1. PHILOSOPHY AND BASIC PRINCIPLES

This volume, the culmination of ten years' work of the British Academy's Major Research Project on the Early History of Agriculture, attempts to draw together into a coherent whole the main theoretical themes advanced in our previous publications, and to illustrate their operation through a consideration of early agricultural development in Europe.

Our last major publication, *Palaeoeconomy*, proposed reasons for the development of the study of palaeoeconomy as a specifically formulated approach to prehistory. Reaction to this suggestion has, predictably, been mixed, and many readers seem to have misunderstood our intent. In an effort to provide some elucidation, and because of their crucial bearing on the present volume, some rehearsal of the arguments is now necessary.

Perhaps the commonest criticism has been levelled at the apparent arrogance of a proposal which seemed to claim pre-eminence for one approach to the subject by disparaging other competing viewpoints. This is an important question, which requires discussion. Unfortunate though it may be, this problem is bound to arise to some extent in any attempt to proselytise. If existing approaches were not deficient in some respects there would be no need to propose others. It was never the intention, however, to imply that palaeoeconomy offered the only acceptable framework for the treatment of archaeological data. Our concern was rather to establish its right to be considered as one valuable and distinct approach; and one that avoided some of the difficulties and limitations, which (it seemed to us) were inherent in other existing approaches. It is axiomatic, of course, that palaeoeconomy, like any selective study, is itself limited. We attempted to give reasons for our belief that these limitations were to a large extent forced upon us by the very nature of archaeological information.

Others argued strongly that palaeoeconomy was simply a part of 'human palaeoecology' or 'cultural ecology' under a different name, and that it was thus already established and in no need of a new name or of elaborate explication. In a way this statement highlights the very point we were trying to make when we suggested that ecological studies were too general in their scope to provide an entirely suitable theoretical framework. For the argument, if pursued rigidly, would suggest that fissioning of subjects would never occur, or at least need not do so. A whole range of modern disciplines – ethology, economics, geography, demography, and a host of others – could equally well be denied separate importance on the argument that they can all be subsumed within the sphere of ecological studies. Yet each of these has derived considerable impetus and advantage from the definition of its own specialised objectives, theoretical structure, and methods. Expressing the case in terms of Clarke's (1972) analysis of the different approaches within the 'New Archaeology' we would hope that a 'palaeoeconomic paradigm' would cull from, and integrate into a more effective whole, the most useful aspects of his 'ecological' and 'geographical paradigms'.

The critical question at issue, and the one that ultimately decides our choice of models, is the nature of archaeology as a discipline: its objectives, and the ways in which it is hoped that these should develop. Do we believe that 'Archaeology . . . is concerned with the recovery, systematic description and study of antiquities' in Clarke's (1968) words? In truth, this rather narrow view does less than justice to his multifarious and original approach, but it *is* representative of a large body of archaeological opinion, as a glance at many of the subject's periodicals will confirm. Is archaeology the time dimension of anthropology? Or are we aiming at a 'who-dunnit' kind of archaeology: an attempt to extrapolate back in time as personalised and 'real' an account of human activity as the wayward data will permit? Or is 'the proper subject of concern the social life of prehistoric man', as one eminent prehistorian has recently emphasised (Clark, J. G. D. 1975)?

Clearly it is vain, given the present state of turmoil in which the subject finds itself, to attempt to lay down a single exclusive gospel as the only route to archaeological salvation; but one can try to isolate those themes and approaches which appear to have more relevance and power for development within the context of the existing climate of thought and available techniques.

One possible avenue which has so far been but little explored is the development of a more specifically

scientific framework for the subject, although the current vogue in conceptual models is beginning to change this. Recent technological advances have, of course, placed within the orbit of many archaeologists a whole range of 'scientific' aids, from computers to bulldozers, but these have so far been employed primarily within the traditional framework of the subject and have not impinged greatly upon its theoretical structure. It is, of course, debatable whether archaeology 'should' try to become a science. The very idea is anathema to some. What is undeniable is that at present the subject cannot be described as scientific except in the very broadest definition of that term, and that a scientific archaeology would generate new aims and objectives, and thus would encourage a reappraisal of the data. Perhaps it would be justified for this alone.

An important prerequisite for such a development to take place is that we should start to search for regularities in the material. It seems reasonable to take Braithwaite's (1953) view that 'The function of a science . . . is to establish general laws covering the behaviour of empirical events or objects' and thus to be able to make 'reliable predictions of events as yet unknown'. For most archaeologists, the excitement and stimulus of the subject lies in its hints at uniqueness and individuality; the particular decorative motif which makes the pottery of a certain site or 'culture' unlike that of any other; the percentage of angle burins which makes a Palaeolithic assemblage an exception; the size and orientation of the gateway to the Iron Age hillfort which are hitherto unmatched; these are the focus of interest, and to some degree the measure of success. Little attention or prestige would be accorded the excavation report which stated baldly that the site and its contents were in all essentials identical with half a dozen others of the same age and area. But regularity and predictability are important, for it is they that indicate the laws or principles which may govern and structure the otherwise uncontrollable variety of our observations. They may, indeed, be taken as key indices of a scientific discipline, and a conscious search for them a *sine qua non* of the scientific approach. Most archaeology is unscientific when judged by these criteria, and we may note that Braithwaite excludes history from his corpus of scientific disciplines as being 'concerned merely with the occurrence of particular historical events'.

