Reviews

G. de G. SIEVEKING and M.B. HART (eds.): *The Scientific study of flint and chert. Proceedings of the fourth international flintsymposium held at Brighton Polytechnic 10–15 April 1983.* Cambridge University Press 1986, 290 pp., 208 ill.

The International Flint Symposium, a long established institution soon celebrating it's 20th anniversary, held it's fifth meeting at Bordeaux last year. The concept of the Symposium has for at least a decade manifested the rapidly growing interest for research across the boundaries of several branches of natural science and archaeology. It has as well been a catalyst for the development of new disciplines working in the ecotone between sciences, as the editors rightly states. The meetings, held about every 4th year since the first in Maastricht 1969, have evidently broadened the scientific base of the institution. The actual book includes, as well as papers by geologists, a number of technical and analytical papers from physicists, geochemists, material scientists and engineers.

The results of the Brighton Symposium are published in two volumes. The papers commented in this review published in *The Scientific Study of Flint and Chert* are, or derive from, those presented at the scientific seminars of the symposium. The companion volume *The Human Uses of Flint and Chert* is planned to follow.

The Scientific Study of Flint and Chert includes no less than 32 papers covering a wide range of comprehensive subjects. This might have been the natural reason not to divide the many papers into groups or sections in the book. The papers are of course well organized and numbered continously throughout the book, but a framework would have been a benefit to the reader. The introductonary paper by *Schmid* on the relevance of flint-stratigraphy to archaeology is an intended key for the nonspecialist to the reading of the socalled lithostratigraphical papers. The review of the following 31 papers is, due to practical reasons, approximately divided into six sections.

The stratigraphical provenance and geological distribution of flint (papers 2–3):
Both papers are geological "case studies" by respectively *Mortimore* and *Wood* and crops at Brandon, Suffolk. This flintmaximum is correlated from the south coast of England up to Yorkshire over a distance nearly the length of Jutland. The flint shifts from giant nodular in the south to continous tabulars in the north. The authors present evidence that the flintmaximum is essentially the same contemporaneous formation.

Processes in relation to the formation of flint and fabric in the chalk (papers 4–11):
The origin of flint is the subject of the paper by *Clayton* on the chemical environment in the Upper Cretaceous chalks. The presented geochemical approach is not new in this field. It has long been known that the formation of flint was caused by organic factors which led to concretion some depth below the floor of the cretaceous sea. Clayton's recent theory comprises that an early diagenetic reduction of sulphate ions to produce hydrogen sulphide followed by the oxidation of the hydrogen sulphide produced, at the interface of reducing and oxidising conditions, crates ideal conditions for the dissolution of calcium carbonate and the precipitation of silica. Voila! – *Kolodny* reflects on the origin of levantine flint based on new isotopic evidence whereas *Williams* describes the cherts of the upper Greensand of Devon. A comprehensive, thoroughly documented study on a classical subject is presented by *Bromley and Ekdale*, demonstrating an up-to-date investigation of flint and fabric in the northwest European chalk. *Felder* has delivered a very short, descriptive paper on rythms, flints and mesofossils in the Maastrichtian Cretaceous in the Netherlands. *Hart, Bailey, Sweiciky and Lakey* are exploring a small, but extremely informative world of microfossils, namely the finds from the fine "chalky" powder often present within cavities in the interior of chalk flints. This study also provides information on the process of flint formation. And besides is illustrated with an excellent "montage" of SEM-photos. *Curry* has investigated the content of foraminiferids in flint pebbles originating from decayed chalk whereas *Glasser and Smith* ask: How did silicious coatings arise on fossil cocoliths? Part of the answer comes from the application of scanning electron microscopy, novel in this field.

Rawmaterial studies in the archaeological perspective (papers 12–14):
The study of flintsources, raw flint and its diffusion through the human network in the past, is an old research tradition within stone age archaeology and traditional etnography. Since the last century interest has been directed into the tracing of flint quarries and regular mines, especially with reference to evidence of "trade" and exchange systems. Again this study is in full bloom. Apropos *Larick* has investigated the types and sources of flint and chert in Dordogne and compared his observations with the scatter of flint types in the palaeolithic strata of the stations in the region. The archaeological application of Larick's work is published in the second volume of the Brighton Symposium. Larick's work is very promising and the same time very problematic. Modern mapping of raw-flint-resources is linked to modern quarries and road-cuts as

well as to accessible farmland. It is still difficult to estimate what was accessible during the upper-palaeolithic. A welcome east European contribution comes from *Takágs-Biró* on raw materials used for the manufacture of stone artefacts in Hungary. The first systematical survey. The use of geochemistry for raw-material-tracing of flint axes by *Bush and Sieveking* is as classical a theme as petrography itself. The subject is of great importance also to Scandinavian scholars, as the neolithic raw-material supply-system here is somehow a pendant to the situation in England, with the exception of the long flint-rich Danish littorina coasts. The hopefully fruitful work carried out nonethe-less faces methodological problems, some clearly pointed out by the authors: "The use of the geochemical technique for determining the provenance of flint axes is based on a number of assumptions some of which can be tested and others accepted" (p. 137). The three basic assumptions are 1) within one mine site the material is reasonably homogeneous geochemically, 2) regional variations occur, and 3) most of the flint axes are derived from a restricted number of mine sites and not from a multiplicity of local sources.

Geological "case studies" (papers 15–21):
These investigations from specific areas in England and Sweden include the study of cretaceous sediments and flint in secondary deposits. The only exception being a paper by *Lidmar – Bergström* on flint and pre-Quarternary geomorphology in south Sweden and south-west England. We are informed that primarily Scanian Cretaceous sediments indicate at least three trangressions from Early Cretaceous to the Turonian. Early Maastrichtian chalk is the source of the well known (local) Kristiansstad-flint. *Gibbard* has studied the flint pebbles in the Quarternary gravels of south-east England. *Catt* has investigated the nature, origin and geomorphological significance of clay-with-flints. Beside the evident geological aspects, clays-with-flint have been a rather rich resource for prehistoric man because of their great range of flint types. *Williams* contributes with a comprehensive study of the periglacial phenomena in the South Downs, drawing our attention to the many factors which have affected the location of flints. *Waton's* preliminary paper deals with the palynological evidence for woodland on the chalk of central Hampshire. *Ellis* reports on the postglacial molluscan succession of the South Downs dry valley. Ireland comes into focus with the paper by *Briggs*. From a middle Scandinavian viewpoint the author deals with an interesting field; transported flints in areas marginal to the chalk outcrops. Concerning this recycled flint, the author brings along one observation, important for rawmaterial classification: "The hard nature of this flint is due to a process of natural selection within the ice, flawed or cracked nodules being broken down to their smallest indestructible particle, both thermally and by percussion. This ensured survival of none but the most stable nodules".

