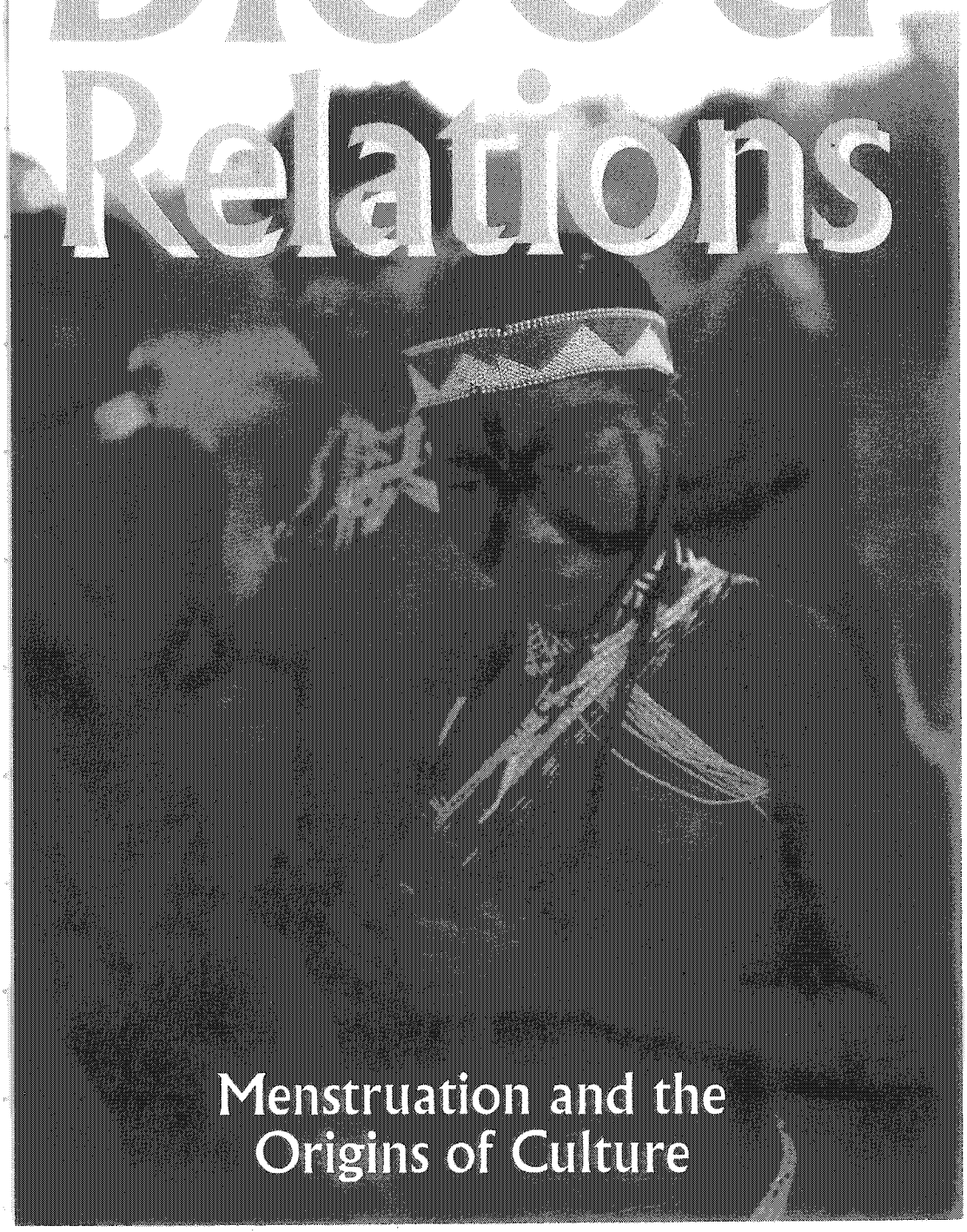
CHRIS KNIGHT

Blood Relations



Menstruation and the Origins of Culture

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'The philosophers have only interpreted *the world in different ways; the point is to change it.'*

Karl Marx, Theses on Feuerbach: XI *(1845)*

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Menstruation and the Origins of Culture

Chris Knight

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To my children,
Rosie, Olivia and Jude

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Preface to the Paperback Edition

By 40,000 years ago, the effects of a symbolic explosion – an efflorescence of human art, song, dance and ritual – were rippling across the globe. The bearers of symbolic culture were recent immigrants from Africa, dispersing so rapidly to encompass the globe that the process has become known as 'the human revolution'. Enough data and sophisticated neo-Darwinian theory now exists to begin to explain this most momentous revolution in history.

Simplistic, sexist stereotypes on the model of 'Man the Hunter' or 'Man the Toolmaker' contravene Darwinian theory. Females are not appendages; they pursue their own independent reproductive strategies, which typically diverge from those of males.

Primate societies are systems of alliances through which individuals pursue their fitness interests. Group-living places a premium on social intelligence, setting up selection pressures for large brains. But among primates, this process is constrained by the very high thermoregulatory, metabolic and obstetric costs of such brains. The exponential increase in brain size characterising the evolution of *Homo sapiens* indicates that, in some radical way, these constraints were overcome.

The costs of brain growth fall over-whelmingly on the female. In the human case, not only did mothers have to secure more and better quality food, they had to accomplish it whilst weighed down by heavily dependent infants. The problem is: how did they cope?

We now know the basic answer. Evolving women succeeded in gaining unprecedented levels of energetic investment from their mates. Success went to mothers who could reward more attentive, heavily investing partners at the expense of would-be philanderers.

A philandering male maximises his reproductive fitness by fertilising as many females as possible. He achieves this by reducing the time spent searching for each fertilisable female, and the time spent with her to ensure impregnation. The human female appears well-designed by evolution to waste the time of any philanderer by witholding information about her true fertility state. Concealment of ovulation and loss of oestrus with continuous

receptivity deprives the male of information on whether his mate is likely to have been impregnated. The longer a male takes to impregnate any one female, the smaller his chances of being able to fertilise another.

A further means of thwarting philanderers is reproductive cycle synchrony. If females synchronise their fertile moments, no single male can cope with guarding and impregnating a whole group. He must concentrate on one at a time. The effect is to maximise the number of males in the breeding system, and hence the amount of male investment available. Ovulatory synchrony in local populations drives the ratio of sexually active males to females in groups towards one-to-one. Sustained male/female bonds on this basis mean greater paternity confidence, hence greater inclination on the part of males to invest in offspring. The evolutionary effect is to discriminate against philanderers in favour of more committed males.

Once ovulation was concealed and oestrus lost in the human lineage, menstruation acquired new significance as a cue. This, however, threatened the stability of the female strategy of withholding information from philanderers. Menstruation in the human case is particularly profuse. It is not something a female can easily hide. In fact it is a complete give-away. It signals a female's imminent fertility – and hence by contrast the *infertility* of neighbouring females who, whilst pregnant and nursing, are not displaying such blood. Males would have been drawn towards any such fertile female within the local area, competing to bond with her at the expense of pregnant or nursing females. Mothers with heavy childcare burdens, lacking the menstrual signal, would then have lost out just when they most needed help.

Cosmetics, according to recent research, were the answer. If there is a menstruating female in the neighbourhood, why not join her? Why not *appear* to be as fertile, painting up with blood-red colours? Ethnographic and historical records show how hunter-gatherer women across southern Africa prevented any young menstruant in their midst from being perceived as an isolable individual. Conjoining with her in a ritual dance, they used red ochre body-paint not only to signal menstruation and fertility, but *simultaneously* to indicate inviolability or taboo, their basic message being: *'No meat, no sex!'*. We know that in Africa, anatomically modern humans were intensively mining, preparing and liberally applying red ochre body-paint 110,000 years ago.

Human symbolic culture emerged out of struggle. Its rituals and myths were expressions of 'counterdominance' – signals for thwarting exploitation by males. The signallers were females, allied with their male kin; their targets were their mates. Culture, in short, was a female invention for the provisioning of babies. Through it, womankind resisted and brought to an end the male's time-honoured biological status as the leisured sex.

January 1995

Acknowledgements

This book could not have been completed without help from many sources.

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I thank Mary Douglas for much personal encouragement and for launching me on my research project in 1976–7 while I was a diploma student at University College London. Thanks also to Alan Barnard for his exceptionally conscientious tutoring during the same period and subsequent twelve years of informal help and encouragement with all aspects of my book. Without such support, my effort to turn myself into an anthropologist would have had to be abandoned at an early stage.

Acknowledgements are due to the British Medical Anthropology Society, the Scottish Branch of the Royal Anthropological Institute, the Institute for Contemporary Arts, the Traditional Cosmology Society and the organisers of the 1986 World Archaeological Congress and the Fourth International Conference on Hunting and Gathering Societies for inviting me to present papers which subsequently became incorporated into this book. In each case, the resulting discussion and criticism allowed me to improve the arguments immensely.

I acknowledge supportive specialist criticism from Tim Buckley (on the Californian Yurok), Vieda Skultans (on medical and menstrual anthropology), Alain Testart (on the 'ideology of blood'), Maurice Godelier (on Baruya menstrual symbolism), Kenneth Maddock (on Dua/Yiritja duality and other aspects of symbolism in Arnhem Land), Roy Willis (on cross-cultural snake symbolism), Joanna Overing (on menstrual myths and many aspects of cross-cultural gender construction) and Stephen Hugh-Jones (on Barasana menstrual rites). At an early stage of the research I benefited particularly from discussions with David McKnight (on the own-kill rule in Cape York Peninsula), and with James Woodburn (on normative menstrual synchrony among the Hadza). Marilyn Strathern, while editor of *Man*, made extensive

comments on an article for that journal which became incorporated into Chapter 12 of this book; it was she who first drew my attention to Tim Buckley's work. Howard Morphy provided information on Yolngu symbolism and suggested the term 'transformational template' to describe my model. Bernard Campbell made valuable and firmly supportive comments on a paper on cultural origins which was later expanded to form Chapter 9 on human origins. Monique Borgerhoff Mülder also constructively (if more stringently) criticised the same paper. Robin Dunbar, Clive Gamble, Graham Richards, Chris Stringer, Robert Kruszynski and Elaine Morgan all read selected chapters within their various fields of competence, offering much extra information and making the final text (whatever its inadequacies) considerably more error-free than it might otherwise have been. None of this implies, of course, these specialists' agreement with what I eventually came to write.

Among many of my students who helped, Max Pearson gave me versions and details of traditional myths from which the argument substantially benefited, while Ian Watts helped me in keeping up to date with the recent palaeontological and archaeological literature on human origins. Lionel Sims read every chapter as it was written and offered many helpful suggestions. Other research assistance came from Chris Catton, Sue Walsh, Isabel Cardigos, Nick Kollerstrom and many others. The text benefited much from Beth Humphries' eagle-eyed and sometimes painfully stringent copy-editing. My Ph.D. supervisors at University College London, first Andrew Strathern and then Philip Burnham, were astonishingly patient with my slow progress in completing the thesis on which this book was eventually to be based. Finally, my warm thanks for the good advice, encouragement and almost equally astonishing patience of Robert Baldock at Yale University Press.

I am often told that the basic idea of my book is 'entirely original'. This is generous but not quite true. I was fortunately able to discuss and correspond with Elaine Morgan over the past ten years, an experience which led me to realise with ever-increasing astonishment the precariousness of the prevailing savannah hypothesis of hominid origins. Close familiarity with the aquatic hypothesis as it developed helped to give evolutionary depth to my initial suspicion that tidal synchrony may have been involved in both the biological *and* sociopolitical dimensions of cultural origins. My appreciation of menstruation as a potentially empowering experience, on the other hand, derived in part from my reading in 1966–7 of Robert Briffault's *The Mothers*. Ten years later, Denise Arnold introduced me to a series of papers on the same theme by members of the Matriarchy Study Group. A year or so after that, the poets Peter Redgrove and Penelope Shuttle published *The Wise Wound*, a literary work of great originality and insight, one of its main themes being the centrality of menstrual symbolism to any cross-cultural understanding of ritual and myth. I had already tentatively reached related conclusions on the basis of rather different lines of evidence, but the germs of many of my ideas

may be traced in part to that book. The authors have since given me invaluable friendship, inspiration and advice.

On a different level, Graham Bash, Keith Veness, Ken Livingstone, Jane Stockton and Ann Bliss – none of them anthropologists – were among the political as well as personal friends and comrades who helped provide the support system necessary to sustain so daunting and unorthodox a research project. My children – Rosie, Olivia and Jude – have been a constant source of strength. Many others over the years – my parents, kin, friends and comrades too numerous to mention – gave me insights, courage and support.

More material in her support than anyone, however, was Hilary Alton, who made it a seven-year personal commitment to see to it that I actually finished. Hilary read every line of this book as it was written, sometimes many times; her insights into my reasoning and her judgements on presentation came to seem to me unerringly perceptive and authoritative. Without her firmness and loving encouragement, these pages would still be one more stack in an apparently unending sequence of never-to-be-finished notes, versions and drafts.

Chris Knight, Lewisham, November 1990

Introduction

> Modern bourgeois society with its relations of production, of exchange and of property, a society that has conjured up such gigantic means of production and of exchange, is like the sorcerer, who is no longer able to control the powers of the nether world whom he has called up by his spells.
>
> Karl Marx and Friedrich Engels, *The Communist Manifesto* (1848)

Humanity now has the power to destroy not only itself but most of the more complex forms of life on earth. No one can measure the scale of threat posed by our unplanned global economy as it hurtles along on its present course. What seems certain is that the future of our planet now depends on conscious planning decisions which we do not yet know ourselves to be capable of taking.

No scientific story about our distant past can avoid this troubling fact about our present, nor escape being shaped by it. Western scientific/industrial culture now holds the rest of creation in its shadow. During the four billion years since life itself first evolved, no living subject has ever held such power or been vested with such responsibility. It is a realisation expressed eloquently by the anthropologist Robin Fox fifteen years ago, when the Cold War was still at its height. 'In the past', he wrote then,

> it has not mattered greatly what people believed about themselves and their societies, since nothing that followed from these beliefs could have endangered the species. Man is now rapidly approaching the point – and it will come in the lifetimes of his children – when, unless he takes his survival consciously into his own hands, he may not survive as a species. This requires a revolution in thinking as serious as the Copernican revolution. Man has to move to a *species-centred* view of the human world he inhabits. And he has to do it quickly – within the next fifty years or even less.

'Anthropology, if it chooses to fulfil its mandate', Fox concluded then, 'can make a more significant contribution to this change in man's view of himself than any other science' (Fox 1975a: 271).

Fifteen years later, with the Cold War replaced by new, less stable structures of conflict, the science historian Donna Haraway has taken this argument a bold step further. She asks: What does it *mean* to be species-centred, rather than merely western-centred or middle-class or masculinist in one's scientific outlook? Her book, *Primate Visions* (Haraway 1989), was published in that 'year of revolutions' when the Stalinist project of 'socialism in one country' finally collapsed, opening up a new and fearsome era of global instability but hopefully allowing the workers' struggle internationally to resume at last its own more autonomous, planet-oriented, course. As if the earth-moving events of 1989 were not enough, Haraway in that year shook the western primatological and palaeoanthropological establishment to its roots by unmasking the contemporary political roots of even the most 'scientific' of modern theories of human origins and human nature.