In recommending a scientific approach to the subject, therefore, we are being unashamedly deterministic, in the broad sense that the postulation of laws presupposes the existence of factors that regulate and determine the way in which the data behave. This does not necessarily mean that we are being deterministic in the more specific sense of excluding *a priori* the possibility of the operation of any random or 'chance' factors. The whole emotive issue of determinism (in its more general sense) reveals itself as a semantic red herring when pushed to its logical extremes. As is so often the case, the source of confusion concerns the relationship between the scale of enquiry and the explanatory devices chosen by the investigator. Carr (1961) neatly illustrated the absurdity of continuing to belabour the determinist *vs* free will debate in historical studies. Hull (1974) points out that whereas 'the basic laws of quantum physics are necessarily statistical' (i.e. non-deterministic) when applied to single particles, they may be deterministic 'for large ensembles of sub-atomic particles'. Our present concern is not really with the philosophical complexities raised by the apparent conflict of deterministic and statistical/probabilistic explanation, however. If archaeology can accept that important aspects of human behaviour are susceptible to scientific enquiry, from which may be derived laws or general principles, we may safely leave aside for the present the question of the precise nature of these principles.

The issue of determinism does have a further significance for us, however. The widespread passionate belief in human uniqueness has led to a situation where the word 'determinist' with regard to human affairs has become a pejorative or term of abuse. Let us at once, and unequivocally agree that man is indeed unique. But then so, of course, are all other organisms, at one level or another. Conversely, few but the most enthusiastic apostles of human free will would deny that there is a broad framework of restraint, of limitation, on our behaviour, which springs from the nature of matter and energy, and the ways in which organisms (including man) can gain access to them. The difficulty arises with the assertion that while all animals are unique, some, like Orwell's pigs, are more unique than others; that the essence of man, the important thing about him, lies in this uniqueness, and that therefore to study him in any other terms is to miss the whole point. Many archaeologists and anthropologists believe that man is raised to such a plane by his intelligence, by his language, and by the fact that much of his evolution is expressed through 'socially transmitted change' rather than through 'biologically or genetically transmitted change', that it is impossible to study him effectively in the same way that other organisms may be studied. From this viewpoint 'the main result of applying the principles of animal behaviour to human societies is to highlight their differences' (Clark, J. G. D. 1975).

This is a value judgement of great emotional appeal. No doubt modern proponents of this view are influenced in some degree by the same feelings which placed the Earth at the centre of the mediaeval universe. What must be accepted, however, is that the proposition can neither be proved nor disproved, although persuasive polemics can be and are marshalled on both sides. There are clearly demonstrable points of close similarity between man and other animals, just as there are distinguishing features. The choice of one of these aspects of humanity as having a pre-eminent significance depends upon objectives and basic philosophical stances, and cannot be justified purely in terms of logical debate. Implicit in the view which gives pre-eminence to human uniqueness is the belief that man is in some degree divine, supernatural; that the most significant part of human behaviour is not subject to the same laws which govern the rest of the universe. This is a proposition based upon faith or inspiration, and not upon data which can be rationally examined and evaluated. 'True' or not, it concerns a subject which by its own definition has placed itself beyond the scope of scientific consideration.

In recommending the development of a scientific archaeology (not as the sole approach, be it noted, but as one valid objective) we are therefore making an important statement about the kinds of behaviour and data that we are attempting to study. It is quite apparent that this will be a selective approach, certain aspects of human behaviour and archaeological data being dismissed as of little or no importance to the particular objectives in view because they cannot be effectively studied by the available techniques within a scientific framework.

ARCHAEOLOGICAL EVIDENCE

This leads us to a consideration of the nature of archaeological evidence. One important aspect of the development of modern archaeology, as with so many subjects, was the particularist nineteenth-century habit of observation, collection, and description as ends in themselves. Consequently the museums of the world are full of trilobites and torques, of butterflies, beads, and beakers, testifying to the energy devoted to that cause, while the libraries are similarly congested with monographs describing these objects. Such activities fulfilled a valuable function at the time, in many instances. Classification of its basic data is an important aspect of the early development of any subject. Archaeology retains perhaps a closer link with this approach to its data than many disciplines, as can be seen from the enthusiastic devotion of so much in the way of time and resources to 'rescue archaeology'. This work has an important place in the overall framework of the subject. It is hard, on the other hand, to avoid the observation that at present it is all too often motivated exclusively by the understandable desire to grab frantically at what we can while it is still there, when more preliminary thought about basic archaeological objectives might perhaps yield results of greater utility in the long run.

