

## Epilogue

In expressing my thanks to Dr. Lech Krzyżaniak for inviting me to give this overview of the conference I would also at the same time like to offer my apologies for its shortcomings of which I am only too aware. So energetically have we pursued our discussions often deep into the night, and so warm has been the hospitality we have experienced here at Dymaczewo that there has been little time in which to marshal one's thoughts on the great amount of information and ideas that have been put before us during the last five days. Never before has there been a major conference as this one, devoted to understanding more about the special circumstances relating to the spread of early food production in Africa. An earlier conference, leading to the publication of the volume *Origins of African Plant Domestication* (J. R. Harlan, J. M. J. de Wett and A. B. L. Stemler [eds.], 1976) dealt with what was known or could be inferred about indigenous food plants up to that time. It took, however, little or no account of the animal domesticates and the direct and indirect effects the possession of these may have had on bringing about the transition from collecting to cultivating some of the plants that were staple foods for the late Quaternary hunting/gathering populations. It is important that both plants and animals have been covered in this present conference, therefore. In fact, there has probably been greater stress on the animal domesticates here than on plant cultivation, not only because of the greater visibility of faunal remains in the archaeological record, and because the pastoral way of life is a more viable one in the largely arid northeastern parts of the continent with which we have been dealing but, also because, except where special circumstances, such as the presence of tsetse fly, preclude the keeping of stock, mixed farming is always likely to have been the best insurance against economic adversity. It is also clear, from the way many of the papers stress the economic data, that this dual emphasis has been the right one and it is the direct evidence of the animals and plants themselves rather than the indirect, from cultural remains or historical linguistics for example, that must always be decisive in showing the true nature of prehistoric economies.

Even with the special expertise many archaeologists possess today it is often difficult, if not impossible, for prehistorians alone to deal adequately with the amount of data the new excavating and recording techniques make available today and more than ever it is necessary to seek the aid of other specialists. This conference has brou-

ght together people who have never talked to each other before — prehistorians, Egyptologists, botanists, palaeontologists, to mention but a few — and it has emphasised that there are many common interests and problems on which light can be thrown by concerted common action through the interdisciplinary approach.

Our meeting here has shown also that science and prehistory are international and are the interest and concern of every scholar working to understand better the record of the past, regardless of nationality. It is the interdisciplinary and international approach to solving our problems and learning more about how domestication came to Africa, the sharing and pooling of expertise and resources that is the surest way to succeed in our goal to solve or get closer to solving some of the problems concerned with the why, when and wherefore of the domestication process. With the dedication and expertise that have been demonstrated here and the friendships that have been made, we can hope to develop new approaches and new teams which will have greatly expanded our understanding of the domestication processes when this conference meets again, as we all hope it will, in four years time.

One might say it is remarkable that we have been able in these four and a half days to say so much about so little. I do not mean this in a slighting way but I think everyone realises that we have so few data that we naturally try to stretch them further than they will really go. One of the most urgent necessities is, I believe, for more evidence obtained through more planned, systematic investigation. We need the empirical evidence from survey and excavation of sediments that will give insight into the life-style of individual groups and communities but the way to obtain this we have found is not through opportunistic digging, just because a site happens to exist in one's study area. If it is the archaeological data that are the cornerstone of our interpretation, it is the planned approach and the use of frameworks provided by hypotheses and the models these suggest that — this conference has shown — is likely to be one of the most profitable research strategies in the future. That we need more evidence is only too true but we should not make this an excuse for not trying to look at that which we already have in the light of behavioural models derived from hypotheses generated by the whole extent of the archaeological data and ethnographic analogies. Models are not interpretations of what went on at a site. We are probably never likely to know this precisely. They are only, as the name implies, models that can be tested against the evidence coming from the sites themselves but which can help towards a better understanding of the roles these sites played in the total procurement pattern of the inhabitants.