I was at the inaugural meeting of the Human Origins Interdisciplinary Research Unit in Sheffield early in 1990 when I realised that behind the scenes, Haraway's book – never mentioned in the formal sessions – was being talked about in hushed, almost reverential, tones. 'It's hard to avoid agreeing', I heard a senior colleague confide, 'that we are all just telling politically motivated fairy-tales'. Haraway has stripped away the fig-leaves, showing that when palaeoanthropologists wrestle with one another over what it means to be human and over how it was that human life first emerged, they are articulating the most deep-seated contemporary cultural longings whilst simultaneously promoting massively powerful (and of course overwhelmingly western) vested interests. As they argue over the meaning of a grooming bout between baboons, over an enigmatic scratch on a fossilised molar or over some vestigial Middle Palaeolithic lunch, what they and their constituencies are really contesting is the right to close off debate about human potentiality – and thereby determine the future of our planet. The primary ideological battleground on which this contest is being waged is that staked out in contemporary sociobiological and other debates over 'human nature', over what it means to be human and over how human life first emerged. 'The Territorial Imperative', 'The Selfish Gene', 'Man the Hunter', 'Woman the Gatherer' – the weaving of such origin myths is a struggle for power.

This book is an intervention in that discourse. Haraway's work has freed me to be explicitly rather than implicitly political. Although employing many of the narrative techniques of my sociobiological and anthropological professional colleagues, mine is a story rather different from the familiar ones. It is told, ultimately, for another audience, to whom I wish to be accountable. Science is, as it has always been, information which gives us power. But *whose* power? Haraway has demonstrated that if it is just men, or

just middle-class people, or just Westerners – then there can be nothing very objective about the 'science'. As a rule, the breadth of the constituency of scientists determines the precise mix of ideology and objectivity in their paradigms. A narrow base yields narrow, biased science; a wider accountability helps correct such distortions of perspective. Science as I understand the term therefore must be, among other things, both anti-racist and feminist (Haraway 1989). More generally, it cannot exist outside the empowerment of oppressed humanity. Human culture has not always been capitalist; neither has it always been dominated by persons with light-coloured skins. In these pages it will be argued further that culture was not invented by – and has not always been dominated by – men.

We humans – according to the narrative I favour – are a very recently evolved species (Stringer 1988; Mellars and Stringer 1989b; Binford 1989). Anatomically modern humans are known to have existed in Africa and the Near East at most 130,000 years ago; humanity's surviving linguistic and other cultural traditions can be traced back, probably, no more than some 45,000 to 90,000 years. The closeness in time of our biological origins and the apparently explosive pace at which cultural evolution subsequently took over have led many modern writers to describe our origins in terms of a 'revolution' – the 'human revolution', as it is often called (Mellars and Stringer 1989a).

This book is a revolutionary Marxist's reconstruction of that event. I am making my political motivation clear on the understanding that the reader has the right to information of this kind. In the wake of Haraway's extraordinarily liberating work, no palaeoanthropologist can any longer write about the 'Origins of Man' (or indeed of 'Woman') as if it were a matter of dispassionate disclosure of 'the facts'. There are no facts in this field – other than those released by courtesy of fiercely contested theories which are in turn packed with political dynamite.

Admitting this at the outset, let me say that from my own chosen political vantage point, virtually everything primatologists and palaeoanthropologists have been saying about human origins since twentieth-century science began addressing this topic has been wrong – and not just wrong in detail, but utterly wrong! On all the major issues, I think that we would get nearer the truth by systematically positing the exact opposite of what the functionalist and (more recently) sociobiological establishments have been telling us.

For example, whilst most authorities still portray the earliest hominids as they were being pictured many decades ago – as tool-using, meat-seeking bipeds striding out with their new technology from dense forest on to the hot, dry savanna – I prefer what is as yet a minority view. I see them as part-time tree climbers, walkers and, in general, as super-adaptable creatures who

amongst other things enjoyed swimming and diving in rivers, lakes, estuaries and along marine shores (Hardy 1960; Morgan 1982). Whilst with dreary unanimity the establishment still posits the 'nuclear family', Victorian-style, as the basic, primordial cultural institution (see Chapter 14), I would posit the reverse – gender solidarity on a scale sufficient to keep husband and wife in separate camps for much of the time. Whilst they stress female 'loss of oestrus' and 'continuous sexual receptivity' (see Chapter 6), I spotlight menstruation and its associated marital and other cultural taboos. Whilst they stress 'Man the Hunter' or his *alter ego*, 'Woman the Gatherer' (Chapter 5), I see evolving palaeowomen using their increasing solidarity to shape the structure of both hunting and gathering, in addition to much else in life.

A final, very important, difference concerns dating. Whilst most socio-biologists and palaeoanthropologists still perpetuate a tradition according to which culture emerged some two or three million years ago, very gradually and contemporaneously with the manufacture of the first stone tools (Holloway 1969; Leakey and Lewin 1977), I would follow Binford (1989) in shifting the dates forward several million years. 'Culture' as contemporary hunter-gatherers might understand such a term is much, much more than the ability to make a stone tool and pass on the tradition. Symbolic culture involves very widespread levels of synchronised co-operative action. It is not merely an 'adaptation'; it does not appear 'naturally' when large-brained, two-legged hominids are set down in a congenial environment. It requires community members' participation in a universe of shared meanings which are not merely technological but also (and here lie the greater challenges) social, sexual, political, mythological and ritual. I think that this multi-levelled intensity of sociality and mental sharing – and this is what I mean by 'culture' in the following pages – was universally and stably achieved at most 90,000 and more probably some 40,000 to 45,000 years ago (Binford 1989; Trinkaus 1989). I also think that it emerged not gradualistically but in a massive social, sexual and political explosion – 'the creative explosion', as it has been called (Pfeiffer 1982).

When palaeontologists and archaeologists nowadays speak of 'The Human Revolution', it is to this relatively *recent* series of momentous events that they are by common consent referring (Mellars and Stringer 1989a). Of course, there are other stories: many specialists would prefer a much more gradualist version of events. But the chief value of a study of human origins, from my political perspective, is that it demonstrates, firstly, that early life was communist (Engels 1972 [1884]; Lee 1988). Secondly, it teaches us that revolution lies at the very heart of what we are. Far from it being the case that 'no revolution can change human nature', everything distinctively *human* about our nature – above all, our capacities for language, self-consciousness, symbolically regulated co-operation and creative work – are precisely the products of that immense social, sexual and political revolution out of whose travails we were born. Whilst this process was finally con-

summated perhaps 40–45,000 years ago, in the earliest phases of the period known as the Upper Palaeolithic, it seems to me self-evident that so massive a human achievement has relevance for those of us hoping for revolutionary change leading to a more peaceful, sustainable and co-operative world order as the condition of our survival today.

In that sense – because I am motivated politically – I am of course constructing a myth. I am doing what all palaeoanthropological storytellers have been doing since the birth of their science (Landau 1991). The test of a good myth, however, is that it is both widely and enduringly believed. Very few of the stories that palaeoanthropologists have so far constructed have passed this particular test. The stories are always changing, and in detail, as I show in this book, they do not add up. This matters: even a fictional plot must work internally if the audience is to suspend disbelief at all.

But while internal coherence may be an important aspect of a narrative's plausibility, it is not the only one. In the game of scientific discourse, despite all the contestants' many disagreements and conflicts, the players have no choice but to adhere, for the duration of particular debates and contests, to at least some agreed ground rules. The rules that matter are those for disputing what kinds of observation are to count as data. 'The facts' themselves will never be stably agreed upon or there would be no game. But the procedures for constructing and verifying them must be shared as common currency at least up to a point. Were it not for some such agreement, in any event, it would be impossible to speak of a scientific community at all (Kuhn 1970). I am one of those who would accept that palaeoanthropology and sociobiology are disciplines which in the main have overcome this particular hurdle; whatever their limitations, they are not just pseudo-sciences. Most importantly, their relationship with a rather widely pooled, commonly accessible database ensures that there are countless antibodies inoculating participants from excess gullibility, constraining rather rigidly the kinds of stories which can nowadays be told.

I write under such constraints. I fully expect my narrative to be vigorously contested. Like any scientific storyteller, however, I live in at least the faint hope that my own particular myth may turn out to become accepted so widely that – whilst it can never be the final word – it forms part of the kernel of all subsequent stories. In our own culture, such a myth would be termed 'science'. In saying this, there is no intention to belittle science, nor to deny its superiority to myth-making. One story is certainly *not* as good as another (Haraway 1989). I am simply registering my view that the ultimate test can only be a social one. Whilst both science and myth are means through which humans become aware of their power, the first differs from the second in that its data confer power upon more than just one minority section of humanity in opposition to the rest. In general, people nowadays will *not* feel sustainably empowered by a story that evades the rigorous testing in the light of evidence which modern science – at least in principle –

demands. The corollary is that if a story survives such testing and in consequence feels so empowering to so many people that conflict over it is largely brought to an end, then it must be a good myth – and under the rules of modern discourse *deserves* to be termed 'science' (see Chapter 14).

Sociobiology: Political Economy of the 1990s

Founded on the premises of methodological individualism, modern sociobiology is – as Donna Haraway (1989) among others has shown us – the supreme mental expression in the life sciences of the inner logic of late capitalism. In this hyper-liberal perspective, social groups, communities, corporations, institutions, cartels, families, mother–child dyads, hordes, troops and even species all disappear. In their place – within a given 'population' – stands the rational, calculating, profit-maximising individual subject.

This entity has nothing to do with the rounded-out organism of commonsense perception, located in space and time, embedded in relationships and subject to death. For sociobiology, the flesh-and-blood individual – the phenotype – is pure agency. What animates it is a set of shadier, more mysterious entities – complex and usually unique sets of molecular, protein-building instructions known as 'genes', whose spatial locations transcend the physical boundaries of individual organisms, and whose only law is to survive death in one form or another and 'stay in the game' at the expense of all contradictory sets of instructions (Dawkins 1976).

Sociobiology triumphed in the life sciences at the start of the 1980s, a decade symbolised, in Britain and the United States, by the coming to power of governments expressing a new, coherent and explicit conservative ideology often known as the New Right (Rose *et al.* 1984: 3). Sociobiological writers characterised the activities of their 'Selfish Genes' (Dawkins 1976) in terms remarkably similar to those used to describe the enterprising moneymakers central to the new current's political manifestos. Even the most austerely academic of books and articles constantly resorted to metaphors derived in the most obvious manner from liberal economics and from modern military theory – giving us, for example, genetic 'arms races', 'investment strategies', 'cost-benefit calculations', 'payoffs' and so on. To many on the left during the 1970s and 1980s, these concepts seemed so ruthlessly bourgeois and right-wing as to preclude the possibility that feminists, socialists, green activists or others could possible learn anything from them (Sahlins 1977; Rose *et al.* 1984).

In the 1990s, however, this situation has begun to change. It has begun to be realised that capitalism is not all negative, and that its vigorous, explicit manifestation in thought can do us all much good. Once again, Donna Haraway is central here, for her book has probably done more than any other single work to clarify for the left what sociobiology has actually

achieved. Particularly in her chapters on the work of sociobiologically trained feminist primatologists (Haraway 1989: 176–9; 349–82), she has taken us beyond the left's knee-jerk complaints about 'biological reductionism' to a new understanding of the paradoxically liberating role which this uncompromisingly 'late capitalist' school of thought has played.

Central to *Blood Relations* is the firm belief that sociobiology's achievements are to a modern Marxist analysis of sociality what the constructs of classical pre-Marxist political economy were to Marx himself. They are the corrosive acid which eats away at all illusions, all cosy assumptions about 'the welfare of the community' or 'the brotherhood of man', all unexamined prejudices about how 'natural' it is for humans to co-operate with one another for the good of all. There is much that is useful in this.

Sociobiology came on to the scene in triumphant opposition to the well-meaning functionalist theory according to which biological organisms are genetically selected for their ability to act for the good of their social groups. This functionalist theory was essentially social-democratic and corporatist: it saw the 'species' or 'group' very much as the prevailing political currents of the period (including Stalinism) saw 'the state'. Just as the various constituent bodies of the 'welfare state' or 'nation' were supposed to pull together for the good of the whole, so the individual organisms making up biological social units were supposed to be 'by nature' inclined to work for the common good.

Like a powerful solvent, the sociobiological paradigms of the 1980s tore into all this, eating away at the supposed co-operative bonds holding together 'species', 'communities', 'hordes', 'mother–child units' and other sentimentally conceived 'holistic entities'. In doing so, sociobiology produced results which to my ears recall Marx's and Engels' words written in 1848 in the *Communist Manifesto* (Marx and Engels 1967 [1848]: 82):

> The bourgeoisie, wherever it has got the upper hand, has put an end to all feudal, patriarchal, idyllic relations. It has pitilessly torn asunder the motley feudal ties that bound man to his 'natural superiors', and has left remaining no other nexus between man and man than naked self-interest, than callous 'cash payment'. It has drowned the most heavenly ecstasies of religious fervour, of chivalrous enthusiasm, of philistine sentimentalism, in the icy water of egotistical calculation. It has resolved personal worth into exchange value, and in place of the numberless indefeasible chartered freedoms, has set up that single, unconscionable freedom – 'Free Trade'.

Now, the point is that Marx found *within* this logic of capitalism – not from something external – the revolutionary antithesis he was seeking. He analysed the works of Adam Smith, Ricardo and the other classical political economists carefully on the understanding that such authors were the leading social scientists of their time, their work representing the cutting edge of *scientific* thought on the issues which concerned him. Refashioned in the

hands of Marx, the findings of these champions of free-market economics were transubstantiated – into a body of theory which validated as never before the notion of men and women as intrinsically, necessarily social, and the future as intrinsically, necessarily communist.

Sociobiology may have a comparable significance for our age. Ideologically right-wing through and through, it incorporates nonetheless much of what is most advanced in current scientific thought on the nature of life. Not only does it seem obvious to me that its political metaphors actually work – that is, they are enlightening, clearly engaging with something actually going on in the natural world. It is equally apparent that the old, functionalist and group-selectionist biological paradigms – counterparts in science of social democracy in politics – in their time were like bad book-keeping. They made it impossible to see what needed to be seen.

When primate social groups were seen as 'functional wholes', the forms of data on which this book depends were simply concealed from view. No one could pick up conflicts of interest between males and females, between parents and offspring, or indeed between social group-members of any kind, since the members of each biological 'community' were seen by definition as harmoniously integrated on the model of the heart, lungs and other parts of a single organism. It took sociobiology with its calculus of genetic interest to reveal female primates, for example, not as passive valuables herded about and organised by dominant males – but as agents in their own right, active strategists fighting for their own genetic goals (Haraway 1989: 176–9). It took sociobiology to dispense with confusing and sentimental terms such as 'the mother–infant dyad', showing that in fact an infant can have rather different genetic interests from its mother – as (for example) when a female needs to wean an existing child in order to make room for another.

Sociobiology does not insist that all individuals are selfish. It would be a crass misreading to confuse the molecular 'selfishness' of sets of genetic instructions with selfishness at the behavioural level on the part of flesh-and-blood individuals. Nonetheless, sociobiology (like revolutionary Marxism) is about struggle and conflict. Whilst not denying altruism in nature, it insists that this constitutes a challenge to our understanding – a *seeming* anomaly which cries out to be *explained*. How much more helpful this is, scientifically, than the view that co-operation is the default condition, so 'natural' that explanation is not really necessary! Had it not been for Barbara Smuts, Shirley Strum, Sarah Hrdy and other sociobiologically informed primatological fieldworkers (see Haraway 1989), many of them what I would term 'bourgeois feminists', the basic concepts of Marxism – of struggle, conflict, contradiction and revolution – would have been inapplicable to the study of monkeys and apes. The relevant data on conflict would simply have been lacking. By the same token, without sociobiology, Marxism would have remained (as it has remained for many decades) inapplicable to palaeoanthropology and to the study of human origins. The concept of 'class

struggle' in particular would have remained boxed in by bourgeois ideology, denied all claims to universality, confined strictly and mechanically to recent cultural history – instead of being seen (as it is in this book, cf. Engels 1964 [1873–86]) as a construct with resonances echoing far back into our evolutionary past.