In the definition of these objectives a point of great significance is that archaeology is essentially imprecise in its dating. Contemporaneity of archaeological sites and objects can only rarely be established in other than the most general terms, even with the modern range of sophisticated dating techniques. The further back in time we go, of course, the wider become the margins of chronological error. While in Iron Age Europe one is used to accepting an uncertainty of a few decades or a century, students of the Upper Palaeolithic must cope with potential inaccuracies of several centuries or even millennia; and those working on the Lower Pleistocene development of man are more than thankful if they can on occasion tentatively date a site, skull, or tool, to within a couple of hundred thousand years.

A second major limitation of archaeological evidence is the degree to which it has been destroyed and distorted since its time of origin. The vast preponderance even of those aspects of life which leave solid and unenigmatic remains must have been destroyed or damaged beyond recovery and interpretation. Those realms of human behaviour which leave no solid traces, or those artefacts which are but obscurely and deviously related to the activities which gave rise to them, offer the richest field for speculation, but perhaps the poorest harvest of testable hypotheses. No one could deny the importance in human behaviour of language, religion, art; of social, moral, and psychological systems and values. However, their interrelationship, and their relationships with other factors such as the environment, the economy, and technology are both complex and poorly understood; so that even in a modern context it is not possible to assess the nature and significance of all the various forces with any confidence.

As far as prehistory is concerned this objective becomes absurd, as few if any of the relevant data are available, or could ever be. Prehistoric art exists, of course, but not one of the competing theories purporting to explain it can be substantiated or refuted, nor is it possible to imagine how this could be accomplished short of conversation with the artists. When one consid-

ers the confusion which surrounds the nature and purpose of modern art, one wonders indeed if this goal would be brought much nearer even if such conversation were possible. Similarly, religion has clearly exerted considerable force in the past, as it does today; but it is hard to envisage circumstances that would permit us to get much beyond the description of those few aspects of belief which become fossilised in monument or artistic representation.

The question of 'social archaeology' requires especial mention, as it is much in vogue today to maintain that the future of the subject lies in the analysis of the social behaviour of prehistoric man. Again, much depends upon what is meant by this. A leading proponent has stated that 'Social archaeology owes its crucial importance to the elementary but basic fact that all archaeological data are the product of the labours of men who not merely lived in society, but acquired their patterns of behaviour as members of social entities' (Clark, J. G. D. 1975). The 'basic fact' is indeed incontestable, but is expressed in terms too general to take us much further in our attempt to develop an effective approach to archaeological studies. It is so generalised a proposition that a similar form of words could easily be used to justify the primacy of environmental, ecological, geographical, or indeed cultural archaeology. A similar statement could moreover be made with equal accuracy of any of a wide variety of social animals, many of which have complicated repertoires of behaviour that develop and are transmitted within specific social contexts. It is revealing here to contrast Clark's view of the relationship between human and animal behaviour with that of Wynne-Edwards (1962), who asserts that the close affinity of man to other animals is particularly apparent from a study of their social behaviour.

Of course, the development of social archaeology has not rested entirely on truisms. Clark and others have gone on to pursue specific objectives in social archaeology. Many of these involve the attempt to derive descent and kinship structure from a detailed consideration of archaeological data, especially patterning in pottery typology. These studies are united by a number of features: the availability of a wealth of ethnographic evidence as an interpretative aid; relatively complete archaeological information; and an unusually tight control over chronological variables due to the choice of recent periods, where the margin of error is less, and where historical data are frequently available for some of the period under consideration. These advantages immediately place these studies in a most privileged position compared with almost all archaeological situations. Furthermore, recent anthropological opinion (Allen & Richardson 1971) seems to indicate that the interpretation of even those favoured bodies of data has failed to take account of the complexity and variability of human social behaviour.

Yet more important, from our point of view, is the growing realisation among anthropologists that many social mechanisms are largely reflections of economic activity, and thus act as dependent rather than independent variables. For while Sahlins (1972) argues that '"Economy" becomes a category of culture rather than behavior, in a class with politics or religion', this seems an unnecessarily parochial viewpoint, and one which takes no account of the many successful attempts to view both economy and society in their wider relationships with the environment. Thus Leeds (1961) found that many detailed aspects of Yaruro belief and cosmology were intimately related to their ecological niche, and that 'This ecological balance in turn limits the human population and socio-economic forms. Nor can any immediate desire or fancy of the Yaruro change the limitations which the ecological relationship imposes'. McLoughlin (1970) in his survey of East African peasant society concludes yet more specifically that 'relationships within the immediate family, within the extended kinship group, and within the broader community have developed over time to guide and regulate the decision making systems for food production'. Rappaport (1968) found a similar relationship between the belief systems of the Tsembaga of New Guinea and their environment, concluding that 'ritual operates to keep the trophic demands of the Tsembaga and their pigs within the carrying capacity of their territory'.

