomically modern humans who eventually broke through all such constraints (Gamble 1986a; Shea 1989; Whallon 1989).

We have noted Foley's one-male harem model of early hominid sociality. Foley himself would concede, however, that his attempts to reconstruct early mating systems are speculative. In particular, he makes no attempt to reconcile his theory with the seemingly incompatible findings of Alexander and Noonan (1979), Paul Turke (1984) and others who have taken into account the unique features of the human female reproductive cycle. Perhaps the most we can be certain of is that like contemporary primates if not more so (Dunbar 1988), *Australopithecus* and *Homo* would have been quite flexible, setting up one kind of mating system under some conditions, other kinds under others. Paul Turke's model does not help us here – it is not concerned with the fossil evidence – but his argument would lead us to suppose that

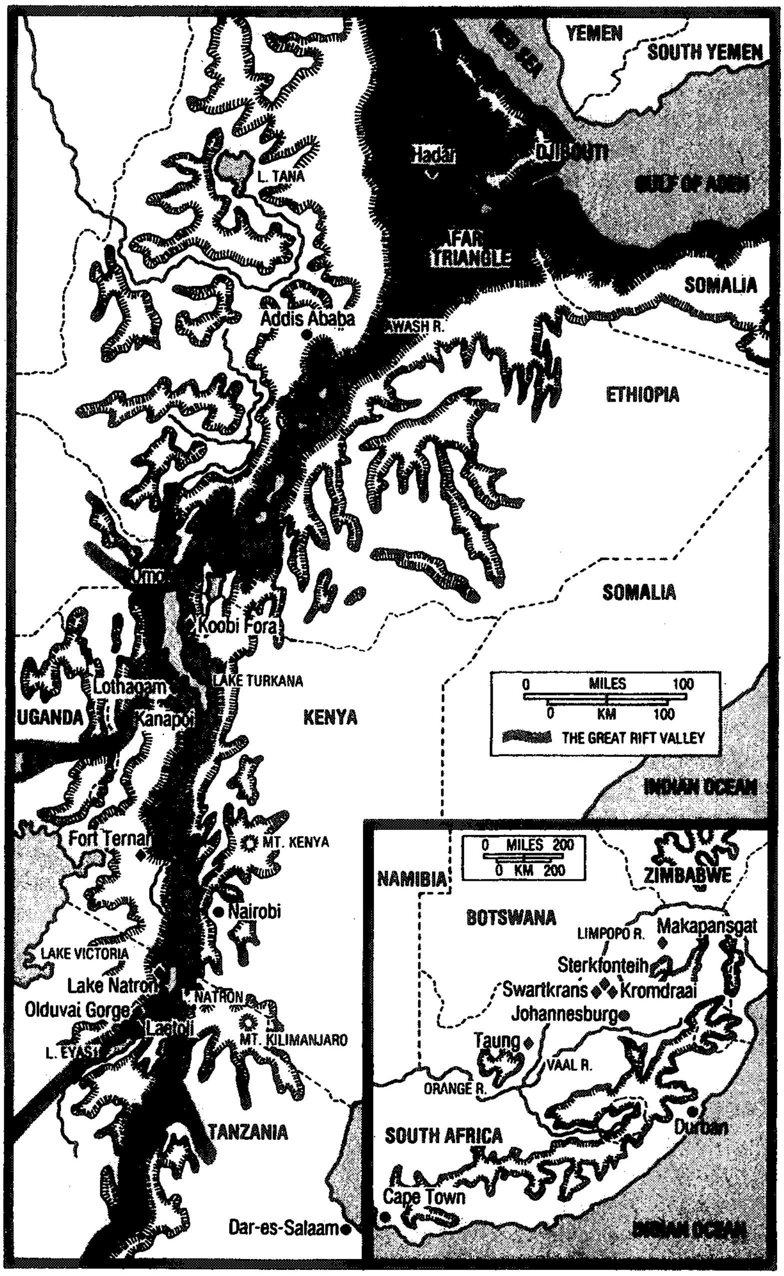


Figure 4 The East African Rift Valley. Today, its floor – shaded in this map – has for the most part dried out, leaving a landscape of bush and dry grassland, although a string of lakes remains. Between 3.5 and 2.5 million years ago, the shaded areas were wetter and in many places flooded, with swamps and dense woodlands bordering the numerous lakes and rivers (map after Johanson and Edey 1981: 13).

where ecological conditions enabled females to synchronise as a means of compelling males to intensify provisioning, larger brains, reduced sexual dimorphism and a more human-like reproductive physiology would have evolved. The interesting question is to ask what these synchrony-favouring ecological conditions might have been.

Synchrony and Subsistence

We may suspect that there must have been something rather special about the East African Rift Valley 'savanna-mosaic' ecosystem of woodlands, grasslands, lakes, estuaries, sea-inlets, islands and rivers in which the earliest hominid fossils have been found (figure 4). Whilst it is possible that this is only an impression created by depositional bias – rivers and lake-shore settings are far better than others in helping to ensure that organic remains fossilise and are preserved – it now seems likely that early hominids really were restricted to roughly this region. Above all, early stone tools, which need no such special conditions in order to be conserved, seem on present evidence to replicate much the same distribution pattern as the fossilised bones. It has been pointed out (Blumenschine 1987: 393) that all known East African Plio/Pleistocene hominid *activity* sites are located in or along ephemeral or perennial watercourses and/or lake shores. These sites would have been in riparian woodlands denser than the tree/bush cover which is found further away from water sources.

Throughout their evolution, hominids seem to have been exceptionally water-dependent primates. The early Oldowan hominids have been labelled 'The People of the Lake' (Leakey and Lewin 1979). What little we know of the function of the battered 'hammers' found from the early levels of Olduvai Gorge seems to indicate that they were used for pounding up '*aquatic tuberous plants*'; phytoliths (the silicified remains of such tubers) have actually been found on the hammers (Binford 1989: 27, citing Isaac pers. comm. 1985). In reporting this finding, Binford adds that the frequencies of these tools 'seem to vary with the presence in the environment of this type of plant, and they are found in the spots where the plants occurred'.

Wymer (1982: 125) comments that throughout human evolution in the Pleistocene, including in Europe and Asia, 'there was a definite preference for rivers and lakes'. Evolving *Homo*'s association with water seems to have lasted right up until the appearance of anatomically modern forms. Among the earliest-known fossils of anatomically modern humans from anywhere in the world are those from Klasies River Mouth Cave on the southern coast of South Africa. These large-brained hominids, who were on the very threshold of establishing culture in its modern, symbolic form, were feeding on a rich diet which included an abundance of sea foods (Wymer 1982: 157; Binford 1984: 19–20). This would have meant high productivity as well as an easy accessibility of foods, potentially leading to that 'mobile sedentism' discussed earlier as a precondition of synchrony. Seals, it should be pointed out, would have provided a wonderful source of very rich, portable food including valuable fats. Clumsy and defenceless on land, they can be picked up and killed with tools as crude as stones or sticks when colonies are assembled on small islands, rocks or reefs along the coast (Lanata 1990).

The most primitive known hominid fossils are those of *Australopithecus afarensis* – 'Lucy' as her most famous representative is known (Johanson and Edey 1981). These fossils come from the East African Rift Valley's northerly end – in particular from the floodplain of an immense, now largely desiccated estuarine region known as the Afar Triangle, stretching between the Gulf of Aden and the lower end of the Red Sea.

The lower-lying areas of this geologically unstable region were partly flooded during the early Pliocene, although rising out of the sea in the Afar Triangle at the northern end would have been a large island – now the Danakil Alps (LaLumiere 1982 [1981]). Adjacent to this island at its north-east end, the Danakil Depression – which was first invaded by marine waters about 6.7 million years ago – only became finally desiccated about 30,000 years ago (LaLumiere 1982 [1981]: 128–32).

If any apes had lived in this initially forested but periodically flooded riverine and estuarine region between the Miocene and Early Pleistocene, we would expect selection pressures to have favoured a creature who could not only climb trees, but also on occasion walk bipedally, swim when it occasionally fell into the water and, when necessary, wade waist-high in search of food, holding the head above water. In other words, whilst there may be little or no real evidence to support the more exuberant, early versions of the 'aquatic hypothesis', according to which unknown Miocene hominids were living an almost dolphin-like or seal-like existence in the sea (Hardy 1960; Morgan 1972), it seems reasonable to suppose that the earliest hominids actually known from the fossil record were well adapted to the wetland, riverine and shoreline environments in which their remains have in fact been found. If despite the attractions lakes, rivers and marine shores evidently held for these hominids, they could not swim – then we would want to know why! It is surely the savanna theorists' idea that our ancestors *could not swim* which is the intrinsically improbable hypothesis that needs to be questioned and tested most sceptically, not the more likely view that, as in our own case, swimming was one of the things that these creatures could do.

In this respect, *Australopithecus afarensis* causes no apparent problems. Lucy – being a female in this sexually dimorphic species – was rather diminutive, with a small, chimpanzee-like brain, straight spine, relatively long arms, a mobile ankle, curved toes and ape-like toe joints. Her feet were broader and larger than those of modern humans – 35 per cent of leg length instead of 26 per cent – giving her a gait described by Roger Lewin (quoted in Morgan 1990: 34) as 'not quite as bad as trying to walk on dry land wearing swimming flippers, but in the same direction'. These features argue against a savanna-dwelling habitual biped, suggesting instead something of an all-round gymnast – a walker who still spent much of her time climbing in trees (Stern and Susman 1983; Susman *et al.* 1984; Susman 1987). Assuming that she could also swim (Morgan 1986, 1990; Verhaegen 1985), her repertoire would be a suite of activities requiring retention of many of the characteristics of brachiators, including the position of the *foramen magnum* (indicating the angle of the head) and a pelvic connection allowing the legs and trunk to form a straight line.

Fossil remains of early hominids along the Rift Valley are found in water-deposited sediments associated with the valley's many ancient lakes, rivers and swamps. The bones and stone tools occur in association with bovids (frequent visitors to waterholes and lake shores), pigs (which root for their food in swamps) and also aquatic species such as fish, turtles, snakes and crocodiles (Potts 1988 and references). When pieced together, the evidence as a whole suggests adaptation to a mixed waterside ecology of foraging and scavenging (Blumenschine 1987) quite different from the 'burning savanna' scenarios of popular writers such as Robert Ardrey in the 1960s.

Despite this, authoritative textbooks still repeatedly use such phrases as 'the hot and arid low-lying floor of the Rift Valley' (Wymer 1982: 63) when discussing the background to hominid evolution. Even so well-informed an evolutionary ecologist as Robert Foley (1987: 189) uses the term 'semi-arid savanna' to describe what he terms 'the particular environment in which the fossil hominids seem to have lived'. Such formulations appear to owe more to the weight of disciplinary tradition than to the data as such.

Palaeoenvironmental reconstructions are notoriously controversial and subject to change almost from year to year. But in terms merely of the sources and conclusions drawn on by Foley himself, the evidence does little to support his model. The site of Tabarin, according to Foley (1987: 195, table 8.1), was a lake margin. The Middle Awash provided 'fluvial conditions'. The site of Hadar, home of 'Lucy', was a lake and associated floodplain, with 'braided streams and rivers'. Omo was a region of dry-thorn savanna 'flanking river banks with gallery forest and swamps'. Koobi Fora was a freshwater lake with floodplains, gallery forest and dry-thorn savanna. Peninj consisted of open grassland surrounding a salt lake, fed by fresh rivers.

Admittedly, in all these localities the climate was subject to pronounced seasonal variation, with many rivers and waterholes becoming desiccated during the driest months – but this only adds to our understanding of the factors compelling early hominids to keep close to the more permanently watered lakes, estuaries and tree-shaded shorelines. In fact, of ten early hominid-occupied Rift Valley habitats listed by Foley (1987: table 8.1), only the reconstruction for Laetoli would seem dry enough to qualify straightforwardly as 'grassland savanna'. Although the evidence is as yet uncertain, it may turn out that this and similar dry and/or elevated areas were occupied by hominids only during the rainier parts of each year. Should this possibility be established, it would confirm our picture of an extremely water-dependent animal.

Olduvai Gorge, in the middle of the Serengeti Plain, is the best-known site. At its centre, about 1.9 million years ago, was a perennial, saline lake about 22 km across. All around were the lake's floodplains, traversed by freshwater seasonal streams and rivers. Fossil pollen, microfauna, geochemistry and bovids from the oldest horizons in Bed I indicate a moist lakeside environment with about 1,000 mm of rainfall per year, closed-canopy vegetation, and isolated patches of grassland and marsh (Potts 1988: 193). Papyrus and other shore grasses were plentiful, and the remains of birds include abundant grebes, cormorants, pelicans, ducks, gulls, terns and wading birds such as flamingos, herons and stalks (Potts 1988: 22). This was a semi-aquatic, mixed-waterside environment if ever there was one. We might indeed suspect it of having been so boggy and prone to flooding as to be best avoided during the rainy months.

It is true that on rare occasions, Developed Oldowan tools have been found on high plateaus above the Rift Valley proper, a good example being a string of sites along the Plain of Gadeb, Ethiopia, where an immense lake existed in Late Tertiary times (Wymer 1982: 63). But again, the lake-shore setting is significant – the tools were found in its sediments – and there is no evidence that this habitat was in any sense arid. In fact, over East Africa as a whole, it seems certain that where dry-savanna regions, high plateaus or mountain slopes were occupied at all, this testifies not to a preference for semi-aridity but to the mobility and adaptability of early hominids – their ability to move from excessively flooded regions during the rainier months when water was everywhere, returning to lower-lying regions as desiccation increased. Even an ape who could wade and swim could be drowned in a sudden flood – and in any event would not want to be wet all the time!

In the Transvaal region, which is also outside the Rift Valley proper, it may be more appropriate to speak of 'savanna environments', but here, too, the term 'mosaic' must qualify this (Foley 1987: 196). The valley of the Vaal River is wide and gently sloping. Gravels in the riverbanks are remnants of former, higher courses, and it is in the oldest of these that early chopper-core tools have been found (Wymer 1982: 72–3). If any these sites was really in open savanna territory, it was certainly not 'semi-arid'. 'Most of the South African sites', as Foley (1987: 196) himself comments having examined the details, 'seem to be at the wetter end of the environmental spectrum'.

Such associations are neither fortuitous, nor a product only of depositional bias. Not a single excavated Plio-Pleistocene site anywhere in Africa supports the view that semi-arid savanna was remotely favoured by the earlier hominids. We have noted that Oldowan stone tools have been found to be distributed in essentially the same ecozonal patterns as fossilised bones – a fact which helps rule out the possible objection that we are mistakenly inferring waterside environments only because fossilisation is favoured by such conditions. In short, it is clear that early hominid subsistence activities were in some necessary way linked to well-watered, well-wooded, highly variegated habitats such as the Rift Valley to an unusual extent provided.

Extremely fertile, volcanically enriched soil and hence abundant vegetation was no doubt one factor which made the Rift Valley such a uniquely favourable spot in which hominids could evolve. The availability of rivers, streams, springs and hence perennially available water was another. 'Man is the most dependent on thermal sweating among the mammals thus far investigated', it has been observed (Newman 1970: 379), 'and may well be the most dependent on a continuing source of water. . . '. Whilst culturally organised modern hunter-gatherers can often survive for many months in seemingly waterless regions such as the Kalahari Desert, obtaining their water supplies from melons, the insides of game animals and similar sources, for pre-cultural hominids such capacities would have been inconceivable.

