

## **From the Jomon to Star Carr: Hunter-Gatherers of East and West Temperate Eurasia, Universities of Cambridge and Durham, 4th-8th September 1995**

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What are conferences for? This question must have passed through many a befuddled mind at many such events, attempting, in the middle of a warm afternoon, to assimilate data from illegible overheads and dimly illuminated slides, against a background of muttered asides from those sitting behind, and thoughts of the next coffee break. Of course, the standard answer is that the bits in between the papers are the most important parts of any such meeting - the chance to renew old acquaintances, make new contacts, talk to people that perhaps one has never even heard of. There was certainly plenty of opportunity to do all this at the Jomon to Star Carr meeting, which, as the title suggests, brought together archaeologists working in the Mesolithic (and to a certain extent late Upper Palaeolithic) of (largely) northwest Europe, with those working on Jomon Japan.

A large part of any conference nevertheless consists of the ostensible reason for our attendance - the presented papers themselves. With over 50 papers given over four days (the fifth spent travelling between the two venues via, curiously, Flag Fen and, of course, Star Carr), it would be impossible to provide even a summary of all the contributions, which is why this review wanders into more general themes.

Many of the most interesting potential themes of the conference were raised on the first day, much of which was occupied by the session on 'Society, Symbolism and Religion'. As a speaker myself in the following day's 'Ecology and Subsistence' session, I was initially affronted by the comments of one of the organisers at the end of this session: tomorrow, we were told, we would have (by implication rather boring) case studies of environment and subsistence, and perhaps would return later to the (more interesting) earlier themes. Unfortunately, with a few exceptions, at the end of the second day I found myself agreeing with his comment; but the same could be said for the sessions on 'Territoriality and Landscape', 'Intra-site analysis', 'Technology and Material Culture', and 'Transitions to Agriculture'. For all these themes I felt that many of the pre-circulated abstracts promised more than was offered - with many participants preferring to present only their data and omit any detailed discussion of the more wide-ranging general conclusions hinted at. Since most of us can take in reams of data much more readily when they are in hard copy, perhaps conference participants should be encouraged to present a general summary of their data and then throw their conclusions open for discussion - which, to return to my initial question, should surely be a major function of any conference. A mandatory requirement should be to make the audience think - and perhaps even participate. Straight data undoubtedly fascinates many, but too much of archaeology consists of sloppy and unexplored conclusions, not explicitly related to the data. That a more critical approach to the connections between data and theory is essential to interpretation was a lesson which we were able to pick up from some of the more interesting (my own bias) papers.

Bias itself was one of the themes of many of these papers. Painful as it may be for many of us to acknowledge that the post-processualists have contributed anything useful

to archaeology, it is probably partly because of their heavy emphasis on the need to acknowledge paradigmatic bias that the topic is beginning to receive the attention it deserves outside the postprocessualist sphere. Papers by Fukasawa Yuriko ('The analogical abuse of the hunter-gatherer'), Liliana Janik ('Social differentiation in fisher-gatherer-hunter cemeteries in northeastern Europe'), Chris Meiklejohn and Erik Brinch Petersen ('Paradigm lost: searching for 'complexity' in the Mesolithic'), and Priscilla Renouf ('Band or band-wagon? Hunter-gatherer complexity and the archaeological record') all looked explicitly at the role that preconceived ideas have had in influencing interpretations of past temperate hunter-gatherers. As the titles suggest, the last three of these, and several other papers in passing, also examined the topic of complexity in hunter-gatherer society; the general conclusion being that this was a concept which had been useful in allowing deviation from the !Kung San model for past hunter-gatherers, but had now reached a point where it was similarly constraining further investigation of the topic. Another closely-linked subject which came in for criticism was the tyranny of the ethnographic record, especially that of recent hunter-gatherers - the biasing effects of varying interpretations of the record on archaeological interpretation was emphasised by Fukasawa, Janik and Renouf, as well as by Simon Kaner ('Occupational histories and social relations in Jomon Japan') and myself ('The archaeobotany of hunter-gatherer subsistence'). The potential influence on interpretation of the particular research standpoint of the archaeologist was amusingly made by Fujio Shin'ichiro ('Why did the Yayoi period begin?') who compared his own (Yayoi-oriented) view of the spread of agriculture in Japan with the rather different (Jomon-oriented) one of Takamiya Hiroto ('Beginning of food production on the island of Okinawa') - the latter summoned to the stage to have his 'Jomon' features compared with the 'Yayoi' (or more typical modern Japanese) physical appearance of Fujio. The lessons for Europeans examining the same transition in the West from the alternative standpoints of studies of Mesolithic hunter-gatherers, or Neolithic agriculturalists (and rarely do the two overlap), are obvious.

Other problems with the current complexity model which were touched on by many speakers were the near impossibilities of determining in the archaeological record unambiguous evidence for many of the crucial indicators required, such as sedentism, social differentiation or stratification, and intensification. And to what extent should such components of 'complexity' (definitely the conference's dirty word) be related at all to what have traditionally been considered 'transitional' (another dirty word) stages, particularly that of the adoption of agriculture? The question of whether sedentism is a necessary precursor to, or a disincentive for, the transition to a farming economy was hinted at, though not fully discussed, and remains one of the biggest areas of uncertainty in discussions of this topic (probably because in some places one theory comes close to being the answer, while in others the other does).