The longer the time scale involved, the more apparent becomes the dependence of social upon economic behaviour. It is true that disciplines which focus on modern human behaviour have many significant advantages over archaeology. The range and refinement of the data available to them are incomparably superior to those from even the best of archaeological circumstances, and they have an enviable capacity in these days of sophisticated mathematical models and hardware to design experiments specifically with a view to testing them against data gathered under controlled conditions. Because of the short time scale inherent in these subjects, however, they are in a poor position to assess the evolutionary significance of the processes they study, to sort out short-term factors and random components from those which may be part of longer term developments. Archaeology is in the converse position.

The apparent conflict between the anthropological and archaeological viewpoints emerges as a question of

alternative chronological focus occasioned by the differing time scales forced on the subjects by their data. To this extent there is no incompatibility between the two views, which merely concern different facets of the same processes. In medicine physiological changes can frequently be explained in terms of neurology, endocrinology, microbiology, and the external environment. Each explanation may be sound, and even complete within its own frame of reference. The profound differences result from the scope of enquiry and scale of causation which is sought.

Similarly in behavioural studies, the answer to the question 'why did A do B?' is never simple. The reasons why an individual or a society acts in a particular way or evolves in a particular direction may be couched in terms of choice, genes, or environment with equal justification, depending upon the point on the scale between proximate and ultimate causation that has been chosen as the objective of enquiry. The decision as to the appropriate framework for studies of human behaviour rests overwhelmingly on the time scale involved. Prehistory must perforce deal for the most part in terms of evolutionary forces and of guiding principles which operate in the long term, while sociology and kindred subjects are constrained to concentrate upon the short term and proximate causative factors.

In querying the emphasis placed by some archaeologists upon social factors we are thus not denying these all significance or a place in the scheme of things. They do appear, however, to be of secondary rather than primary importance among the long-term determinants of human behaviour, which are the concern of this book. Social behaviour is sufficiently flexible and volatile to vary widely (in its own terms) but on such a local and short-term scale as to place it effectively beyond the reach of archaeological investigation. Examples of this are innumerable. One has only to consider the worldwide impact of industrial upon pre-industrial economies to be aware of the fragility and – necessarily – adaptability of social institutions. We must also remember that a number of social systems may be developed to accommodate a single range of basic economic and environmental variables. Cole & Wolf (1974) point out that 'One finds an effective use of an entire alpine valley by a communal organization whose members hold all resources in common; by a division of a valley into two or more interdependent communities, each of which exploits some aspect of the environment . . . or by a series of independent households, each holding exclusive rights to a portion of each ecological zone'. They then go on to document such variations in the organisation and behaviour of two villages within 2 km of each other in the same valley. Wilson (1975) has suggested that the widespread social flexibility of man is strongly selected for genetically, a factor reinforced by the existence of multiple adaptive peaks appropriate to a number of different social systems. The point is therefore not to question the existence of such variability, or its importance to certain kinds of study; but rather to suggest that such fineness of detail is in most, if not all cases, inaccessible to archaeology, which may more usefully base its enquiries upon more stable phenomena.

PALAEOECONOMY

What are we left with then? It seems that there may be distinguished certain aspects of prehistoric human behaviour that are susceptible to a scientific approach which consciously searches for regularity, for organising principles; and a category of data that is not amenable to this approach. It is important to make this distinction, and to realise that doing so does not imply a judgement as to the relative value of the two approaches. They are simply 'other' and have different interests and objectives.

It is worth re-emphasising, however, that the nature of archaeological information suits it better to some kinds of enquiry than to others. Archaeology's coarse and indeterminate chronology, and the precarious control it has over the quality and completeness of the information available, severely impede the analysis of short-lived or individual phenomena. On the other hand, the subject is uniquely placed among the disciplines which deal with man to observe and analyse long-term trends and factors in past human behaviour which appear to be of abiding significance.

Among the aspects of past human behaviour which are particularly appropriate for analysis in terms of a long-term, evolutionary approach, economic behaviour clearly has a great importance. The economy is the primary adaptation whereby life is maintained and populations survive and grow. It is thus a biological linchpin exposed to the full force of natural selection, and as such it is to be expected that the impact of economic necessity or advantage will be widespread and profound in human behaviour. Many people, and some modern economists among them, would protest that economic necessity is too rare an occurrence to have acted as a stimulus, or to provide us with an explanation of behaviour. Studied in the short term, economic behaviour frequently appears to operate according to 'satisficer' rather than 'maximiser' models.

Satisficer models were primarily developed by economists and geographers dealing with short-term decision-making in industrialised economies and large business corporations. Starting from the realisation that man has neither the sensory nor analytical equipment to compute the precise 'maximal' or 'optimal' strategy in a given set of circumstances, it was found that individuals tend to settle for a 'satisficer' solution to a problem, one which achieves the necessary goals to a level satisfactory to the person concerned, but not necessarily to a level which could not be improved upon.