In the hot, seasonally rainless region which was Plio-Pleistocene East Africa, uninterrupted proximity to water was for evolving hominids an absolute necessity (Foley 1987: 106–7). Humans shed mineral-rich body salts in addition to precious water through their profuse sweating, and must drink copiously for this reason as well as because human urine is dilute and little water can be stored in the body (Verhaegen 1985). None of these features suggests a savanna adaptation. In fact, the precise salt/water composition of human urine (a quite different mix from that of dry savanna- or desert-dwellers, whose water-conserving urine is sticky and concentrated) leads Verhaegen to conclude that our ancestors 'probably lived once near salt or brackish waters or at simultaneously or successively different aquatic habitats, e.g. first in a freshwater and afterwards in a salt water environment . . . but certainly not in a very dry habitat (savanna)' (1985: 25).

To the extent that hominids were situated along the shores of saline lakes (such as many of those in the Rift Valley) or the sea, they would have needed to focus around estuarine regions which provided drinkable water. The evidence is that they did.

Drinking water would have been only one utility associated with such sites. Water has other uses, too, and it must be significant that early hominids situated themselves not just along riverbanks or springs but more specifically at the points at which these entered large, often saline, lakes. What could have been the special value of such sites?

Some modern human analogies are pertinent here. Unless cultural norms, freshwater crocodiles or other factors intervene, humans from childhood onwards find bathing an enjoyable way of keeping cool, of cleaning the body and of playing. Diving can be pleasurable once learned. Traditional coastal peoples such as the diving women of Korea and Japan (Hong and Rahn 1967) go as deep as 75 feet to gather shellfish and seaweed, while Polynesians can dive into deep water, hide behind plants – and catch passing fish with one hand. Given that humans are clearly genetically equipped to cope extremely well with swimming, diving and even underwater childbirth (Morgan 1982), we have every reason to suppose that such facilities have been at least intermittently adaptive for millions of years.

Subsistence considerations point in the same direction. Human brains are large and brain tissue is 60 per cent polyunsaturated fat. Such brains are in energetic terms exceedingly expensive and need constant replenishing. The savanna grasslands of early and middle Pleistocene Africa would have held only a miserable supply of the fats essential to nourish such organs. Some nutritional chemists (Crawford and Marsh 1989) have pointed out that this effectively rules out a savanna adaptation for hominids who were as yet unable to practise efficient hunting. Hominids whose brains were expanding, these authors find, must have been collecting marine foods and gathering seeds and fruits along rivers, lakes, seashores and estuaries which contained not only foods rich in polyunsaturates but also a wealth of essential trace elements and minerals washed off the land. We could perhaps link such considerations with the fact that whereas the small-brained robust australopithecines apparently evolved their huge grinding teeth and other special adaptations in the drier regions of East Africa, the gracile forms and *Homo* evolved in the wetter parts, close to rivers and lakes (Foley 1987: 210–14).

In fact lakes, marshes and shorelines can provide abundant foods of all kinds – weeds, edible bulbs, aquatic birds and their eggs, turtles, small reptiles and much else. As Plio-Pleistocene African lakes and pools contracted or even sometimes dried out in the driest, most difficult, months of the year, they would often have been teeming with stranded fish and other creatures; collecting these in addition to birds or other small-to-medium-sized game would have been for hominids a high-yield strategy in otherwise stressful months. It is worth adding that because of the salinity, plant resources may have been sparse in many such areas, prompting an increasing need to exploit non-vegetable foods (Foley 1987: 209, 212). A very omnivorous diet would conform with what we know about the gracile hominids' teeth (Foley 1987: 211).

Little technology is needed to exploit shoreline resources. In Australia, coastal Aborigines using extremely simple tools combine the eating of molluscs with fish, turtles, birds, wallabies, snakes and lizards, and they collect yams, water chestnuts, fruits, eggs and wild honey. The economics of mollusc gathering are revealed by a study on the shores of Arnhem Land (Meehan 1977), in which women and children were each able to take 8.5 kg of shellfish per trip, of 29 sorts, but mainly of a single species, *Tapes hiantina*. For the energy expended, Ebling (1985) comments, it seems unlikely that male hunters could have secured a better return. Although they may have a rather low food value for their weight (Bailey 1978: 39), shellfish contain valuable minerals as well as other nutrients, and as part of a varied shoreline diet can play an important role. Over Australia as a whole, in any event, the pre-contact population density of Aborigines is closely correlated with rainfall – except along coasts and on islands, where it is higher than would be predicted (Birdsell 1953).

It used to be thought that an absence of Plio–Pleistocene shell-middens indicated that early humans were not using aquatic foods. But this was in the 1970s, when theorists were still looking for 'home bases' as diagnostic features of early hominid activity. The collapse of this paradigm has radically changed the picture. It is now realised that if early hominids were diving or wandering along shores, sometimes catching fish in shallow waters, selecting edible weeds or seaweeds, or cracking crabs or bivalves with stones and eating the flesh as they went, locally concentrated middens would never have arisen. Indeed, since stone tools need not have been used, at most sites it would be extremely surprising to find any archaeological traces of such activities at all.

Hominids, Evolution and Water

There is nothing extraordinary about the theory that in a coastal, estuarine or riparian woodland environment, water as such would have been an important component of the total system of environmentally linked selection pressures acting on evolving hominids. Wild chimpanzees are much less water-adapted than humans, but even they have been seen wading in streams, drinking from lakes, feeding on aquatic plants – and mating ventroventrally in very shallow water (Nishida 1980). On the other hand, if chimpanzees waded any deeper or foraged and travelled through trees overhanging rivers and lakes, they would frequently fall in and drown. Chimpanzee infants as well as adults lack the fat-based buoyancy of humans, their bodies are not streamlined – and for numerous reasons connected with breathing control, the shape of limbs and so forth, no one has ever trained a chimpanzee to swim. The only primate known to be a good habitual swimmer is the proboscis monkey, which inhabits an ecological zone comparable to that which Blumenschine and others have envisaged for Lucy and other early hominids. Proboscis monkeys live in mangrove trees in the coastal swamps of Borneo. They climb trees which overhang the water and swim, but occasionally, and especially at low tide, they are seen on relatively dry land – on a mud flat or sandbank. It can hardly be a coincidence that proboscis monkeys are the only primates able to practise a form of sustained bipedalism very similar to that of humans, in one Japanese documentary film being shown 'walking calmly on the ground through the trees in single file' (Morgan

1990: 46). Periodic inundation of their habitat is, of course, the incentive for such bipedalism. As Morgan (1990: 46) points out:

> For proboscis monkeys crossing a stretch of water a couple of feet deep, walking upright offers only one single advantage, but it is an offer they cannot refuse. It enables them to breathe, whereas if they walked on four legs, their heads would be under water.

Bonobos are widely regarded as the most human-like of all the great apes. Whilst they are not known to be swimmers, their near-bipedalism has seemingly evolved under comparable selection pressures. Bonobos often feed whilst wading along streams in their periodically inundated natural habitat (de Waal 1991: 182–6).

Any hominids evolving in the northern regions of the Rift Valley would have been under selection pressures shaped in part by the waters which periodically flooded wide areas – particularly when the Danakil microplate became detached from both the African and Arabian plates at the beginning of the Pliocene, allowing waters from the Red Sea and Gulf of Aden to flood into the Afar Triangle (LaLumiere 1982 [1981]: 128). Lucy's skeleton was found among the remains of crocodile and turtle eggs and crab claws. It is perhaps also worth noting Foley's (1987: 26) support for the view that a mixed *Australopithecus afarensis* group who died in this region 3 million years ago was a whole family, whose members had all been drowned together in a flash flood (Johanson and Edey 1981; Johanson *et al.* 1982). In fact, most experts now believe that the bones are not those of a single family but were accumulated separately over many years. Nonetheless, drownings must often have occurred, and certainly the need for wading or occasional swimming would explain why the very earliest hominids failed to adopt the restrictively terrestrial but otherwise extremely efficient locomotory technique of quadrupedalism.

The need to move within three contrasting media – trees, open ground and water – would have hastened the tempo of evolutionary development, particularly if small populations became stranded for periods on islands with water all around. As less efficient swimmers occasionally drowned, the survivors would have displayed increasing hairlessness, a thick subcutaneous fat layer, chubby and buoyant babies, streamlined body contours, downwards-facing nostrils, a descended larynx, unusually good control over breathing, enhanced diving abilities – and the many other clearly water-adaptive characteristics which make humans such unusual primates (Hardy 1960; LaLumiere 1982 [1981]; Morgan 1972, 1982, 1984, 1986, 1990; Morris 1977; Verhaegen 1985). *It is simply impossible to believe that hominids who selected the Afar Gulf and various Rift Valley lakesides as their favoured habitats could have been unable to swim.* On the other hand, if they could swim, it is impossible to believe that selection pressures associated with this mode of locomotion could have been irrelevant to their evolution.

Admittedly, attempts have been made to explain some of the features listed above without reference to water-associated selection pressures. But in general they have not stood the test of time.

In the 1960s, the development of bare skin and profuse sweating as a thermoregulatory system was ascribed to the exigencies of survival in a hot, tropical, dry savanna environment. This was seen in terms of the needs of overheated males hunting or foraging strenuously under a hot sun (Morris 1967). The fact that leopards, lions, hunting dogs and other savanna-dwelling predators failed to lose their fur under such conditions was not seen as a problem.

In a recent variation on this theme, Wheeler (1984, 1985, 1988) argues that both bipedalism and humans' relative hairlessness evolved as a thermoregulatory system enabling foraging hominids to run out into the open savanna to obtain food under the noonday sun – when rival scavengers and predators were keeping cool in the shade. Wheeler (1988) makes the important observation that chimpanzees with their short muzzles are poorly equipped to keep their brains cool by panting, and that the ultra-large human brain is particularly heat-sensitive. He suggests that by keeping upright, retaining heat-reflective hair on the head and by sweating from the rest of the body in the absence of fur, humans have developed an efficient set of mechanisms for keeping the body and hence also the brain cool and avoiding sunstroke during activities under a blazing sun.

This hypothesis is interesting and valuable, yet taken in isolation it is inadequate. Firstly, the loss of body-warming fur would have affected hominids on a day-round basis – including in the bitter cold of night – as well as at noon. If shivering in the small hours were the price to be paid for comfort under a blazing sun, it is not at all clear that in the habitat envisaged, the benefits of the new system would have outweighed the costs (Morgan 1990: 60–1). In fact, of course, it is known that enhanced body fat has in humans compensated for fur loss, so at least the plumper furless hominids might have kept warm. But fat-insulation is not discussed by Wheeler (1985, 1988). Humans' enhanced subcutaneous fat layers – whilst useful for a swimmer – would surely be unwelcome during strenuous activities under a tropical African noonday sun. It seems mysterious how this *particular* configuration could have been the product of the selection pressures that Wheeler envisages.

A further objection is that human females have less body-hair than males. They are also on average substantially fatter (Pond 1987). Wheeler does not posit a sexual division of labour for his noonday foragers, but still his model would surely predict the opposite. Had humans lost their body hair primarily in order to monopolise a foraging niche involving running in search of highly-clumped food over partially sunlit ground (Wheeler 1988), then males – as the more mobile sex – ought surely to have evolved furthest in this direction. They ought to be less hairy than human females – the reverse of what we actually find.

In other ways, too, Wheeler's speculation fails when it comes to the fine detail. As Morgan (1990: 80–91) has intriguingly shown, human sweating is actually nothing like that of a chimpanzee which began foraging under a noonday sun. Humans have lost most of the scent-emitting *apocrine* glands which are found all over the bodies of chimpanzees, and through which they (like other large mammals) produce their temperature-regulating sweat. Extraordinarily, humans sweat using the *eccrine* glands which in other primates are relatively few in number and are found mainly on the palms of their feet and hands, where they produce tiny amounts of moisture to assist in gaining a grip and have nothing to do with thermoregulation. In contrast to other primates, humans have eccrines which vastly outnumber the apocrine glands. These eccrines (which in chimpanzees are scattered thinly over the whole body) produce a form of sweat which – bizarrely for a savanna-dweller – takes several minutes to appear in response to overheating and is also extravagantly wasteful of both salt and water. Incomprehensible in a savanna context, this would make sense if humans were the descendants of an early hominid which – like any mammal adapting to water – lost most of its apocrines. Scent trails cannot be left in water, whilst sweating of any kind is less necessary for habitual bathers. It would seem that in proportion as our ancestors later began foraging in hot, dry conditions and needed to sweat profusely to keep cool – they had nothing but their eccrines to fall back on. Add to this the fact that humans have an astonishingly weak sense of smell (Stoddart 1986 and references), anomalous in a savanna scavenger but quite typical of swimming mammals (Morgan 1982: 97, citing Martin 1979) – and the pieces of the jigsaw nicely fit.

In short, the aquatic hypothesis (Hardy 1960; Morgan 1972, 1982, 1990) offers far simpler explanations than Wheeler's, and has the added advantage of accommodating Wheeler's own substantive findings. Wheeler (1988) himself is not averse to postulating very early selection pressures *pre-adapting* evolving hominids for the bipedalism whose thermoregulatory advantages he has documented:

> Our ancestors may well have been predisposed to walking upright by virtue of the time that their ancestors had spent hanging about in trees. Brachiating – swinging by the arms – means the animal has to hold its body in a vertical position for long periods of time; primates that brachiate often adopt a more upright posture on the ground than true quadrupeds (Wheeler 1988: 62).

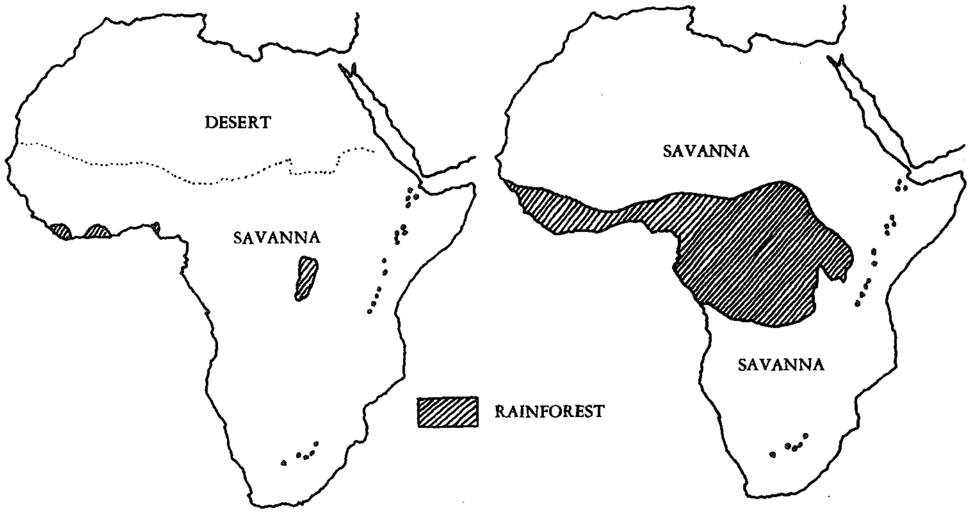
Add 'moving through water' to 'brachiating' in the above passage and the problems are solved. Wading through water no less than swinging through trees would require the traveller 'to hold its body in a vertical position for long periods of time'. The upper body's buoyancy in water would help sustain such a stance. It is well-known that semi-aquatic creatures tend to adopt a more upright posture on the ground that true quadrupeds – as the examples of penguins, proboscis monkeys and beavers nicely show (Morgan 1982: 53–4). Postulating at least occasional swimming, moreover, has the added advantage that it explains the development of the characteristically human layer of subcutaneous insulating blubber about which Wheeler says nothing at all. Interestingly in this context, and puzzlingly in view of his concerns over hominid overheating, Wheeler (1985) dismisses the idea that early hominids could have been swimmers on the basis of a single unsupported supposition. A hairless primate the size of an early hominid, he writes, would have got *far too cold* in the tropical waters of East Africa!