Qualitative studies of flint, quartz, and obsidian (papers 22–29):
These technical and analytical investigations, expressions of frontier research, apply a number of new techniques and terms quite new to archaeologists like for instance nuclear magnetic resonance and plasma spectrometry. *Krinsley and Trusty* have dealt with sand grain surface textures using scanning electron miscroscopy. The method, controlled by laboratory simulation, must be judged as an important tool in the environmental reconstruction and even gives clues to the (quite exotic) interpretation of the socalled fine particles on Mars (p. 205). *Linde* has worked in the same field with specific interest in quartz, flint and obsidian grains in experimental glacial, water or wind transportation. The paper by *Bull* is purely procedural and is also dealing with environmental reconstruction by the study of sand grains using SEM. As a small curiosity he believes possible not only to detect but also to compare the grain surface textures produced by experimental grinding to those found in glacial-ground sediments. *Shalley and Marshall* are with the help of SEM focussing on the simulation of aeolian quartz grain surface textures. With respect to environmental geology this refined identification is most promising to future studies of archaeological sites. *Whalley and Orford* deliver new practical methods with the aid of microcomputers – analysing and quantifying two-dimensional images. Plasma spectrometry as a method of flint-source determination is introduced by *Thompson, Bush, and Ferguson*. The method appears promising being both accurate and efficient! It seems much more difficult to evaluate the potential in the study by *Symons* on the application of Magnetic Resonance in the investigation of cherts. *Griffiths, Seeley, and Symons* report on electron spin resonance (ESR) signals in chert. ESR is a refined technique for determining past heating temperatures. The last paper in this round by *Ögelmann* deals with thermoluminiscence dating. TL dating of burned flint is a developing technique. More accuracy can be expected in the near future.

The impact of frost on flint (papers 31–32):
These two studies have an evident value for future environmental interpretation naturally with special regards to palaeoclimatic factors. *Lautridou, Letavernier, Lindé, Etlicher, and Ozouf* have studied porosity and frost suspectibility of flints and chalk based on laboraory experiments and field investigations. Finally *Sieveking* together with *Clayton* has investigated the question of frost shatter and its effects on flint microstructure.

As mentioned, the volume covers empirically and methodologically a subject, which regarding techniques is extremely varied. This of course influences it as a book, some of the papers could as well have appeared in specialised periodicals. Anyway, it also demonstrates the effect of what could be described as the continuous conceptual undulations of archaeology. The growing interest in technical research in archaeology must be seen in contrast to two decades of (necessary) hyper-theory. Technical studies such as these, which are also now expanding within the important field of experimental archaeology, have acquired considerable importance and it is hoped they have come to stay.

The papers are altogether an indispensible contribution to the ongoing clarification of the many "maybe's" in the archaeological study of flint. Besides, thanks are due to both editor and publisher for the solidly equipped and well printed book. It will be used.

Bo Madsen

KARL-ERNST BEHRE (ed.): *Anthropogenic Indicators in Pollen Diagrams.* A. A. Balkema, Rotterdam 1986, 232 pp., 22 foldout diagrams.

It was Johannes Iversen, in his paper from 1941, Landnam i Danmark's Stenalder, who was the first to show that man was responsible for the vegetational changes which can be detected in pollen diagrams at the beginning of the Sub-Boreal period. In 1981 Behre published an article concerning which pollen types can be used as indicators for various kinds of cultural landscape in the past. In 1982 a working group was formed with the aim of further improving these criteria and they presented their results at a symposium in Wilhelmshaven in 1985. This book contains 17 papers from the symposium, 12 in English and 5 in German.

The papers cover research carried out in Scandinavia, Germany, Poland, Czechoslovakia, Holland and England and are concerned exclusively with the problems of detecting and demonstrating agriculture. The much weaker, but still fascinating interactions, between mesolithic hunter/gatherer populations and the vegetation do not get a mention. The book is clearly intended for pollen analysts and I will therefore try summarise and comment upon some of the aims and results in the articles which will be of interest to archaeologists interested in interpreting pollen diagrams for themselves. All the radiocarbon dates given are uncalibrated.

Four papers are concerned with Northern Scandinavia. K. D. Vorren claims to be able to recognise an early phase of human activity in northern Norway beginning as early as 5500 BP, whilst the archaeological evidence clearly demonstrates barley cultivation and pastoralism at around 4200 B.P. Vorren presents a list of the few often rather uncertain indicators in this, in agricultural terms, marginal area.

P. E. Kaland's research is concerned with the origin of heath in western Norway. It was around 4300 B.P., or perhaps earlier, that the first woodland clearances took place and from that time onwards the heath which replaced the woodland was burnt on a regular basis and used for grazing. This was the case for the whole of the heath area which at one time extended along Western Europe's coast (including western Jutland). Kaland rejects the idea that the heath has a climatic origin.

I. Vuorela's work shows that slash and burn started in southern Finland around 4000 B.P., but that even later, when much larger areas were affected, the evidence for this in the pollen diagrams is very weak due to the poor pollen dispersal (filtering) from the clearances to the lakes and mires from which the pollen samples were taken.

Y. Vasari and K. Vaananen describe another technique used in Finland during recent times, whereby the the water level in mires is raised during the summer by means of dams. The aim is to promote the growth of rushes and grasses for harvesting as hay. This technique could quite easily have been used here in Denmark in the past.

B. Aaby's paper 'Trees as anthropogenic indicators in regional pollen diagrams from eastern Denmark' is the only Danish contribution in the book and therefore demands a more detailed treatment. Aaby discusses pollen diagrams From Holmegårds Mose (South Zealand), Fuglsø Mose (Djursland) and Bundsø (Als). The first two are raised bogs and as such can be used to produce radiocarbon-dated pollen diagrams; something which is all-important in studying vegetational history.