Gradualism, Genes and 'Memes'

A modern tale of human origins must conform to various narrative conventions if it is to be heard. Usually, this involves an element of gradualism. The gradualism which seems inescapable stems from the need for consistency with contemporary Darwinian theory. My story will convince no one if it seems to be contradicted by the basic laws of genetic inheritance, random mutation and non-random differential selection which – as I am quite capable of accepting – have governed evolution on this planet since life itself began some 3–4,000 million years ago.

Admittedly, many palaeontologists and evolutionary biologists nowadays describe themselves as 'punctuationists'. But even those who see in the evolutionary record long periods of stability which are on rare occasions 'punctuated' by sudden bursts of change (Eldredge and Gould 1972) hold that their 'sudden' changes are in fact strung out over immense periods, each quantum leap or 'speciation-event' consisting of barely perceptible modifications stretched across hundreds of thousands if not many millions of years. No one can tell a story about a new species of primate, for example, which leaps into existence from one generation to the next. All evolutionary theory is inherently gradualist in this basic sense, and must remain so if it is to have any credibility at all.

On the other hand, virtually all evolutionary biologists are believers in radical and – on a geological timescale – 'sudden' change, although some may feel that such events are extremely rare. Richard Dawkins in his bestseller, *The Selfish Gene* (1976), stresses two such very rare or 'abnormal' events – two events in the course of which something utterly new seems to have appeared in the known universe. One of these is the origin of life. The other is the origin of culture. Since (as Dawkins himself suggests) the first may have something to teach us concerning the second, it is perhaps worth touching on the problem of life before pursuing any further the main topic of this book.

Almost all biologists agree that life as we know it had only *one* origin, giving rise to a *single* history, characterised by shared derived characteristics such as the genetic code and the universal molecular symmetry of metabolised sugars (see, e.g., Margulis 1982). A much-contested contemporary scientific question is whether *modern humanity* and *culture* as we now understand this term had a single origin and a single history in something like the same way (Mellars and Stringer 1989a).

What might there be in common between life's origins and the emergence of culture? Many thinkers have linked these two processes. If we accept Dawkins' version, as presented in *The Selfish Gene* (1976: 208), the 'genetic takeover' accomplished at life's birth was not to be the only one ever to occur:

> As soon as the primeval soup provided conditions in which molecules could make copies of themselves, the replicators themselves took over. For more than three thousand million years, DNA has been the only replicator worth talking about in the world. But it does not necessarily hold these monopoly rights for all time. Whenever conditions arise in which a new kind of replicator *can* make copies of itself, the new replicators *will* tend to take over, and start a new kind of evolution of their own. Once this new evolution begins, it will in no necessary sense be subservient to the old.

With the origin of culture, according to Dawkins, there was launched just such a novel form of evolution, based on the immortality not of the gene but of the 'meme'.

A successful 'meme', according to Dawkins, is a portion of cultural tradition – say, a tune, an idea or a catch-phrase – which survives in the memories of successive generations of humans and is capable of evolution at a very rapid pace. Just as genes propagate themselves in the gene pool by leaping from cell to cell, so according to this view, memes propagate themselves in the meme pool by being transmitted from brain to brain through a process which, in the broad sense, can be called 'learning' or 'imitation'.

History or cultural change, in this view, is basically the evolution of memes. Because the differential selection and preservation of memes has little to do with the genetic constitutions of the individuals who memorise them, it follows that cultural evolution is in Darwinian terms a quite peculiar thing, and that in gaining an understanding of it 'we must begin by throwing out the gene as the sole basis of our ideas on evolution' (Dawkins 1976: 205). Some rudimentary examples of 'cultural' or 'memic' evolution can be found in birds and in monkeys, but as Dawkins (1976: 204) points out, '. . . . these are just interesting oddities. It is our own species that really shows what cultural evolution can do.' The appearance of humanity, in this view, opened the door to a 'new takeover' by memes – in effect a seizure of power by the new replicators, ending or at least transcending the tyranny of the old, blind genetic replicators (Dawkins 1976: 208, 215). It was rather like the origin of life all over again – but on a new, higher level. In any event, something utterly new had once again begun to happen. There was a leap to a new level of determinism, requiring for its analysis a distinct – more-than-biological – kind of science.

I intend to draw on this parallel between 'genes' and 'memes' not because I find the analogies to be entirely convincing (for variations on the theme see

Cavalli-Sforza and Feldman 1981; Lumsden and Wilson 1981; Boyd and Richerson 1985; Rindos 1985, 1986), but because this way of looking at matters helps to validate my own narrative of a 'human revolution' which transported evolution *beyond the parameters of ordinary Darwinism*. The 'memes' concept implies that just as a theory of life's origin must explain where Darwinian principles came from when they had never operated within our part of the universe before, so my book must explain where the still more complex phenomenon of memic immortality came from. Dawkins stresses that no theory of life's origin can 'contradict the laws of physics'. But he also stresses that such a theory will have to 'deploy these laws in a special way that is not ordinarily discussed in physics textbooks' (Dawkins 1988: 15). The corresponding logic applies to the task I have set myself here. Naturally, *Blood Relations* must not contradict the laws of Darwinian natural selection. But it must deploy these laws *'in a special way that is not ordinarily discussed in biology textbooks'*. Biology – even sociobiology – will not be enough.

Nature and Culture

It will be clear that the notion of a human revolution both validates and to an extent depends upon the peculiarly western cultural construct of a domain called 'nature' which stands in polar opposition to a different domain known as 'culture'. Since I am convinced that it stands for something real, I like this distinction and intend to respect it. With his notion of 'memic immortality', Dawkins has both replicated this cultural construct and refined it, helping those of us who value it to perform the difficult task of determining precisely where the boundary between 'nature' and 'culture' should be drawn.

An implication of Dawkins' argument is that in deciding whether palaeoanthropological events belong on one side or the other of the divide, what matters is not whether memes are occasionally replicated. What matters is (a) their centrality in maintaining the continuity of social structure and (b) whether true immortality is open to them. As noted earlier, many creatures can pass down memorised patterns from one generation to the next. Vervet monkeys in the Amboseli National Park, Kenya, for example, have been observed to dip dry *tortilis* pods during a drought into the sap-filled well of a *tortilis* tree, a technique which makes the parched pods much more nutritious and edible (Hauser 1988). This technique is not an element in the ordinary species-specific behavioural repertoire of the vervets; it has to be invented by an individual during a particular drought and then passed on to others via imitation. Why, then, can we not speak of vervet monkeys as having 'culture'?

Part of the reason is that such learning-dependent skills are peripheral to the political determinance of structure. However great their survival value, they are marginal to the maintenance of social-structural continuity from one generation to the next. This means that although behavioural patterns may

fluctuate, seasonally or in other ways, there can be no real, cumulative *social evolution* beyond that which is chained to the slow evolution of genes.

Linked to this limitation, such learned skills tend to circulate only in limited pockets of time and space before being forgotten. In the case of Hauser's vervets, social groups are so small that the pod-dipping technique 'has a high probability of disappearing by chance alone' (Hauser 1988: 341). A period of drought has only to end for this element of collective wisdom to get forgotten, although it will very probably be reinvented by some other individual or individuals in a subsequent drought and shared within one or more small groups all over again. The important point here is that memes under such conditions can experience no real evolution. There is no widespread, universalistic information pooling and therefore no progressive accumulation of memes – only the endless rediscovery within small groups of what previous generations may already have known.

It is this kind of limitation which – according to my origins narrative – anatomically modern humans transcended in the course of those momentous events which led up to and were consummated in the Upper Palaeolithic revolution whose reverberations began rippling across the world between 40,000 and 45,000 years ago. Whilst chimpanzees have been shown to have preserved and developed a surprisingly rich traditional knowledge of the use of medicinal plants (Sears 1990), and whilst there can be little doubt that archaic humans such as the Neanderthals had palaeotraditions, palaeolanguages and perhaps also palaeorituals (Marshack 1989), my point is that communication between local groups prior to the Upper Palaeolithic was poor (Gamble 1986a). Memes could replicate themselves and accumulate, but only patchily, within small, circumscribed, scattered and often isolated social units. The real breakthrough – the 'creative explosion', as it has been called (Pfeiffer 1982) – was made when new and extended patterns of social interaction allowed such local boundaries to be transcended. At that point, in a process which we might liken to 'freedom of the press' or 'ideational free trade', memes could circulate freely over such distances that it no longer mattered (from a memic point of view) whether a particular local population survived: so many intercommunicating populations preserved at least something of the basic pool of memes that memic immortality as such was now assured. In my story, the human revolution was finally consummated when – paralleling life's establishment of the infinite immortality of genes – events opened up channels for the transmission of memes across what were in principle indefinite expanses of space and of time.

Agreements, Contracts and the Cultural Domain

Symbolic culture as I understand the term, then, has its basis in the immortality of whole sets of extremely complex memes – culture-constituting instructions shaped not just by behavioural interaction between organisms

and their environments, and derived not only from the genetically based phylogenetic conservatism of the species, but shaped also through the relationship between these and a highly specific, rich and accumulating fund of collective wisdom or tradition materialised in technology, design, language, art, ritual, kinship and so on.

What were the conditions which had to be established to enable such complex memic patterns to be preserved? Central to my argument is politics. There could be no memic immortality in the absence of the essentially *political* capacity to establish agreements, rules and contracts. No human kinship system, no economic system, no religious community and indeed no cultural institution of any kind could function without these. Although my focus will be essentially upon the notion of 'blood' contract, let me for the moment leave aside this dimension and consider 'contract' in general as a novel evolutionary possibility.

Not even the simplest of collectively agreed or sanctioned contracts can occur in nature. Despite constructs such as kin selection (Hamilton 1964) and (in the case of large-brained creatures) reciprocal altruism (Trivers 1971), sociobiological theory insists that plants and animals do not and cannot adhere to 'agreements'. Instead, each organism is programmed to pursue its genetic interests and – except when mistakes are made – to allow nothing to get in its way. This (according to sociobiological doctrine) remains the case no matter how great may be the ultimate costs of such activities to the group or community to which each individual belongs.

Dawkins (1988: 184) drives home this point vigorously in a fascinating anti-socialist discussion concerning plants. 'Why for instance, are trees in forests so tall?', he asks, and replies:

> The short answer is that all the other trees are tall, so no one tree can afford not to be. It would be overshadowed if it did.

Dawkins points out how difficult we morally minded humans find all this. As we examine the situation at any point in the course of the struggle for sunlight, it becomes obvious that the tree community as a whole has gained no more light than would have been available had each tree stayed short. We might well ask: Why don't the trees co-operate? As Dawkins puts it:

> if only they were *all* shorter; if only there could be some sort of trade union agreement to lower the recognized height of the canopy in forests, *all* the trees would benefit. They would be competing with each other in the canopy for exactly the same amount of sunlight, but they would all have 'paid' much smaller growing costs to get into the canopy. The total economy of the forest would benefit, and so would every individual tree.

Yet this seemingly logical solution has never been hit upon. Neither trees nor any other plants or animals have ever come to realise the immense potential benefits which, theoretically, could stem from mutual self-restraint

and solidarity in the interests of all. Disappointingly for those who would root a co-operative world political system in a benevolent 'nature', there is never in the animal world a collectivity capable of imposing global harmony or 'rational planning'. Such planning might seem 'objectively necessary', but as Dawkins continues in his tree discussion: 'Unfortunately, natural selection doesn't care about total economies, and it has no room for cartels and agreements.' There has simply been an 'arms race' in which forest trees became larger as the generations went by. At each stage, there was no intrinsic benefit in being tall. The only point was to be always just that little bit ahead of one's neighbours.

I have characterised Dawkins' discussion here as 'anti-socialist'. In a way, at least by implication, it is. But Dawkins makes his case without for a moment suggesting that it therefore makes no sense for humans to take collective action, form trade unions or collaborate to protect the global environment. He is not against – say – trying to save the large whales (whose genes are quite different from ours) from becoming extinct. His point is simply that no other species would artificially and through collective action try to impose self-denying regulations to curb the long-term effects of short-term competitive profit-seeking.

To me, it seems fruitless to deny this. But the implications are not necessarily 'reactionary'. They must seem so only to those who require their constructs of what is 'moral' or 'socialist' to match a model supposedly afforded by 'nature'. What logic is there in this? Surely, the point is that we speaking primates are not plants or animals but culturally organised *humans*. This means, on the one hand, that we have evolved to a potentially catastrophic degree the power to upset the balance of nature on our planet, destroying the Amazonian and other tropical rain forests, puncturing the ozone layer, polluting our atmosphere, altering the climate and threatening our own and many other biological species with complete extinction in the event of nuclear war. But it also means that the competitive pursuit of short-term 'selfish' interests is emphatically *not* the only political logic of which we are or have been capable.

Solidarity and Memic Immortality

Blood Relations is designed to show how it was that in evolving our biologically improbable languages, kinship systems, rituals and taboos, we humans have shown that we *are* capable of establishing 'artificial' rules which are in the interests of whole clans, interconnected bands, villages or entire communities, and enforcing respect for these. Although there is always some tension between personal interests and wider collective ones, we are and always have been capable of precisely that concern for 'total economies', and precisely that power to form trade unions or other contractual alliances which, as Dawkins points out, are not to be found in the natural world.

It would be a truism to say that solidarity in a general sense – including clan solidarity, tribal solidarity, ethnic rebellion, nationhood, class solidarity and other forms – has been a vastly important component of all human history up until now. No human sociobiology which failed to take account of such phenomena could claim to have much of interest to say to social historians or sociologists. Yet of course there are good reasons why sociobiologists have chosen not to focus on such things. Their science is an attempt to explain all social life in terms of constraints imposed by the 'selfish' self-replicatory interests of genes. This works well in the study of insects, and even in the study of primates. Up to a point, it also works in the study of ourselves. But only up to a point.

'We, alone on earth', Dawkins writes in concluding *The Selfish Gene* (1976: 215), 'can rebel against the tyranny of the selfish replicators'. The difference between ourselves and other creatures is that we *can* transcend the level of determinism which is represented by competition between genes. Unlike trees competing for sunlight, we humans *can* form trade unions or comparable bodies. We *can* act with conscious foresight in our collective long-term interests, instead of remaining wrapped up in our short-term individualistic pursuits. Where the 'total economy' of our planet is concerned, the idea of taking collective responsibility for it may seem a novel and daunting political challenge, which we have barely begun to rise to. Yet it may be precisely such a new cultural 'leap' that is required if our own and many other species' genes are to have any future at all.

In any event, it is part of the argument of this book that our power to make and to enforce life-enhancing collective agreements has been with us since the very inception of culture. My task in the chapters which follow is to investigate how such abilities could have arisen.

Language

Politics must be centre stage in any discussion of 'memes'. This is because a condition of memic immortality is at least a *relative* absence of political conflict. If two primates are fighting, then for the duration of hostilities there will be little 'meeting of minds' and therefore little if any memic sharing or interchange. By contrast, two close allies – perhaps in a coalition directed against a third – are likely to be sharing and exchanging memes as a matter of course. Where a coalition is large, the likelihood of memic survival within it becomes magnified correspondingly.

It is an obvious point, but one which has been all too often missed. It has a bearing on the question of the origins of language – 'the most remarkable and characteristic of all human creations' (Renfrew 1987: 1). With many others, I take the view that our species did not become fully human until the abilities of advanced reasoning that language helps to foster were fully developed (Binford 1989: 36; Mellars and Stringer 1989b; Renfrew 1987: 1; Cavalli-Sforza *et al.* 1988).