Bipedalism doubtless helped minimise overheating, just as it is useful in food-carrying, tool-use, peering over long grasses and many other activities. But understanding the many ultimate benefits is one thing; tracing the *initial* functions and selection pressures – as Wheeler in his discussion of brachiating in effect concedes – is quite another. We can accept that an increased capacity to forage under a hot sun would have conferred selective advantages – without, however, postulating 'noonday foraging' as the novel niche responsible for the ape/hominid evolutionary divergence. It might be thought difficult under any circumstances to persuade heat-sensitive, large-brained and increasingly fat-insulated hominids to choose noon as the time of day to maximise their foraging activities in the tropical African sun! On the other hand, apes becoming bipedal under combined arboreal/terrestrial/aquatic selection-pressures, drinking copiously, keeping cool by washing, by finding a shady tree or by taking a splash – such apes would have been under strong pressures to evolve precisely the out-of-water thermoregulatory system which Wheeler has so usefully described. In the water, our ancestors would have had few problems in keeping cool. Each time individuals emerged, evaporation would have extended the cooling effects. Enhancement of the technique of drinking copiously and intermittently so as to be able to 'bathe' when necessary through sweat-evaporation can in this light be seen as an extension of the bathing adaptation. The blubber which would gradually have evolved would have served as an aid to buoyancy (particularly necessary for young babies, which are surprisingly fat in the human case) and also as an out-of-water insulation functional in cool periods and at night. To this it should be added that whilst a marked absence of subcutaneous scalp fat combined with abundant heat-reflecting scalp hair fit Wheeler's scenario nicely – such features would equally be predicted of an ape whose postcranial body-hair initially diminished to reduce water-drag. The head is the one part of a swimmer's body which remains above the water-line, and which would therefore need cooling (or alternatively, keeping warm) in a completely different way from the rest of the body. In short, whilst the thermoregulatory hypothesis has some strong points, it is not a complete solution to the problems which need to be addressed; in this context, there seem to be no good reasons why Wheeler's and Morgan's complementary insights should not be combined.

The advantage of taking aquatic selection-pressures into account is that it allows us to view early hominids not as specialists adapted to just one narrow niche – but as flexible creatures capable of climbing trees, foraging on open ground and food-gathering in water as well. It is perhaps possible to envisage sex-linked differences in this context, with males at one evolutionary stage or another more likely to forage inland on dry savanna because of their greater mobility, while females with their offspring stayed closer to protective trees overhanging water and/or resource-rich shores. The sex-linked pattern found in modern humans – with women on average both fatter and less hairy than males – would be consistent with the possibility that evolving hominid females were marginally more dependent on swimming and/or shoreline foraging, although a more probable explanation for women's extra body fat is simply that it provides an energetic fallback needed particularly for mothers who must carry babies in the womb and provide milk at the breast in bad seasons as in good. That might mean that the subcutaneous fat which evolved in the first instance under semi-aquatic selection pressures at a later stage turned out to enable human females more reliably to feed their young.

The restriction of early hominid sites to just one riverine, lake-ribboned and estuarine geological zone confirms the scenario. In Plio-Pleistocene Africa, there were plenty of savanna environments outside this restricted region, particularly in the colder periods (Foley 1987: figure 5.10, citing Bonnefille 1984; Roberts 1984). Tropical rain forests were much less widespread than was previously thought, disappearing almost completely during each ice age although from time to time covering a large swathe of central and south-western Africa (see figure 5). Most of Africa has for a very long time been savanna, and the proportion has been increasing over the past 10 million or so years (see references in Foley 1987: 110–17). If the survival of evolving humans presupposed no more than good hunting or scavenging conditions in this kind of environment, then the geographical distribution of early hominid sites ought at various times to have extended widely across almost the whole of the African continent.

The actual, highly restricted distribution pattern suggests that open savanna-foraging played little role. We have seen that humans' poor sense of smell, reduced apocrines, salt-and-water wasting eccrine sweat glands, constant thirstiness and generally extreme water dependence in tropical climates would have been maladaptive and indeed inexplicable in the context of a simple savanna adaptation. On the other hand, an ordinary, non-aquatic forest or woodland adaptation would leave us wondering why our ancestors ever stopped being apes. We can conclude that evolving early hominids – unlike culture-bearing anatomically modern humans – were extremely discriminating as to where they could live. The quite peculiar mosaic of conditions prevailing in the Rift Valley between the Miocene and Late



a: glacial periods

b: interglacials

Figure 5 Approximate ratio of rain forest to savanna in Africa during the Pleistocene. In earlier periods the rain forest shown in the second map extended still further. Mountain forests and additional woodlands bordering lakes and rivers are not shown. Superimposed on both maps, the dots indicate the points along the Rift Valley where early hominid stone tools and/or fossil remains have been found (modified after Foley 1987: 112–13).

Pleistocene uniquely met their requirements. It was these specific conditions which were responsible for the initial divergence and unique characteristics of the hominids – characteristics increasingly differentiating them from their ape-like cousins.

The Shores of Eden

Some of the best-known early hominid fossils date back to as much as 3.8 million years ago, and were found in the Afar Triangle (Johanson and White 1979). No South African fossils as old as this have been found. Within the Afar, 'Lucy' and her associates settled some 3 million years ago along the Awash River, which flows down the Rift Valley at its northern end. Although fully bipedal (even if not quite in a 'modern' sense), they had no more than chimpanzee-sized brains, and were not yet making stone tools. With neither fire for cooking nor a stone technology for bone-breaking, they could hardly have been proficient open-country scavengers. A diet radically different from that of chimpanzees, however, is indicated already by their rather different teeth. There has been little agreement among palaeontologists on what these teeth were for. Electron scanning microscope use-wear studies indicate micro-flaking, pitting and scratching. It is as if Lucy were a chimpanzee-like fruit-eater – except that she also ate many other kinds of food, including coarse, gritty items which have not so far been identified (Johanson and Edey 1981: 363–4). An unconfirmed possibility is that these were often sand-contaminated lake shore foods including bulbs, aquatic tubers (Binford 1989: 27), and perhaps shellfish.

Others of Lucy's kind must have followed the Awash up to its source and beyond. As they did so, such hominids would eventually have discovered other inland shores, leaving their remains at such present-day sites as the Omo River, Koobi Fora, Lake Turkana, Olduvai Gorge, Laetoli, Makapansgat, Sterkfontein, and Taung (LaLumiere 1982 [1981]; Leakey and Lewin 1979). Those who remained behind, on the other hand, appear to have continued evolving under probably more strongly aquatic selection pressures along the banks of the Awash or in the partly flooded Afar Triangle itself, with offshoot groups periodically moving up the valley and sometimes finding ways of surviving in drier regions as the earlier robust australopithecines seem to have done.

The process of evolution and migration along the valley continued through the Pliocene and into the Pleistocene. Some of Lucy's ape-like but relatively intelligent tool-making migratory descendants – earliest *Homo* – stopped about two million years ago beside lakes at Koobi Fora and Olduvai Gorge (Leakey and Lewin 1977, 1979). Later still, hominids with even larger brains began moving along the valley; around a million years ago, some were able to migrate right out of Africa altogether (figure 6a), eventually reaching China and Java, where they acquired more and more of the derived features of *Homo erectus* (Wood 1984; Groves 1989).

Even later, anatomically modern humans had begun evolving within the Afar/northern Rift Valley area. Among currently favoured candidates for the earliest anatomically modern humans are some fragmentary fossils from Omo Kibish, Laetoli and Lake Turkana, some of which may be up to 130,000 years old. The precise age and status of these fossils may need corroboration (Rightmire 1989), but it is at least possible that anatomically modern humans came to exist in the middle-to-northern part of the Rift Valley some tens of thousands of years before they appeared anywhere else. They would then have moved northwards and southwards, gradually replacing or perhaps sometimes interbreeding with the native populations in each newly entered area (figure 6b).

Much of this is speculative. New evidence is likely to change our picture substantially. Nonetheless, what seems stably established is that from the Miocene onwards until the Last Interglacial, the Rift Valley retained its unique position as the evolutionary cradle of large-brained, gracile hominids.

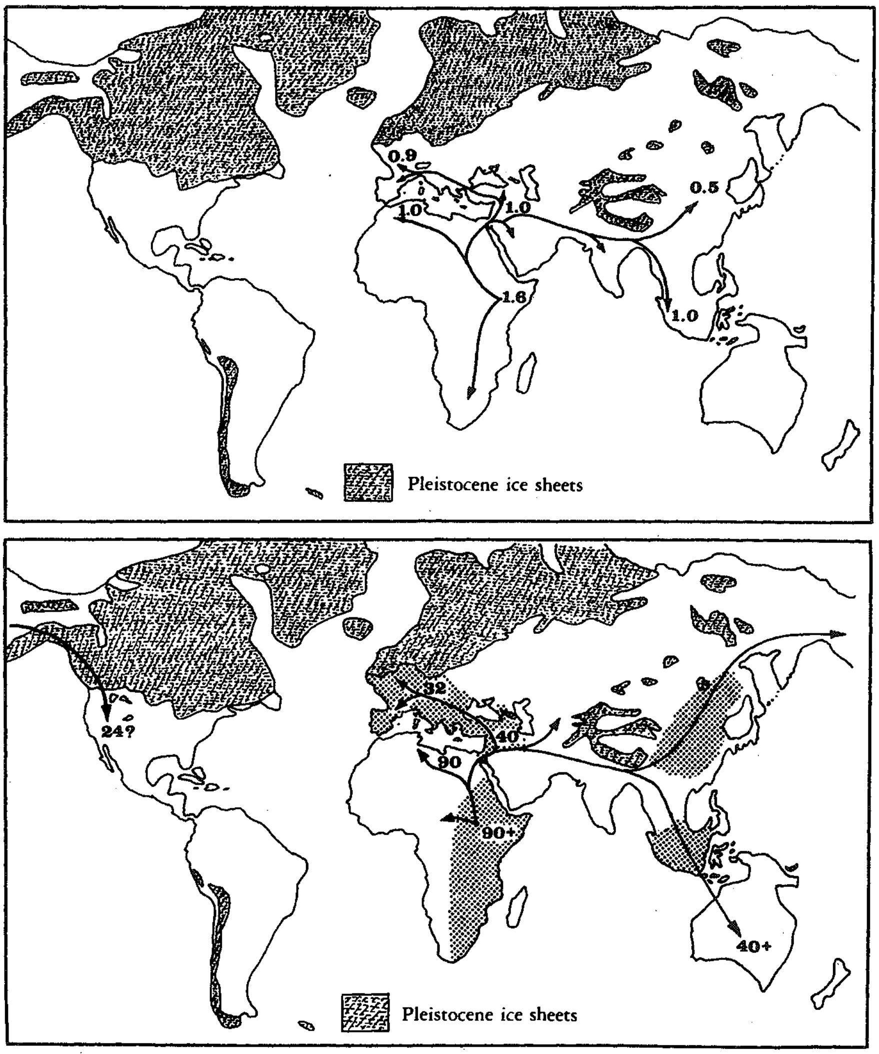


Figure 6 'Out of Africa'. *Top*: dispersal of pre-sapient humans. The lines suggest routes and local dates of arrival (in millions of years before the present). Hatched areas indicate ice-cover at glacial maxima. *Bottom*: dispersal of anatomically modern humans (dates in thousands of years). Also indicated (dot-patterned areas) are resident archaic populations encountered by the moderns: archaic *Homo sapiens* in Africa; Neanderthals in Europe and the Near East; East Asian archaic populations in China and Java. Note that in all periods the ultimate source of dispersal is seemingly from the northern end of the Rift Valley (modified after Foley 1987: 264–5).

In this region and nowhere else in the world, selection pressures – which I am here linking with Turke's ovarian synchrony scenario – promoted intensified parenting (including a growing *paternal* input) and hence gave rise to a succession of bipeds with more and more neotenous features, and larger and larger brains. Each time a new type was produced, certain descendants of these evolved hominids may have been forced by population pressure to migrate southwards and sometimes northwards in successive waves, replacing their less neotenous, usually smaller-brained local antecedents – hominid cousins who in their local (drier) habitat had not evolved in similar directions or at the same rate. Acceptance of Turke's model would imply that the northern half of the Rift Valley had something about it which enabled pre-cultural or proto-cultural hominid females to sustain ovarian synchrony more consistently than was possible elsewhere.

Synchrony, Tides and the Moon

It was noted earlier that women's higher levels of body fat and decreased body hair are both water-adaptive features, suggesting that evolving females may have had some special need to maintain proximity with expanses of water. In the light of Turke's model, we may speculate that this was connected not only with the requirements of an area-intensive mode of foraging – but also with females' need for environmental cues unique to shoreline habitats, and of direct relevance to the maintenance of ovarian synchrony over wide areas.

In recent decades, researches have substantially increased our understanding and awareness of biological clocks (Cloudsley-Thompson 1980). Almost every living organism can become entrained by – locked in step with – oscillations in its natural environment, provided these have set up selection pressures which have endured for long enough for the capacity to evolve, and provided social factors such as the prevailing mating system do not produce counteracting pressures to avoid synchrony.

Tidal movements provide an extremely reliable, predictable environmental rhythm (or *Zeitgeber*, to use the technical term) for all organisms which live close to large expanses of water (Cloudsley-Thompson 1980: 74, 75, 91–5).

Tides are of course a lunar effect, spring tides occurring twice a month – at full and again at new moon, when the gravitational pulls of both moon and sun combine. Contrary to some classical writers including Darwin (1871: 1, 212n), most terrestrial mammals have physiological cycles showing no link at all with such rhythms. Primates, however, have menstrual cycles. Whilst with few exceptions these are not in fact genuinely lunar, there is at least the basis for tidal phase-locking here – should selection pressures act in that direction powerfully enough and for sufficiently long. For what it is worth, a minor medical study of human births at St Thomas'

Hospital, London – chosen for the analysis because of its situation beside the tidal Thames – showed significantly more deliveries at the flood tide than at the ebb (Rajasingham *et al.* 1989). Confirming that the tides may have been directly involved, a similar study at a hospital 3 km from the river showed no such effects (Chamberlain and Azam 1988). Such isolated findings of 'lunar effects' using small samples are notoriously variegated and unreliable, however (Rotton and Kelly 1985; Culver *et al.* 1988). In the absence of improved statistics, any grounds for suspecting tidal selection pressures operative in the evolution of the human menstrual cycle must come from logical considerations based on rather different kinds of data.

The most important logical consideration is simply that any form of sustained and generalised reproductive or menstrual synchrony would require a reliable external cue. Many environments – such as the floor of a tropical forest – would not provide environmental cues of the kind necessary for consistent menstrual synchrony to work. The open and often moonlit shores of lakes and seas would provide such cues. In this context it seems worth recalling that the theoretical underpinnings of Turke's theory rest essentially on Nancy Knowlton's (1979) studies of *shrimps*, her findings constituting perhaps the only conclusive demonstration yet made that females can impose monogamy on males by totally synchronising their reproductive cycles . In the case of shrimps, at least – as of prawns, sea-horses and many other sea creatures (Cloudsley-Thompson 1980: 91–8) – it is definitely tidal and/or directly lunar influences which provide the proximate cues necessary for females to synchronise with impressive precision.