This is all undisputed. Everything here again depends on the time scale of study, however. The long-term development of economic behaviour seems to be more profoundly influenced by the exigencies of natural selection than by limitations on the mentality of individuals. A particular beetle or bandicoot doubtless operates suboptimally in many cases; and, who knows, may on occasion act in accordance with satisficer models. This does not release them from the biological Sword of Damocles which threatens that if beetles or bandicoots in general fail to tend towards an optimum subsistence strategy, then they will be supplanted by other beetles, bandicoots, or their competitors.

There is a danger that, in looking for adaptiveness and an approximation to the optimum in human economies, we may be interpreted as relying upon a Panglossian argument that all human relationships with their environment are for the best in this best of all possible worlds. This is far from the case. It will certainly be seen that in our discussion of ethnological and archaeological examples we search for the adaptive significance of economic arrangements. This is by no means the same as suggesting that the fit is always perfect, or that maladaptation may not arise and persist for a while. Among human communities, individual and community choice, periods of unusual affluence when economic factors seem of little direct significance, deliberate pursuit of less than profitable goals, these are all observable in the short individual and historical time scale, although perhaps a little less frequently in subsistence economies than might be supposed. It requires a conscious effort to view human behaviour on a longer, evolutionary time scale, but it is a necessary effort. It is easy to be dazzled by the present-day affluence of industrialised societies, and to be blinded by anthropological and historical examples of uneconomic behaviour. Recognition of these should not, however, prevent our perception that there are observable long-term trends, which override the individual and short-term variability. These trends may be attributed to the operation of evolutionary forces, and *from this particular viewpoint* the short-term deviations may be dismissed as 'noise'. As we shall see, the tendency of human as of animal populations to press upon their available resources – to exceed their optimum population level in Sauvy's (1969) terms – is a powerful incentive in the long term for the development and maintenance of well-adapted productive economies.

THE RÔLE OF POPULATION PRESSURE

The proposition that economic behaviour may be seen in evolutionary terms should perhaps be examined a little more closely. What, for example, is there to prevent or even to discourage a community or population from behaving in an 'uneconomic' fashion, even in the long term? Why should not certain societies maintain unproductive economic systems even on a prehistoric time scale? Our view is that, at least as a general rule, they are forced by internal or external population pressure to adopt more productive patterns of exploitation.

Population pressure is inherent in human, as in other animals' existence. This will be taken by many as a most contentious statement, and indeed much recent anthropological dogma proposes the precise contrary. A conclusion frequently drawn from such studies as Lee's now classic work on the !Kung Bushmen (1968, 1972a) is that most 'hunter–gatherers' have the inherent ability to adjust their population size to a level well below the theoretical carrying capacity of their environment, and to maintain this relationship by a variety of social and other mechanisms. In this way a state of quasi-permanent relative affluence is inferred, and this inference is boldly extended to pre-Neolithic man as a whole. Modern industrial man is seen as having lost this capacity for self-regulation, a process believed by many to have originated with the Neolithic 'food-producing revolution'.

Some have sought to support this proposition by pointing to the existence of comparable mechanisms in many species of animals. It is now well-established that there exists a wide range of physiological and behavioural syndromes in the animal kingdom whose overall effect is to regulate population density. These vary widely in their nature, and not all of them are directly density-dependent; but they do all exercise a degree of control on the animal numbers. This evidence has been coupled by some workers with the ethnographic observations to argue for a major dichotomy between on the one hand pre-Neolithic 'natural' man, who, like other animals, was spared the impact of population pressure by the intervention of these barriers to uninhi-

bited population increase; and on the other hand Neolithic and more especially industrial man, who, having in some mysterious and unexplained way lost these evolutionary responses despite their obvious adaptive value, were exposed to continuing and increasing demographic stress.

Let us briefly examine the position. As we have said, there is a growing body of evidence and an increasing degree of acceptance that animal populations are regulated in their numbers by a wide variety of mechanisms. Most workers are agreed that food supply is the overriding ultimate limiting factor, although simple 'Malthusian' control may occur rather infrequently. The proximate cause is often more subtle and complex, operating in such a way that the animal population may be viewed as self-regulating. One authority has even seen the need to control population size as the primary factor giving rise to social behaviour in animals (Wynne-Edwards 1962). Some have taken this to mean that population pressure does not normally occur in animals, as the inhibitory mechanisms to population growth are initiated at levels well below the theoretical carrying capacity, and the population is therefore perpetually buffered against attaining levels where pressure will be experienced.

A number of considerations suggest an alternative view. The first is a general point that may usefully be illustrated by reference to an analogy suggested by the use of the term population *pressure*. Gases are frequently considered as exerting or being at certain pressures, a characteristic which is relevant whether or not the pressure is sufficient to burst the container or to liquefy the gas. Similarly, population pressure need not necessarily predicate far-reaching or cataclysmic reactions in animal populations. From this point of view we may see the very ubiquity and significance of regulatory mechanisms as evidence of the widespread existence of population pressure, rather than as indications that such pressure rarely occurs. This is not simply a semantic device for denying the obvious. Upon close examination it is hard to see the justice of an argument which infers directly from the existence of self-regulatory demographic responses in animals to an absence of any population pressure. How far such a suggestion is from Wynne-Edwards' thesis can be seen from his discussion of periodic population eruptions, such as those studied so extensively in certain Arctic species. Here, emigration is specifically invoked as a 'means of rapidly reducing population density in any area where the optimum has been exceeded'.