Pollen analytical investigations in recent years (Andersen et al. 1983) have shown that subsequent to the introduction of agriculture, the development of the vegetation is very different from one part of the country to another. The time is past when Denmark's vegetational history could be presented with one diagram from eastern Denmark and one from western Jutland.

The so-called elm decline occupies a significant part of the discussion in Aaby's paper. At Holmegårds Mose there is an elm decline at about 4800 B.P. and agricultural activity is apparent in the centuries which follow until a near total regeneration of the woodland takes place around 4200 B.P. Strikingly, there is a second elm decline at 3100 B.P., something which is unique to Holmegårds Mose. There may be some reason to doubt the dating of the first elm decline to 4800 B.P., which is 200–300 years later than in the rest of Northern Europe. It may be that raised-bog peat can produce anomalous radiocarbon datings. The development of the vegetation in the early Sub-Boreal at Holmegårds Mose corresponds closely to that in St. Åmose, another inland site on Zealand (Troels-Smith, 1960) where new radiocarbon dates place the elm decline around 5100 B.P. whilst the subsequent 'Iversen's Landnam' begins around 4800 B.P. Aaby argues that both elm declines seen at Holmegårds Mose are due to human disturbance of areas with damp soil where elm in particular grows. He discounts the involvement of climatic change, soil deterioration and, in part, elm disease. The latter, particularly in view of its present devastation of the elm population in northern Europe, should probably be ascribed greater significance. Can anything other than a widespread disease explain that: –

1. The elm decline is synchronous across northern Europe.
2. It lasts a very short time (50–100 years).
3. It is preceded by very different vegetational development from one locality to the next.
4. If elm regenerates at all in an area, it is usually several centuries later.
5. It occurs in areas regardless of whether agriculture had been introduced by that time or not (H. J. B. Birks 1986).

I should perhaps also be mentioned that there is a record of Scolytus, the beetle responsible for spreading Dutch Elm Disease, from St. Åmose in deposits dating to immediately prior to the elm decline (Stockmarr, cited in Robinson and Dickson 1988).

Aaby describes the importance of the major tree species as anthropogenic indicators and concentrates in particular on the expansion of beech. Beech reached Denmark around 3100 B.P. but its frequency in a specific area subsequent to this is very much dependant on agricultural activity within that area. It grows on well-drained soils, the same soils that are so well-suited to cultivation.

At Holmegårds Mose, where the accumulation rate of the peat can be determined by means of radiocarbon dating, it has been possible to construct an absolute pollen diagram, which in contrast to a traditional percentage pollen diagram, shows

directly how many pollen grains fall on a square centimetre of bog surface in a year. It appears that the Atlantic primaeval forest with its closed canopy had a low pollen production which increased when the clearances in the forest began and the individual trees received more light. This resulted in the few herb pollen grains, which are indicators for the first tentative agricultural activity, being further under-represented in a normal percentage pollen diagram. It is only when the landscape became completely open that tree pollen production begins to fall again. It is therefore very difficult 'in interpreting a pollen diagram' to evaluate the changing relationship with time between areas of woodland and open land.

H.-J. Beug has investigated the earliest agriculture (around 6400–5900 B.P.) at Luttersee near Göttingen. The very abundant Linear Band Ceramic sites on the European Loess are normally not registered in pollen diagrams. He has found evidence for arable agriculture, but that there is nothing which suggests pastoral activity. As elm rises during the period of human activity, it cannot have been used as a source of leaf hay. It is only later that a 'normal' elm decline appears.

W. Groenman-van Waateringe continues the traditional Dutch activity of investigating buried soils under burial mounds. In order to find out more about the possibilities for grazing animals during the Neolithic, she collected surface pollen samples from nature reserves grazed by domesticated animals and compared them with the vegetation in the area. Cattle and sheep are grazers and they tolerate only small amounts of woody (lignaceous) material in their food. In contrast, red deer and especially roe deer are browsers and they can digest much greater quantities of woody material. When she compared the modern pollen spectra with fossil spectra from under Funnel Beaker Culture burial mounds, she found that the potential grazing available for cattle was very limited. The burial mounds lie in areas of disturbed open woodland on dry sandy soils, where woody heath species made up a large part of the ground cover. In areas which were not disturbed grass was very rare on the dark woodland floor. Grazing must, therefore, have been limited to river valleys and other low-lying areas, although the heather-dominated open woodland was an important area for winter grazing.

It should be added that the Dutch investigations have shown that specific agricultural practices are not linked to particular neolithic cultures (Casparie and Groenman-van Waateringe, 1988) as has been proposed by Troels-Smith for example.

K.-E. Behre and D. Kucan recount the pollen analytical investigations associated with the extensive interdisciplinary research project at Flögeln which lies between the mouths of the Weser and Elbe. This is a low-lying sandy area surrounded by mires. Excavation has produced evidence of occupation from the Neolithic through to the Middle Ages. Pollen diagrams have been constructed from raised bogs at various distances from the settlement and from kettle bogs within the occupied area itself. The first clear occupation of the area is registered in the pollen diagrams around 4400 B.P. This is the advent of the Funnel Beaker Culture and it corresponds to Iversen's Landnam phase in Denmark. There are indicators of both arable and pastoral agriculture. Heather expanded in the area as early as the Neolithic, a phenomenon also known from western Jutland (Andersen et al. 1983). It should be made clear however that Iversen's Landnam phase is not synchronous in its expansion. It has been dated variously from the early Neolithic right up into the Bronze Age and is therefore connected with several cultures.

What is remarkable about this research is how poorly the settlement shows up in the pollen diagrams from outside the occupied and cultivated areas. Just 100 metres from agricultural fields, indicators in the pollen diagram are rare and in another diagram, some 3 km distant, evidence for the presence of a large medieval village is completely absent. All this must be taken into consideration when choosing sites for pollen analysis. Without pollen diagrams from small mires and lakes, it is not possible to gain information about human influence on a local scale.