In the case of evolving human females, we can construct a narrative along similar lines. *Females who synchronised to escape monopolisation by alpha males found themselves drawing on tidal cues.* The earliest hominids, we have seen, arose and for several million years evolved in the Rift Valley and along the shores of the Afar Gulf. Assuming that females were already tending to synchronise for sexual-political reasons (Turke 1984), the ovarian cycles of closely associated females in this setting could hardly have escaped selection pressures to mesh in with the movements of the moon and any tidal rhythms, however slight. *Indeed, we might even turn matters the other way around, and suppose that it was some kind of tidal effect which provided the necessary cue for ovarian synchrony on Paul Turke's model to become set up in the first place.*

Direct lunar/tidal influences on the human body (for a survey of the medical and psychological literature see Rotton and Kelly 1985; Culver *et al.* 1988; Kollerstrom 1990) are at best weak (see the following section). Even on clear nights or at the sea's edge, they would be easily overridden by other factors such as those of sexual politics. Regardless of the moon or the tides, synchrony would *not* occur if the prevailing mating system rendered it maladaptive.

But conversely, if coastal females were beginning to synchronise with each other for their own sexual-political reasons, then any external cues with an appropriate periodicity would automatically have acquired special significance. If it was important not only that local groups synchronised on a local basis, but also that neighbouring groups synchronised to a single schedule over a wide area, then it would have become vital for all to converge around a shared external rhythm capable of acting as a 'clock'. It would hardly have mattered how weak were the signals – if females needed to receive them, then selection pressures would have acted powerfully to develop the necessary senses. Oysters, shrimps, prawns and other natural organisms which synchronise through internal body-clocks must continuously reset these using environmental cues – which may be vanishingly weak to human senses. Deprived of such cues, the organisms tend to drift out of phase over time (Cloudsley-Thompson 1980: 6–21), although sea-horses rather amazingly continue to lay their eggs at full moon even when in laboratory tanks, deprived of any evident source of information on the lunar cycle. (For a discussion of this and other examples see Kollerstrom 1990: 157.) When the early human populations of this book's narrative migrated along or up the sides of the Rift Valley, eventually ending up far from coastal shores, faint tidal effects in lakes or even direct cues from the moon itself would almost certainly have proved sufficient to preserve the synchrony essential to their mating system – although direct lunar cues would have been weaker and less reliable than tidal ones, the moon's light being blocked out during periods of thick cloud.

In this context, the ovarian synchrony model is strengthened by the fact that the human female menstrual cycle – virtually alone among primate cycles – is a body-clock with precisely the correct average phase-length to enable lunar/tidal synchrony to be maintained.

Lunar Cycles

If the human menstrual cycle were genuinely linked with the moon it would be rather surprising, for such a correspondence is not normal, either for primates or for mammals in general. Although many invertebrate marine animals, certain fish and some frogs and toads concentrate their reproductive activities at specific lunar phases (Bunning 1964; Cloudsley-Thompson 1980: 90–100), few terrestrial mammals appear to be in any way synchronised with the moon.

We saw in Chapter 6 that hamadryas baboons synchronise not only within harem units but also more widely, a degree of synchrony characterising whole bands and even troops (Kummer 1968: 176–9). However, there is no generalised synchrony: troops in different localities are cycling on different schedules from one another. Hamadryas baboon menstrual cycles are longer than those of humans – on average between 31 and 35 days (Hrdy and Whitten 1987: 372–8) – which suggests that neither lunar changes nor the tides have entrained baboon cycles to a locality-independent fixed rhythm, at least not in evolutionarily recent times.

Mating at full moon has, however, been reported of certain diurnal prosimians, such as *Lemur macaco*. In one often-quoted study (Cowgill *et al.* 1962), researchers reported that of 15 matings observed in their laboratory, 13 fell within 5 days before or after a full moon. Their lemurs' first three oestrous periods, after they were moved from Madagascar to the northern hemisphere, were out of phase, perhaps because of disorientation caused by travel, but after this, six of seven oestrous periods overlapped a day of full moon.

Alison Jolly (1967: 3–14) subsequently studied wild lemurs of a different species, *Lemur catta*, and concluded that evidence from the wild 'agrees with the moon hypothesis, but hardly proves it'. In each troop, the females came into oestrus annually, mating for a few days, all at the same time, the period roughly overlapping with full moon. But this may have been coincidental, and Jolly (1967: 13) simply concludes that 'there is a challenge to elucidate the mechanism of synchronous breeding, whether social, by day length, or by the moon, as well as the evolutionary function of synchronous breeding.'

Isolated reports aside, consistent lunar synchrony appears to be rare among primates and perhaps non-existent. Neither do many primate cycle lengths match the 29.5 day duration of the synodic lunar month (see Table 7.1).

It could be argued that the figures for primate cycle lengths indicate a roughly lunar/tidal pattern, but if so there are numerous divergences. Any hypothetical lunar baseline would have to be seen as a trait which has been largely overridden in the course of primate evolutionary speciation. From the table it would appear to be principally the smaller primates which have departed most radically from what one might suppose to be a rough 28-day to 30-day norm.

In humans the situation is intriguing. Although few contemporary western women cycle in a way which has anything to do with the moon (see next section), woman's reproduction physiology differs from that of most other primates in being theoretically consistent with tidal synchrony. The key biological condition for synchrony of successive cycles is of course that the cycle length should match the moon's. In women, this condition is met with precision.

Among the most careful investigations of human menstrual cycle length ever conducted was that of Gunn and associates (1937); their data, when properly arranged, gave a mean of 29.5 days (see Arey 1954; Menaker and Menaker 1959; Menaker 1967; Criss and Marcum 1981; Dewan *et al.* 1978). This is exactly the length of time it takes for the moon to pass through its phases as seen from the earth. The figure has been confirmed by Treloar (1981; Treloar *et al.* 1967), who compiled well over 270,000 cycle lengths of

Table 7.1 Cycle lengths in non-human primates

Species	Cycle length (days)
Ring-tailed lemur	39
Tarsier	24
Common marmoset	15–17
Lion tamarin	14–21
Goeldi's marmoset	21–24
Red howler	16–27
Squirrel monkey	7–25
Gray langur	27
Barbary macaque	31
Rhesus macaque	29
Japanese macaque	28
Vervet monkey	33
Talapoin	33
Patas monkey	32
Yellow baboon	32
Olive baboon	31–35
Chacma baboon	31–35
Hamadryas baboon	31–35
Gelada baboon	35
Lar gibbon	30
Orangutan	31
Common chimpanzee	37
Pygmy chimpanzee	28–37
Mountain gorilla	28
Lowland gorilla	31

Source: Hrdy and Whitten (1987: 372–8).

women throughout all ages of reproductive life. In a more recent study, Cutler *et al.* (1980) again confirmed the 29.5 day average.

In western contexts, human menstrual cycle lengths vary widely. Only about 28 per cent of reproductively active women show a 29.5 ± 1 day cycle length. On the other hand, cycles of this length tend to be the most fertile ones (Vollman 1968, 1970, 1977; Treloar *et al.* 1967, Treloar 1981). The finding that there is a positive correlation between fertility and precision of lunar phase length has been described as an 'intriguing biological coincidence' (Cutler *et al.* 1987).

A related finding is that heterosexual women who have regular weekly sex tend to have significantly more 29.5 ± 3 day cycles than women who have either sporadic or celibate sexual patterns (Cutler *et al.* 1979a, 1979b). According to Cutler and her colleagues (1987), weekly sex is usually sufficient to set up 29.5 ± 3 day cycles. Male pheromones may be involved (Cutler *et al.* 1986; Preti *et al.* 1986).

Lunar Phase-locking: Negative Evidence

Much evidence suggests that if women needed to phase-lock themselves to a lunar schedule and could set up the appropriate conditions for this, physiology would do the rest. In other words, there is nothing in women's genetic constitution to prevent the moon from acting as a *Zeitgeber* (exogenous synchroniser) of their cycles. Whether or not the moon in practice acts in this way depends on many factors – above all, it would seem, on the prevailing system of kinship and marriage, which may either sustain or preclude the possibility of women's remaining in close contact with one another after marriage.

The fact that women *in contemporary cultures* fail to synchronise is well established. In an analysis of 11,807 menstrual onsets at the turn of the century, the Swedish researcher Arrhenius (1898) concluded that these were more frequent while the moon was waxing than when it was waning. But nearly forty years later, when Gunn and associates (1937) analysed 10,416 menstrual events, they expressed their disappointment in having to conclude that there was no justification for asserting any connection with the moon. A more recent investigation by Pochobradsky (1974) analysed over 6,000 menstrual onsets, mostly of women living in Czechoslovakia, and concluded likewise that 'women in the study menstruated and ovulated independently of the phases of the moon'.

There is of course the theoretical possibility that non-western statistics would produce different results. In a study conducted in 1982 (Law 1986), 826 young female volunteers with normal menstrual cycles living in Beijing and Guangchow in China were asked to record their cycle lengths and dates of menstrual onset over a period of four months. It was found that 'most menstruations occurred during 4 days around the new moon'. This was quite a strong statistical effect. When the lunar month was divided into equal periods averaging four days each, the results broke down as shown in Table 7.2. In other words, over twice as many menstruations occurred during the new moon four-day period as during the four-day full moon period. But until comparable studies have confirmed such results, few conclusions can safely be drawn.

One female researcher in the United States has regularly found the opposite effect to that claimed by Law (1986), women in her sample showing a tendency to menstruate at full moon whilst ovulating at new (Cutler 1980a; Cutler *et al.* 1987). A possibility is that these women were more subject to artificial lighting than the Chinese subjects studied by Law, but even then, the results would seem puzzling. In any event, there is a clear need for large-sample statistical studies which control for variables such as

Table 7.2 Correlation of menstrual onsets with lunar phase

Four-day period	No. of menstruations	%
New moon	234	28.3
New moon: first quarter	104	12.6
First quarter	87	10.5
First quarter: full moon	77	9.3
Full moon	95	11.5
Full moon: last quarter	83	10.0
Last quarter	77	9.3
Last quarter: new moon	69	8.5

Source: Law (1986).

exposure to artificial light. None has so far been conducted. Ethnographic data of possible relevance to this issue will be surveyed in later chapters of this book.

Birth Records and the Moon

If women tended to menstruate at around new moon, they should give birth at full moon, the mean length of pregnancy measured from the last menstrual onset being nine synodic months plus a half. Again, there is no evidence that this happens.

The best-known studies to test this in a western context were carried out in the 1950s and 1960s (Menaker and Menaker 1959; Menaker 1967). Records of half a million live births in New York City between 1948 and 1957 showed more births occurring in the half-cycle centring on full moon, although there was only a 1.35 per cent difference between the figures for the two half-cycles. Taking half a million births between 1961 and 1963 in the same city, it was again found that more births occurred in the half-cycle centred on full moon. In this case, however, the two half-cycles differed by only 1.01 per cent, falling only just within the standard (1 per cent) margin of statistical significance.

Others (Osley *et al.* 1973) later reported similar results. But daily birth data for the years 1972–3 compiled from the records of the Vancouver General Hospital gave no indication of a birth peak related to full moon (Schwab 1975). Still more recently, a French team (Guillon *et al.* 1986) looked at hospital records of almost 6 million births in France between 1968 and 1974. They found a slight tendency for more births to occur during the dark moon and fewer to occur during the moon's first quarter – a finding in conflict with those of the Menakers.

Possible Photic Entrainment of the Menstrual Cycle

We have examined statistical studies of women in modern industrialised cultures, and have found no evidence of a significant correspondence between lunar phase and events in the menstrual cycle. However, a circalunadian version of Paul Turke's synchrony model would not predict this. Modern cultural conditions are unlikely to resemble even remotely the conditions of shore-dwelling evolving protowomen in the East African Pleistocene. The model would predict merely that modern women should be found to possess the physiological capacity to synchronise with one another through the moon – given ideal conditions of exposure to moonlight, to the tides or to an appropriate artificial cue. Falsification of the hypothesis would require medical evidence that women lacked such physiological potentialities. It is to this question that we now turn.

In a pilot experiment to test for the effects of nocturnal light on a human female, Dewan and Rock (1969) subjected a 26-year-old woman to overnight lamplight from days 14 to 17 of her cycle (day 1 being that of menstrual onset). She had to keep her room quite dark during sleep for the rest of the month. Under this treatment her cycle, which had been varying between 33 and 48 days, regularised to between 29 and 31 days. To check that this was not coincidental, several women were then subjected for a few months to a similar regimen of nocturnal light while also supplying control data (the subjects receiving no nocturnal illumination). As before, a 100W lamp-bulb was kept on once per month overnight, from nights 14 to 17 of the women's cycles, this being 'an artificial simulation of the effects of full moon . . .' (Dewan *et al.* 1978: 582). Again, the treatment worked: eight of eleven subjects showed a narrower range of cycle lengths than when not manipulated, a quarter of the experimental cycles achieving a lunar cycle length.

The theory that light triggers ovulation seems well-founded, and would accord with the Chinese findings noted earlier (Law 1986). This would mean that under ideal conditions, ovulation should occur at full moon, menstruation at new.

The moon's light is some 300,000 times weaker than the sun's, and also many times weaker than the 100W bulb used in Dewan's experiments. Theoretically, however, this need pose no problem. Studies of humans living artificially in near-total darkness have shown that quite miniscule amounts of light are sufficient to entrain the body-clock which regulates the daily alternation between sleep and wakefulness (Moore-Ede 1981).

Humans are of course primates, whose ultimate ancestors were nocturnal, arboreal insectivores resembling tree-shrews. High up in trees, their periodic exposure to moonlight may have been adaptively significant. For example, movement through branches could well have been impeded on moonless nights, so that courtship behaviour tended to intensify during the better-lit nights around full moon. All this could help explain what may turn out to be a baseline of lunar periodicity beneath the variability of primate reproductive physiology as a whole. Whilst all this is speculative, we do know that in Malayan forest rats there is a strong tendency for conceptions to be most frequent in the period before full moon. This is true for the nocturnal forest species and, to a less marked degree, for house rats and the rats on an oil-palm estate, but not for day-active forest squirrels (Cloudsley-Thompson 1980: 100, citing Harrison 1954).

The hormone melatonin has been shown to inhibit ovulation in rats as well as in some monkeys. The synthesis of this hormone is inhibited by light (references in Dewan *et al.* 1978). Consequently, it has been argued that a possible mechanism for ovulation-synchronisation by means of exposure to nocturnal light exists (Dewan *et al.* 1978). If true, this would mean that early hominids in the Rift Valley could have standardised their synchrony even hundreds of miles from tidal shores, merely by sleeping out under the moon. Once again, however, it must be stressed that we have no independent evidence for this. The nearest we have to evidence is the fact that the menstrual cycles of modern females are clearly light-sensitive and have the same average phase-length as the moon.

The Moon and Culture: Some Hypotheses

J. L. Cloudsley-Thompson (1980: 100) is a leading authority on biological clocks. He keeps an open mind on the evolutionary origins of the human menstrual body-clock, declining to rule out the possibility that it may be the manifestation of what was once a true circalunadian rhythm. He suggests that a civilised, indoor life with artificial lighting may now be the factor which prevents most women from synchronising. This would make contemporary women's typical failure to keep in step with the moon 'unnatural' – a product of civilised artificial lighting and culture.

Unfortunately, this view is contradicted by persuasive evidence that a randomisation of menstrual cycles with respect to the moon has long been typical in most known human cultures, whether 'civilised' or not. Such evidence suggests that it is mating systems – what social anthropologists term 'systems of kinship and marriage' – which are the primary determinants, not light availability considered in isolation.