The cross-cultural nature of the conference, though perhaps disappointingly little in evidence formally in papers and discussions (perhaps because of linguistic difficulties, maybe even cultural reticence), nevertheless provided considerable food for thought. The value of the Jomon to the west as an area for comparative study was touched on in many (mainly western) papers. At least one Japanese speaker, however, emphasised that the period is neither Mesolithic nor Neolithic, but is the Jomon, and perhaps should not be used as a direct analogy for the often more poorly preserved past of northwest Europe. The biases introduced when depauperate Britons look for help to Denmark (but can't read all the literature), or when Danes look to the Jomon (but can't possibly hope to

follow all the intricacies of debate in the Japanese literature), are something which should be borne closely in mind by those who subscribe closely to such archaeological analogy. The differences in approach and funding between different areas was brought home by our visit to Star Carr - a ploughed field of fast-eroding peat with no definite plans for future large-scale excavation (at the time of the conference). In contrast, we saw pictures from Japan of a 16km stretch of motorway being excavated in full. From eastern Europe there were tales of the impossibility of affording more than the bare minimum of radiocarbon dates. I wondered to what extent cultural differences (I have already hinted at the apparent reticence to take part in discussions on the part of at least the more junior Japanese participants) had an effect on regional interpretations - it was hard *not* to subscribe to the stereotypical view of the Japanese when they (almost) all sported smart dark-grey suits (or the (rare) female equivalent), and, incredibly, snapped away with noisy power-driven cameras while their colleagues were presenting papers. The Norwegians were almost universally young(ish) and be-jeaned; while the Danes sported beards or moustaches and woolly jumpers. The Brits were more diverse, but perhaps it was just that there were enough of us to allow age, sex and social status-related patterns to emerge.

So, were any useful conclusions reached? At one point during the debate on complexity, when semantics raised its head and debate touched on the problem that many of the arguments stood or fell depending on one's definition of terms, Erik Brinch Petersen suggested that we were all little more than a bunch of so-called scientists walking around in circles; and there were times when many must have agreed with this analysis. But it is possible to tease from the tangle of ideas generated by a conference of this scale some ideas for forward movement: the need to deconstruct (or at least re-name, to enable a fresh approach) monolithic concepts such as that of complexity (and perhaps also 'Mesolithic' or 'Jomon'), in order to look at individual components of systems, and not to assume that other components are *necessarily* related; the need to begin looking for variability in adaptations within the broader archaeological patterning; the need to consider the environmental, social, cultural and historical contexts of ethnographic examples used as analogies; the need to think about temporal changes as processes of adaptation rather than transitions on some kind of evolutionary trajectory; and the need to look at precise chronological sequences in closely defined areas. All this, of course, does little more than introduce a new paradigm, of which there are aspects which are perhaps as dangerous to 'objective' analysis as any of the previous one's - the frequent reference to the need to examine 'adaptation/s' in particular, begging questions which were raised in biology in the 1970s, and the anti-'evolutionary trajectories' stance perhaps blinkering research from attempting to clearly distinguish between valid explanations for 'progress' (building on previous successes or failures) or its absence, and explanations which are purely teleological.

One of the most frequently stated elements of what I am taking to be the 'new paradigm' was the importance of not reading too much into the presence in archaeological contexts of the remains of domesticated animals or plants. Whereas previously such a presence has immediately been hailed as the origin of 'agriculture' in any one area, most speakers who touched on this subject were at pains to stress that this probably represented an *addition* to a subsistence system already in place, with many suggesting that for considerable periods of time a low level of domesticates were incorporated within a system in which wild plants or animals persisted and often dominated. Asle Bruen Olsen ('Sedentism and agricultural practice in the Late Stone Age hunter-fisher society of

western Norway') even suggested coining the term 'Nesolithic' to cover such examples. Liliana Janik and Martin Jones discussed this idea, and some of the possible ecological underpinnings for such a persistence, in detail in their paper on 'Foodwebs and modes of production in fisher-hunter-gatherer societies'. My own doubts about this element of the new paradigm rest not so much on its unlikelihood (like many other elements it falls somewhere between the extremes of previous paradigms and is therefore probably a step in the right direction), but on the paucity of evidence, certainly from the archaeobotanical side, for the likely proportions of food that might have been derived from either wild or domestic sources. The real trouble with all our models, as Priscilla Renouf noted in her discussion on North American hunter-gatherers, is that the data are often not good enough to tell whether models are correct or not. In that case, maybe I'm wrong, perhaps conferences should dispense with theory and other confusing epistemological concerns altogether, and just stick to presenting data after all, until there's enough of it to say something useful about ...

Whatever the case may be, the organisers should be congratulated for putting together such a stimulating meeting, and the proceedings that arise from this conference, expected to appear in mid-1997, will undoubtedly provide a rich source of data and ideas on recent developments in temperate Eurasian hunter-gatherer research.