The best indicator of human occupation and activity is Plantago lanceolata (rib-wort plantain), but Behre and Kucan point out that in many cases its affiliation is with arable fields rather than common and pasture grazed by cattle. Plantago lanceolata is actually a common weed of fallow fields.

E. Lange has analysed pollen samples from a ditch surrounding the Slavic fortifications in northern East Germany. The samples were taken respectively from north, south, east and west of the sites and those samples lying closest to the potential arable land, clearly show the highest values of cereal and Plantago lanceolata pollen. It is also striking how quickly the values of the two indicators fall away with distance from the cultivated areas (100% of tree pollen down to 3% over a distance of 270 metres). It should however be added that no account is taken in the research of the prevailing wind direction. Lange uses the relationship between values of cereals and Plantago lanceolata as a simple indicator for the relationship between arable and pastoral agriculture. Behre (1981) doesn't recommend the use of this and other more complicated indices, in particular because the pollen production and distribution of the various indicator species is unknown.

B. Berglund *et al.* have examined recent pollen spectra from sites in southern Sweden which are exploited in various ways with regard to grazing and harvesting of hay. Pollen analysis was carried out on samples from moss polsters and the surrounding vegetation was recorded at various distances from the sampling point. Various computer-based methods were used in comparing the pollen spectra and the vegetation, but is was not possible to differentiate between the two types of cultural landscape. Additional landscape types must be investigated in future research before it will be possible to use numerical methods to interpret fossil pollen diagrams. Berglund's research also gives the possibility of assessing the pollen productivity of common agricultural indicator species. An important result of the research was to show Plantago lanceolata to be greatly under-represented.

Computer analysis has also been utilised by Judith Turner in order to study the variations in anthropogenic indicators from seven European sites. The analysis do not help directly in the interpretation of the pollen diagrams but they do concentrate attention on the relationships between the occurence of some less common indicators.

The last two articles which will be commented upon here

concern the relationship between pollen analysis and plant macrofossil analysis and their respective potential for giving information about various forms of past land use. U. Willerding lists the range of methodological problems involved. First and foremost there is the difference in the level to which pollen and macrofossils can be identified. Seeds and fruits can normally be identified to species, whereas pollen is more commonly only possible to genus. Broadly speaking, pollen diagrams give an impression of the extent of the occupation, duration and land use, but if we want precise information about which crops were grown or which wild plants were exploited, then it is plant macrofossil analysis to which we must turn. The problems can be illustrated by considering the possibilities for identifying cereal species. Only rye pollen can be identified with certainty whereas the other cereals can be assigned to one of two groups. The first includes some wild grasses, barley and one species of wheat (einkorn), the second includes the other wheat species plus oats. In addition, rye is wind-pollinated and produces large quantities of pollen, whereas the other cereal species are self-pollinating and their pollen is first released on threshing or grinding. Accordingly they only register in pollen diagrams from within the settlement area. On the contrary cereal grains can usually be identified to species.

Behre (1981) presented 30 pollen indicators (identified to species, genus or family) for various forms of land use. According to Willerding, 300 macrofossil species can be determined within these indicator groups. On the other hand, the limitations in the use of plant macrofossils clearly lie in the preservation conditions. Seeds and plant remains can only be preserved under waterlogged conditions or by charring.

Willerding's final point is to outline difficulties in reconstructing former cultivation methods. The use of different types of plough in the past has, for example, resulted in a different weed flora composition from that we know from modern agriculture.

K. Wasilikova has investigated the relationship between charred plant remains from archaeological layers and pollen analysis from lake and peat deposits from a large number of Polish sites. She finds that the frequency of certain macrofossils in many cases act to highlight the corresponding pollen type's value as an anthropogenic indicator. As an example of this she presents an investigation of the charred plant remains from a pit from a settlement dating to the Lenyel Culture, together with pollen and plant macrofossil analyses of contemporary uncarbonised material form an adjacent small lake in a river valley. In this way she is able to separate plants that are closely associated with human presence and plants that appear as a result of natural changes, in water level for example.

In short, pollen analysis and plant macrofossil analysis have both their advantages and disadvantages, but if they can be used together, then it is possible to reach a much better understanding of the part played by human populations in the development of the vegetation and landscape through time.

This important book contains papers covering methodical reviews through to specific investigations. Something which would have been desirable is a chapter which tried to put the individual articles into context, whilst outlining the present state of the art within this research area and pointing the way forward. Apparently no-one dared! [Translated by David Robinson].

Charlie Christensen

REFERENCES

ANDERSEN, S. T., B. AABY and B. V. ODGAARD 1983: Environment and Man. Current Studies in Vegetational History at the Geological Survey of Denmark. *Journ. Danish Archaeology,* 2: 184–196.

BEHRE, K.-E. 1981: The interpretation of anthropogenic indicators in pollen diagrams. *Pollen et Spores,* 23: 225–245.

BIRKS, H. J. B. 1986: Late-Quarternary biotic changes in terrestrial and lacustrine environments, with particular reference to north-west Europe. In: B. E. BERGLUND (ed.), *Handbook of Holocene Palaeoecology and Palaeohydrology:* 3–65. John Wiley & Sons Ltd.

CASPARIE, W. A. & W. GROENMAN-van WAATERINGE 1980: Palynological analyses of Dutch barrows. *Palaeohistoria,* 22: 7–65.

IVERSEN, J. 1941. *Landnam i Danmarks Stenalder.* Danm. Geol. Unders., 2, 66: 1–68.

ROBINSON, D. E. & DICKSON, J. H. 1988: Vegetational history and land use: a radiocarbon dated pollen diagram from Machrie Moor, Arran, Scotland. *New Phytologist,* 109: 223–251.

TROELS-SMITH, J. 1960: *Ivy, Mistletoe and Elm, Climate Indicators-Fodder Plants.* Danm. Geol. Unders., 4, 4: 1–32.

COLIN RENFREW: *Archaeology and Language. The Puzzle of Indo-European Origins.* Jonathan Cape, London, 1987. 346 pp.

This book exemplifies what happens when a systemic theoretical framework of autonomous social change is applied to "solve" the puzzle of Indo-European origins. It illumintates more clearly than previous works by Colin Renfrew not only the the potential, but also the limitations of such an approach.