The social anthropologist Tim Buckley's (1982) work on the Yurok Indians of California will be examined later in this book, since it indicates the possibility of a Yurok tradition of widespread lunar phase-locking which broke down at some point in the fairly recent past. It may well be that similar patterns of synchrony were widely prevalent among Amerindians and others until quite recently, and indeed some evidence of this will be surveyed in Chapters 9–14.

Nonetheless, such patterns were not in recent times universal and probably would have been unusual even among those hunters and gatherers least influenced by western culture over the past few centuries.

Given Aboriginal Australians' long resistance to farming and horticultural influences, their continent might have seemed a good place to look for synchrony, yet it is certainly not the case that all Aboriginal women everywhere synchronised their periods with one another or with the moon in the recent pre-contact period. There is in fact little evidence for synchrony except in coastal Arnhem Land, where traditions of synchrony linked with 'the rainbow snake' have lived on until recent times, in Western Australia in the form of certain suggestive rock-paintings, and in Central Australia where myths depict synchrony as a basic feature of the ancestral Dreamtime (see Chapters 12–14). Such evidence is of course significant. But if a pattern of maintaining synchrony were once widespread, it must have started breaking down in Australia as in most other parts of the world millennia before the emergence of modern civilisation or the sustained use of artificial lighting.

One conclusion which adherents of the lunar hypothesis might draw is that gender politics and mating systems have in most regions changed dramatically since the Late Pleistocene, and that the pressures these exert have always overridden all other factors in determining whether synchrony will occur.

A slightly different view has been put forward by Cambridge experimental psychologist Nick Humphrey, a figure well known for his pioneering work on the social functions of primate intelligence (Humphrey 1976; see Byrne and Whiten 1988). Humphrey's hypothesis was put forward to explain the results of a survey which he had helped to conduct.

Together with a third-year undergraduate assistant, Humphrey (1982) asked 500 students, 150 of them women, to indicate the phase which they believed the moon to be in on that particular day. The women among them were also asked to say when their last menstrual period had occurred. The results were surprising.

The answers concerning the moon were quite wrong. The students had no real idea which phase the moon was in. Two-thirds said it was waning, when in fact it was waxing. Men had a particular tendency to view the moon as waning. But strangely, women's answers bore a systematic relationship to the positions they were in within their menstrual cycles. In the words of Humphrey (1982):

> Around the middle of the cycle, around the time we would guess they were ovulating, they showed a strong shift over to seeing the moon as waxing – a very significant effect. And during the menstrual period they showed an even more significant tendency than men, who were the control group, to seeing it as waning.

Interestingly, this was only true of women who were having normal cycles; those who were on the contraceptive pill – who were menstruating every month but not producing an egg – did not show the effect at the middle of the cycle, although they did show it at menstruation. As Humphrey himself stresses, it would seem difficult to explain these findings except on the assumption that women entrained their cycles to the moon's phases at some time in their evolutionary past.

From all these studies, the conclusion which emerges is that women are probably capable of entraining their cycles to the moon's phases, but conditions have to be ideal. If the moon's weak light were to be found entraining women's cycles unconsciously and automatically under artificially lit modern conditions, it would be surprising indeed. The evidence is that it does not happen, or happens so rarely and unpredictably as to amount to near-suppression of any 'lunar effect'.

Early humans sleeping in trees overhanging expanses of water in tropical Africa would doubtless have been more easily influenced – if, that is, light from the moon or tides can affect the human metabolism at all. But even in their case, sexual politics acting in a negative direction would have overridden any synchronising effects. If Foley (1987) is correct and early hominids were unavoidably organised in one-male harem units, then widespread synchrony would have been maladaptive, since it would have intensified harassment by setting female harem members in severe sexual conflict with one another for the few available males (Chapter 6). Females aiming to minimise such harassment within the constraints set by the system would have done best to ignore any lunar or tidal environmental cues.

On the other hand, in those 'multi-male' cases where such constraints had been overridden and synchrony was occurring, evolving humans are unlikely to have been passively reliant on the tides, the moon or any other cues. Some female populations would have been synchronising widely, others would have been synchronising less consistently. The physical impact of moonlight or of tidal cues would have been quite secondary in deciding such matters. If certain populations of protowomen were synchronising consistently not only with one another but also with the moon and tides, it would not have been because in the localities concerned such environmental rhythms were so powerful as to entrain or enslave women. It would have been because detecting the appropriate cues in order actively to synchronise was in these particular females' sexual-political interests.

We know from Chapter 6 that had evolving human females needed synchrony sufficiently, they would have maintained it at least on a local level even far from the coasts or under cloud-covered skies. On the other hand, where females had access to the appropriate cues and were required to synchronise, but lacked the resources to sustain their spatial proximity, problems would have arisen. Evidence from contemporary hunter-gatherers suggests, as we will see, that as anatomically modern women left their former coastal environments for the continental hinterlands in the course of the Upper Palaeolithic revolution, they did encounter severe problems. Their vegetationally sparse new habitat was not capable of sustaining large groups of females all foraging together within a restricted area. On the other hand, they needed a solution which enabled them to survive in this habitat without leaving their ancient traditions of tidal synchrony behind. Since synchrony's old conditions were vanishing, anatomically modern protowomen had to seek ways of preserving their menstrual and reproductive harmony – their 'witchcraft' or 'magic', as it would become conceptualised – in novel ways. In the end, they broke their umbilical cords, abandoned their ancient shoreline habitats – and in the new situation used massage, sweating, ritual bathing, dance, night-long firelight and moon-scheduled celebratory sexual intercourse to augment any effects that nature's weakened clocks on their own might have had. Using such extraordinary new 'artistic' devices as body-paint, sound-making instruments and elaborate choreography, they sustained and intensified their synchrony to the point where the harnessing of male provisioning energies could match the challenges of the new environment in which they lived, releasing child-burdened females from the need to find their own food for themselves. It was in the course of this woman-inspired process that symbolic culture – forged centrally in what social anthropologists term 'the ritual domain' – was at last born.

Chapter 8
Between Water, Stone and Fire

> No social order ever disappears before all the productive forces for which there is room in it have been developed; and new, higher relations of production never appear before the material conditions of their existence have matured in the womb of the old society. Therefore, mankind always sets itself only such problems as it can solve; since, on closer examination, it will always be found that the problem itself arises only when the material conditions necessary for its solution already exist or are at least in the process of formation.
>
> Karl Marx, Preface to *A Contribution to the Critique of Political Economy* (1859)

The emergence of human culture was a revolutionary event. To say this is not new: a succession of authoritative writers have spoken of 'the human revolution' in this context (Hockett and Ascher 1964; Montagu 1965; Holloway 1969; Collins 1976; Mellars and Stringer 1989). Until very recently, however, the idea has seemed less than convincing. The concept of revolution has seemed to be belied by the extreme gradualism of the prevailing palaeontological and archaeological scenarios (Chapter 5). If a human way of life began to be established in the Plio-Pleistocene, yet was still being established two million years later towards the end of the Pleistocene, how can this lengthy and very gradual process be termed a 'revolution'? Can a revolution last two million years?

As we have seen, however, the dates no longer pose such a problem. The scenarios of the 1970s and early 1980s are now largely discredited. Few believe any longer in the gradualist theory of a two-million-year long epoch of 'steady progress' towards a human lifestyle. It is now widely agreed that in the million and more years prior to the Upper Palaeolithic, any discernible cultural advance or 'progress' was in most areas exceedingly and indeed quite astonishingly slow (Binford 1984). Since late in the 1980s, on the other hand, molecular biologists have been producing exciting new evidence that all anatomically modern, symbolic-culture-bearing humans are the genetic descendants of a single fast-developing sub-Saharan African population which first appeared only some 200,000 years ago (see pp. 269–72). This new information makes postulated events in the distant Plio-Pleistocene now seem rather less relevant.

A Recent Perspective

In this and the following chapters, it is not intended to dwell further on the early biological preconditions of the human revolution. Although key elements of Turke's sociobiological model will be drawn on, it is intended to focus on the social processes underpinning the later, cultural, stages – the development of variegated tool-kits, of logistic big game hunting, cooking, systems of notation, art, dance, music, ritual and, in short, the final, stable establishment of a cultural way of life in its fully modern, symbolic, form some 45,000 or so years ago.

Compared with Plio-Pleistocene frameworks, this recent perspective imposes fairly rigid constraints upon the weaving of 'just-so' stories. Speculative narratives whose only requirement is to conclude with a picture of the known end-result can be quickly dismissed; we have far more solid information on the various stages in the transition to modern humans than for any of the earlier major transitions in the hominisation process (Pilbeam 1986; Mellars 1988). We can test our models because, firstly, modern humans are still living today so that our biological make-up can be directly studied, physiologically, psychologically, sociobiologically and in other ways. Secondly, the fossil record for the Late Pleistocene is quite good. Thirdly, archaeological finds – including evidence for the appearance of self-adornment, burial practices, ritual and art – constitute potentially decodable messages yielding information on at least some aspects of the symbolic and social structures of the prehistoric communities we are interested in.

Fourthly, a focus on modern humans renders hunter-gatherer studies fully relevant for the first time, so that social anthropologists' cross-cultural findings can act as a further check on our model-building. Whilst no surviving human culture can constitute a model of earliest *sapient* life, it is not unreasonable to suppose that certain recurrent patterns – for example the striking near-universality of 'classificatory' modes of reckoning kinship, or the prevalence of mythological patterns of something like the kind isolated by Lévi-Strauss in his *Mythologiques* – convey information on traditions stretching back to the last ice age. Actual human social formations have certainly changed and diversified virtually limitlessly since that time, but it is also true that cultures can resist change to an astonishing extent, particularly where religious ideology is concerned. An extreme example is the Northern Australian Aboriginal cult of the rainbow snake, chronicled in rock art as extending in an unbroken tradition for up to 9,000 years (Flood 1983). In the concluding chapters of this book, we will draw heavily on evidence of this kind.

Tool-making: The First Two Million Years

Stone tool-making, fire-tending and an increasing dependence on meat food were central to the human revolution. However, in their earliest manifestations these potentially momentous developments apparently did little to revolutionise social and political life. Rather, it seems that for millennia, our ancestors maintained sociopolitical continuity with their primate past – at the cost of missing out on the full potential of the technological advances they were experimenting with. It was to be two million years after the first stone tools and perhaps a million or more from the harnessing of fire before the evolution of technology, physiology and brains would eventually create the material conditions for a breakthrough to symbolic culture.

The tool-making traditions of the Lower Palaeolithic are known as Pre-Oldowan, Oldowan, Developed Oldowan, or (more generally) 'chopper-core' industries. Modified beach pebbles, such tools were of all shapes and sizes: there was no symmetry and no repertoire of standardised patterns. These first tools, according to one psychologist's report (Wynn 1988: 277), 'do not argue for an intelligence greater than that known for apes'. About a million and a half years separates the first of these industries in East Africa from Oldowan industries of Middle Pleistocene date. Crudely flaked pebble implements characterised the beginning of this immensely long period, beginning anything up to about 3.0 million years ago; tool-kits of essentially the same type were still dominant at the end of it. In the words of one specialist 'It is difficult to comprehend such slow development. Man had certainly evolved physically: he was now bigger both in stature and in brain capacity' (Wymer 1982: 98) The puzzle is to explain why, despite substantial biological evolution, very little technological advance appears to have taken place in all this time.

About 1.4 million years ago however, an industry appears with implements known as 'hand-axes'. These tend to be well-made, symmetrical, bifacial tools of pointed or oval shape, all made to a standardised pattern. Unlike the pebble tools, their manufacture has been estimated to have required levels of intelligence far beyond that of any ape (Wynn 1988).

The hand-axe traditions are known as 'Acheulean', and are the characteristic products of *Homo erectus* (who first appears in the fossil record of East Africa about 1.7 million years ago), although it is again significant that technological evolution clearly lagged behind biology, the earliest hand-axes dating back to 1.4 million years at most. Hand-axe-using groups seem to have begun moving out from Africa into southern Eurasia about a million years ago – about 500,000 years after the first appearance of hand-axes in the archaeological record.

The most extraordinary feature of the Acheulean hand-axe tradition is its monotonous uniformity. It might have been expected that local conditions – the availability of different plant resources or species of game, for example –

would have given rise to specific, localised methods of foraging, in turn reflected in locally distinctive specialised tool-kits. But instead, the same basic design for a 'hand-axe' is replicated unimaginatively all over the world – from southern Africa to northern England, from Spain to India.

Most specialists admit to having little idea of the function of these tools. The puzzle has indeed been described as 'the greatest enigma of Lower Palaeolithic archaeology' (Wymer 1982: 102). The problem would lessen if it could be demonstrated that these implements facilitated greater hunting success. But no such evidence exists.

Hand-axes were not good hunting weapons. Too heavy to be hafted to spear-shafts or thrown, they do not even look like particularly good cutting tools, although at least occasional involvement with the butchery of elephants and other large *scavenged* animals has been documented (Binford 1987; Villa 1990: 302n). Some of the large, pointed hand-axes could also have been used for digging – but as Wymer (1982: 103) points out, 'experiments show that they are not much use in this respect, and far less efficient than a suitably shaped stick'.

The commonest form of hand-axe is a very small, poorly made tool that 'does not look useful for anything' (Wymer 1982: 103). Yet in certain levels at some sites – for example Olorgesaile in Kenya, and Swanscombe in England – such axes are extremely numerous, almost to the exclusion of any other implements (Wymer 1982: 103, 106). One theory is that the 'axes' were not primarily tools at all – their basic function was to act as a source of flint from which to chip off usable, sharp-edged flakes from time to time (Hayden 1979).

It used to be argued that because they are so stereotyped, hand-axes provide evidence for true cultural life. Their standardised symmetrical shapes, according to a well-known formulation of this idea (Holloway 1969, 1981), represent the human collectivity's imposition of 'arbitrary form' upon the environment, indicating the presence of hominids capable of constructing and enforcing grammatical, social, moral and technological 'rules'. Other authors – such as Jolly and Plog (1986: 289) in their popular textbook on archaeology and evolution – envisage *Homo erectus* possessing not only hearths and home bases but also 'language, ritual, complex social relationships, and refined tool-making techniques'. They base such inferences in part on the uniformity of hand-axe designs; this is stated to be a sign of cultural-level learning, transmission and diffusion of techniques and traditions.

Such interpretations are almost certainly wrong. If the hominids of this period were cultural, we would expect uniformity on some levels – but also much greater diversity on others.

Had the hand-axe makers been involved in a truly collectivist, cultural framework of action, this would have released individuals from the need to replicate one another's activities: numerous different roles could have complemented one another in the joint pursuit of common goals. Moreover, with sufficient co-operation and trust, there is no fear that particularly valuable tools will be appropriated by some competitor or rival. There is no need to carry tools on one's person at all times – even precious implements can be left at caches or with trusted allies or kin until they are needed. Again, this allows for a much more variegated community-wide tool-kit than when each individual must guard against theft and carry everywhere a full personal survival kit. Collectivity, in other words, means less need for 'all-purpose' tools. There is no need for each individual to limit the tool-kit to the personally required bare essentials – to one or a few multi-purpose tools portable enough to be kept close to the body and guarded at all times.

Finally, although a cultural framework implies standardisation of tool-kits within local communities, it also produces wide diversity between distant communities as these adapt in different ways to contrasting local conditions. The same factors also lead to relatively rapid stylistic and other changes over time. We do not expect to find tools of essentially identical design being replicated over an area stretching from Britain to India for over a million years. The homogeneity and conservatism of the hand-axe tradition suggests 'that it as yet retained the character of a general species-specific behaviour, not subject to cultural level processes of stylistic differentiation, formal classification and fairly rapid change' (Richards 1987: 281; see also Binford 1989: 28–9).