Another thing that I think, therefore, came out of this conference is the fact that the site we excavate is but one small surviving material part of a system of social and economic behaviour of a prehistoric group of people — men, women and children. How did they use their resources and what was the rest of the area like over which they ranged? Why was the site located where it is? And what were the respective roles played by its inhabitants? We cannot try to answer these questions unless we look at the way our prehistoric populations used their space. This has been very

well demonstrated by the work about which we have heard of the Combined Prehistoric Expedition, of the Polish team at Kadero and of those working in the Aca-cus and in East Africa. We can only succeed in analysis of space through the inter-disciplinary approach. What were the resources that were exploited from a particu-lar site? Was exploitation the reason why the site was situated where it is or was it for social, religious or other purposes? Knowledge of palaeogeography, hydrology, fauna, flora, raw materials and many more of the phenomena that go to compose the ecosystem is what is needed in order to be able to try to reconstruct the ways in which the land and its resources were used; it is the behaviour behind the artifacts that is important and exciting and not the tools or pots themselves.

So again, we come back to the team approach and the need for archaeologists to become acquainted with and knowledgeable about the plants and animals and hu-man adaptations in the area today as well as the extent to which these may have differed in the past. Ethnography is the key to understanding human behaviour just as geomorphology can provide basic knowledge of the palaeoenvironment that is an essential clue to understanding prehistoric adaptations. Several speakers called attention to and made good use of ethnography and the interpretations ethno-archaeology makes possible but there has not, I feel, been nearly enough of this especially since a number of the papers presented have emphasised the extraordinary continuity of culture and ethnicity in the areas with which we have been dealing. If it is the natural sciences that give us our methodology it is anthropology that en-ables us to interpret the evidence in terms of human behaviour. While it is clear that we could make far more use of ethnographic evidence than has been done up to now, we should remember that we are never likely to find a one to one relationship be-tween ethnographic analogy and any past situation. It does, however, enable us to identify with people, their societies and human reactions and so enables us to narrow down considerably the possible interpretations of the archeological record.

To turn to a few of the highlights and specifics: — we have all been greatly im-pressed by the exciting evidence from Wadi Kubbaniya and sites in the Western Desert of Egypt, from Capeletti Cave and other sites in the Maghreb and by that from those pastoral settlements along the Nile in the Sudan and in East Africa that have produced some of the first direct evidence of domesticated animals and plants. But what is a domesticate? animal or plant — and what are the genetic and other processes that have to be gone through before domestication is arrived at and which leave a recognisable record? To this, I suggest, we have not given enough attention. Are the animals and plants from the early sites we have been hearing about indeed domestic forms, or could the evidence be interpreted in some other way? How much manipulation by humans is first necessary to produce a cultigen or domestic animal, and how long does this process take? It rests for further research — the recovery of more complete remains, better dated samples, in even better contexts — to show how far our interpretations today will be confirmed or will need amendment.

The palaeobotanists and palaeontologists have told us of the problems they have

in making positive determinations as between wild and domestic forms on the basis of the fragmentary archaeological remains and especially in the case of plants, from pollens and impressions in pottery. We should take due heed of their warnings against jumping to over-optimistic or too hasty conclusions while, at the same time, redoubling our efforts to obtain additional and more complete remains in archaeological contexts that can provide the crucial evidence needed for specific identifications. In the predominantly arid environment of northeast Africa, there is good reason to expect that, so far as plant remains are concerned, our efforts will be rewarded by the routine and regular use of flotation and dry sieving methods in all future excavation of early Holocene settlement sites.

There seems to be emerging in northern Africa, in the millennia between 7,000 and 3,000 B. C., evidence of two major prehistoric Culture Areas, and, if we look at the continent as a whole, we can see at least two more — those of West and East Africa. The northernmost lies in the winter rainfall region between c. 20°N latitude and the Mediterranean coast. This is characterised by small blade technologies and the adoption of domestic animals (first small stock and then cattle), little evidence for cultivation except in the Mediterranean coastal region, and an emphasis on hunting and, probably, on a very mobile social organization. In the inland region of the Maghreb and the northern Sahara this cultural entity is characterised by the Neolithic of Capsian Tradition and in Upper Egypt and Nubia by several comparable and local small blade industries.