This general point aside, other evidence suggests that population pressure must be considered an important potential causative agent in animal behaviour. Indeed, as has been extensively documented by Lack (1954), there are numerous instances of food shortage acting as a limiting factor on animal populations. Lack discusses examples from birds, mammals, fish, and insects. It is perhaps easy to get an impression, in many cases, of a comfortable gap between animal populations and the carrying capacities of their environments. A number of points must be taken into account, however. Seasonal and annual variations in available resources need to be considered. Pressure of population on resources is frequently only experienced at one or two seasons of the year, as in winter, and during the period when the young must be fed. Such pressure may only become acute in particularly poor years, when resources are less abundant than usual. Thus periods of severe stress will be intermittent, and irregular, even though they may occur quite frequently enough to be of great evolutionary significance. Some consideration also needs to be given to the recognition of the relevant factor in the environment. As a broad generalisation, where food is limiting, populations will be related to the abundance of *the least available necessity at the worst time of the year*. It is no use measuring one staple resource and expecting that a close relationship between this and population levels will necessarily emerge.

To some extent the efficiency of exploitation in terms of energetics is also a consideration. A corollary of the concept of an optimum population level is that increased population will automatically result in a fall in economic return per unit of labour input: the 'law of diminishing returns'. Either standards of living will decline, or individuals must work harder to maintain the same level. For this reason considerable pressure can in some circumstances be experienced by an exploiting population long before deleterious effects are necessarily apparent in the resource population. Thus those populations of wolves that subsist mainly upon moose, caribou, or Dall sheep are thought to be limited primarily by the difficulty of catching their prey. Higher wolf populations would be under great population pressure because they would run out of prey they were physically able to catch. They would have to subsist on less food, or devise ways of catching fitter prey, or reduce their own numbers. Nevertheless it is most unlikely that a higher rate of predation, if this were the eventuality, would seriously jeopardise the prey populations, which could adjust to the additional burden of exploitation by an increased rate of reproduction, by maintaining slightly lower population levels, or by surrendering fewer individuals to other causes of mortality.

Precisely the same considerations are relevant in the case of human populations. The !Kung Bushmen provide a useful example, both because they have been studied in great detail for a number of years and because extensive use has been made of the resulting data by anthropologists and archaeologists. A number of features of !Kung life led to most interesting conclusions regarding the relationship of their population to its available food resources. Lee (1968, 1972a, 1976) was able to show that, contrary to much established anthropological doctrine, this particular group of 'hunter–gatherers' showed many signs of relative subsistence affluence. Life expectancy was fairly high; infanticide occurred but was not common, while senilicide was very rare. Clinical malnutrition was rare or absent. Food was obtained with remarkably little expenditure of effort, at least in terms of man-hours. Considerable surpluses of the staple mongongo nut were observed to remain unexploited.

This evidence has been interpreted by Lee and others in a number of ways. Two propositions of particular importance have emerged, the two seeming superficially to be very similar, but in fact carrying crucially different implications. The first proposition is that hunter–gatherer populations are characteristically maintained at a level below that which would start to deplete the food supply. This seems to be a perfectly acceptable inference; and indeed, when one considers the general picture, it must be so. If population levels, and thus intensity of exploitation, were as a general rule so high as to diminish the available food supply, then a very few seasons would lead to the classic Lotka–Volterra predator–prey oscillation, with the attendant likelihood of extermination of the predator – man, in this case. Such a crude pattern must have been as rare an occurrence in man as it seems to be for other animals. Human economies must, like other natural exploitation systems, have as a rule been *efficient* in Wilkinson's (1975) terms; that is to say, they will have been regulated so as to ensure sustained yields in the long term.

The second proposition takes the form of an apparently acceptable minor extension of the first. It is argued that 'since food supply appears to be abundant in modern hunters' and that 'since it appears that some observed hunter population densities have become stabilized at a mere fraction of the numbers that could be supported by the food supply' (Lee, 1972b), food supply is not the limiting factor involved, which must thus be sought elsewhere. Various social mechanisms, such as the wide spacing of births, are commonly proposed as the agents involved.

It may be the case that the apparent superabundance of mongongo nuts indicates a surplus of food to !Kung Bushman requirements, although Lee's data are insufficient, in time depth at least, to demonstrate this satisfactorily. As was mentioned above, however, it is of critical importance to take into account fluctuations in the level of resources from season to season, year to year, and decade to decade. Lee himself (1976) obviously realises this:

> a hunter–gatherer group may be able to satisfy subsistence requirements within 100 km^2 for four years out of five but it will still go out of business unless it has access to a much larger area during the fifth year. And in order to ride out environmental fluctuation over the course of 50, 100 or 200 years, the area to which the group must maintain access must be even larger. . . . However, little of this long-term perspective is visible to the observer.