However uncertain the results, it is intriguing to examine fossil hominids such as the Neanderthals for signs of the physical ability to articulate the range of sounds which modern humans can pronounce (Arensburg *et al.* 1989; Lieberman and Crelin 1971; Lieberman 1988, 1989). It is also useful to seek to identify the basic 'design features' common to all human languages – features distinguishing them from the communications systems of animals (Hockett 1960; Hockett and Ascher 1964), or to debate whether the primary channel for earliest human language was gestural or vocal (Hewes 1974; Hill 1974). But such questions concerning the mechanics of language are obviously secondary as far as the real theoretical problems are concerned.

A human linguistic system is made up of 'memes'. In the case of language, these are phonetic rules, syntactical rules, semantic rules and 'pragmatics' – sets of conventionally agreed relationships between what participants hear or say and what they are supposed, consequently, to do. If these latter rules – insufficiently discussed in theories of language origins – are not respected, language itself cannot evolve. In short, the creativity behind language 'arises not from linguistic skills narrowly conceived but from sociality and the social matrix in which one lives' (Carrithers 1990: 202). Or as the linguistic philosophers Bennett (1976) and Grice (1969) among others have shown, human speech is possible only against a *logically prior background of social interaction and sociality*.

Language is 'a product of the collective mind of linguistic groups' (De Saussure 1974 [1915]: 5). It 'exists only by virtue of a sort of contract signed by the members of a community' (De Saussure 1974 [1915]: 14), and has no existence apart from that contract. It has frequently been observed (for example by Wescott 1969: 131) that the word 'communication' comes to us from the Latin adjective *communis*, 'common'. This word, in turn, is derived from a reconstructed Indo-European verbal root **mey-*, 'to share' or 'to exchange'. For a speech community to emerge, it is necessary that intelligent hominid individuals should *share understandings*, and that these mental sharings should extend even to those sensitive areas – such as food and sex – which are most liable to provoke the kinds of conflict which would otherwise lead to blows. The sharing of understandings, the sharing of wealth such as food and a downgrading of the role of violence are all in this context interconnected. 'Language', as the French anthropologist Pierre Clastres (1977: 36) has cogently put it, 'is the very opposite of violence'; speech 'must be interpreted . . . as the means the group provides itself with to maintain power outside coercive violence; as the guarantee repeated daily that this threat is averted'.

Such a capacity for transcending physicality has obviously less to do with the genetic constitution of individuals than with the political/social/sexual situation in which they find themselves. To the extent that, in any community, issues between individuals or groups are decided purely or primarily physically, language not only cannot evolve – it loses all its relevance.

This was perhaps the most important lesson to emerge from the many attempts made some years ago to teach chimpanzees to speak (for a survey see Desmond 1979). For example, when Roger Fouts (1975: 380) and his colleagues taught American Sign Language to the chimpanzees 'Booee' and 'Bruno', explaining to them how to ask politely for food, everything worked well – for as long as it was *humans* who were called upon to make the culturally required responses. Once the animals were left to give and take food or other valuables between themselves, their newly learned skills were left hanging in a cultural vacuum, deprived of any meaning or use:

> The food eating situation has turned out to be somewhat of a one-way ASL communication because neither of the two males seems to want to share food with the other. For example, when one of the two chimpanzees has a desired fruit or drink the other chimpanzee will sign such combinations as GIMME FRUIT or GIMME DRINK. Generally, when the chimpanzee with the desired food sees this request he runs off with his prized possession. (Fouts 1975: 380)

So much for asking. Any chimp seriously wanting food or drink, then, must forget linguistic subtleties and fight for its objectives using hands, feet or whatever other instruments are available.

It is undeniable that compared with chimpanzees, humans have more highly evolved speech areas in the brain, and that the capacity to learn any language has a major genetic component. But this must not obscure the essential fact that the conditions for language's relevance have always been political. The problem for Booee and Bruno was not their inadequate linguistic competence or training. It was their lack of involvement in a wider system of cultural meanings. The two animals were not citizens within a chimpanzee republic; neither were they 'classificatory brothers' within a chimpanzee counterpart of an exogamous clan. Their rights and duties were not codified in the name of a higher authority; neither had they entered into any moral contract regarding the sharing of valuables such as food or sex. It was for these reasons that they lacked a social universe capable of making human language even remotely worth learning – except, of course, for those periods during which they were entirely cocooned as individuals within an artificial, fully cultural, human foster-family. Just as one does not speak to one's enemies, so there would be little to be gained from conversing with a calculating rival who opposed one's own interests at every point. A growing child who got hit in the face by its parents on requesting love or support would develop only the most stunted of linguistic skills. No one can sustain the use of speech for very long unless there are others ready not only to listen, but to act with at least reasonable predictability in accordance with agreed rules on the basis of what is said.

Human language is in this context utterly dependent on the rest of

culture, and has no function in its absence. 'Without language, culture could not exist; but without the rest of culture, language would have no function' (Trager 1972: 6). For language to work, in short, there has to be a deeper, sub-linguistic level of mutual understanding already built up in relation to the most important things and underpinning any agreement on the more superficial level of purely linguistic usage.

It is for this reason (and not just out of considerations of space) that I have chosen in this book not to concentrate heavily on the topic of language, despite its evident centrality. Instead, I have focused on what I consider to be the political conditions essential for language's emergence. I have stressed that languages are spoken effectively only within coalitions which can evolve into stable 'speech communities', and that therefore the important thing is to explain how coalitions of the necessary stability and scope could have been formed.

Primeval Soups and Coalitions

Primates' calculations of genetic self-interest frequently induce them to form coalitions. These are not exactly 'trade unions' or contractual 'agreements', but they seem to be the closest we can get to these in the natural world. When one primate forms a coalitionary alliance with another, each member of the alliance supports his or her partner when in conflict with a third party, *on the understanding that this will be reciprocated should the need or opportunity arise*. It is possible for individuals to 'renege' on such understandings, but there can be no doubting their reality for those who enter into them. An animal who reneges on a coalition partner is taking a risk, since the victim will remember the event and perhaps refuse needed support in the future (see, for example, Harcourt 1988).

Within stable coalitions, evolving protohumans could have shared certain understandings and passed on proto-cultural traditions such as methods of foraging or tool use (Wynn 1988; Hauser 1988). By contrast, where individuals were left to fend for themselves in a behaviourally stark battle of each against all, 'memes' – to return to Dawkins' term – would not have had a favourable medium within which to replicate themselves.

Some kinds of memes may have been transmissible even between hostile individuals. A particular weapon-using technique, for example, might have been copied by one contestant following defeat at the hands of a better-armed rival. However, memic immortality even on this level would be favoured by defensive coalitions and alliances – male chimpanzees probably accomplish their highest cognitive levels in the course of 'warfare' between rival co-operative groups (see references in Alexander 1989). More neutrally, technical foraging tricks may have been relatively easily transmitted, the only requirement being sufficient mutual tolerance to allow imitation to take place. Again, however, the likelihood would be that even these would soon

be forgotten unless the techniques were dispersed widely within coalitions which met frequently and in mutually supportive contexts.

Much more resistant to transmission, however, would have been memes *which specified something about how society should be organised.* These could never have percolated through a population riven by boundary disputes, inequalities or power conflicts, for the simple reason that the dominant and subordinate, those in one coalition and those in the next, would have had such very different interests and perspectives. Wherever a *political* or *sexual-political* meme travelled from brain to brain, replicating itself in identical copies, it could only have been because the individuals so connected already possessed much in common. They must already have shared the same social and political interests, providing them with a common vantage-point from which to view their world. In this context, the fact that so many widely dispersed contemporary populations of hunter-gatherers (among other peoples) share mythologically and ritually codified memes of this kind says much about the scope of coalition-forming which the human revolution must have entailed.

A coalition is a situation-dependent, temporary and informal agreement to share power, rather than fight over it. To the extent that individuals share power, political memes can be freely transmitted between them. In this context, we can posit a simple relationship: the stronger, the more stable and the broader each coalition the greater the likelihood of the spread of rudimentary political and other memes within it. Extensive and strong coalitions would have been the complex components of the new 'primeval soup' – as Dawkins (1976: 206) terms it – within which culture's 'new replicators' could have begun to evolve towards take-off point.

Emergence of the Human Coalition-forming Capacity

The specific *sexual-political* concept of coalition-forming central to this book represents the development and extension of an idea first suggested by the biological anthropologist Paul Turke (1984). Turke's field of interests explains my subtitle, *Menstruation and the origins of culture.* His aim was to explain the emergence of the human female reproductive system; his basic finding was that somewhere along the road towards fully human status, evolving hominid females must have systematically formed coalitions of a particular kind. What was special about these coalitions was that the females within them *synchronised their ovulatory cycles with one another.*

Published in the journal *Ethology and Sociobiology*, Turke's article was a contribution to a long-standing debate on the evolution of human female reproductivity. I had long felt that there was something explicitly competitive about the manner in which female chimpanzees and many other primates display their brightly coloured, swollen genitals at or around the time of ovulation. By the same token, my guess had been that the human condition of ovulation concealment and absence of sexual swellings had evolved in the

context of a less behaviourally competitive sexual-political dynamic. To be more precise: I had long felt that inter-female *gender-solidarity* had had something to do with the unusual and characteristic features which the human female showed.

Turke's article seemed to me to translate such intuitive guesses into the language of science. A system in which a few 'alpha-males' monopolise the bulk of the female population may not be as common as was once thought (for a discussion, see Haraway 1989: 304–15; 349–67), but neither is it usual for all males in a population to have equality of access to the available mates. To the extent that the receptive females in a given primate community tend to be monopolised by only the more dominant males – Turke pointed out – they must tend to keep *out* of phase with one another. This is because each dominant male can adequately satisfy the females he consorts with only on condition that they come into receptivity not simultaneously but in turn. Should all of his females come into oestrus simultaneously, their demands over the next few days would be unmanageable and he would risk losing them to neighbouring rival males.

Turke is prominent among those sociobiologists who have looked at human evolution from a female-centred theoretical standpoint, viewing evolving protohuman females not as passive reproductive resources but as active agents in their own right. He argues that evolving protohuman females would have had compelling reasons to reject anything resembling an 'alpha-male' system. Such a system would in effect have 'wasted' the potential usefulness of all the unmated, less-dominant males. Faced with heavy child-care burdens and requiring as much male provisioning and parenting assistance as possible, evolving protowomen would have needed to approximate towards a situation in which inter-male differentials (in terms of reproductive success) were minimised. Selection pressures on biological features such as duration of sexual receptivity would have acted to favour those females who resisted their separation from potentially useful males, including males behaviourally less inclined towards fighting and/or direct struggles for dominance.

Imagine, writes Turke, a group of evolving hominid females who are under pressure to maximise their harnessing of the energies of even the least dominant adult males, each insisting on the support of at least one male for herself. It would then be logical for them to synchronise their ovulatory cycles with one another in groups – a course which at the same time would lessen direct sexual competition among themselves. In these circumstances, pronounced sexual swellings would not be predicted. Indeed, Turke goes on to show that it would be in such females' interests – if they wished to keep their partners with them – to dampen their signals markedly, eventually concealing the moment of ovulation completely and extending receptivity uniformly throughout the cycle. This, of course, is what human females do.

Without entering into the details of this argument here (see Chapters

6–9), let it be said simply that on reading Turke I felt that this model had more than the virtues claimed for it by its author. Firstly, it seemed to me to represent a sociobiologist's discovery of the virtues of a kind of 'egalitarianism', in that it envisaged a levelling process in which inter-male as well as inter-female status differentials were progressively minimised. Females and males according to Turke's model still had counterbalanced gender-specific interests, but within each gender group, enhanced levels of mutual tolerance and reciprocity must by implication have prevailed. Involvement in the synchrony envisaged by Turke would have demanded of each individual – male and female – a very high degree of co-operative *awareness of others*. Although he himself did not treat the emergence of large, gender-specific coalitions as a factor underpinning the transmission of memes, it seemed to me that Turke had successfully defined some of the basic sexual-political preconditions under which memic evolution could have evolved towards take-off point.

Secondly and equally importantly, I soon realised that with its emphasis on *ovulatory synchrony*, this particular model solved a number of theoretical problems I had been grappling with for years. These were not restricted to evolutionary biology; they extended to palaeontology, archaeology and to my own more familiar terrain of social and symbolic anthropology.

The Myth of Matriarchy

To explain the full significance to me of Turke, I must retrace my steps a little and return to the subject of political belief.

The myth on the basis of which I first became drawn to anthropology was that of Friedrich Engels in *The Origin of the Family, Private Property and the State* (Engels 1972 [1884]). Any reader of this book will without difficulty recognise my narrative as a version of that tale. Human society was originally communist; men and women were free and equal; sexual and other forms of oppression were at first unknown.

Engels (1972 [1884]: 49) argued that whilst primate societies were sexually competitive and incapable of sustaining solidarity, the transition to earliest human life placed solidarity first. This solidarity was not just a matter of technical co-ordination or co-operation in the hunt. So powerful were the first forms of solidarity that even sexual jealousy was transcended: whole groups of kin-related women were 'married', collectively, to whole groups of men (Engels 1972 [1884]: 49–50).

Because of such 'group marriage', according to the Engels myth, no one could know who the father of a particular child was: only the mother was known. Consequently, kinship tended to be traced only through the female line. The result – skipping a few stages in Engels' argument – was 'the matrilineal clan', whose features Engels derived from Lewis Morgan's ac-

count of matriliny among the Iroquois Indians. The 'matrilineal clan' or 'mother-right gens' (as Lewis Morgan usually termed it) was a group of women and men united by blood, descended from a common ancestral mother, sharing joint ownership in land, longhouses, children and other valuables, and based on a strict rule stipulating marriage outside the clan. Following Morgan, Engels characterised the internal life of an Iroquois longhouse as a form of 'communism'.

Clan solidarity – according to this view – always split or cut across the biological family, since a husband would always belong to and owe his primary loyalties to one clan while his wife and children belonged to another. Following Morgan, Engels insisted that it was vital to an understanding of human history and prehistory to accept this priority of the unilineal clan over 'the family'. In the beginning, marriage as modern Europeans understand this term was unknown. A husband acquired neither unconditional property rights in nor authority over his wife and her children. Instead, a man's kinship rights were in his sister and her children, just as his shared rights in clan property were rights in the resources of his own matrilineal clan, not his wife's. The virtue of this system, for Engels, was that it precluded the exploitation and oppression associated, at a later historical stage, with the emergence of 'the family, private property and the state'.

In my early twenties, when my political allegiances were just forming, I needed this myth because without it I could see no way of making communism seem either reasonable or possible. All the other accounts, as far as I could see, were so many different anthropological ways of rooting the contemporary social order in 'nature' or in the very foundations of earliest cultural life. Belief in these other myths, it seemed to me, must rule out any hope for real change in our contemporary world. If it was true that the nuclear family – basic cellular unit of modern capitalist society – was also central to nature and to all historical forms of kinship organisation to date, what hope was there of replacing it nowadays by something else? If male dominance in the family had always existed, what hope was there for fundamental sexual change? If domestic life could never be social and collective, with real love and solidarity extended beyond its contemporary nuclear family bounds, what hope was there for self-organised community life, sustainable workers' power or a future without the state?

But although I needed Engels' wonderful myth, I could not make it work. In my mid-twenties it rapidly became evident to me that bourgeois anthropology had gained complete hegemony in this area in the period since Engels' death, and that by the late 1960s no Marxist had made even a faintly credible attempt to keep abreast of developments in order to keep the story alive. As I began tussling with the literature, I realised that my political world was divided between comrades who knew nothing about anthropology but supported Engels all the same, and others who were familiar with the recent literature and had therefore abandoned the myth.