The Culture Area immediately south of this occupied the summer rainfall region south of the 20°N line and stretching south to the West African Sahel. Since the northern boundary of summer rains extended further north in the early Holocene than it does today, this boundary in Egypt was probably around the latitude of Aswan and it is likely that these northern parts of the central Sahara received rain in both the summer and winter months. If the number of sites in the desert is any indication, this area must have been a highly favourable environment for humans and animals at this time. The many lakes and streams that now filled the basins and depressions in the central and southern Sahara provided an abundance of localized water sources that contrasts with the generally drier habitats of the Neolithic populations in the northern Sahara and produced a different kind of economic adaptation that made extensive use of aquatic resources wherever these existed. Was this hunting/fishing/collecting economy one that developed independently of those in the north and was it the use of water resources (fish in particular) that was the crucial factor leading on to domestication there in the late seventh millennium B. C.? If so, did domestication spread northwest into Upper Egypt and the Delta as the few dates we have at present suggest might have happened? Perhaps; but maybe new findings will decide otherwise and the new work now beginning on the Predynastic settlements in Egypt could turn up evidence for farming as early as any to the south and shed light on the extent to which external influence and/or migration may have had a hand in introducing agriculture to the Nile. With all the new work going on

and projected in the desert and along the Nile we should, in four years' time, be nearer to answering some of these questions that we have no means of answering today.

Since cattle and small stock in East Africa had to come from the north, clearly the chronology for domestication along the Upper Nile and in Ethiopia is the key to the chronology of domestication in the tropical highlands and the northern desert of Kenya. At present, 8 - 7,000 years B. P. seems rather too early a date for the introduction of cattle to East Africa; 4,000 years B. P. presents no problem; but we need to find and identify the people who brought the stock into East Africa and the Horn. Was this a matter of migration or of stimulus diffusion? The concept of the "advancing frontier" provides a useful framework within which to examine the evidence and the knowledge coming from ethnography, which indicates that we can expect to find different peoples and different economies juxtaposed in the same region of East Africa, helps to confirm the mosaic nature of the prehistoric pattern that is emerging.

Impressive also are the population studies on the human skeletal material we have heard about and the way pathology and age studies of human bone can provide clues to nutrition and so to changes in the staples used from the balanced diet of hunter-gatherers to the more unbalanced one sometimes found with food producers. The great abundance of skeletal material provides a tremendous opportunity to learn more about nutrition when examined in conjunction with the understanding of the cultural changes that the archaeologists can provide and one should urge that further studies of this kind be undertaken in the future. We as prehistorians have a duty to show the physical anthropologists the nature of the food base and diet the results of which are manifest in the skeletal remains they are studying.

What of the future? Where have the proceedings of this conference suggested that we might focus some of our energies? To mention a few — a filling in of the "two thousand year gap" in the Sudan; continuing and developing research on the Predynastic settlements and the demographic patterning in the Egyptian Nile; concentrating more on learning what the settlements were like and the technologies involved rather than digging more graves and cemeteries for the pots and other objects they contain; learning more of the patterning and use of valley and desert resources through the use of ethno-botany and animal behaviour studies. Again, tracing the direction of movement between desert and Nile; was this a pattern of lateral movement in and out of the valley to the desert and back, or alternatively was it a movement up and down the Nile or, again, a combination of both? As we have heard, a clue to movement is provided by the distribution of raw material sources and this is likely to be one of the best ways in which we might determine territorial ranges and, when analysis of style in lithic and ceramic artifacts is included, in which we might identify ethnic boundaries. Next, development of spatial analysis studies is clearly essential to show prehistoric patterns of land use. We need to be more carefully selective of sites for excavation for what they can yield and it is clear that substantial parts of the site will need to be excavated to show the plan of the settlement,

house plans, and social patterning, to distinguish different activity areas and to determine from faunal and plant analysis the economic base. Developing taphonomy studies identifying the different agencies and phenomena that have been at work in accumulating and dispersing the archaeological residues is another way in which we can greatly expand information about a site. We should pay just as much attention to the areas of low density remains as to those of high concentrations since only in this way can we hope to understand the overall pattern of a camp or settlement and its significance within the whole exploitation system.