Colin Renfrew has played a major role in transforming the perception of european prehistory by drawing the methodological and theoretical consequences of the breakdown of the traditional chronological framework triggered by the radio carbon revolution. This happened mainly during the 1960's and was summarized in the now classical book from 1973 "Before Civilization". As a result migrations and diffusion were more or less abandoned from the scene of European prehistory. The locus of change was rather to be sought in internal conditions, whereas information exchange accounted for major cultural innovations, such as the Corded Ware Culture. This approach, however, was not accepted in most of Central Europe, and among those who studied Indo-European language there developed a research tradition which still tried to identify language, ethnicity and migrations in terms of material culture.

In "Archaeology and Language" Colin Renfrew attempts to incorporate this last bastion of more traditional research into his systemic framework of internal forces of change. After having presented the problem in a short history of the study of indo-european origins and language, he presents a number of theoretical models to account for social and language change. Since migrations on a priori grounds are not accepted as having played any major role, except in the initial expansion of agriculture, the final model for the spread of indo-european languages is predictable in terms of the theoretical framework chosen. Indo-european language was linked to the expansion of agriculture and developed its various typological branches in the continued process of expansion and transformation of the Neolithic and Bronze Age societies. The model is a logical outcome of the approach presented, which remains its prime justification, since there is no closer empirical examinations of the complex relationships between social and economic change and changes in material culture and language.

Colin Renfrew has to be congratualated, however, not so much for presenting a convincing case, but for insisting on understanding language in terms of its social environment, and by raising the issue of the relationship between the two. Personally I have serious doubts as to the possibility of tracing language change on archaeological grounds, and I have very mixed feelings about the potential ideological uses and misuses of such studies, although Renfrew clearly distance himself from such positions. Since language after all is an essential part of cultural identity, and also of etnicithy, it is indeed important to develop a framework to replace the traditional and simplistic correlations between culture and language. Renfrews study, however, does not itself move far beyond such simplistic models, except that they are of a different, and more sophisticated nature. In one chapter about the Celts, where historical sources makes it possible to talk more firmly about some of these problems, he tries to expose in more detail the relationships between etnicity and language, but even here the examination remains at a rather general level.

Since the book has already been extensively reviewed, I shall rather point out what I consider to be the significant, and implicit message of Colin Renfrews study. It demonstrates more clearly than before the limitations of an internal social framework of change, and of a simplistic evolutionary perception of prehistory. It rests on the basic assumption that with the exception of the spread of agriculture there were no migrations until European societies reached a level of social organisation that allowed conquest migrations to take place. Since we have ample written evidence of migrations from the Celtic period onwards, this level of archaic state formation was not reached until that time. The history of european social evolution is seen as progressing from stable tribal societies to more turbulent archaic state formations in a rather linear fashion, the Iron Age establishing an evolutionary fixpoint.

It is indeed remarkable that a theoretical archaeologist like Colin Renfrew, a firmly believer in the priority of internal forces of change, uncritically accepts that the appearence of the first written accounts from classical civilizations about European societies marks a significant evolutionary change within these societies, rather than within the societies producing the texts. And it is indeed remarkable that these accounts are taken on evolutionary face value to such a degree, that Renfrew excludes himself from raising the question if such a level of social organisation could not have been reached at a much earlier time in European prehistory. The fact that migrations flourish as soon as we have written evidence, and continue to do so, should lead not only Colin Renfrew, but a whole generation of British archaeologists, to ask themselves if this might not be a rather normal state of affairs, also before the time when written records give testimony to them.

What I suggest then, is that it is not possible to progress any further in understanding the relationship between language, ethnicity and social change before we are able to identitify in a much more convincing way the arthaeological correlates of social change in space. And such an understanding is not possible before we include migrations as well as information exchange in our theoretical and methodological framework. Only by considering all possible mechanisms of change can we choose between them. (It ought to be unnecessary to stress, that I do not propose to explain change by reference to migrations, but rather see migrations as important symptons of certain processes of change, due to internal contradictions, a necessary addition to the model of peaceful change, due to information exchange, which presently dominates).

The fact that Colin Renfrew on *a priori* grounds precludes himself from asking some of the most pressing questions arising from his study, is a logical outcome of remaining faithfull to his own paradigm. This, however, is also the strength of the book, since it exposes the limitations of its underlying theoretical assumptions.

One may ask if there is a deeper logic or ideology behind Colin Renfrew's perception of European prehistory. There is in two important aspects. First, it can be argued that the model of peaceful internal change and information exchange arose as a reaction against the militant migration model, that was politically and historically discredited after two world wars. In much the same way as the migrationist pardigm reflected dominant perceptions of its own time, so does the "welfare" model of Colin Renfrew (and of most of New Archaeology). It corresponds to dominant perceptions of peaceful change through information exchange and international cooperation in the period after World War Two. I suggest that time is now ripe to consider the world from a more balanced perspective of both conflict and harmony, information exchange and migrations.

Colin Renfrew's perception of European prehistory also in other important aspects reflects present ideological trends, especially the programmatic stressing of Europe as an area of independent development from the earliest times. Like Gordon Childe he is a firm believer in the prehistoric roots of European progress and civilization, which ultimately explains industrialization and the european expansion over the globe. We do not owe our identify or our historical abilities to either Asiatic nomads or Near Eastern civilizations, except in a very remote past. But after that historical incident such influences were internally transformed and remained genuinely european. Althouth Renfrew is careful to stress that such processes were the outcome of shared environmental conditions, and shared experiences over the millennia, there is (at page 6) a

moment of emotional outlet that reveals his dedication to a common European ancestry: "These lands have been our lands, and those of our forefathers, for thousand of years longer than is widely thought. Many of the features, then, which define the Irish, or the Spanishness of the Spanish, or the Britishness of the British, go back very much deeper . . . This, I think, is a fundamental change in perspective, and one which carries many interesting implications with it." I should replace "interesting" with "dangerous". Here Renfrew is not far from Kossinna and he would do well in rethinking its implications.

In conclusion I find "Language and Archaeology" an extremely important book in its paradigmatic exposure not only of the limitations of what has been and still is a dominant theoretical framework, but also of its underlying ideology. It will be interesting to see what effects this will have when the battle of Indo-European origins is over.