Despite important advances (for example Villa 1990), the problem of explaining the hand-axe traditions has still not been solved. What we do know is that with the arrival of the Neanderthals, a wide variety of standardised tool shapes for the first time began to evolve, and that with the emergence of modern humans, this variety increased radically whilst hand-axes totally disappeared. The efficient and co-operative hunters and gatherers of the Upper Palaeolithic had no use for such implements at all.

To survive for a million years with basically the same technology can be seen in its own terms as no small achievement. Cranial capacity increased by about 20 per cent over those years, so presumably social complexity was also increasing. But by cultural-historical standards, the hand-axe people appear locked in a kind of 'time-warp', incapable of more than a snail's pace of technological advance. It is impossible to avoid the question: Why?

In the light of the primate evidence surveyed in Chapters 4 and 5, we can glimpse the outlines of an answer. If Lower and Middle Pleistocene hand-axe makers were socially and sexually organised in *anything like* the manner of baboons or chimpanzees, the problems posed by a weapons-technology would have been daunting. We have only to imagine Goodall's 'Satan' equipped with a hand-axe to appreciate this. The danger would have been that hand-axes or other weapons would have been used not as 'collective hunting implements', and not only as all-purpose tools for cutting and pounding, but from time to time also as instruments for settling scores, as males battled with one another for meat and for access to females along the lines which Parker (1987) indeed suggests. Our ancestors' evolving weapons technology would then have been turned dangerously inwards, instead of being directed outwards towards external nature as it is (at least for the most part) among modern human hunters and gatherers. Wymer (1982: 106) may be hinting at this sombre possibility in writing in this context that 'tradition may have outweighed rational behaviour'. He continues:

> There are so many puzzling factors about hand-axes that the answers may well be outside a straightforward, rational explanation and lie in the realms of human behaviour rather than function.

When hand-axes were first discovered in European gravel pits, they were popularly described as 'fighting stones'. Wymer comments that the implements 'would have been useless as hand weapons, unless hunters were fighting each other, which may have occasionally happened' (Wymer 1982: 103). Whatever its scientific merits, the idea that hand-axes were used in fights has always had a certain popularity (see, for example, Lorenz 1966: 208).

Archaeologists are not usually trained to think in sociobiological or primatological terms. But perhaps they have been mistaken to assume that every 'human' artefact-type must have had a positively useful 'function' in relation to 'the species' or 'the group'. Group functionality may be relevant once the cultural-symbolic stage has been reached, but there is little to suggest that tool-makers during the Middle Pleistocene were 'cultural' in anything like a modern sense (Binford 1989). Consequently, a wholly different conceptual framework seems to be required.

If we are dealing with a non-cultural evolutionary process, then it seems appropriate to use a sociobiological approach. We should set out from the individual as the unit of selection, not 'the species' or 'the group'. In this context, we should ask how possession of a hand-axe might have contributed to an individual's genetic fitness.

Homo erectus was a heavy-faced, large-jawed creature with enormous brow ridges (Collins 1986: 149–50). Compared with both *Australopithecus* and modern humans, his skull was extraordinarily thick: 12.5 mm in the case of the Swanscombe occipital, 11 mm for the parietal. Values for Zhoukoudian exceed 18 mm on occasion, whereas the figures for most modern humans are little over a third of this (Collins 1986: 148–9, and references).

No doubt the need for heavy chewing and use of the teeth as tools was partly responsible for the large teeth and jaws, but why was 'Beijing Man's' skull in places three times as thick as ours? Like the massive brow ridges, this feature suggests at least some function in terms of self-protection, possibly in the context of occasional fights. Some fighting does not seem intrinsically improbable: sexual dimorphism was by modern standards pronounced, a fact which has led many writers to infer that *Homo erectus*, like other early hominids, had some kind of polygamous mating system in which the more dominant males gained access to the most females (Foley 1987: 171 and references; Parker 1987). Could it be that the million-year-long Acheulean tradition represented a period in which, in many localities, every male simply 'had to' be the owner of a hand-axe or other weapon, as much for reasons of personal and sexual security as to facilitate hunting or foraging?

We do not have to envisage constant *Homo erectus* violence for an interpretation along the lines suggested here to seem persuasive. Although there has been a long-standing controversy over 'Beijing Man's' alleged 'cannibalism' (Poirier 1973: 140; Binford 1981), hand-axes are not found at Zhoukoudian, nor at other far eastern *Homo erectus* sites. Yet it is these specimens of *Homo* which have the thickest skulls of all. Perhaps the Eastern groups used weapons made of materials which have not survived. Where these *or* stone hand-axes were used, they may well have been all-purpose tools, used for opportunistic hunting, butchering and various other activities, but capable of being used in self-defence when necessary.

All this would fit well with Parker's (1987) 'sexual selection' model of hominid evolution (Chapter 5). In the light of all that we know of the size, shape and distribution of hand-axes, and in the light of what seems to be a picture of social and economic near-stasis throughout this immensely long period, it is a tempting (even if only partial) explanation. It links a plausible set of productive and other functions for these strange tools with male behaviour of a kind which does not seem too difficult to envisage, which is familiar from an indefinite number of primatological accounts, and which – in the period preceding the 'human revolution' – might well have constituted a sexual-political brake upon social and economic development.

Fire

The development of pyrotechnology presents a similar set of puzzles. Eventually, as we will see, fire proved an important factor assisting proto-women in defining and defending their own domestic space, this achievement in turn underpinning the immense sexual-political and symbolic changes associated with the Upper Palaeolithic revolution. But it is surprising how long it was before females apparently succeeded in making full use of cooking fire as an economic and political resource.

Except in northerly regions and tropical rain forests, fire is one of the natural hazards which most animal life must periodically face. Bush-fires are particularly common in the drier savanna regions of tropical East Africa (Foley 1987, citing Harris 1980). For most animals, such fires are extremely frightening, the only appropriate response being flight. But this is not always so. Even when flames are raging through the bush, falcons and kites may hover over them to hunt fleeing birds and insects. Later, quadruped predators visit the smouldering remains in search of prey; and later still, ungulates venture near to lick at the salted ashes. 'Most animals', comments Goudsblom (1986: 518–19), 'enjoy the warmth radiated at night by the site of an extinguished fire'. For early hominids, the task would have been gradually to build on such familiarity, extending or preserving local fires by feeding them, slowly gaining an increasing measure of control.

Without fire, meat reserves cannot be kept overnight at a campsite. Apart from other problems, it has been pointed out that bears and wolves are attracted by the smells, posing a danger to sleeping offspring (Schaller and Lowther 1969: 335; Potts 1984b, 1988). For millennia, one of the few things capable of *reliably* keeping carnivores from non-arboreal sleeping-sites may have been the visible blaze of a fire.

In addition to providing warmth, protection and nocturnal light, fire can in principle be used to dry out materials, to harden wood, or to preserve food by drying or smoking. Among fire's other uses, well-timed grass-burning may amount to something close to farming, in the sense that the new shoots may tempt game within range of hunters – a technique skilfully developed by Australian Aborigines with their firesticks (Hallam 1975). Alternatively, grassland can be fired over a wide area so as to encircle herds of game.

Finally, fire can of course be used for cooking, a process which removes toxins from plant foods (Leopold and Ardrey 1972; Stahl 1984) and makes meat and bone marrow easier to consume. In enabling each group to extract more from its surroundings, increased cooking efficiency would have allowed bands – perhaps most significantly their female members with dependent offspring – to remain longer in each occupied locality before having to move on.

Because humans are the only animals to control fire, we are handicapped in constructing models of its early use: materials for cross-species comparisons are not available. It has been suggested that several different Plio-Pleistocene hominid species may originally have been involved with fire, each using it in its own, species-specific way (Barbetti 1986; Gowlett *et al.* 1981; Brain and Sillen 1988; Goudsblom 1986). Chimpanzees being rehabilitated into the wild in Senegal have been observed to manage camp-fires in a rudimentary manner, and to collect and eat roasted seeds after a bush-fire (McGrew 1989: 16, citing Brewer 1978), so the idea that early *Homo* or even *Australopithecus* may have achieved this level is perhaps not far-fetched.

A recurrent mistake has been to project modern concepts of fire use back on to the distant past. Whenever early fire traces have been found in association with hominid remains, writers have imagined a dutiful husband bringing meat for his wife to cook in the glowing embers of their camp-fire.

The tendency has been to associate fire almost automatically with a home base, with food-sharing – and with a sexual division of labour on the model of modern hunters and gatherers.

In fact, there is no evidence for domestic fire until about 250,000 years ago, whilst *structured* hearths – for example, deep pits lined or banked around with heat-conserving stones – do not make their appearance until considerably later (James 1989: 9). All the evidence indicates that despite the presence of occasional shallow fires, the camps of early hominids were radically different from those of modern hunter-gatherers, being rather more akin to the temporarily occupied, ever-shifting sleeping sites of chimpanzees.

During excavations at the Swartkrans cave in South Africa late in the 1980s, burnt bones were found and dated to about 1.0 to 1.5 million years ago – an astonishingly early date if the burning indicates the artificial control of fire. The bones were found in association with tools of the Developed Oldowan tradition. The archaeologists responsible for this excavation (Brain and Sillen 1988) stress that fire in this cave was 'a regular event' in the period before *Australopithecus robustus* had become extinct; since robust australopithecine remains are also found in the same levels, it has even been suggested that *robustus* was the fire user.

Another site giving an early claimed date for fire use is Chesowanja, near Lake Baringo, in Kenya. Here, in sediments dated to over 1.4 million years, a 'hearth-like' concentration of stones is said to have been found, associated with lumps of burnt clay. However, no burnt bones were found at this site, and it may be that a smouldering tree trunk set alight in a bush-fire was responsible for the burnt clay (Gowlett *et al.* 1981, 1982; Gowlett 1984; Isaac 1982).

Other claimed early fire sites have been Yuanmou in China (Jia 1985), FxJj20 at Karari at East Turkana (Isaac and Harris 1978), and Gadeb in Ethiopia (Barbetti *et al.* 1980). Some of these sites may be more than a million years old (Gowlett 1984: 182), but even where this has been confirmed, Binford and his students dispute whether fires at such early dates were produced by hominids. It seems significant that there is a complete absence of evidence for fire use at Olduvai Gorge in Tanzania. On the basis of this and other negative evidence, James (1989: 4) has in fact argued that the baked clays, charred organic remains and other finds at sites such as Chesowanja must have been produced by natural fires or volcanic activity.

Numerous claims for European Acheulean and pre-Mousterian hearths – such as Lazaret (Alpes-Maritimes), Pech de l'Azé (Dordogne), Orgnac III (Ardèche) and Grotte de Rigabe (Var) – have been made, the sites being dated to the Riss glaciation, about 360,000 to 330,000 years ago. The site of St. Esteve (dated to about 500,000 years) in Provence has fire traces. Vértesszöllös in Hungary had charred bone in the occupation deposits of perhaps 400,000 years (Kretzoi and Vértes 1965), while scattered traces of charcoal were found at Torralba and Ambrona in Spain, dating to perhaps 360,000 years (Freeman 1975; Collins 1986: 253). Outside Europe, the deposits at Zhoukoudian probably date to about 480,000 years ago, and reveal apparent sporadic traces of fire throughout, possibly produced by *Homo erectus* (but see Binford and Ho 1985). However, a reanalysis of the literature by James (1989) suggests that many of these claims are questionable: there is no firm evidence for *domestic* fire, he concludes, prior to 250,000 years ago. Such a date would at least enable us to include one of the most celebrated (if still not fully authenticated) of all early 'campsites' – Terra Amata in southern France, which has produced the earliest claimed indication of an artificially constructed shelter of some kind associated with the use of fire. On a beach near Nice about 230,000 years ago (Wintle and Aitken 1977), several huts are said to have been built by shoreline foragers over a period of about a century, one floor above the remnants of another, often enclosing a charred area (De Lumley 1969; Villa 1983). Several burned mussel shell fragments have been found in the deposits (Villa 1983: 80–1). The claimed 'hearths', however, are usually described as 'unprepared'. It is not until much later – generally as part of the Middle-to-Upper Palaeolithic transition – that hearths shifted from being thermally inefficient shallow depressions or flat surfaces that would radiate little heat to effective structures (including stone-lined pits) which would have cooked food effectively and conserved heat for extended periods of time.

Until fire could be kindled at will, there would have been strong incentives within each local group to ensure that at least one accessible fire, somewhere, was kept constantly burning. In seeking an ethnographic analogy, Oakley (1958) notes one modern Northampton family who claim to have kept their cottage peat fire burning without a break for 200 years! To keep an Early or Middle Pleistocene fire burning for months on end would have been an immense challenge; in meeting it, a section of society – presumably mainly older individuals and females – would increasingly have had to be entrusted to remain behind during extended foraging expeditions to protect and feed the fire. Unfortunately the *political* preconditions of such a division of responsibilities have rarely been properly examined.

Wherever fire-using hominids were governed by a primate-style social and sexual logic, fire as such may only have added to the problems touched on in Chapter 5. It is even possible to envisage a scenario in which early hominids treated the resource as a scarce value to be competed for and from which to exclude rivals. Selection pressures may in this context have favoured males who strove to keep close at all times to 'their' females and, by implication, to 'their' fires. This would not have enhanced hunting efficiency. In an atmosphere of sexual mistrust, how many males would have been prepared to go away from their females and associated camp-fires, staying out in the cold overnight on an extended hunting trip? In male eyes, would the possible benefits have outweighed the risks?

Much evidence suggests that problems of some such kind may not have been fully solved until the final establishment of symbolic culture by anatomically modern humans. It seems that although they had loosely prepared, temporary hearths and camps, neither *Homo erectus* nor the Neanderthals were capable of the kind of organisation in which the group can periodically split into distinct parties each with its own logistic task (Binford 1980, 1981, 1983, 1984, 1989; Binford and Ho 1985; Binford and Stone 1986; James 1989). The probability is that females in early populations were simply not permitted to stay in charge of a constantly burning fire while males went off to hunt. At its worst, we may suspect, the picture was just the opposite. Males were tempted to keep close to the fire at all times, and because of their insecurities, kept taking 'their' females and fire with them whenever they moved. Not only would this have been bad for mothers with young babies. Constant movement dictated by foraging concerns would have done little to ensure that precarious fires stayed alight.

In other words, fire's potentialities may at first have been constrained by the limitations of a basically primate-like social system. And if all this was the case, then we can say that inseparable from all the other problems was the probability that to begin with, fire was – like much of the rest of life – basically under male political control. Females might have seemed ideally placed to take power in this domain, assuming the responsibilities of 'guardians of the hearth'. But prior to the Upper Palaeolithic revolution, the female sex had not yet wrested fire away, established its semi-permanence at a given site, and made it the focal point of a specifically female domain.

The Neanderthal Problem

The final, culturally-expansionist phase of the human revolution seems to have been entered about 45,000 years ago (Binford 1989). In the Near East and in North Africa, anatomically modern humans make their appearance in the archaeological record from about 100,000 years ago. At the time of writing, the evidence suggests that from this time onwards, they lived in the Near East *without* fully developed symbolic culture for something like 60,000 years – a period during which the long-standing Neanderthal occupation of Europe was not affected. Why the thriving Near Eastern modern humans did not break out into Europe during this lengthy period is something of a mystery (Stringer 1988). We know that towards the end of this period they spread across Asia quite rapidly – fully cultural modern humans were already in Australia as early as 40,000 years ago and perhaps even before that. One conclusion which seems reasonably safe is that events in Europe were peripheral to the processes in which culture as such was born.