Experimental archaeology along the lines of that described for the Predynastic knife can also make a significant contribution. That is to say experimentation in the manufacture of artifacts, in understanding the reduction process and the range of artifacts that result; experiment also in the way artifacts were hafted and used. This leads on to edge-wear studies, of course, and this is, I believe, one of the most significant breakthroughs of the 20th century. Now we can begin to know something about the functions of the stone tool equipment. But it will not be easy and, so far as edge-polish studies are concerned, no small amount of experience and skill are essential before identifications will be made in which we can have confidence. With edge-damage studies goes that of conjoining pieces — of stone artifacts in particular. The recently published work on the Terminal Palaeolithic site at Meer (Belgium) shows what exciting new potential for knowing the nature, number, and sequence of activities engaged exists in this kind of study. Also an urgent need for palaeo-environmental evidence is the recovery of cores from lakes and other favourable localities to provide the climatic reconstruction and chronology of the last 10,000 years. The core from Selima Oasis could be most significant here and the crater lake at Jebel Mara is another possibility for obtaining a core to show late Quaternary climate and environment. We need to know what was growing round the settlements that we think contain domestic plants and animals. We need pollen evidence, therefore, and the study of phytoliths could also help not a little.

It has been a very great pleasure to meet and hear from Egyptologists. They have a major contribution to make to understanding the domestication process along the Nile and the part played in this by the desert or by exotic influences from outside the continent. It is of great importance to interest them in our problems and to enlist their support. We prehistorians, in turn, can be of no small help to them in providing an understanding of the cultural substratum antecedent to the Predynastic. Some of the methods developed to help prehistorians — such as thermo-luminescence dating and neutron activation analysis of pottery to determine age and sources — can also be of use to Egyptologists. With the growing interest in the way in which Ancient Egyptians lived, rather than in what happened to them when they died, we should be able to look forward to a new collaboration between Egyptologists and prehistorians in the years ahead.

In conclusion, if the amount of substantial evidence still leaves much to be desired, its very paucity helps us to value and appreciate all the more the contributions

of those to whom we owe that which we do already possess. Not only is this the cornerstone on which our constructs depend but it serves, also, to confirm that the evidence is surely there to be found and provides the incentive to make us go out and find more of it and recognise its worth. Knowledge and understanding are unlikely to come quickly but each new piece of evidence will have its place in helping to reconstruct the Neolithic way of life. Whatever the discipline we represent we are here because of our active research interests in learning more about how food production came to Africa. To me, and I hope to all of you, this is one of the most exciting and challenging problems in African prehistory today and, with the number of new studies projected or already under way, we can be certain of even more eventful discoveries in the next four years as knowledge of palaeoclimates in northern Africa becomes more precise and the new technique of recovery and analysis adopted by the archaeologists are put into more general practice to produce more meaningful reconstructions.

This has been a great conference and the informal and most friendly atmosphere in which our meetings have been conducted has, I think, gone a long way to advancing the work and to informing us on where we stand today in our attempts to trace the pattern of domestication in northeast Africa. We owe this to the foresight, energy and excellent administration of Dr. Lech Krzyżaniak and Dr. Michał Kobusiewicz. We are greatly indebted to them and to their charming and efficient staff of helpers for the conception of the conference and for making it possible for us to be here. Especially do we thank them for all that they have done in making us so welcome and helping forward our deliberations to a clearer understanding of what the Neolithic is all about. We will carry away the most pleasureable memories of the hospitality and kindness that we received during our stay in Poland and will look forward with anticipation to the appearance of the Proceedings that will be the definitive statement and record of the extent of our knowledge and ideas at this time.

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