My main need was to find some real evidence for that sex-related 'solidarity' at the heart of earliest culture which had seemed to me to be the fundamental, indispensable kernel of the Engels myth. If necessary I could dispense with virtually any other aspect of the story; for political reasons I could not let go of that.

Engels Regained

In 1966, perhaps three or four years after assimilating Engels, I discovered an article by Marshall Sahlins in the *Scientific American* entitled 'The Origin of Society' (Sahlins 1960). The tone seemed weighty and authoritative, the article's privileged positioning within the journal signalling (as I thought) Sahlins' status as a leading expert on this issue. I at once realised the piece was exactly what I needed.

Just as Engels had written, primate societies were hierarchical, competitive, ridden with conflicts over sex and food. There were echoes here of the situation under capitalism, but – according to Sahlins – traditional hunter-gatherer societies were quite different. They were strongly egalitarian, based on a sharing way of life, and at their heart were rules or taboos which ensured that values such as sexual access and food were not nakedly fought over, with the strongest monopolising the most, but distributed fairly. Sahlins implied that a revolution – 'the greatest reform in history' – had been responsible for the momentous transition from a primate to a human way of life. Under the logic of what Sahlins termed 'primate dominance', sex had organised pre-human society. With the establishment of earliest culture, society at last succeeded in organising sex.

With the Sahlins model firmly in my head, it now seemed easy to save my basic myth, evolving it directly from the Marxist political vision to which I was by now attached. I concluded that in primatology there were two sexes – just as under capitalism there were two contending classes. Only one of these two groups was responsible for the *material* production which sustained life's continuance over the generations. It seemed clear to me that the responsibilities involved in female pregnancy and lactation were as heavy as the male's sperm contribution was light, and that in this context one could think of males on a biological level as the 'leisured sex', escaping most of the costlier tasks associated with the replication of their genes by getting females to do the work for them. Female primates, in other words, functioned in my mind as 'Labour'. The fact that, nonetheless, male dominance among primates could be pronounced was also not unfamiliar: did not the leisured classes in history usually dominate those whose labour sustained social reproduction as a whole?

As I structured the field through these and similar political preconceptions, I found that the most fruitful course was to be completely uninhibited

in applying my Marxist grid. In this context, while female reproductivity was 'Labour', primate male dominance functioned in my mind as 'Capital'. Dominance was the primate mothers' own reproductive produce – their own male offspring – alienated so as to act as a force opposed to themselves. In Marxist terms, I saw nothing unorthodox about these ideas. In his Preface to the first edition of *The Origin of the Family*, Engels (1972 [1884]: 26) had written that 'production' in the first instance includes pregnancy and childbirth – 'the production of human beings themselves, the propagation of the species'; moreover, he himself (albeit in a rather different context) had emphasised (p. 75) that the first class oppression known to history coincided with that of the female sex by the male.

Other aspects of my grid seemed to fit. Some years before sociobiology had stressed the inevitability of conflict between female and male gene-replicating strategies, I was ready to assume on doctrinal grounds the divergence of 'Labour's' interests from those of 'Capital'. As I read up on primate politics – discovering dominance hierarchies, 'alpha' males controlling 'harems' of females, infanticidal males tussling with lactating mothers to decide the fate of rival males' offspring, breeding season battles between male sexual 'haves' and their rival 'have-nots' – I saw irreconcilable contradictions and class struggle everywhere. One of the very earliest books I had read had been Solly Zuckerman's harrowing description of what he termed 'the social life of monkeys and apes' (in reality the story of a pathologically distorted Hamadryas baboon community artificially created in the London Zoo). As I read this in 1967, it reminded me of some of Lenin's descriptions (which I happened to be reading simultaneously) of inter-imperialist rivalries exploding at the expense of workers everywhere during the First World War. On the bloody battlefields of Monkey Hill in Regent's Park Zoo, the males fought one another so viciously for the right to control females that within a few weeks most mothers and their offspring had been killed (Zuckerman 1932).

Becoming human, it seemed obvious to me, meant escaping all this via some kind of revolution. I took this idea literally. It meant the overthrow of Capital by the Proletariat – which I translated as the overthrow of Primate Male Dominance by Female Reproductive Labour. In what follows, I will refer to this as my 'mythical' version of the story central to this book. It was *Blood Relations* before I began worrying about what specialists in the field might have to say on such topics – my story in the period before I had started testing it and transforming it under collective pressure from comrades, friends and, eventually, professional colleagues.

Within this myth-like 'initial version' of the theory, the culture-inaugurating overthrow of Dominance could only be accomplished by Solidarity. The oppressed category of females had to resist their former sexual/reproductive exploitation and found a new, egalitarian order. They had to end the situation in which they had to do all the work. *They had to force*

the leisured sex to help in child care for the first time. The obvious thing for them to do in this context was what any oppressed, revolutionary class in such a situation must do – win over to its side those members of the ruling class who in fact have an interest in change. I pictured the females as reaching out to the 'outcast' males – those excluded sexual failures who had lost out in the battle for females. These were the ones who had nothing to lose.

I soon realised that I could introduce at this point an economic element: meat. The outcast males, cut off from sexual attachments, would be mobile and free. The 'overlords' would be immobilised – chained down by the need to guard jealously their 'private property' in the form of females and slow-moving young. Because hunting (as I reasoned) requires unfettered mobility, the outcasts would occupy the most privileged position from which to exploit this new source of food. My conclusion was that any circumstances which might make meat-eating worthwhile would turn the tables on the dominant males in any group, destabilising the political hierarchy by enhancing the bargaining power of the formerly subordinate hunter-males. It all seemed to me so simple: in such a situation, the females would have needed meat; the meat-possessing subordinate males would have needed sex. What would have prevented the two sides from coming together? Only the sexual jealousy of the Tyrant Male. He had to go. He was duly overthrown. The transition to culture was consummated in that revolutionary act.

From this point on, the narrative evolved on the basis of its own mythic logic. Having thrown off one Tyrant Male on account of his uselessness as a hunter, the females – I reasoned – would have needed to continue to rely on the same revolutionary gender solidarity to prevent yet another male from occupying the old Tyrant's place. The solution seemed obvious, and again stemmed directly from my political grid. Organise strike action! This was the way for the females to demonstrate that *their bodies now belonged to themselves*. Just as the females in effect must have sexually boycotted the defeated Tyrant, so they would again have had to go on sex strike given any future signs of dominance-like behaviour in any of the males who were now allied to them sexually. More precisely: any male who approached seeking sex *without* first joining his comrades in the hunt would have had to be met with refusal. No meat: no sex. I already knew that in hunter cultures in the ethnographic record, a preliminary period of sexual abstinence was usually thought central to success in the chase, whilst bride-service was almost always the condition of men's on-going marital rights.

No matter how great the females' potentiality to *enjoy* sex, I reasoned, this would not have been the point. Babies – and therefore feeding them – would ultimately have had to come first. Sex would have had to be subordinated to economics. And the logic of strike action would have necessitated collective vigilance in this respect: within any female group, the sexuality of each would have become – by the inherent logic of strike action – the concern of all. Women *as a collectivity* now would have possessed and been responsible

for the value their sexuality represented. This seemed to me to conform nicely with Sahlins' (1960) formulations in connection with the need for 'society' for the very first time to organise 'sex'. In my narrative, however, it was not 'society' in a general sense which had organised the sexual availability of its female members: it was women themselves.

Organising a sex strike meant joint action. No individual could freely offer herself to a male as and when it suited her. The living 'instruments of production' were now socialised – self-socialised. I was intrigued by the thought that this idea had the potential to explain not only the morality-laden intersection of sex and economics in all cultural institutions of marital alliance, but also those complexes of sexual self-control, self-awareness, potential 'shame' and 'embarrassment' so central to all human cultures – features obviously unknown (as I had noted as a young boy when visiting zoos) among monkeys or apes.

For the first culturally organised humans, sexuality could no longer be under purely personal control. The availability of one's body was of potential concern to *the whole group*. There were elements of repression in this. Freud (1965 [1913]) on this score had not been entirely wrong. But there was nothing necessarily patriarchal about the repressive forces which now came into play. The culturally necessary inhibitions came from within. Contrary to the Victorian myths, so-called 'modesty' or 'morality' had not been imposed on earliest women by men: women had imposed it on themselves (producing mirror-image responses among culturally organised men) as a condition of their own solidarity and power.

The model gave me the cultural incest/exogamy taboo. Women imposing their sex strike did so – according to my model – to inhibit the sexual advances of all non-hunter males. Their own adolescent male offspring would have come into this category, and the forms of sex-excluding solidarity so generated would have endured to produce unilineal clans. Finally, the same story gave me matrilineal descent. I was pleased to discover from my myth that, contrary to Engels, the internal solidarity of the matrilineal clan was based on much more than 'ignorance of paternity'. It was an inescapable *political* consequence of the gender solidarity central to the revolution and to the cultural logic which this had set up. Men were not included in the same political camp as their wives and children for the simple reason that camps were defined by gender solidarity. Naturally, whenever women went on sex strike. they *included* their male offspring within the boundaries of their coalition but *excluded* their male sexual partners. Inevitably, this meant at least two matrilineal 'coalitions' or 'clans'.

Matriliny: a Fathers' 'Own Offspring' Taboo

Impressed with the internal logic of the model, I nonetheless knew that I would have to learn something about recent developments in anthropology,

archaeology and palaeoanthropology to see whether it all worked. In other words, satisfied though I was by my myth as pure myth, almost entirely independent of any modern data, I did retain a sufficient sense of perspective to realise my story's private status and total incommunicability to others in the absence of sufficient actual knowledge.

I began with the matrilineal clan. If my theory were right, then matriclans in the ethnographic record would be predicted to operate in a certain way, exogamy rules and food-sharing rules interlocking according to an underlying structural pattern specified in my myth. By the time I had combed through Robert Briffault's *The Mothers* (1927) and then Schneider and Gough's *Matrilineal Kinship* (1961), I felt broadly satisfied that my story was safe. The logic apparently worked. In a matrilineal clan system husbands can usually be divorced easily, and tend not to have rights in either children or *food-stocks* in their wives' households. On the other hand, these husbands *have to provide food for these households*, often under the authority of wives' brothers who have a stake in the home. A frequent reason for divorce or sexual refusal is alleged laziness on the part of an in-marrying husband.

This logic was exactly what my model had already given me: a situation in which women and their male kin organise a sexual rebuff to 'outsider' males unless they provide food (in the model's case, meat). The food which is taken in from these 'outsiders' then becomes the shared property of matrilineal 'insiders' – offspring and uterine kin of the woman. Drawing out the implications of my model, I reasoned that if the in-marrying males were to be allowed to eat meals in their wives' household, it ought to be as a favour and on sufferance, not as a matter of their rights in the food-stocks as such. This should not be a problem for men, however, provided they could always go to their sisters' or mothers' households and be sure of rights there.

I drew a diagram (figure 1) to illustrate two matrilineal moieties interconnected in this way. The conceptual grid seemed to accommodate the data, with a little squeezing here and there. Virtually every account of a matrilineally organised community that I consulted confirmed that men *did* retain rights throughout life in the household property and offspring of their mothers/sisters, and that there *were* various taboos or inhibitions against helping themselves to provisions within the households of their wives. The logic of matriliny, I decided, implied on an economic level that what husbands produced for their wives or wives' kin was food which they themselves had no right to appropriate, while that over which they exercised shared rights (in their mothers' or sisters' households) was always produced by other men than themselves. The same applied to children: those whom men fathered were never 'theirs', while 'their own' children were those born to their sisters – having been fathered, of course, by other men.

My diagram brought out the fact that these rules dovetailed into one another to define the logic of what I termed – following Mauss (1954) – 'total exchange'. In this, the rule *denying men rights in the produce of their own labour*

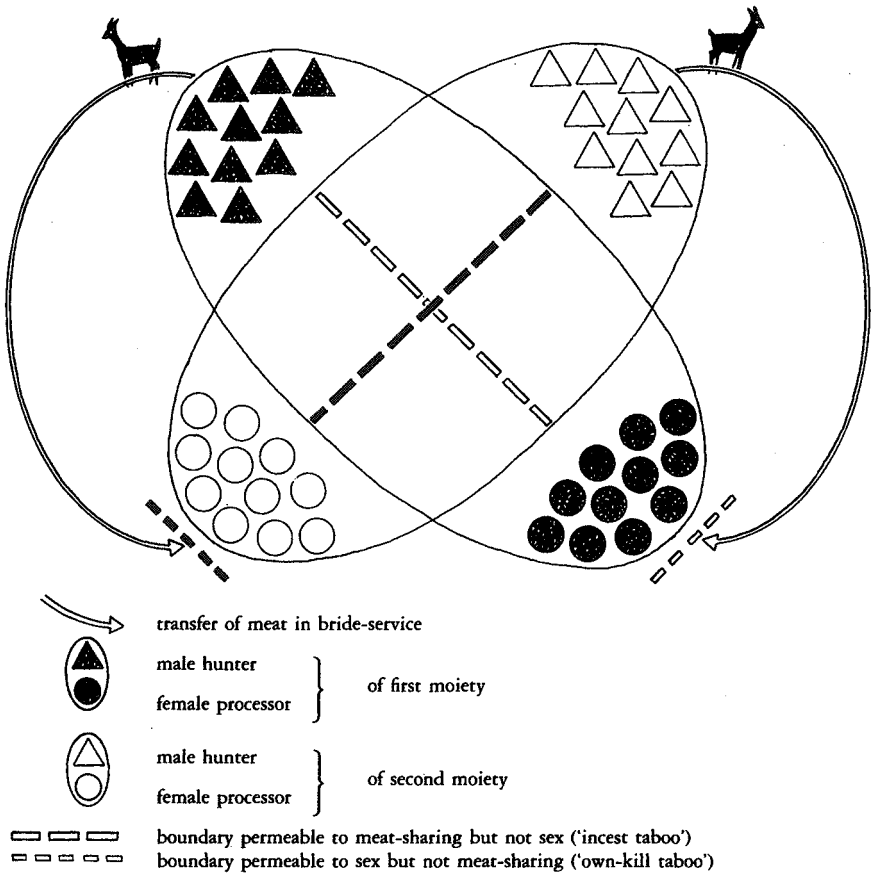

Figure 1 Mutual dependence of marital exchanges and the exchange of meat. 'Total exchange' results as each moiety is prohibited from appropriating its own 'flesh', whether human or animal. Just as humans born in one moiety can be maritally enjoyed only by the other, so the privilege of distributing meat produced (hunted) by men in one moiety is reserved for members of the other.

was an economic counterpart to the exogamy rule which *prevented people from having sex with their own kin*. In each case, what was at issue was a principle of exchange – this always resting, of course, on some factor acting to inhibit people from consuming their own productive or reproductive output. None of these exchange rules, it seemed important to note, would have had to be constructed through arriving at specific, separate, 'agreements' or 'contracts'. They were no more than emergent properties or dynamic consequences of that basic commitment which was rooted in the logic of the strike.

But of course, I quickly became aware that matrilineal clan systems are somewhat unusual in the ethnographic record, and that very few social

anthropologists any longer give credence to the view that matriliny was once universal. Although my myth *yielded* matriliny, duality and exogamy, I was aware that with few exceptions twentieth-century social anthropology was no longer in conformity with Morgan or Engels in treating such things as central to culture's initial situation, and that informed critics would seize on this to marginalise my story should I prematurely attempt to publish it.