The European Neanderthals only became extinct about 30,000 years ago, presumably as a consequence of changes in their environment brought about by the eventual arrival in Europe of modern humans – with whom there seems to have been little or no interbreeding (Stringer 1988). Populations of modern humans came up from the Levant and seem to have begun percolating into Central Europe from about 38,000 to 42,000 years ago, establishing a new tradition of tool-making known as the Aurignacian, which was characterised by heavy retouching and a proliferation in the production of intricate tools made from antler and bone. After some delay, these peoples then began to spread into Western Europe, completely displacing the Neanderthal former inhabitants over a period of perhaps 3,000 years (Dibble 1983; Leroyer and Leroi-Gourhan 1983; Stringer *et al.* 1984; Harrold 1988). Although this meant a relatively sharp break in continuity, the new arrivals in Central Europe at first lived sparsely and with a material culture containing significant elements taken from the Mousterian traditions of their Neanderthal predecessors (Hoffecker 1988; Straus and Heller 1988).

In the 1960s and early 1970s (Breuil and Lantier 1959; Maringer 1960; Solecki 1975), it was widely agreed that the Neanderthals offered grave goods to their buried dead, believed in an afterlife, cared for the sick, engaged in bear cults and other totemic rites, spoke complex languages, hunted big game, cooked and shared their meat – and were the forerunners of modern humans both genetically and in terms of cultural tradition. In short, there were no radical differences to be discerned between Neanderthal lifestyles and those of their 'modern' descendants, so that the notion of a sudden 'human revolution' establishing symbolic culture made very little sense.

In the 1970s and more particularly the 1980s, this view came under sustained attack. One contribution came from two researchers (Lieberman and Crelin 1971) who examined Neanderthal skulls and concluded (mistakenly, it now seems: Arensburg *et al.* 1989) that their supralaryngeal vocal tracts would not have enabled them to produce the full range of sounds necessary for human speech. Other investigations led to the claim that female Neanderthals must have had a radically different reproductive physiology from modern humans, with a gestation period of perhaps thirteen months instead of the modern human nine (Trinkaus 1984). More recently, even the long-accepted idea that the Neanderthals buried their dead has been challenged (Gargett 1989), whilst others have shown that we have little more than isolated, fragmentary and disputable suggestions of Neanderthal necklaces or other items of personal ornamentation (Chase and Dibble 1987).

Findings of this kind converged with others touched on earlier in this book. As mentioned already, from the late 1960s onwards, Lewis Binford began arguing forcefully that 'culture' in its modern sense could not have arisen until the Upper Palaeolithic revolution, and that the Neanderthals had settlement systems and subsistence strategies quite different from those of contemporary hunter-gatherers and other modern humans. Although Binford probably overstated his case, archaeologists recently have been much less ready to see evidence for hunter-gatherer-like behaviour in the Middle Palaeolithic archaeological record.

Despite these findings, until late in the 1980s it was still widely assumed that there was a direct ancestor-descendant relationship between the Neanderthals and modern humans, at least in the Near East, where the two populations had long been known to have lived close to one another in space and in time. It was therefore a shock to discover that even this idea would probably have to be abandoned.

Perhaps the most decisive event in this connection was the publication of a brief report in the journal *Nature*, in 1988. It described the use of the new thermoluminescence technique to determine the age of burnt flints in Mousterian levels in Qafzeh Cave in Israel – levels which had earlier yielded some anatomically modern ('Proto-Cro-Magnon') fossils (Valladas *et al.* 1988). It turned out that the levels and hence the fossils were 92,000 years old – twice as old as had previously been guessed. Such 'modern' people could not possibly have evolved from the local Neanderthals – because they were not younger, but about 30,000 years older than the earliest known Neanderthals in the region (Stringer 1988)! On the basis of this and other evidence, it is now known that anatomically modern humans were living in the Levant as far back as 90,000 to 100,000 years ago, whereas Neanderthals arrived in the region – perhaps retreating from the intense cold in Europe – only about 60,000 years ago. In at least one cave, there is evidence that modern humans eventually moved out, to be replaced by Neanderthals, who were in turn replaced much later by a new population of culture-bearing moderns. The Neanderthals then appear to have become extinct.

All of this information – and particularly the age of the Qafzeh fossils – gave the severest of jolts to Regional Continuity as a model of human evolution in this part of the world. As the London Natural History Museum's Chris Stringer (1988) was quick to point out:

> The palaeoanthropological implications of such an age are enormous. . . . Evolutionary models centred on a direct ancestor-descendant relationship between Neanderthals and modern *H. sapiens* must surely now be discarded, along with associated schemes designed to explain such a transition.

The finding that resident modern humans and intruding Neanderthals coexisted in the Near East side by side for about 60,000 years, apparently with little if any interbreeding, has led some writers (Stringer 1988; Foley 1989) to suggest that the two groups must have been entirely distinct species, not sub-species of *Homo sapiens* at all.

The Qafzeh dates delivered the heaviest blow to an orthodoxy which had retained at least some of its credibility until late in the 1980s. Under these circumstances, 'Regional Continuity' could not even be resuscitated by the astonishing discovery in France of fossil Neanderthals who had evidently been making and using Upper Palaeolithic stone tools. In an earlier period, this would undoubtedly have been taken as ruling out any real gulf separating Neanderthal from modern cultural traditions. But it is now widely suspected that the Upper Palaeolithic (Chatelperronian) technology of the Neanderthals at Saint-Césaire testifies not to an autonomous local attainment of full cultural modernity – but to the impact of newly arrived modern humans upon an ancient Neanderthal lifestyle. It seems as if the retreating Neanderthals at first began to learn advanced tool-making patterns from the new arrivals, although this did not prevent them from becoming extinct a few thousand years later (Harrold 1989).

Despite superficial appearances to the contrary, all of this can probably be reconciled with Marshack's (1989) eloquently argued view that the Neanderthals had for millennia been wholly in possession of the *capacity* for symbolic culture, even though this capacity in their case never became fully *realised*. We can rephrase this distinction in the light of Richard Dawkins' (1976) comparison between 'genes' and 'memes' (see Introduction). The breakthrough to cultural evolution required not just the localised replication of sophisticated symbolic memes. Memes had to be able to circulate freely over vast areas. Only this could guarantee that they did not die out with the extinction of particular local populations. And only this could guarantee the necessary element of 'immortality' – that is, guarantee that memes did not die almost as fast as they were born, but instead became widely exchanged, pooled and subject ultimately to global evolution. The late Neanderthals in each inhabited European district seem to have been in principle capable of almost any symbolic invention. But each of their most unexpectedly 'modern'-seeming artistic or other advances – many of which Marshack (1989) has beautifully documented for us – seems to have occurred only in a localised way, usually disappearing in the place of its origin *before* it could become part of the cultural heritage of all Neanderthals as such. This was the Neanderthals' handicap. The capacity for a universalistic *collective pooling* and hence *indefinite cumulative evolution* of cultural knowledge was displayed only by those anatomically modern humans who evolved in Africa and the Near East, eventually displacing the Neanderthals in the earliest stages of the Upper Palaeolithic.

African Eve

No less decisive in revolutionising our recent origins models has been the rise of palaeogenetics – the use of molecular biology to work out past genetic relationships. Although fierce controversies remain, there is now strong support for the belief that contemporary racial diversity is superficial, all anatomically modern humans being not only one species but a very homogenous and recently evolved one. Modern Chinese people – according to this view – are not the direct genetic descendants of Peking Man, any more than modern Europeans are highly evolved Neanderthals. Instead, all contemporary humans, from Hudson's Bay to Ayers Rock, are the descendants of a small population of fully modern humans from Africa who broke out and fanned across the world only a few tens of thousands of years ago.

The most influential studies in this connection were conducted in the late 1980s, most spectacularly in the form of an analysis of sequence variation in modern women's mitochondrial DNA. The mitochondria are tiny energy-generating organs found outside the nucleus of every cell, their location determining that their DNA can be transmitted only matrilineally. Whenever a female has no daughters, therefore, her mitochondrial genetic inheritance is lost – her line simply comes to an end. Rebecca Cann and her colleagues (Cann *et al.* 1987) deduced on logical grounds that if all of us, throughout the world, were to trace our lines back far enough, the ancestral tree would keep converging until it reached a point. In other words, the mitochondrial DNA now immortalised in us all must ultimately flow from just one ancestral mother.

When measurements of mtDNA from women of different racial origins began to be taken in the mid-1980s, the amount of sequence variation seemed astonishingly small for all modern human populations. One surprise was that the average variation between any two racially distinct groups was much lower than inter-individual variation within each group. In other words, any two Eskimos, or any two Aboriginal Australians, would be likely to have mitochondrial sequences differing much more widely than the average differences separating Eskimos as a whole from Aborigines (or Europeans, or Papua/New Guineans etc.) as a whole.

Assuming mtDNA mutations to be largely neutral – that is, assuming that they make little difference to the fitness of individuals, and so escape the influence of natural selection – then their occurrence and accumulation must be mostly a function of time. The more variability a population possesses, in other words, the older it is. Modern humans show a small (0.57 per cent) variability across all populations, indicating a remarkably recent common ancestor. Since the rate of mtDNA evolution for a wide variety of vertebrates is 2–4 per cent per million years, and since there is much evidence that this also applies to humans (Stoneking and Cann 1989), the human results suggest a common ancestor living between 142,000 and 284,000 years ago.

Although the worldwide mtDNA variation is small, within this restricted range the African gene pool shows greater variation than that of any other group (Stoneking and Cann 1989: 22, table 2.1). Caucasians, for example, show an internal variation of only 0.23 per cent, compared with a 0.47 per cent variation in African populations. This indicates that the evolution of modern humans has been occurring in Africa longer than elsewhere. In fact, it seems that the descendants of 'African Eve' – postulated common ancestress of all modern humans – at an early stage split into two major lines of descendants: (a) the ancestors of several African fully modern groups and (b) a line ancestral to the remaining fully modern African groups, in addition to all the world's other racial groups.

If the transition from anatomically archaic to modern humans had occurred more or less simultaneously in different parts of the world – as the gradualist proponents of 'Regional Continuity' had always held – then the various populations of archaic *Homo* in Asia and Europe would all have made major contributions to the modern human gene pool (Wolpoff *et al.* 1984; Wolpoff 1989). Any common ancestor of all modern humans must have lived before the period when *Homo erectus* populations were first beginning to migrate beyond Africa with their hand-axes or other tools. In that event, the observed pan-human variation in mtDNA and in other genetic phenomena should be very wide, indicating that our common ancestor lived not a mere 200,000 years ago but something more like a million years ago. The fact that such wide mtDNA variation between populations is not observed has been an important factor in persuading many specialists to abandon the theory that the world's various racial groups could have descended locally from middle Pleistocene populations of *Homo erectus*, or from the Neanderthals. Instead, within each continent or region, all contemporary racial groups seem to be recent immigrants from some restricted point of origin within Africa (for both sides of the continuing controversy, see papers in Mellars and Stringer 1989b).

Current molecular research, then, is profoundly changing our understanding of evolutionary timescales and of the genetic background to human cultural origins, and the next few years are likely to see exciting further developments. 'African Eve' herself would appear to be little more than a logical construct. She may before long fade from fashion, and has in any case recently been joined by a perhaps still more ephemeral 'Adam' (Lucotte 1989). Nonetheless, the population to which either construct refers presumably existed, and we can usefully ask what it was about this population which destined the mitochondrial inheritance of one of its number to become immortalised in every single living member of the human species.

We know that Eve's genetic constitution was under selection-pressures leading to the anatomy and physiology of modern woman. We know that modern offspring mature slowly compared with Neanderthal offspring (Trinkaus 1986; Bromage and Dean 1985; Dean *et al.* 1986), and that male parental contributions must have been increasing to cope with the consequent added parenting burdens. Linking this to our previous discussion of Paul Turke's (1984) model, we might infer that if evolving protohuman females ever followed the ovarian synchrony strategy, then Eve and her kin must surely have been doing so as they diverged from related hominid forms. Although not yet fully 'cultural', such mothers would have been prioritising child care, synchronising their cycles, concealing ovulation, extending receptivity – and thereby harnessing to an ever-increasing extent the available provisioning energies of males.

A final inference can be made. Our findings in Chapters 5 to 7 would indicate that when, finally, the 'home base' institution in its modern form did appear, it was because an age-old, primate-derived sexual-political obstruction associated with male sexual dominance had at last been removed. This obstruction had never completely prevented the rise of ovarian synchrony and therefore of a kind of 'human solidarity': but it had restricted it to those populations inhabiting a small number of rather special, semi-aquatic or in any event resource-rich habitats. Overcoming this restriction involved a revolution in the most literal sense of the word – a relatively sudden change involving a redistribution of power and a radical transformation of all social, sexual and also spatial relationships.

Background to Revolution: Foraging Strategies and Shores

It is widely agreed that the emergence of symbolic culture involved 'the replacement of ape-like systems of interpersonal dominance . . . by systems of at least relatively egalitarian, stable, and reliable relations of rights and obligations' (Whallon 1989: 449). Such an overthrow certainly merits description as a revolution. Yet however decisive an event or process this may have been, it is now clear that Upper Palaeolithic humans had no need to invent either egalitarianism or solidarity. Paul Turke's model (Chapter 6) enables us to appreciate that the extraordinary scale of internal social harmony required for the final, Upper Palaeolithic consummation of the human revolution already had an ancient evolutionary pedigree. Gender solidarity of the kind expressed in ovarian synchrony had been a powerful evolutionary factor from the moment when evolving humans' anatomy and reproductive physiology had begun acquiring modern form.

What, then, was distinctive about the human revolution's final consummation? Much could be said about this, because the transition from Middle to Upper Palaeolithic levels of technological competence brought with it what one writer has termed 'the creative explosion' (Pfeiffer 1982) – the emergence of personal ornamentation, art, ritual, dance and much else besides. Understanding such changes in symbolic behaviour, however, requires delving into their material roots in subsistence strategies and in climatic and other environmental change.

According to my own preferred narrative, the point of departure was a situation in which evolving humans were still practising area-intensive foraging strategies. While this may not necessarily imply riverside or shoreline settings, I think there is evidence that such habitats were strongly favoured.

A glimpse into an early Mediterranean setting of this kind is provided by the site of Terra Amata, near Nice in southern France, where, as mentioned earlier in my discussion of fire, what may have been a shelter was built on the Mediterranean beach about 230,000 years ago (Villa 1983: 55). The people – probably archaic *Homo sapiens* – seem to have based their subsistence largely upon the hunting of selected young or weak animals; they also ate marine resources such as fish and shellfish (de Lumley 1969: 45; Villa 1983). The presence of water-lilies, whose bulbs are edible (Dimbleby 1978: 28), may have been another attraction of the beach-site. 'If the conditions were suitable for water-lilies', comments McKay (1988: 48),

> perhaps other edible plants with similar restrictions on their habitats were also growing. We cannot be certain, but it is not unlikely that along the salt-wasted coast there were little oases where streams created small gardens of edible plants. Gardens that could, perhaps, sustain a hunter-gatherer band for a few days, and which served as ideal camp-sites, whilst the men hunted and the women harvested vegetables and shell-fish.

Analysis of hearth positions and reuse suggest eleven perhaps-seasonal visits to this site, each of two or three days duration (de Lumley 1969; Villa 1983). All this could indicate a lifestyle perhaps not radically distinguishing these hominids from their Lower Pleistocene ancestors evolving along the shores of the Afar Gulf and the wetlands and lake shores of the East African Rift Valley. It will be remembered that one of the few hints we have as to the food eaten by the Bed I hominids at Olduvai was that they used 'hammers' to pound up aquatic tuberous plants (Binford 1989: 27).