Kristian Kristiansen

KLAUS RADDATZ: *Der Thorsberger Moorfund. Katalog. Teile von Waffen und Pferdegeschirr, sonstige Fundstücke aus Metall und Glass, Ton- und Holzgefässe, Steingeräte.* Offa-Bücher, Neue Folge Bd. 65 (127 text pages with 50 text figures and 109 full-side plates with drawings and photographs of artefacts.

Klaus Raddatz's work is one of those good books which is not to be read but simply used. It comprises a complete and fully illustrated catalogue of the artefact classes noted in the title, and for each of the 1,111 finds which the catalogue lists there is a thorough description and informative illustrations. References are also given to catalogues and registers in which the material has previously been included together with the key points in archaeological literature where the objects in question are depicted or discussed. The foreword informs us that a further catalogue volume is to be published, covering the "Gürtelteile und Körperschmuck" of the Torsbjerg find, and although these classes were comprehensively discussed in the author's well-illustrated study of 1957 (Offa-Bücher, Bd. 13) it is certainly desirable that they too should be presented in a full catalogue following the same clear lines as this one, for with these catalogues available future research will have easy access to a find-group which is as important for Iron-age studies as it is difficult to approach in the storerooms and exhibitions of the museums in which it is kept. Thus there is good reason to be grateful to the author because he, with a lifelong study of the material finds of the period at his back, has taken upon himself this great and quite altruistic task.

By way of introduction Raddatz informs us that the catalogue originally should only have included the metal artefacts in the Torsbjerg find which represent a soldier's armour and weaponry, but that he was forced to recognize in the meantime that leather and wooden objects which fall into this category must also be included. Just as well, because the extraordinary preservative conditions in Torsbjerg mose have indeed delivered to us quite unique artefacts of organic material. Pottery and wooden vessels too have found a place in the catalogue although there cannot really be shown to be any necessary connection between pottery- and weapon-deposits in the bog, but other important find classes are still kept out of the study. This includes the coins from the site, and textiles and leather objects "which do not belong to the warriors' gear or whose function is indeterminable". It is difficult to see any real logic in this selectivity, and many will search in vain for catalogued information about the costume pieces and leather footwear from Torsbjerg mose.

Something crucial and new is that in preparing these catalogues Raddats has had access to Engelhardt's diaries from the excavations in Torsbjerg in 1858 and 1860 and to the handwritten and fully illustrated archive with which Engelhardt, from 1852 to 1864, spanned the accession list of 'Den Kongelige Samling af nordiske Oldsager i Flensborg'. From here many valuable items of information concerning finds and contexts have been taken which are supplied as verbatim quotations in connection with the descriptions of objects; similarly a majority of the text figures are reproductions of Engelhardt's drawings. Various mysteries in the registration of the finds hitherto can now be cleared up on the basis of the original descriptions, and one must join Raddatz in deploring that they have been made available to research so late. The cause of this is not however as stated in the introduction, that they lay neglected in the archives of the National Museum in Copenhagen "an unerwarteter Stelle – falsch abgelegt". This is a misunderstanding, for the history of the missing archives of the Flensborg collection is more peculiar (M. Ørsnes, Foreword to C. Engelhardt, *Sønderjyske og Fynske Mosefund,* bd. 1, Kbh. 1969; J. Ilkjær and J. Lønstrup, Flensborgsamlingens skæbne, *Hikuin* 1984). Just as Engelhardt got the major part of the Flensborg collection transported to a hiding place on Sjælland in 1864 when an Austro-Prussian army invaded Denmark, so too he took care of the diaries and catalogues which he personally had written through twelve busy years' work. But while the collection of archaeological material was subsequently found and handed over as stipulated in the terms of the peace treaty, the archives unquestionably remained in his possession. In 1935 his daughter delivered a sealed package to the National Museum with the condition that it might first be opened after her death, and when the seal was broken in 1966 it proved to include Engelhardt's surviving writings. With a liberal interpretation of the conditions which were still attached to the use of these the results of Engelhardt's labour could finally be made available for continued research upon the great southern Jutish votive sites. In Volume 1 of this periodical Ilkjær and Lønstrup showed how the evidence of Engelhardt's diary can contribute to a reassessment of the character of the deposits in Torsbjerg mose, and the essence of the diary and catalogues' data is now made immediately accessible in Klaus Raddatz's large, indispensable Torsbjerg catalogue. [Translated by John Hines].

Mogens Ørsnes

KRISTIAN KRISTIANSEN (ed.): *Archaeological Formation Processes. The representativity of archaeological remains from Danish Prehistory.* Nationalmuseet, Copenhagen, 1985. 280 pp.

This book sets out to examine in very considerable detail the post-depositional factors that have affected the archaeological record of Denmark. It does not discuss the actual formation processes of that record, the ancient deposition of material culture, and in this the title of the book is perhaps a little misleading; the sub-title is accurate. The idea behind the book is laudable – it is high time that at least some of the bias in the archaeological record that we work with is examined in depth. There are so many factors that operate in the formation of the record and in its gradual or sudden decay, that every effort should be made to comprehend some of the actions and processes. Of course, it is not enough to state baldly that all our distributions of ancient activities are incomplete, we have known that for over 200 years ever since the first antiquarians began to comment on something more than mere pots and bronzes. So some of the comments in this book are blindingly obvious – we have what we have found, we do not have what we have not found.

Archaeology in recent years has gone into, and perhaps through too fast, the source critical analysis approach, probably because such analysis reacts against two concepts. The first was antiquarian archaeology, the random opportunistic collection of data and the acceptance that they all must mean something, and we could ignore the gaps, inconvenient although they were at times. The second was processual archaeology, where again data were collected, but selectively, and were manipulated by applying rules of behaviour into yielding a picture without gaps. This is perhaps too abrupt a dismissal of both approaches, but the book's contents apply so firmly to both that perhaps an assertion here will in fact make the point. Each of these camps works by its own rules, of what it can do, and it cannot therefore admit that its data base is fundamentally flawed; incomplete it can be, partial even, but basically unreliable and primarily unusable, never. To accept this last would be to render illogical and unintelligible any conclusions or theses advanced by either camp. The obvious example for each is the distribution maps so beloved by archaeologists of whatever persuasion, or generation. How many times have we seen maps, not only devoid of landscape constraints or encouragements, but blithely, blandly and blatently interpreted as if gaps never existed, if indeed gaps there were. That is the problem for maps, if gaps there were.