Reviewing the great early twentieth-century controversy over 'matrilineal priority', I felt far from satisfied with the mid-century and contemporary consensus on this issue. Boas' material on supposedly recent patriliny-to-matriliny changes among the Kwakiutl Indians I soon found to be by general consent irrelevant, since neither unilineal recruitment nor exogamy characterised descent groups in the region (see, for example, Harris 1969: 305). And the other main allegedly seminal contribution – Radcliffe-Brown's (1924) paper, 'The Mother's Brother in South Africa' – had long ago been demolished by G. P. Murdock (1959), who had succeeded in showing, firstly, that Radcliffe-Brown's new 'solution' to the problem of the avunculate was little more than a play on words whilst, secondly, the tribes Radcliffe-Brown was discussing almost certainly *were* matrilineal in the relatively recent past. It seemed to me strange that late twentieth-century anthropologists should continue to accept the anti-evolutionist paradigms of theorists such as Radcliffe-Brown and Boas on the matriliny issue even though the grounds on which these conclusions had been arrived at were now known to be at least questionable and at worst spurious.

Still, I felt that the nineteenth-century myths about a 'primitive matriarchy', along with their *ways* of arguing for the more scientific concept of matrilineal priority, were now past history. Despite my lingering sympathy with these myths in some of their aspects, I did not relish the idea of taking on the modern social anthropological establishment by attempting directly to revive them. I chose instead another tack.

The Hunter's 'Own-kill' Rule

I concentrated on the economic implications of my model. If matriliny was, in effect, an 'own-offspring rule' – a rule denying men rights in their own sexual 'produce' – then the economic counterpart of this (in my model's terms) was an 'own-produce rule', denying men rights in whatever they themselves had produced by way of food. In the context of a hunter-gatherer lifestyle, this translated into a rule prohibiting hunters from appropriating their own kills.

When I had first formulated my model, it was without any evidence for any such rule in the ethnographic record. I had been made aware of 'sharing', but that was not the same. My model specified that central to human culture's initial situation – indeed, just as central as the incest taboo itself – was the expectation that men should not have kinship rights in the

household property of their wives, and that this should imply their inability to appropriate for their own use the meat which they themselves had killed. Men killed game animals, not to eat the meat, but to surrender it to the opposite sex as a condition of their sexual rights. If my origins story was to survive I had to find evidence, at some level within the cultural domain, for the centrality of this complex in which sex and economics intertwined.

Anyone who turns to the ethnographic record determined to prove the existence of what she or he wishes to find will rarely be disappointed, and I was no exception. Over several years during spare moments in a life taken up mostly with political activism, combing through all the ethnographies I could find, I piled up example after example of what I termed 'the hunter's own-kill taboo', writing patiently and in neat handwriting in a series of very thick, hard-backed, notebooks. By the time I arrived as a diploma student in the Anthropology Department at University College London, I had collected scores of these; they appear in this book as the basis of Chapter 3. My first tutor, Alan Barnard, appreciated the 'own-kill' concept in structuralist terms and helped me to integrate it effectively with Lévi-Strauss' (1969a) conceptualisation of incest prohibitions as rules of exchange in *The Elementary Structures of Kinship*.

It was in this period that I made what for me seemed an advance. I succeeded in recodifying 'totemism' (cf. Lévi-Strauss 1969b) as an expression of the same principle of exchange. Over the years, it had become clear to me that most hunters in hunter-gatherer cultures in fact *do* quite often eat their own kills, or eat their kills of certain species, or eat certain parts of the animals which they kill. If my rule existed at all, then it was as if people in most cultures had long since concluded that *rules are there to be broken*. But to break a rule is not to deny its existence on any level. I was aware of reports from all over the world of hunters' apparent feelings of guilt over the taking of life or the killing of bears, deer or other game 'for base motives'. Hunters in classical accounts would apologise to the animals before killing them, or offer prayers or gifts to them after their death (Hallowell 1926). And of course, I also knew of rituals of 'sacrifice' in accordance with which people took the life of an animal in order, not just to eat it, but to make an explicit gift of its life 'to the gods' (Hubert and Mauss 1964).

Increasingly, I felt able to discern my 'hunter's own-kill rule' in all of these manifestations. What they expressed in common was a reluctance on the part of hunters simply to *kill and eat*. Although my taboo as such was not universal, in other words, its logic – its underlying 'structure', to use Lévi-Strauss' term – apparently was. The rule as such was still visible, *even in the stratagems through which it was evaded or negated*. I reasoned that if economic conditions had changed from those of culture's 'initial situation', then there may have occurred a collapse or weakening of the earlier chains of reciprocity necessary if the old exchange rules were to work. In these circumstances, people may have have felt compelled to begin breaking the rule against

eating one's own kill, just as features such as strict exogamy, duality or matriliny also broke down. Under such conditions, the ethnographic record as actually found could well have been arrived at.

Faced with difficulties, hunters would violate neither their extensive incest taboos nor the own-kill rule suddenly or wholesale, in an all-or-nothing way. Instead, they would eat away at these rules' more inconvenient consequences whilst retaining others, evading the stricter interpretations whilst on a formal level respecting them, making 'exceptions' – in the case of the own-kill taboo – of this species or that one, this part of a killed animal or that one, apologising always to 'the spirits' for infringements which seemed particularly difficult to justify.

Anthropologists had not properly understood the welter of different 'respect' or 'avoidance' relationships towards animals or their flesh in traditional cultures, I felt, because they had apprehended each institution separately, as if it were a particular local anomaly. The reality was that all of these avoidances were central to culture's default condition. In that context, they were not anomalous. They were as 'natural' (given culture) as the incest taboo. In other words, given culture in its default state, all meat was 'avoided', 'taboo', 'totemic', 'sacrificial' and so on. All raw or unprepared meat was exchange value, not use value. If people in *contemporary* hunter-gatherer and other cultures felt free about killing-in-order-to-eat with respect to *any animals at all*, then it was this weakening of the 'own-kill' taboo – not the logic of gift-giving behind 'sacrifice', 'totemism' and so on – which was the 'anomaly' to be explained.

In this book I have chosen to begin with this account of 'totemic' phenomena because it helps illustrate, perhaps better than anything else could, the distinctiveness of my palaeoanthropological aim. I am seeking to get beneath the ethnographic record to an underlying structure which helps explain its more seemingly anomalous features. I want my model to illuminate the hunter-gatherer ethnographic record as a whole, shedding light not only on kinship and economics but also on dance and trance, myth and magic, ritual and art. But above all, I want *Blood Relations* to seem to the reader to constitute a satisfying explanation for the genesis of *this structure itself*. That would make it a narrative adequate to the *richness, variability and specificity of culture*, rather than a model which seemed helpful in accounting for just a few selected, highly generalised, statically conceived 'universal features' such as (to take some examples from conventional palaeoanthropological accounts) 'the sharing way of life', 'pair-bonding' or 'consciousness'.

Sociobiology and Feminism

It was only from the late 1970s onwards – a decade or so after I had first glimpsed my 'human revolution' idea – that sociobiology became a powerful influence in both palaeoanthropology and primatology. The versions of

primate politics underlying my original myth were not sociobiological at all. The narratives I had explored had been those of Solly Zuckerman (1932), Sherwood Washburn (1962), Irven DeVore and others for whom primate social structure meant essentially 'Dominance', whilst 'Dominance' meant essentially the political supremacy of *males*.

Haraway (1989: 176–9) has beautifully described how such early primatological fieldworkers who shaped my vision in those years simply failed to 'see' females as active subjects. They saw baboon or chimpanzee females basically as valuables to be fought over by males, whose noteworthy actions alone shaped and structured the entire social field. With equal insight, Haraway has shown how this blindness was served and masked by the functionalist biological paradigms of the period – paradigms which obfuscated analysis at the micro-level of individual motivation by dealing not with individuals but with supposedly functional wholes such as 'troops', 'hordes', 'bachelor bands', 'harems', 'mother–infant dyads' and so on.

The first systematic attempt to apply what was to become known as sociobiological theory to primates – according to Haraway (1989: 176) – was a thesis entitled 'Natural selection and macaque behavior', based on fieldwork on the island of La Cueva, Puerto Rico, in 1970. The author was Barbara Smuts, who as an undergraduate had been strongly influenced by one of sociobiology's founding prophets, Robert Trivers. In her thesis and her subsequent work, Smuts explored the doctrines of Bateman (1948), Williams (1966; 1975) and Trivers (1972) – later developed within primatology by her colleague, Richard Wrangham (1979, 1980) – according to which males and females have radically different genetic interests. Since this is an absolutely key concept, it is worth dwelling on Smuts' and others' use of it here.

Simplifying somewhat, the underlying idea is that males can have virtually limitless offspring and should therefore try to inseminate as many females as they can, whilst females can only produce a small number of babies, and should therefore concentrate not on consorting with one male after another but on ensuring that any offspring born should actually survive (Bateman 1948). Another way of putting this would be to say that, after a period, females should be uninterested in seeking more male sperm. The substance is plentiful, and enough is enough. But males should not have the same attitude towards fertilisable female ova. These are scarce. From a male's genetic point of view, more should always be welcome. If another female *can* be made pregnant, she should be. What 'limits' a male's genetic fitness, therefore, will tend to be the restricted availability of receptive females. What 'limits' a female's genetic fitness, however, is *not* a mirror-image of this. It has little to do with the availability of sperm – and much more to do with such things as food, shelter and other means of keeping existing offspring alive. Primate females, then, should distribute themselves within their environment motivated mainly by 'economic' concerns; males, by

contrast, should map themselves in accordance with the search for receptive females.

This new, starkly Darwinian, way of looking at matters immediately introduced *gender* into all attempts to examine how food availability or other ecological constraints affected primate social organisation. The crucial point was that such constraints affected males quite differently from females. Sociobiology had clarified this, and the result was a sudden realisation – exploited by Smuts as well as by other feminist-minded primatologists – that the old primatological fieldwork had left immense blank spaces of ignorance as to how females pursued strategies to ensure the survival of their young. To ask how ecological constraints shaped social organisation, it was now realised, would mean studying female action in its own right. Simply to say that females were herded around 'functionally' by males, incorporated within 'harems' or 'mother–infant dyads', was no longer enough.

It is in this context that Haraway makes one of the most vital points in her entire book. The new explanatory framework, she writes (Haraway 1989: 176), began to revolutionise primatology from top to bottom. The absence of data on female–female interactions and female behavioural ecology began to be remarked upon in the literature, and young graduate students began to plan their field studies to explore such topics. In addition, primate workers began to understand that sociobiological explanatory strategies destabilised the centrality of male behaviour for defining social organisation. 'Female reproductive strategies began to look critical, unknown, and complicated, rather than like dependent (or entirely silent and unformulated) variables in a male drama, she writes.'

Female observers, continues Haraway, pressed these points with their male associate in the field and in informal networks. In general,

> since the men were not taking many data on females, they were not in a position to see the new possibilities first. In general, the women had the higher motivation to rethink what it meant to be female.

During interviews with Haraway, several of the women reported to her that it was the atmosphere of feminism in their own societies which had made it seem personally and culturally legitimate to focus scientifically on females for the first time:

> Men also reported the same sense of legitimation for taking females more seriously, coming from the emerging scientific explanatory framework, from the data and arguments of women scientific peers, from the prominence of feminist ideas in their culture, and from their experience of friendships with women influenced by feminism. (Haraway 1989: 176)

Discussing how Barbara Smuts, Jeanne Altmann, Adrienne Zihlman, Sarah Blaffer Hrdy, Shirley Strum and other western women have recently revolutionised primatology, Haraway concludes that these middle-class feminists

have projected their own political grids in the most fruitful imaginable way upon the primates they have been studying. These women were hostile to the notion that 'woman's place is in the home' – *and proceeded to remove all female primates from the maternally nurturant 'dyad' relationships in which they had previously been embedded.* As scientific primatologists, they were go-getters, assertive intruders into a male scientific world, determined to prove that they were 'as good as' – if not better than – any man. They succeeded – and cast their female primates as active strategists in an identical mould.

And all this was thanks to late capitalism – or rather, to its highest expression in primatological thought. As Haraway (1989: 178–9) concludes:

> Plainly, sociobiological theory can be, really must be, 'female centered' in ways not true for previous paradigms, where the 'mother–infant' unit substituted for females.

The 'mother–infant' unit had not been theorised as a rational autonomous individual; its ideological-scientific functions were different, 'located in the space called "personal" or "private" in western dualities'. The sociobiological kind of female-centring, states Haraway,

> remains firmly within western economic and liberal theoretical frames and succeeds in reconstructing what it means to be female by a complex elimination of this special female sphere. The female becomes the fully calculating, maximising machine that had defined males already. The 'private' collapses into the 'public'. The female is no longer assigned to male-defined 'community' when she is restructured ontologically as a fully 'rational' creature, i.e. recoded as 'male' in the traditional explanatory systems of the culture.

The female ceases to be a dependent variable when males *and* females both are defined as liberal man. The result – notes Haraway – was the construction, in both human and non-human primate forms, of a liberated 'female male'.

The Class Struggle: Point, Counterpoint

Although I could not have formulated matters so clearly at the time, by the mid-1980s I was dimly aware of many of these subtleties, which so closely challenged and yet vindicated my own evolving myth. Although I scarcely understood its scientific complexities, sociobiology by this stage did not simply repel me, despite its obvious political roots. Indeed, I warmed to its ideological excesses. They seemed to promise for the first time a publicly communicable way of validating my own narrative. If the stockbrokers, the company directors and the bourgeois feminists could be uninhibited about projecting their pure political constructs into primatological and palaeoanthropological debate – then how could they object to a socialist doing the same? Obviously, it seemed to me, they could not object *on*

principle. The bone of contention could only be the extent to which – if at all – our respective grids worked.

I was happy about seeing *primates* as 'primitive capitalists', not only genetically but to some extent also on a behavioural level. I point-blank refused to see hunter-gatherer *humans* as maximising, competitive calculators within the same mould. It was here that I parted company with sociobiology. Their importation of the constructs of our own culture, it seemed to me, was unfair and one-sided. If you could have calculating, maximising capitalists operating in human origins narratives, why could you not *also* have militant trade-unionists? If you could have profits and dividends, why not also industrial action, pay bargaining and strikes? Our world, I reasoned, was a mixture of conflicting forces and political dynamics, not 'pure' capitalism. If scientists were going to transpose the modern world's constructs on to other cultures, on to nature and on to our own most distant past, why stop half-way? Why just take the selfish, competitive bits? Why not harness the whole of modern industrial culture in its entirety – class struggle, trade unionism, movements for socialism and all?

I felt equally ambivalent about bourgeois feminism. Influenced by friends and comrades who were feminists, I naturally felt feminism of any variety to be a liberating political force. But the currents I felt politically closest to were not those which Haraway in her discussion of primatologists describes. For the women I was closest to (many of them involved in the Greenham Common anti-Cruise missile campaigns of the early 1980s), the construction of 'female males' was not what the struggle was all about, any more than joining the capitalists was the essence of working-class emancipation. The struggle was more about refusing to collaborate with the whole masculinist political set-up, organising autonomously as women, drawing on support for real change from the wider class struggle – and fighting to bring men as allies into a world transformed on women's terms.

The Menstrual Dimension

I was at first unaware that my Labour versus Capital myth could or should have anything to do with menstruation. Although in 1967 I had read Briffault's *The Mothers* (1927), in which traditional beliefs about menstruation and the moon figured prominently, my politically shaped need to vindicate what I saw as the Marxist orthodoxies made me shy away from such themes. Nonetheless, I must have unconsciously absorbed from Briffault an awareness that menstruation can be positively viewed, perhaps internalising a hint that it might once have had something to do with that all-female 'strike action' on which my origins myth relied.