The southern coast of the Mediterranean, especially in Morocco and Algeria, was similarly occupied, the site of Sidi Abderrahman yielding contemporary remains of the hominids themselves (Wymer 1982: 124). At the Libyan coastal site of Haua Fteah, mounds of sea-shells have been found buried with Mousterian tools dating to about 80,000–70,000 BP (McBurney 1967). Whatever else it may imply, such evidence suggests that Mousterian and earlier hominids tended to favour coastal economies and did at least occasionally eat seafoods.

In most parts of the world, shorelines have changed substantially over the past 10,000 years. Rising ocean levels coinciding with the end of the last ice age have often destroyed evidence of Late Pleistocene human occupation as cliffs, perhaps honeycombed with inhabited caves, have collapsed into the sea. But along the southern coast of Africa, tough Palaeozoic rocks have withstood the batterings of both time and crashing waves, changing scarcely at all since the Middle Pleistocene. It is the evidence from Klasies River

Mouth which has given us some of the oldest known fossils of anatomically modern humans – presumed descendants of 'Eve' – in addition to outstandingly early dates for stone tools indicating a level of lithic competence approximating that of the Upper Palaeolithic. And it was this same evidence, as Binford (1984: 20) notes, which 'forced the recognition that early man was using aquatic resources for a long period of time prior to the Late Pleistocene'.

Compared with earlier Acheulean peoples in the same region, there is evidence that the Middle Stone Age peoples at Klasies River Mouth were becoming less narrowly restricted to valleys and to the coastal platform, and were beginning to collect game and gatherable resources up in the adjacent plateaux (Deacon 1989: 557). Nonetheless, the evolved, anatomically modern but pre-cultural hominids of Klasies River Mouth lit their fires and foraged along the seashore, killing penguins, scavenging seasonally washed-up seal carcasses (Marean 1986) and eating other marine creatures in addition to vegetable foods. At the Klasies main site there is no archaeological evidence for fishing. But as a supplement to the carbohydrate-rich geophytes (buried plant foods) dug up in the surrounding mountains and apparently encouraged by controlled burning, small- to medium-sized terrestrial game animals were hunted and eaten, and quantities of shellfish were consumed as a rich and necessary source of minerals and other nutrients (Deacon 1989: 558–9).

We do not know whether the females at Klasies River Mouth at various times were organised into harems monopolised by single males, chimpanzee-style multi-male systems or other arrangements. However, Binford's (1984) healthily sobering view that they were still constrained within the parameters of a basically primate-like system seems too extreme. It would seem more likely that periodically or perhaps even continuously, the females in this locality had escaped many of the severer problems associated with competitive primate sexuality, and that they had done so by synchronising along the lines suggested by Paul Turke (1984). It is difficult to think of types of evidence which could decide between these possibilities, but information concerning sex ratios and measurements of sexual dimorphism might possibly prove relevant. There is some evidence that in the warm period preceding the Last Glacial, the Klasies hominids showed pronounced sexual dimorphism, with very robust males and much more gracile females (Deacon 1989: 556). Then, as colder weather intensified with the onset of the last Glacial, there is evidence that selection pressures against dimorphism set in. This might indicate an increase in synchrony in the later period, reducing inter-male physical competition for mates, but this is of course guesswork. Perhaps the most we can say is that since the skeletal anatomy of even the earlier Middle Stone Age Klasies hominids had become basically modern, the soft-tissue sexual anatomy and physiology of the females may also have reached modern form. If Turke's (1984) model can be relied upon, this would in turn imply that some time, somewhere, ovarian synchrony had been playing an important role in these females' lives.

If females were synchronising and by that means maximising male help – and it is hard to see how these hominids could have attained anatomically modern form without this – then along the coasts they were probably supplementing their own collected resources with occasional medium-sized land mammals brought to them by males and cooked on fires. Whilst male provisioning may have been reaching a relatively high level, however, there is nothing to indicate a rigid sex division of labour at Klasies River Mouth. Females would have collected what foods they could, and males would have done likewise as they provisioned their offspring and mates. Groups of kin and offspring may have slept in caves like those which have been excavated (Singer and Wymer 1982), doubtless enfolded in skin blankets and huddling together for warmth around a fire.

In this and similar coastal areas, population densities may have been locally high – perhaps rather higher than in the more mountainous hinterland regions which were also occupied. On that analysis, Middle Stone Age anatomically modern females would have been maintaining the togetherness necessary for synchrony thanks to the rich coastal environment and an area-intensive foraging strategy. Permanent movement into the surrounding mountains would have posed challenges which led to greater dispersal.

Out of Africa

As anatomically modern humans began to spread out from Africa across the world between 90,000 and 40,000 years ago, they were almost certainly capable of living inland from lakes or coastal shorelines where necessary. According to my narrative, however, *wherever possible* they at first opted for easier ways of feeding themselves and maintaining their togetherness – ways that involved retaining area-intensive foraging strategies close to river valleys, lakesides and shores.

The Nile Valley is the corridor along which anatomically modern humans probably filed as they moved from Africa into adjacent parts of Eurasia some 70,000 or more years ago, shortly before beginning their colonisation of the world (Bräuer 1989: 148; Howells 1988: 226). The fertile banks of the Nile, comments Howells (1988: 226), would have been 'hospitable at all times regardless of continental climates . . . '. There are alternative routes – such as the short sea crossing over the Strait of Bab el Mandeb into Southern Arabia; but these would have presupposed familiarity with swimming-logs or rafts (Clark 1989: 580).

Sea crossings cannot be ruled out, for an expertise with watercraft evidently extends back to the very earliest stages of the Upper Palaeolithic. Indeed, in order to explain the surprisingly early expansion of anatomically modern humans across Asia to Australia, it is necessary to picture small groups travelling along the edges of the Indian Ocean and other coasts, periodically following and crossing rivers, inlets and estuaries. The final step to Australia involved a daunting 90 km sea crossing between Timor (on the Sunda continental shelf) and Greater Australia (Sahul). Rhys Jones writes:

> My own scenario is that in the period just prior to the colonisation of Australia – say 40 kyr ago – there were people living on the shores of Sundaland, in the mangrove swamps and using the river mouth for resources. They had an adequate technology of inshore watercraft, perhaps rafts made of bamboo palm or other suitable materials. Random events such as storms and currents sometimes swept people off into the ocean, where under suitable conditions of wind and current they made new land falls. The odds against any one such episode being successful might have been high, yet given enough time the entire archipelago could be colonised. (1989: 755)

Once in Australia, the same coastal economy was maintained. Among the favoured hypotheses for the gradual colonisation of Greater Australia is that of Bowdler (1977, 1990), according to whom the first immigrants moved south along the coasts of this vast continent, before expanding their territory by following major river and lake systems and soon colonising the interior.

The American case was probably similar. 'In both America and Australia', comments Bednarik (1989: 109–10),

> it seems entirely plausible that first entry was by small numbers of people who were adapted to coastal economies. Hominids of the Lower and Middle Palaeolithic are generally credited with a penchant for near-coastal, riverine and lacustrine environments. . . . For a people occupying a new continent there may have been little incentive to shed their coastal economy and penetrate the hinterland until such time as coasts and major river courses were settled to capacity.

Bednarik's conclusion is important: it is likely that until coasts, estuaries and major river valleys were settled to capacity, newly settled modern humans in all continents tended to retain their ancient evolutionary preference for resource-rich estuarine and/or shoreline homes.

We have seen that shoreline economies were favoured not only by evolving hominids in the East African Rift Valley during the Plio-Pleistocene, but also by much later, large-brained tool-makers as these moved out of Africa into Eurasia, Australasia and even the Americas. It would be an exaggeration to state that a restriction to such settings characterised all sapient humans prior to the Upper Palaeolithic. But although many archaic populations of

Homo successfully penetrated the higher and drier regions of the great continental hinterlands, it would seem that this was always and everywhere achieved only at a price. If our earlier arguments about the more robust, derived features of Eurasian *Homo erectus* are correct, then in the more arid or otherwise marginal habitats which they were able to colonise, archaic humans – who owed their brains ultimately not to local conditions but to the peculiar circumstances of their African origins – would have been obliged to adapt locally in ways which did not involve significant further neoteny, gracility or encephalisation. Instead, increased dispersal, a corresponding decline in social complexity and new, strenuous physical demands would have led to enhanced robustness and to a certain emphasis on physical at the expense of highly sophisticated communicative/social skills. This, in any event, is one interpretation which can be put upon some of the super-robust features of *Homo erectus* in Asia and the earlier specimens of archaic *Homo sapiens* in Europe and elsewhere – in particular the massive limb bones, enormous brow ridges and astonishingly thick skulls (Collins 1986: 148–50).

From about 200,000 years ago onwards, these problems evidently began to be overcome. No longer did dispersal out of Africa and into cooler regions entail losing touch with those conditions (conducive to ovulatory synchrony) which, within Africa itself, had led to the initial evolution of large brains. Certainly, Eve's descendants – or at least a group of them – avoided pressures to evolve in non-modern directions of the kind characteristic of *Homo erectus* in Asia or the Neanderthals in Europe. Yet it was long before a solution was arrived at which made ice age Eurasian and other continental hinterlands positively favoured as habitats by anatomically modern humans. Such humans were biologically adapted to the tropics and subtropics, and – for as long as they remained in roughly comparable climatic conditions – it would seem that there was little pressure on them to undertake a 'cultural revolution'.

In fact, wherever the origins of the Upper Palaeolithic have been adequately researched, it turns out that an episode of severe cold, desiccation or both was in some way connected with it. It was lowered primary productivity which triggered the change – a momentous cultural transition rooted, I believe, in a cold-triggered, forced abandonment of area-intensive foraging patterns and riverine/coastal ecosystems. This involved a genuinely revolutionary 'leap' to new transpatial, non-territorial forms of social organisation, in turn made possible by symbolic communication systems of an entirely new kind.

Culture and Cold

Let us examine more closely the background to this revolution – its relationship to long-term climatological change. We will see that everywhere, the decisive events were associated with periods of combined dryness and cold.

In addition to its many other riches, the Middle Stone Age site at Klasies River Mouth has yielded some astonishingly early dates for tool assemblages which, in a European context, might almost be labelled 'Upper Palaeolithic'. The sophisticated blade-making technology known as 'Howieson's Poort' – dated to about 70,000 BP (Deacon 1989: 554) – coincides with the onset of a glacial period and worldwide regression of sea levels, bringing with it substantially deteriorating environmental conditions in southern Africa (Clark 1989: 573; Deacon 1989: 560). It was evidently this deterioration which triggered the cultural advance. The Klasies deposits reveal a sudden lowered frequency of gathered shells at this time. This coincided with a smaller proportion of seals among the faunal remains and a larger proportion of bovids, equids and geophyte plant remains – all indicating a partial abandonment of marine foods and a move towards inland collecting (Marean 1986: 366). Thereafter, the climate improved, and technology reverted to simpler forms for tens of thousands of years. Then at about 40,000 BP came the next major technological advance, which this time proved permanent. Again, cold weather had something to do with it. The change from the Middle Stone Age technological stage in southern Africa to that of the Later Stone Age coincides with the onset of the Last Glacial Maximum – the most extreme environmental conditions of the late Pleistocene (Deacon 1989: 556). This eventually led modern humans such as those at Klasies River Mouth to abandon their coastal economies altogether in favour of gathering and hunting in the hinterland (Deacon 1989).

A comparable pattern can be discerned in the northern part of the continent. Severe desertification affected the Sahara about 75,000 years ago – at the start of the Last Glacial (Clark 1989: 573) – at a time when Neanderthal populations were retreating from the severe cold in Europe and expanding or shifting their range by entering the Levant (Bar-Yosef 1989: 604). Many former inhabitants of the Saharan region seem to have migrated into the Middle East at this time. In the Central Negev Desert, the Upper Palaeolithic revolution came at a time when severe desiccation associated with global cooling was setting in; eventually, this area had to be abandoned because it was so barren – but by this time people had moved on, now equipped with revolutionary new cultural forms which they took with them (Marks 1983).

In Europe, too, a deteriorating climate seems to have tipped the scales in favour of modern humans and their associated Upper Palaeolithic symbolic cultural traditions. Gamble (1986a: 367–83) has shown that considerable differences existed in the responses of European Neanderthal and anatomically modern populations to the establishment of polar desert conditions of low terrestrial productivity. Whereas the probably light-skinned, physiologically cold-adapted Neanderthals appear to have abandoned Central Europe during the glacial advances of about 70,000–50,000 BP, modern human populations, who, given their African origins were at first probably dark-skinned or even black, persisted in the face of similar severe cold conditions between 35,000 and 12,000 years ago. The final displacement of the last Neanderthals by modern humans in central and south-west France occurred in a particularly cold phase (Harrold 1989: 689).

At first sight, it might seem that given their tropical origins, probably dark skins, warmth-adapted physiologies, ultra-dependent offspring and labour-intensive child-care burdens, anatomically modern females would have been hit particularly hard by the onset of cold weather. In addition to such requirements as thick clothing, the cold, windswept plains now inhabited would have demanded higher levels of mobility and a heavier reliance on hunting. In the previous chapter we took account of environmental factors making it difficult to imagine how Paul Turke's ovarian synchrony model could have worked under such conditions at all – certainly not if females were forced to disperse widely and forage inland and in isolation from one another. Besides undermining synchrony, the cold/dry conditions would also have undermined any competitive female strategy of granting favours to males in return for favoured access to foraging space. Of what use would a few square metres of ground have been – if insufficient food for survival could be found in such patches? What use personal feeding space, if the decisive requirement had become access to roaming herds of game?

In short, it is hard to imagine how the new conditions could have been anything other than negatively experienced by females – unless, of course, they could find some way of compelling the opposite sex to do massively more of the travelling, hunting and related tasks for them. Yet the evidence is that soon after the start of the last ice age, the harsher conditions not only failed to block the expansion of modern humans into new regions – they positively facilitated such expansion. Despite their tropical origins, modern humans with their warm clothes, semi-permanent dwellings and well-controlled domestic fires embraced the snowswept plains and tundra of ice age Eurasia as if such spaces had been made for them. We must conclude that females in these regions were guaranteeing their subsistence requirements by relating to males in a wholly new way.

Paul Turke Reconsidered: Synchrony and the Ice Age

The requirement, we can now see, would not have been for culturally organised humans to invent entirely unprecedented patterns of synchrony and area-extensive gender solidarity. Instead – as our findings in the last chapter now suggest – the task would have been *to preserve synchrony under entirely new conditions*. The sociality built up over preceding millennia and responsible for the unique reproductive physiology of the human female would have had to survive the transition from the rich tropical shoreline environments of hominid ancestry – to the much less hospitable environments of the continental hinterlands of the last glacial epoch.

In the Levant at around 70,000–80,000 BP there is evidence that whilst the Neanderthals were still restricted to moving within highly productive, often-coastal ecosystems, anatomically modern humans in the same period were able to transfer to less productive mountain and desert zones (Shea 1989: 622). Previously, entering such zones might have prompted local extinction, the retreat of hominid populations to more resource-rich areas or (if survival were possible at all) much greater mobility and dispersal into small groups, with a corresponding loss in sociality. The final consummation of the human revolution, by contrast, was achieved not through retreat but through some extraordinary process of meeting the new challenge. As a result, humans became able to live almost anywhere. In the Levant (as, perhaps, in other regions where anatomically modern humans existed), an extraordinary revolution occurred when for the first time extended communities proved that they could traverse and embrace immense areas of space – *without* losing the high levels of sociality which their area-intensive former foraging traditions had sustained.