The point is, of course, that one simply cannot make a reasonable assessment of population–resource relationships without taking this long-term perspective into account.

In fact, examined closely, much of Lee's own evidence and that of others working on the Bushmen seems to indicate a considerably greater degree of population pressure on food resources than is commonly supposed. The misapprehension seems to have arisen as a result of the failure to take sufficient account of conditions at the lean period of the year and to isolate the true limiting factor involved – which is clearly *not* the availability of the mongongo nut. In a semi-desert area such as the Kalahari it seems likely that water will be one of the most important resources, and indeed Yellen & Lee (1976) state that the 'scarcity of permanent water is the most crucial limiting factor with which the !Kung must contend'. In other, drier parts of the Kalahari, water is even more stringently limiting. Silberbauer (1972) found that the G/Wi Bushmen had surface water available for only six or eight weeks of the year, having to rely on vegetable sources of fluid for the rest of the year. Not only is water shortage itself limiting; it has an important effect upon the availability of food. The dry, lean season of the year lasts for about three months, from August to October. By the end of this period 'The areas within easy walking distance of the waterholes become depleted of choice foods, and diet consists largely of roots, edible gum, and whatever less desirable foods may be found' (Yellen & Lee 1976). Detailed medical examination (Truswell & Hansen 1976) seems to show that, although clinical malnutrition is a rarity among the !Kung Bushmen, a general level of undernourishment is the norm. The people are all thin, and are shorter than their genetic

potential would almost certainly allow. Because 'In these lean months, the foods eaten are mostly roots and bulbs, which would be expected generally to contain much water but to have low caloric density', these authors conclude that 'the only nutritional weakness . . . is a shortage of energy (calories) usually in the dry spring season'.

These points illustrate the importance of a careful consideration of seasonal and annual variation, and the other factors involved. It is especially noteworthy that the water shortage acts in two important ways in the case of the Bushmen. Not only is it clearly a limiting factor itself, particularly in years of drought when a number of bands have to share the same waterhole. Just as important, if less obvious, is its effect in restricting the possible range of dry season exploitation to a day's walking distance (round trip) around the few permanent waterholes. During the lean period of the year food supplies beyond this radius are inaccessible, and thus no matter how rich they may be they are irrelevant to the question of the relationship between population and resources.

The ethnographic record supplies many similar instances of populations relatively closely adjusted to the available food resources. Baumhoff (1963) describes such a case from California. Damas (1972) says that 'Famine was not uncommon in the aboriginal period' among the Copper Eskimo, and there was a high incidence of female infanticide. Starvation was widespread among the Inuit Eskimo in the winter of 1957–8 (Hoffman 1976). 'Securing food is a constant problem and a never-ending concern' among the Mistassini Cree, and 'times of starvation are vividly remembered' (Rogers 1972). The Guayaki, according to Clastres (1972), undergo food shortage in their winter lean season. The importance of taking annual variations into account is evident from Gardner's (1972) discussion of the Paliyans. 'Their natural environment in a good year is bounteous far beyond human need . . . While this allows them a wide margin of safety in times of distress, conditions do sometimes become so difficult that inter-regional migrations are necessary.' Carneiro's (1961) work on the Kuikuru is frequently quoted as supporting the suggestion of a general absence of serious population pressure under aboriginal conditions. Here, as with the !Kung, it is questionable whether a sufficiently detailed analysis is available to substantiate the claim. The data certainly seem to support the view that considerably more manioc could be grown, and that thus many more calories could be extracted from the environment. Manioc, however, like most root crops, is so poor in protein as to be a negligible source of this most essential nutrient. It therefore seems at least possible that the limiting factor involved is availability of protein, which seems to come mainly from fish. This view is certainly suggested by Lathrap's (1968) review of the evidence. Barth (1960) notes that in southern Persia 'the pastures of the whole of Fars are utilised nearly to their total carrying capacity'. Elsewhere in Iran, Salzman (1972) describes a situation in which pressure of population on resources is such that the sale of labour in towns and the cultivation of marginal land are necessary to maintain the mobile pastoral economy through the lean period of the year.

This list could doubtless be expanded endlessly; many of the examples quoted above came from a single publication. On the other hand, there is also a large number of cases in the literature where it is claimed that there is no evidence for a high degree of population pressure on resources. This apparent contradiction illustrates the limitation of ethnographic evidence for our purposes. From the point of view of archaeology's concern with the long term, the time depth of most ethnographic studies is woefully inadequate. For this reason we must depend primarily upon theoretical arguments, although it does also seem that there is considerable ethnographic evidence in support of our view that human populations, like those of other animals, frequently exert considerable pressure upon their food resources.