Source critical analysis is in essence a destructive game. The players arrive, enthusiastic and optimistic. After the game, which they always lose, they are dispirited and deeply pessimistic. They can be no other if they are true believers in the reality of the archaeological record. It is my belief that many archaeologists have taken one look, played one game, and have abandoned their high-flung optimism about rewriting human prehistory, and retreated into the antiquarian pursuit of basic raw data, manipulating these only so far as they can see them; when tempted to pursue the data into the unknown, they fear to step into the dark court. This book may help them to take that step.

Of the contents of the book I will say little except that it is a standard layout, with historical introductions, then discussions of agrarian developments, and examinations of the archaeological data by period or monument type. It might have been instructive to conduct this examination by region, over time, by antiquarian presences or absences, by the social conditions, rather than by artificial archaeological types. Most chapters are illustrated by various diagrams arranged by decades, showing not what happened to the monuments or artifacts in question, but what was going on around and sometimes on them. So if, for example, an artifact type was consistently found through the first half of the 19th century, and not thereafter, there must be some reason why this is so. It could be any combination of several reasons, of course, from removal of the deposits in which the artifacts lay, to changes in the way the deposits were worked, to loss of antiquarian interest, and so on. All of this is quite straight-forward and we have known most of it, conceptually, for a long time. We have not, however, known much of it in detail, in the particular, which is why the book is a catalogue and an encyclopedia, to be consulted at the moment of research into any of the special periods or any of the artifact types.

So far so good, but can we take the process further? We probably can, and if we cannot, a policy of despair will have triumphed over the innocent yearnings of most archaeologists, to understand the past. It is not the aim of the book to advance new theories about how such an approach might develop, but the seeds are planted throughout the book. Individually, each area of archaeological enquiry should be able to devise a set of rules and possibilities, to be applied before a single word is written on the culture-history, or the processes at work, of any particular episode of the past. Predictive archaeology is the name of the game, and one loosely constrained by general concepts, but tightly controlled at places within these concepts by the particular characteristics of the evidence. Just as the individual chapters of the book reflect not only the archaeological evidence, and the cultural milieu in which it was found, but also the preoccupations of the particular author, so too will the assessment of the evidence be influenced by future authors as individuals and as a part of a society going through its own episodic and ill-controlled evolution. The reflection of this is seen throughout the book, in the TRB, in the Bronze Age burial, and in other chapters. Social, political, economic and religious factors were all at work here, unevenly and unequally through Denmark, and it might well have proved instructive, if not constructive, to have asked some authors to develop models of uncertainty levels for their data that were more specific than those presented here. The result would of course be a reverse model of reliability.

This book should not be dismissed because it uses a sledgehammer to crack an old nut. The nut was big enough to deserve it. The book poses old questions once again, but now examines them in depth. If post-depositional factors can be identified in such a way as to make it possible to quantify and define the archaeological record, new opportunities for interpretation will be created. If as a result of this work, archaeologists still feel in their bones that quantification and qualification are not possible no matter how one analyses the factors, then it is back to

opportunistic antiquarianism. Some of course will never admit it, and will go on as blind optimists – a dangerous combination of the two approaches, antiquarian processualism.

J. M. Coles

PETER UCKO: *Academic Freedom and Apartheid. The story of the World Archaeological Congress.* Duckworth 1987. 305 pp. 16 plates. Price in UK: £ 9.95.

This book presents the history of the Southampton World Archaeological Congress in 1986 as seen through the glasses of Peter Ucko. Thus it is a highly biased personal experience, as the author clearly states in the preface, but also a highly interesting and important book, well written too. It gives a fascinating inside view of how unprepared archaeology was to the political realitites of the present. As a testimony of that it provides an important snapshot of some of the major transformations archaeology is undergoing in the 1980's.

The background to that is the explosive global expansion of archaeology, especially within conservation archaeology. The economic motor of archaeology today throughout the world is national legislative and political frameworks for rescue and conservation. Consequently most archaeology today is taking place in political/administrative – that is non-academic – environments. That goes hand in hand with a renewed significance of archaeology as creator of cultural and historical identity for many new nations around the world, and for ethnic groups and indigenous peoples. This latter development, which has taken place within the last 20 years, has many parallels to the expansion and consolidation of archaeology in Europe in the late 19. and early 20th century.

Since however the existing world archaeological organisation, IUPPS, is governed mainly by european archaeologists with little or no insight into these processes, the organisation has been unable to adapt to the changes and new developments. Thus Southampton revealed the clash of contradictions within the world archaeological community – between Europe/North America and Africa/the Third World, between old and new archaeological traditions and between old and new realities of archaeological practice. (As a consequence of that a new archaeological world organisation has now been established, along the old one). Although academic freedom was the issue, a lot more was hidden behind the controversy. Thus when the increasing international pressure against South Africa finally in 1985 reached and interferred with the Southampton conference, it acted rather as a catalyst to set in motion these wider contradictions. This is not to deny the significence of the problem of academic freedom, but the way the various actors handled the problem suggests that there was more to it than ethics and principles.

The problem of archaeological and political practice has undoubtedly come to stay, and we now have to learn to handle it in a conscious and responsible way. It can be regretted that so much personal antagonism had to accomplish the controvery, and the book testifies that all sides are to be blamed for overreacting, rather seeking to establish positions than to compromise. But then, it can be hoped that it sharpened the consciousness among archaeologists about the realities of principles, practice and politics, none of which can stand alone.

Southampton may thus be said to represent the real loss of innocence – the loss of political innocence. Peter Ucko's book has captured some of the drama that this created, which makes it an interesting historical document, and as such also a subject for historical source criticism.

Kristian Kristiansen

Fennoscandia Archaeologica, published by the Archaeological Society of Finland. Editor J.-P. TAAVITSAINEN.