It was in 1977–8, during my first year as an anthropology postgraduate in University College London, that the topic suddenly began seeming more pressing.

Just before going to college, I had become aware of a small women's publishing network, one of whose pamphlets, 'Menstrual Taboos' (Matriarchy Study Group, n.d.), had attracted my attention. The historical and personal accounts by women in its pages made me aware that there were activists in the women's movement who were drawing on Briffault and other writers from an earlier generation in reasserting myths about a 'primitive matriarchy' – myths with at least some potential relationship, as I thought then, to my own evolving narrative. Although my gender, politics and antipathy to religion – even 'Goddess' religion – kept me from close contact with this particular separatist group, the tangential encounter was helpful in strengthening my awareness that women could find menstruation empowering. Menstrual experiences were not necessarily ups and downs to be disguised, suppressed or flattened out with artificial hormones. It seemed to me clear, in any event, that it was only an extremely *masculinist* and *non-periodic* culture which could impose its one-sided constraints so deeply as to make women conclude that it was they – but not men – who would have to suppress and deny their own biology as the condition of feeling liberated. There was a link in my mind between refusing the construct of the 'female male' and determining that cultural liberation *ought* to give women the chance (where they wish to) to validate and derive social pride, status and power from uniquely female experiences such as childbirth and menstruation.

At about the same time, I came across Martha McClintock's (1971) article in *Nature* documenting for the first time that closely associated women tend to synchronise their menstrual cycles. Like most men, I had not known this – had not known what nearly all women, I now realise, take to be an unremarkable fact of life. In my ignorance, I had had problems in integrating menstruation into my palaeoanthropological model of gender-based 'class solidarity' or 'sex strike'. My difficulty had been that women's menstrual cycles, as far as I had known, were necessarily randomised. Effective strike action, by contrast, presupposes joint action on the picket line. How, then, could menstruation coincide with the 'trade unionism' of my treasured origins myth?

The McClintock article solved this particular difficulty for me. Where women have solidarity, their cycles *automatically* tend to synchronise. The logical link between menstruation and strike action was now secure. My myth was vindicating itself and taking on new forms.

Then, during my first year as a diploma student at University College, Mary Douglas gave a lecture on 'male menstruation', citing in particular Hogbin's (1970) book on the Wogeo Islanders, *The Island of Menstruating Men*. The story made a searing impression, as with some gusto Douglas shocked her student audience with vivid descriptions of warriors and hunters preparing themselves for armed action, purifying themselves from weakening 'contamination' with the opposite sex – by gashing their genitals so as to make the blood flow in streams.

I was puzzled to understand why men should want to do this – particularly as I learned with growing astonishment that comparable practices were central to secret male initiation rites over immense areas of Australasia, Melanesia, Africa and the Americas. If menstruation were necessarily emblematic of feminine weakness, why should men want to emulate it? Not satisfied with the various psychoanalytic theories I came across (Bettelheim 1955), I suspected that on some level it was because menstruation had been for women not 'weakness' at all. Menstruation had been culturally constructed as a source (perhaps even *the* symbolic source) of ritual power – power which these 'menstruating men' were now motivated to usurp and appropriate for themselves.

Then, not long after my first excitement at making these discoveries, Peter Redgrove and Penelope Shuttle published *The Wise Wound* (Shuttle and Redgrove 1978). Although I felt not wholly sympathetic with the Jungian, far from Marxist style and tone, as a cultural influence upon me the book's impact was tremendous. Little was said about menstrual collectivity or synchrony – the emphasis was intimate, sexual, personal. Yet it became abundantly clear to me that my own myth now could connect up with massively powerful echoes in both modern and ancient literature, art, ritual and myth. Shuttle and Redgrove's poetic and psychological insights into the menstrual dimensions of the Holy Grail legend and countless other pivotal narratives now seemed not merely astonishingly original but also familiar and inevitable. They were my original Engels myth, Briffault's myth – and now my own evolving origins myth working itself out in my head.

The Language of Blood

In the light of all this I revised my cultural 'initial situation', incorporating – now – the symbolism of blood. It all seemed neat and parsimonious. For babies to be conceived, the sexes had to come together. For efficient hunting to take place, they had to separate. If both successful hunting *and* conceptions were to occur, the sexes had to alternate between conjunction and disjunction. Periods of sex strike and of marital togetherness had to alternate. I assumed that this alternation must have been socially synchronised, rather than a matter for individuals to decide autonomously within couples. It also seemed unlikely that any such alternation – if women had anything to do with it – could have disregarded the menstrual cycle. The default condition. I reasoned, must have been one in which the sexes came together when women were fertile.

This meant disjoining – organising each sex strike – during menstruation. Synchronised cyclicity would have made of this a collective rhythm. Had women required some external standard by which to regulate their synchrony, they could have used the moon. I was not a believer in mystic lunar influences; the moon was just the only appropriate clock. It may also have

made sense for men, intermeshing their own rhythms with those of women, to regulate their hunting schedules in accordance with a lunar calendar. In addition to the obvious sexual/reproductive advantages of keeping in tune with women, hunters – particularly when winter daylight lasted only a few hours – may have benefited by maximising their overnight travel and associated exertions when there was sufficient moonlight to significantly lengthen the effective day. I reasoned that this might help explain the explicitly lunar attributes of hunters' deities such as the Greek Artemis and the Roman Diana, in addition to hunting ritual and folklore throughout much of the world, (see Chapter 10).

It was necessary not only for the *sexes* periodically to conjoin and disjoin. The same also applied to relations between *men and game animals*. Hunters had to encounter animals in order to kill them. But they had to be subsequently separated so that others could obtain the meat. In this context, I had been intrigued for some time by certain implications of Lévi-Strauss' *The Raw and the Cooked* (1970; see Chapter 13). I was also fascinated by the work of the French Marxist anthropologist Alain Testart (1978, 1985, 1986), in which he posits as central to culture's initial situation an 'ideology of blood' linking menstruation recurrently with hunting blood. Both Lévi-Strauss' and Testart's findings could be nicely integrated with my model.

Blood had been constructed, during the course of the human revolution, to signal inviolability or 'taboo'. In my narrative there was nothing complicatedly 'symbolic' about this. Women just went on sex strike at the biologically appropriate period – during the time of month when menstruation normally occurred. Any man noticed to be blood-covered might then have been suspected of 'strike-breaking'. Like a rapist or murderer, he would have had 'blood on his hands'. Assuming that men wanted to avoid suspicion, this consideration would have motivated the shunning of menstrual stains. By this route, women's blood as such would have become conceptualised as 'taboo'.

The hunter-gatherer ethnographic record, I knew, was replete with examples of 'hunters' taboos' which seemed to assume magical connections linking women's menstrual condition with men's hunting luck, or women's blood and the blood of raw meat (Testart 1985, 1986; *cf.* Lévi-Strauss 1970, 1978). To explain all this, I put the finishing touches to my model.

Somehow – I supposed – in the course of evolution it had become established that blood was simply blood. That is, it made no difference where the blood came from: it was conceptually all the same. The blood of rape, murder or strike-breaking, the blood of the hunt, the blood of menstruation or of childbirth: it was all in the final analysis just blood.

However this identification had been arrived at, the important thing was that once the confusion or perceptual merging had been accomplished, an extraordinary result would have been achieved. Given my arguments about menstruation and the theoretically necessary inviolability of women's sex

strike, no form of blood could have been equated with menstrual blood without the most potent of consequences in evoking 'respect' or in conveying the notion of 'ritual power'. *Raw meat, after all, could have marked a man with bloodstains just as easily as could contact with a menstruating woman.* The only way to remain above suspicion might have been for men to remain wary of contact with blood of any kind – particularly when women were around. In accordance with this logic, all raw meat may have become in effect labelled 'unavailable' (in ethnographic translations 'taboo', 'totemic', 'sacrificial' and so on) within the vicinity of the camp. It would have stayed taboo for as long as it remained uncooked – just as women remained 'taboo' whilst menstruating.

Sex Strike as 'Elementary Structure'

Having constructed this new version of my model, I treated it rather as Lévi-Strauss planned to treat his long sought after but never actually delineated 'universal structures' of human culture. The model afforded a simple logic from which the more complex cultural constructs could be derived. The logic as such corresponded to culture's 'default condition'. It represented culture's simplest form, its state of rest, its point of departure. Everything started here, and nothing could be understood unless this initial situation were known. Other anthropologists' models of an 'intial situation' – almost all of them based on the supposed centrality of the 'nuclear family' – were not entirely banished, but their significance was now changed. In accordance with my logic, couples in the initial situation naturally conjoined, so that something at least vaguely corresponding to 'the individual family' existed. But they also separated. And it was in ensuring this separation that *culture as such* established its force. Heterosexual bonding naturally occurred, but culture's logic of gender solidarity periodically overrode this. Father-child links could develop, but the logic of the sex strike put blood symbolism and matriliny first. Individualist, self-seeking, profit-maximising tendencies could be tolerated within limits – but community-wide solidarity finally had to prevail.

With this model in place, I saw most aspects of kinship, ritual and mythology in traditional cultures as expressive of its logic. Most hunter-gatherer cultures, as far as I could determine, *did* sustain menstrual avoidances of one kind or another, *did* see prior sexual abstinence as essential to hunting luck, *did* link such abstinence with menstrual avoidances, *did* construct mythological connections between the blood of women and the blood of game animals, *did* draw a sharp conceptual distinction between raw meat and cooked – and so on.

To attempt to isolate invariant cross-cultural uniformities on any level is risky, since 'exceptions' will always be found. Certainly no custom, rule or meaning in one culture is ever really 'the same' as its supposed counterpart in another. But within my narrative – as I had learned from Lévi-Strauss –

'shared structure' had nothing to do with identity on the level of what could be recorded or observed. It meant, on the contrary, precisely a logic of perpetual alternation, opposition, variation, contrast. My structure was a *transformational* template: a set of constraints governing the pattern in accordance with which change and diversification could proceed. Far from blurring distinctions, such a unitary, all-purpose template is like a common standard of measurement: unlike incommensurable standards, it allows one to discern *with precision* just how meanings, rules and customs differ from and are logical transformations of one another. Only a unitary standard can reveal what diversity really means.

I started working on my doctoral thesis to show that Lévi-Strauss' unusually opaque, cumbersome, scarcely read, yet awesomely ambitious *Mythologiques* (1970, 1973, 1978, 1981) could profitably be reinterpreted along these lines. All of his strange stories about 'bird-nesters' alternating perpetually between 'earth' and 'sky' could be seen as accurate expressions of my template. The myths traced in graphic imagery the perpetual alternation of men and women between sex strike (often coded as life in the 'sky') and marital conjunction (renewed contact with 'earth'). When the heroes were in the sky, they lost their wives to male rivals whose 'incestuous' claims had to be challenged at a later stage. When they descended to earth, they got their wives back again. When they were in the sky, they felt 'raw'; once back on earth, they felt, if not always 'cooked', then at least desirable and available to the opposite sex once more. Sky-stranded heroes came into contact with blood, bloodstained faeces or – in one prominent case – celestial wives whose synchronised menstruation regularly caused all the world's women to menstruate at the same time (Lévi-Strauss 1981: 565). None of this kind of thing happened back on earth, where proper food could be eaten and blood-free marital relations enjoyed.

The Rainbow Snake

I took it that the source of these stories was not just my model in the abstract. The myths could hardly be pure 'collective memories' of a cultural initial situation which had long since passed. To have survived, the tales must have had a living point of reference in the present. My menstrual sex strike must have been on some level *still operative* within the cultures which replicated these myths. It seemed clear to me that ritual action was central to the forms this living tradition took.

This was clearest in the case of that category of ritual action most obviously connected with the myths, as well as most indisputably central to social structure in the cultures concerned. I concentrated on initiation rites.

Where hunter-gatherers focused mainly on *female* initiation rites (as was

generally the case among the San and some other African groups), then the mythico-ritual structures were not necessarily oppressive of women; the connection with the model tended to be simple and self-evident. When a young woman first menstruated, her blood was taken to be potentially beneficial, immensely powerful – and intimately connected with male hunting success (Lewis-Williams 1981). The flowing of blood set up a structure of sex-excluding gender solidarity. Hunting success depended on this solidarity. That was all. The model was expressed straightforwardly.

But when hunter-gatherers – as in much of Australia – put the major emphasis on *male* initiation ritualism, the situation was more complex. The major structural feature remained gender segregation. Every ritual began with the women and children, as a group, repulsed in some symbolic sense from the space occupied by ritually active men. Marital sex was still for a period prohibited. Blood was still used to signal this segregation and to mark the gender-defined boundaries. There was still the same connection with hunting success. *But the 'menstrual blood' was now that of men.* Boys had to have their flesh cut to allow the blood to flow. This was what initiation was essentially about. It was now large groups of men whose 'menstrual periods', deliberately synchronised, kept women away *and thereby preserved the boundaries of the cultural domain.* And compared with the southern African (San) situation, all this made a vast difference to the whole atmosphere of the rituals, to their politics, to their 'naturalness' or apparent informality – and to all relations between the sexes, between the generations and between those within each gender-segregated group.

Where 'male menstruation' had become the rule, real women's menstruation became feared as a threat to men's supremacy. Men, now, needed to organise their ritual sex strike *at women's political expense*, actively inhibiting women from replying in kind. This meant challenging women's freedom to exploit the symbolic potency of real cyclicity, real life-giving blood, real reproductive labour. The symbols and hence values were all taken over by men, who to ensure their rule strove always to atomise the productive sex *at the point of reproduction.* Like workers denied collective control over their own labour, mothers were prevented from synchronising their cycles or menstrual flows, prevented from benefiting collectively from the potency of their bodily processes. Menstrual seclusion rules were expressive of this, as menstruants – now said to be saturated with immensely *dangerous* ritual power – were hedged around with restrictions and elaborately marginalised. Male menstruation, the associated mythologies never tired of explaining, is positive, magical, empowering and conducive to good hunting luck. Female menstruation is just dangerously polluting and should be treated as far as possible as a private affair despite the cosmos-endangering properties of the blood.

It was in the north-east Arnhem Land classical ethnographies that I found all this most breathtakingly illustrated. Here, as I described in a reanalysis published in the early 1980s (Knight 1983), men 'menstruated' synchron-

ously in the course of their most important rituals. Whilst expressing their power in this way, they did what they could (although not always successfully) to prohibit women from replying in kind. Only by covering themselves and one another in their own 'menstrual blood' whilst excluding women from doing the same could men safeguard their claimed monopoly of ritual power. And as they enveloped themselves in both blood and symbolic potency, men thought of this experience in terms of being 'swallowed' by an immense 'rainbow' or 'snake' – a creature alleged to be of immense *danger* to living women should they ever become too closely involved. Perhaps the most marked feature of this 'snake' was its arousal in the presence of female body odours: 'The rainbow serpent, a good consumer of smells, is associated with female bodily emissions related to reproductive processes . . . ' (Buchler 1978: 129). In being placed in awe of the 'snake' construed as an alien monster, women were made to *fear their own blood-potency*. Their own reproductive powers were being alienated from them – taken from them, turned into their opposite and constructed as a force opposed to all women – in the most dramatic imaginable way.

This 'snake', I realised, could not have been simply a male invention. Not only was it too feminine, too maternal, too wrapped up in the language of women's odours, babies, bodily fluids and associated powers (Buchler 1978). It also corresponded too closely with my own model of the force at culture's roots. The 'snake' was an ancient menstruation-inspired construct which men had taken over for their own use. It was 'blood relations' in masculinized form.