The significant question, as far as studies of man are concerned, thus emerges not as 'why do human populations experience population pressure, while other organisms do not?' but 'why are many modern human populations apparently less sensitive to the lower levels of pressure than many animal populations, thus critically exceeding the optimum population–resource relationship with such dramatic short-term results?' It seems that an important distinction needs to be made between on the one hand *recurrent (or, on an archaeological time scale, sustained) population pressure*, which occurs so frequently in nature as to be treated as a general rule; and on the other hand, *sustained population increase*, which occurs in rather more specialised circumstances. Human development provides many striking examples of this latter phenomenon.

It must at once be admitted that in the present state of knowledge we can offer little beyond a brief and generalised discussion of the possible reasons for this state of affairs. One difference between man and other organisms is the degree to which his subsistence is technologically oriented. An obvious consequence of a technologically rather than a physiologically based eco-

nomy is the capacity for much greater rates of change and increases in productivity. One should perhaps point out here that radical changes in animal subsistence behaviour, sometimes permitting far higher populations to be maintained, are by no means unknown. The successful exploitation of man-made habitats; the colonisation of inland ecological niches by some species of seagulls; the dramatic expansion in the range of the fulmar (*Fulmarus glacialis*) in Atlantic Europe; these provide well-known instances of successful opportunistic changes in animal behaviour leading to higher overall population levels. Man stands out, however, both by reason of his sustained long-term population increase, and by the degree to which he has been able to raise his economic productivity, and thus his population, per unit area. It is hard to see how this phenomenon can be dissociated from man's technological development, the means whereby this increased productivity has been achieved, and the ever-increasing population levels sustained.

It cannot be overstressed that we do not feel there to be any cogent evidence for the separation of non-agricultural from agricultural and industrial man in these matters. Evidently the rate of technological progress, as the rate of population increase, has accelerated with time, but there seems no reason to suggest that essentially different forces or balances come into being with the development of modern economic systems. Evidence for rising populations from the Neolithic onwards is widespread, if patchy. Some data will be discussed in great detail later in this volume. Evidence from the earlier periods is inevitably more scanty and harder to interpret, but certainly the number of occupation sites known per unit area shows a distinct tendency to increase with time. We must, furthermore, take into account man's dramatic ecological and geographical diversification. From his tropical savannah origins he has successfully colonised an impressive number of land masses and environments, extending his range by a factor of tens if not hundreds. Occasionally more revealing evidence emerges. Wendorf's (1968) excavation of a Late Pleistocene cemetery in Nubia in which about half the bodies showed signs of a violent death is extremely suggestive of warfare. Other hypotheses are of course possible, but perhaps less likely. As Wendorf himself points out, accepting the evidence at face value, localised extremes of population pressure provide a reasonable – in our view the most plausible – explanation.

Indeed, the widespread historical evidence of endemic warfare in parts of North America, both among 'hunters and gatherers' and among agriculturalists, ought to make us suspicious of any generalised proposition concerning the absence of population pressure or stress at certain levels of economic development. Where the evidence exists, the warfare is as a general rule specifically for economic motives. Hickerson (1965) says that in the Upper Mississippi valley in the eighteenth and nineteenth centuries 'Warfare was a function of competition over game', and was in fact one of the mechanisms whereby the local human populations adjusted to the available resources. After only three years of enforced truce between the Sioux and the Chippewa famine became widespread, resulting eventually in a return to warfare. Larson (1972) has shown that in the southeastern United States 'suitable agricultural lands were a critical resource in some areas . . . during the Mississippi period, constituting a causal factor and the primary objective of the apparently endemic warfare in the area'. There is confirmatory evidence from other areas and time periods. Browman (1976) has argued convincingly that the Wari imperial expansion in the Junin department of Peru, *c* A.D. 400–500, and the economic change from llama pastoralism to potato agriculture which accompanied it, were substantially caused by demographic pressure. Reichel-Dolmatoff (1961) showed that under aboriginal conditions in Colombia populations were so closely adjusted to the available resources that 'the 80-inch isohyet [which divided the zone of single-maize cropping from that of multi-annual cropping] was practically a military frontier'. Warfare was one of the primary mechanisms of population regulation, and 'In many sub-Andean chiefdoms cannibalism was simply part of the food quest'. The intra- and intertribal warfare widespread historically in parts of New Guinea has been explained in terms of competition for land (Brookfield & Brown 1963). Vayda (1961) has suggested that there is generally a sound ecological function for warfare among swidden agriculturalists. Although in individual cases other explanations may be appropriate, it is surely reasonable to view such warfare as an extreme form of intraspecific competition, a phenomenon which seems hardly likely to arise in the absence of considerable and widespread population pressure.

We should similarly reject any too easy assumption that twentieth-century man has lost the capacity to regulate his populations by means of inherent biological mechanisms. The growing worldwide interest in artificial means of birth control and the rise of such groups as the Zero Population Growth movement might be viewed as expressions of these forces. In Formosa, Hong Kong, and Singapore the birth rate dropped significantly and steadily in the early nine-