In 1982 a new archaeological periodical, *Fennoscandia Antiqua,* was initiated in Finland. Its first and only volume contained five articles dealing with finds from a Stone Age site in Lapland, Stone Age economy in Finland, a Comb Ware pot ornamented with a human figure, a copper ring obviously found on a Stone Age site, as well as a report on excavations on a hill fort.

In 1984 the Archaeological Society of Finland took over the publication of the periodical, which was renamed *Fennoscandia Archaeologica.* Before looking more closely at the contents it may be appropriate to give some bare facts about this new publication.

So far four volumes have appeared containing 32 articles: 21 articles are in English, eight in German, two in Swedish and one in Finnish, but all of them are furnished with an English abstract. Thus it is the hope of the editors that this series will serve to establish contacts also with scholars abroad. Most of the articles deal with archaeology, but also such topics as geology, palaeobotany, protection, linguistics, numismatics, ethnology/anthropology and arctic seafaring are represented. The bulk of these contributions discusses subjects dealing with Finland, but there are also several papers by Russian scholars about Russian archaeology.

In *Fennoscandia Archaeologica,* volume one 1984, there are articles on Comb Ware pottery with bird motifs in Finland, on some Finnish Bronze Age finds, as well as a survey of swords and daggers from the Finnish Bronze Age, including neutron activation analyses. Five new finds of skis from Finland are published with ^{14}C datings showing that they belong to the late Iron Age. Two contributions deal with palaeobotany, i.e., pollen-analytical records of cereals in Finland and their dating as well as examination of grain from an Iron Age site.

Palaeobotany is also the subject of two papers in volume two (1985). Palaeobotany is a highly developed discipline in Finland, and the Finnish capacity within pollen analyses and the study of prehistoric grain seems imposing, at least viewed from a Danish angle.

The volume in question also contains an article on the history of protection in Finland, as well as an excavation report of a late Iron Age site. The late Iron Age in Karelia is also discussed in a survey of the present state of research. Finally a 12th century coin hoard is reconsidered; the hoard containing

imports from both Karelia, Russia and Scandinavia was found already in the first half of the 19th century.

In volume three (1986) there are articles on clay figurines from Åland, an early Iron Age cairn and a Medieval stone church. There is also an interesting article on the technique used in carving rune inscriptions, from a geologist's point of view. But attention should also be drawn to several articles dealing with "eastern connections", as well as to some contributions by Russian scholars. At least eastern connections is still a sadly neglected field of research within European Prehistory. Here, however, Pyheensilta pottery is seen from a Russian point of view, and the eastern contacts of northern Fennoscandia in the Bronze Age is discussed. A paper treats prehistoric lacustrine pile dwellings in north western Russia whereas another contribution discusses environment and settlement in the same area during the Holocene. The problem of the discovery of Spitsbergen, who did it and when (?), is seen from a Russian angle and it remains to be seen how Scandinavian scholars will react towards the proposals put forward here.

Volume four of *Fennoscandia Archaeologica* (1987) is the last volume which has so far appeared. Again, there are several interesting contributions by Russian scholars, including an article dealing with the new excavations of the Ryurik Gorodishche near Novgorod. This was an important trading center during the 9th-loth centuries A.D.

Among the articles there is one deserving mention of its own. It is a discussion on human expansion into northern Europe, including Finland, after the Ice Age, dealing with archaeological, anthropological, genetical and linguistic evidence. Especially the question of the origin of the Finnish language is interesting. Such an approach may look old fashioned or even far fetched within modern archaeology. Yet, thinking of Colin Renfrew in his new book, *Archaeology and Language: the Puzzle of Indo-European Origins* (1987), it seems that there is now again an interest in combining archaeology and linguistics, and the article in question should be welcomed, since it is certainly also worth considering why such areas as Finland and Esthonia did not become Indo-European.

Finally it is worth drawing attention to an article dealing with A. M. Tallgren in *Fennoscandia Archaeologica* volume two (1985). This distinguished Finnish archaeologist was the editor and contributor to the periodical *Eurasia Septentrionalis Antiqua* which appeared between the two world wars. He did more than any other archaeologist to inform scholars all over Europe about the prehistory of eastern Europe, and thus his life-work may serve as an example to the present periodical. Considering Finland's geographical position this would only be natural. Yet, Finland is not only situated between east and west, but also between the Continent and the Arctic region and there are thus many other archaeological phenomena which might be considered in this new periodical.

Svend Nielsen

Documents d'archéologie française (*DAF*), Nos. 1–6. Editions de la Maison des Sciences de l'Homme. Paris 1985–.

We would like to call attention to this new series, which in every respect meets the standard that can be expected of the best modern archaeological reports. Its aim is to present monographs, theses, and conference reports on themes relating to French archaeology. Also more general topics like archaeological prospecting (as in vol. 3) enter the scope of the series. Quick publication at moderate costs is intended, all of the volumes being paperbound and in quarto. Five regional committees choose the works for publication, backed by the ministries of culture, education, and technical science, as well as the French central research agency, the CNRS. From the series we quote the following titles:

No. 1. Jean Gascó: *Les installations du quotidien.* – Domestic structures of two rock-shelters in Languedoc ranging from the Mesolithic to the Bronze Age.

No. 2. Jaques Lasfargues (ed.): *Architectures de terre et de bois.* – Private estates of the western Roman sphere.

No. 3. Alain Ferdière & Elisabeth Zadora-Rio (eds.): *La prospection archéologique.* – Papers presented at a conference on landscape archaeology in Paris in 1982.

No. 4. Patrice Brun: *La civilisation des Champs d'Urnes.* – A critical study of the Urnfield Culture in the Paris Basin.

No. 5. Jean-Luc Fiches: *Les maisons gallo-romaines d'Ambrussum.* – The excavation and analyses of a Gallo-Roman residence inside the oppidum of Ambrussum.

No. 6. Colette Bémont & Jean-Paul Jacob (eds.): *La terre sigillée gallo-romaine.* – A remarkable systematic presentation, by more than forty archaeologists, of the terra sigillata industries in France (with notes on Swiss and German sites).

At least three of these volumes are concerned with problems of general interest to the European archaeological forum. A more consistent use of summaries in English (only supplied in vol. 3) would make the series even more useful. However, it is to be welcomed as a handy and up-to-date source of information that is very close to the focus of present French archaeology.

P. O. Nielsen