In this connection, the core of the evolving thesis and perhaps the most exciting ethnographic finding to which my model had led me was that what functionalist-minded fieldworkers of an earlier period had thought of as an Aboriginal construct symbolising 'sex', 'weather-change', 'water', 'phallus', 'womb' or some other ready-made category familiar to Europeans – this so-called 'Snake' was nothing of the kind. Its meaning was not a thing. It referred not to something external to the human subject. It was – I decided when the first dawnings of understanding began to hit me – pure subjectivity. It was solidarity. It was my class struggle. It was the picket line, the blood-red flag, the many-headed Dragon of resistance. It was the overthrow of Primate Capitalism – the triumph of the great Sex Strike which had established the cultural domain. It was women in solidarity with 'brothers', not husbands; men in solidarity with 'sisters', not wives. It was women *as women*, one hundred per cent themselves, bringing sons and brothers into their own world. It was the blood of clan solidarity and kinship, flowing, shimmering, sustaining each participant in birth as in life – whilst pulsing on through mothers and daughters beyond death. It was the pulse which had linked us once to the reason of our being – to a primordial class struggle which had finally reached its culture-creating goal, to be at one with the moon, the tides, the seasons and all other fluctuating, living/dying things.

All this had first hit me in a rather heady way during the autumn of 1980, and for a while I just let the reveries flow. Once I had stopped dreaming and resumed academic work, the task was to see what were the usable insights among the various connections I had made.

As, over the next few years, I read all I could find on the woman-loving, terrifying, magical 'rainbow' or 'snake' at the heart of so much Aboriginal cosmology, I admired more and more the myth-makers' precision in describing the rhythmic logic at the root of their world. Rather as Lévi-Strauss had shown in his analyses of Amerindian mythology, it seemed clear to me that these beautifully rich and complicated stories *never made mistakes*.

To begin with, for the storytellers to describe their magic as 'snake-like' made good sense. Solidarity's concrete manifestation in its default state, according to my model, had been menstrual synchrony, and therefore cyclicity. A humanoid, maternally functioning 'snake' was the perfect zoological metaphor here. Not only – I thought – does a snake with its venomous bite inspire respect, just as any effective picket line must! It also has the correct shape. Its parts are egalitarian, its head like its tail, each segment seeming to be the equivalent of any other. Moreover, what creature on earth, as I reasoned, could connote cyclicity more appropriately than this flowing being, coiling itself up in spirals, undulating its way across water or land, sloughing its skins and so seeming to move between one life and the next? Appropriately for a menstrual metaphor, most snakes have a quite extraordinary sense of smell (Buchler 1978: 125, 128–9); it is after smelling the two synchronously bleeding Wawilak Sisters' blood that the great copper-python Yurlunggur, in the best-known of all Aboriginal rainbow-snake myths, incorporates these dancing, synchronised, women into its body (figure 2). The northern Australian zoological water-python most directly associated with the rainbow snake (Worrell 1966: 99), acts by swallowing its victim whole, as if taking it into its immense super-womb – a kind of birth process played backwards. What a perfect way of describing how women's menstrual flows, within the terms of my model, reasserted the primacy of blood links, of maternal solidarity, of involvement in a picket line of interconnected wombs!

That the Aborigines were not thinking primarily of water-pythons seemed, however, equally clear – despite the authoritative zoological identification of the creature as 'Liasis fuscus Peters' (Worrell 1966). I agreed with the Upper Palaeolithic art specialist Alexander Marshack (1985) that 'the Snake', here as in other parts of the world, was a way of describing cyclicity – *especially lunar cyclicity*. It was an elaborate, wonderful, extravagant metaphor – not just a reptile. This was evident from the fact that in descriptions, the Aborigines insisted on adding that 'she' or 'he' or 'it' was not only snake-like but also 'like our Mother' (or like some other senior relative) and immense as no real snake could possibly be. The *myndie* of the Melbourne area 'could

Figure 2 Jurlungur the Rainbow Snake and the Two Wawilak Sisters. The Snake is shown approaching the heroines and their offspring, then swallowing them, and finally departing filled with their flesh. Note the patterning to the left of the vertical path of footprints leading to the women's sacred hut. This symbolises the menstrual blood which aroused the snake. Bark painting. Yolngu, North East Arnhem Land (redrawn by the author from a photograph by Mountford 1978: 77, Fig. 22).

ascend the highest tree and hold onto a branch like a ring-tailed opossum, or he could leave his home and stretch his body across a great forest to reach any tribe, with his tail still in the Bukara-bunnal waterhole' (Mountford 1978: 31–2). Like an expanding picket line incorporating ever more supporters within its ranks, or like a wave of revolutionary militancy rippling across a political landscape, this creature in its various local manifestations 'swallowed' whole communities of humans as no real snake ever could. In Aboriginal conceptions, it also felt aroused by menstrual blood to an extent unknown in zoology, lay behind the birth of *human* babies rather than just snakes – and was decidedly *human* in its 'incestuous' (that is kinship-oriented) matrimonial preferences.

For the Aborigines to say that their world had been created by this magical power – as they did – seemed wholly appropriate within the terms of my own myth. Yes, gender solidarity and synchrony *had* established culture. These stories were, in their own way, good science. By comparison, the interpretations of most pre-structuralist social anthropologists seemed sadly uninformative. I felt immense admiration for the erudition of those early Australian ethnographers such as Spencer and Gillen (1899, 1904, 1927), as well as for marvellous scholars such as W. E. H. Stanner (1966) and W. L. Warner (1957), whose writings shone on every page with respect for all that the Aborigines had achieved. But none of this stopped me from feeling irritated by the condescending scepticism of most of the social-anthropological fraternity, a colonially spawned establishment which had exposed the Aborigines' secrets for my benefit as a library reader in England – only to dismiss the sacred myths as at best functional constructs, at worst incomprehensible irrationality.

It seemed futile to deny the irreversibility of the initial betrayal: no one would suggest burning the monographs which, as part of colonialism's violation of native Australia, had recorded so many details of Aboriginal secret/sacred life. Morally, all participants within the dominant white culture were thereby implicated; there can be no adequate atonement, no easy way out. I am a part of that culture. Yet given colonialism's *fait accompli*, it seemed to me on political grounds that the best attempt at recompense was to vindicate the native narratives within their own terms, showing to the best of my ability their *status as science*.

A refusal to read Warner's (1957), the Berndts' (Berndt and Berndt 1951; Berndt 1951, 1952) or Stanner's (1966) relatively sensitive and wonderfully informative early works on 'the secret life' would have benefited no one. On the other hand, for me to have intruded even vicariously into the Aborigines' secrets and then to have remained silent would have been to collude in the colonialist betrayal, contributing to the initial cruel exposure something perhaps even less forgivable – my own culture's arrogant dismissal of the precious knowledge which these fragile native patterns had the potential to transmit to us all. The Aborigines who had confided their secrets to individ-

ual befriended anthropologists, trusting, perhaps, that after colonialism's destruction of much of their culture at least some of its highest accomplishments could have been immortalised safe in our hands – these Aborigines ought surely to be remembered, even if recompense is no longer within anyone's power.

The best we could do, I thought, was to *listen* to what, through their myths, these cultures still have to say to all of us across the planet. Such a course seemed validated by much of Lévi-Strauss' work. I particularly liked his statement (Lévi-Strauss 1968: 351) that 'what we are doing is not building a theory with which to interpret the facts, but rather trying to get back to the older native theory at the origin of the facts we are trying to explain'. For me, of course, the 'native theory' at the origin of these facts was a sexual-political version of my 'class struggle'.

The myths allege that ritual power originally belonged to women. 'We took these things from women', as a learned Aboriginal put it during a performance of the great *Kunapipi* ceremony, referring to the cult's jealously guarded secrets (Berndt 1951: 55). Such storytellers knew the meaning, value and *truth* of their precious narratives, I thought, better than did the puzzled, functionalist-indoctrinated anthropologists who had arrived by boat and plane from a culture cut loose from its roots in its own Dreamtime past. *'White man got no dreaming, Him go 'nother way. White man, him go different, Him got road belong himself'* (Stanner 1956: 51).

As I read through the corpus of traditional, mainly functionalist, anthropological attempts to come to terms with the logic of the rainbow snake, I was struck by the meagreness of the great classical attempts at interpretation. Radcliffe-Brown's (1926) view that the snake represented 'water' seemed unconvincing, as did Warner's (1957) idea that it represented 'the weather', or Berndt's (1951) suggestion that it was a 'phallic symbol'. What seemed unacceptable was these theories' reductionism – their striving to reduce the richness of the Aborigines' own logic to some far simpler construct already familiar at a superficial level to Europeans.

All too apparent was these interpretations' one-sidedness and inadequacy to match the rich detail and complexity of 'the Snake'. What kind of a 'snake' was it if people could *participate in its body by dancing*? Why was this shimmering, rainbow-coloured, male/female creature recurrently described as *'incestuous?'* Why was it depicted as having kangaroo-like prominent *ears*? Why would it so often be a rainbow or rainbow snake which sent floods or thunderbolts as punishment for abuse of the game animals, for attempting to cook meat during menstruation time – or for selfishly consuming one's own kill? Why did humans in need of ritual power have to allow themselves to become *'swallowed'* by this 'snake'? Whence came the persistent associations with life/death alternations, birth and rebirth, the tides, blood-streaked floods, the moon and ancestral dancing women? Such questions were neither answered nor even asked. Above all, I noticed that although one bizarre

detail was recurrently stressed by the Aborigines themselves, and usually reported in the ethnographies without comment, no one had attempted to explain it. If this 'snake' was basically a 'phallic symbol' or an emblem for some functional utility such as 'water', why was it always *so thirsty for supplies of real or surrogate human menstrual blood*?

Within the framework of my myth, all this seemed as would be expected. Kinship indeed could not function without blood. Reasserted kinship solidarity was indeed the conjoining of blood with blood, like with like. Blood-marked women with their kin were – within the specifications of my model – very much the guardians of all life-blood, and therefore of game animals protected by blood-encoded rules and taboos. Mother-son and/or brother-sister unity could very appropriately be depicted as 'incestuous' – and as condensable into the image of a 'mother-with-phallus' or 'male-with-womb'. In this context, heterosexual distinctions indeed might be expected to fade away, power during a sex strike being derived not from gender polarity or difference but from the fact that when kinsfolk act in solidarity they can experience themselves 'as one'. Water was – as many early accounts had noted – certainly central to the rainbow snake, but how clear it was from the Aborigines' own accounts that this was not just ordinary water but sacred water, the water of life, womb fluid – the menstrual flow *mingling* in myth and imagination with the surrounding streams and waterholes on which life in reality depended, 'swallowing up' men and women in its synchronising, rhythmic power!

The snake the Aborigines depicted was always rhythmical, tidal, cyclical – synchronised with the changes of moon and season, dark and light, night and day. Cyclicity was absolutely central to her (Maddock 1978b: 115). I noticed that when native artists depicted her/him, they would often use cyclical motifs which *could* be interpreted as waterholes or snake-tracks or yams, breasts, wombs etc. etc. – but that such meanings were never fixed or pinned down (Munn 1973). Almost anything, to the Aborigines, could be part of cyclicity, part of synchrony, part of rainbowness/snakiness. Just as the arch of a rainbow mediates between earth and sky, dry season and wet, sunshine and rain, red colours and violet or blue – so ceremonial life across Australia, it seemed to me, had carried humans in an orderly way from one season or time of month to its opposite, from the 'raw' phase of each ritual cycle to the 'cooked', from blood to fire, kinship connectedness (often coded as 'incest') to marital life. The aim was always to bring humans and nature into rhythmic connectedness and synchrony. Ritual was the endeavour to activate all living beings as vibrant participants within 'the Snake'.

Finally, as if these Aborigines had never heard of functionalist preoccupations with 'the family', their highest divinities were, I noticed, consistently non-heterosexual, blood-empowered, anti-marital. Rainbow snakes always seemed to violate their communities' exogamous laws, conjoining 'incestuously' only with their own blood, their own kin or flesh. She/he/it was

said by informants in the various accounts to be not only 'incestuous' (that is, in correct sex-strike fashion, hostile to normal, heterosexual, exogamous marital intercourse) but also 'like a rainbow', 'like our mother', 'like power', 'like metamorphosis', 'like the Dreaming' and like many other shimmering, changing, life-creating or life-devouring things. Moreover, female rainbow snakes seemed 'masculine'; male ones were regularly depicted with female attributes such as 'wombs' or 'breasts'. I wondered how any of this could be reconciled with the views of those seeking in Aboriginal religion evidence for the centrality of heterosexual 'pair-bonding' or the 'nuclear family'. I noticed that in this area, there were simply no functionalist theories at all.

Turke's Model and the End of Sociobiology

In this book I have drawn on Paul Turke's model of ovulatory synchrony to show that – despite my objections to sociobiological attempts to construe hunter-gatherers as primitive bankers, estate agents or property developers – sociobiology itself has arrived at the very threshold of the egalitarian model which is central to my account.

When I first read Turke's 1984 article on ovulatory synchrony, I was as excited as I had been when I had discovered Sahlins' *Origins of Society* almost twenty years earlier. Since it corresponded so closely with my preconceptions, it was not that I was learning anything really very new. But Turke, I realised, was a respected figure in sociobiology. If this school of thought represented intellectual 'late capitalism', then – and here was the really exciting thought – here was that capitalism at last transcending itself. Followed through consistently, Turke had demonstrated, the logic of primatological sociobiology had led to the point at which gender egalitarianism amounting to a kind of communism had been found to lie at the roots of the human condition.

I discovered all this, however, only after my Ph.D. thesis had been completed in 1987. Had I been able to write it all over again, I might have couched it more strongly in Turke's sociobiological terms. I might have been able to make my myth seem rather more respectable – even sociobiologically conventional – had I taken as my starting point his own formulations for describing coalition-building, synchronously cycling females. The theory as such need not have suffered. As far as I can discern, there are no fundamental incompatibilities between Turke's version of the evolutionary role of synchrony and mine. I would certainly have needed to extend and draw out the implications of his model, but this, I think, could easily have been achieved.

In my lighter moments, I have sometimes pictured to myself how events might then have ensued. Obtaining funding from the great United States and other western grant-conferring institutions, I might have written a fully authorised sociobiological theory of human cultural solidarity – of the evolution of matrilineal and other clans, the establishment of menstrual

taboos, the emergence of hunting ritual, the invention of initiation rites, the appearance of dance and trance, magic and myth, poetry and song. In tune with the mood of post-1989 capitalist triumph over the world as a whole, I could have assisted sociobiology in its project not only to hegemonise these areas but to redraw the map of all the biological and social sciences, helping it out of its traditional zoological ghetto. The discipline could then have been released from its unfortunate confinement, as it might seem, within the myopic perspectives of wasps, gulls, macaques, chimps or the more chimp-like purposes of human beings – and allowed out at last into the expansive realm of culture, the domain of humans acting *as only we humans can*. In that context, E. O. Wilson's (1975) grandiose dream of abolishing anthropology as a separate discipline and uniting biology with the humanities within the framework of a *single science* might even have been accomplished – although, of course (since at the moment of emancipation the experience of revolution could hardly have been avoided), not in quite the manner he envisaged!

Although I was unable to rewrite the thesis along these lines, following sociobiology's logic even to its self-transcending conclusion, I have been given a new chance to update my story in the writing of *Blood Relations*. I am still not satisfied that I have made sufficient use of this opportunity. The book remains something of a compromise, with some sections taken largely from the thesis while others have been completely rewritten. Improvements could still be made. My patient publishers, however, will not allow me to keep revising indefinitely. Unfinished as it will always be, this is my current version. I am sure you have your own favourite myths. Here for your enjoyment is mine.