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### **The Palaeolithic Societies of Europe**

Palaeolithic societies have been a neglected topic in the discussion of human origins. In this book Clive Gamble challenges the established view that the social life of Europeans over the 500,000 years of the European Palaeolithic must remain a mystery. In the past forty years archaeologists have recovered a wealth of information from sites throughout the continent. Professor Gamble now introduces a new approach to this material. The archaeological evidence from stone tools, hunting and campsites is interrogated for information on the scale of social interaction, and the forms of social life. Taking a pan-European view of the archaeological evidence, he reconstructs ancient human societies, and introduces new perspectives on the unique social experience of human beings.

CLIVE GAMBLE is Professor of Archaeology at the University of Southampton. He is the author of several books, including *The Palaeolithic Settlement of Europe*, published by Cambridge University Press in 1986.

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For  
KIM HAHN and CATHERINE FARIZY

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## PREFACE

Two conferences were the inspiration for this book. The first, held in Bradford in 1981, examined European social evolution from an archaeological perspective. I gave a paper, a fragment of which is included here in Chapter 1, but did not feel that there was anything to be gained from publishing it since anything prior to the Neolithic looked like a makeweight; a fact borne out by the editor who concentrated his considerable energies on providing additional discussions on every period but the ones dealing with hunters and gatherers. The truth is I had very little to say and I began to wonder why.

The second was the 1986 conference on hunting and gathering societies held in the armchairs of the library of the London School of Economics. My expectations here were higher since the conference pedigree was blue blood. This was the descendant of the first *Man the Hunter* conference in 1968 which gave us the model of the modern hunter gatherer. While invited to present papers the archaeologists were however entirely excluded from the published conference volumes unless they happened to address social issues. Only one did.

While it was often difficult to stay awake in the comfort of the LSE armchairs as experts on the !Kung/Zhu/Ju/'hoansi hotly disputed what to call them and trainspotted unashamedly about which individuals were present at a waterhole on 26 August 1965, I did have one rewarding conversation.

I asked an expert on the Australian Aborigines what he would like to hear from archaeologists? 'How could we make a contribution that would keep anthropologists awake?' I inquired.

He didn't know for sure, 'But something social would be nice', he replied.

'But what do you mean by social?'

'Well, anything with some kinship in it', he stopped. 'Ah, but you archaeologists can't do that, can you?'

There is not much kinship in what follows but there is a good deal about what a social approach means for the study of 500,000 years of one continent's prehistory. As in my previous book *The Palaeolithic settlement of Europe* (1986), I acknowledge the difficulty of defining Europe and employ a nine cell regional model to standardize the problem. Where I depart in this volume is the time range explored. Instead of drawing the discussion to a close at the conventional date of 10,000 BP, I have opted instead to close at 21,000 BP just prior to the last glacial maximum. There are two reasons for this decision. One is the length of this book. To include the late glacial would have made it overlong and

unwieldy, and would have restricted the detail which is necessary. Secondly, by moving the cut-off point back in time I rather hope to confuse what is at best an arbitrary division and at worst a convention which prevents the Palaeolithic from being considered as part of later prehistory simply because it took place before the interglacial got underway. Why 500,000 years of prehistory should be frozen out of later syntheses by the weather report seems ludicrous. So, shifting the boundary back in time to a position where it cannot be taken as either arbitrary or indeed significant, but just plain convenient, is not a result of cussedness or exhaustion but is chosen to make a point; social life never stopped in Palaeolithic Europe, it never stood still for the climate and it certainly wasn't invented at some juncture. I plan to give a full treatment to the late glacial in another book.

In the decade which has passed since the publication of *The Palaeolithic settlement of Europe*, there has been a wonderful explosion of major surveys and excavations. The quality and quantity of data necessitated a complete rewrite rather than a second edition. Also my own interests had moved on. I am increasingly disappointed by two models which govern Palaeolithic analysis and which I have characterized to students as the stomach-led and the brain-dead interpretations of the period. The first still dwells on the importance of calories and is fascinated by the abilities of early hominids to kill animals with bits of wood and stone. The other attributes change to a gradual awakening of the hominid grey matter as though they had trouble for many long millennia not only in tying their shoelaces, but also in finding their feet. In effect what both views are saying is that a prime mover can be found to explain the archaeological record. This may be something like the development of co-operative behaviour, a fancy bit of new technology or the appearance of language which confers an advantage and explains new patterns. Arguments using prime movers have never worked in archaeology and they are even less effective in the Palaeolithic. What I have attempted here is to provide a context for generating patterns and in my opinion the only acceptable framework must be social. I will dismay many by championing the individual and presenting an interactional model for the performance of social life. I make no apologies, either for that framework or for the new vocabulary which must accompany it if we are to break away from the theory-laden facts which prevent new analysis. I am also unapologetic because while impressed by the virtuoso skills now evident in the excavation of open and rock-shelter sites from the Palaeolithic, I nonetheless have to ask, what is all this precision in recording artifact coordinates and taphonomy about? What do refitting studies tell us apart from the fact that we can do them and convince grant bodies to fund us? What social archaeologist in the Roman period would bother to invest the same energies in routinely refitting pot sherds on the scale now evident for the Palaeolithic? Have we in fact run out of things to do and questions to ask? I don't think so. What follows is partly an experiment in asking how such precision will allow us to move away from stomach-led and brain-dead explanations and instead

direct our analytical ingenuity to the richness of the social data our evidence contains.

Many people have helped me with the ideas and information which follow. The invitation from Wil Roebroeks and Gerhard Bosinski in 1992 to be a co-ordinating member of the European Science Foundation Network on the earliest occupation of Europe provided not only three years of travel, seminars and conferences, but the most rewarding academic environment for the discussion of Palaeolithic matters. The other network co-ordinators, Manuel Santonja, Lars Larsson, Alain Tuffreau, Luis Raposo, Nikolai Praslov and Margherita Mussi, will, I hope, approve of some of the results which I have included here. Our ESF minder, Max Sparreboom, deserves a special thanks for making so many things happen so efficiently.

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Finally, it is a great sadness that Catherine Farizy, who was also part of our ESF family, and Kim Hahn, who helped me so much as a struggling postgrad student, will not be able to give me the benefit of their opinions on what I have written. This book is part of their legacy to Palaeolithic studies and I dedicate it to their memory.

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