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The structure of British Neolithic society

a response to Clive Ruggles and Gordon Barclay

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1. INTRODUCTION

Background

Clive Ruggles and Gordon Barclay (2000) clearly are not persuaded, first, by the author's arguments for the hierarchical structure of late Neolithic society in Britain or, second, by the evidence he has collected for the genuineness of the long celestial alignments postulated by Alexander Thom. Although their 'reply' was primarily provoked by his article in this journal on the Orkney chambered tomb Maeshowe (MacKie 1997), the main focus of their attack is broader — his book written a quarter of a century ago (1977).

In brief two main arguments were put forward there. The first is that the archaeological evidence for the late Neolithic period could *by itself* be re-interpreted to give a picture of some kind of hierarchical organisation in which a professional priesthood played a prominent part; sites like Durrington Walls and Woodhenge were — in this new scenario — the obvious residences and training centres of this élite. The second argument was based on the work of Alexander Thom (1967; Thom & Thom 1978) whose discoveries (made mainly from exact measurement) about the geometrical and astronomical qualities of the standing stone sites seemed to provide clear evidence for the intellectual capabilities of the priesthood concerned. To this evidence was added that from several fieldwork tests of Thom's ideas carried out at standing stone sites by the author.

This paper is also in two main parts. The first includes some brief comments on Barclay's detailed archaeological arguments against the scenario just outlined but it also makes the point that the author even then was not the only British archaeologist to argue for an hierarchical society in the Neolithic. The second part discusses — again fairly briefly — Ruggles' objections to independent evidence of the author's which appears to support Thom's ideas but also asks one fundamental question: how valid is Ruggles' own resolute 'downgrading' of Thom's work on which, it appears, his unvarying scepticism about this supporting evidence is based? No-one has previously asked this question in a British archaeological journal with the result that his approach seems to have been accepted without question by interested colleagues (Ashmore 1999; 2000).

The author is very grateful to both Barclay and Ruggles for taking the trouble to respond in some detail to his ideas. In a case like this, when one is in a minority among ones colleagues, it is much easier to evolve one's thinking — and to abandon untenable ideas while holding on to those that still seem good — when they are openly challenged in this way. One hopes that the critics will feel the same way about this riposte.

The present approach

It should be obvious from the foregoing that the author does not see any great virtue in using this 'response' to engage only in further arguments about the details of this controversy (although brief comments must be made on the more important pieces of evidence). A much longer essay would be more appropriate for that. It seems more useful to begin by going back to some basic principles of archaeological research. For example both Ruggles and Barclay seem very positive and confident in their refutations of the author's views, but in view of the fragmentary and incomplete nature of the relevant evidence can such confidence really be justified? Another obvious question is — are the protagonists in this dispute taking account of *all* the relevant evidence or are they sometimes tempted to emphasise only that which

supports their respective cases?

2. THE STANDARD ARCHAEOLOGICAL EVIDENCE

The argument from analogy

It can be argued that, when dealing with mute archaeological evidence, there is a limit to what can be directly inferred from it (Mackie 1977: 7–12; Hodder 1982). When seeking clues among this evidence which illuminate the intangibles of vanished prehistoric ways of life — language, belief, social organisation and so on — there are and can be no direct answers; whether one realises it or not one has to draw *analogies* with recent simple societies which have an approximately similar technology and economy, or with the alleged universalities of human behaviour. To go beyond describing past technologies and economies, and the environments in which these flourished, our discipline has to become a branch of anthropology, in the broadest sense of the word, and to use the vast amount of evidence about living, functioning simple societies that anthropology has assembled.

A primary aspect of the whole argument concerns what *kind* of analogy is most appropriate to explain the late Neolithic evidence. In addition to matters of archaeological detail (referred to again below) Barclay objects to the author's view that late Neolithic society in Britain was hierarchical and priest-dominated, and particularly to the analogy he tentatively drew with the Classic Maya civilisation of Central America. Although this is asserted to be essential to the whole argument this is not the case, though the author admits that the Maya analogy was probably stressed too strongly. The book concerned was after all written 24 years ago and to argue then for any kind of hierarchical society was daring; the Maya were invoked simply to show that a technologically Neolithic people did not necessarily have to be simple intellectually.

The obvious fundamental differences — like the absence of writing and of elaborate life-like stone carving in late Neolithic Britain — were stressed (MacKie 1977: 208–11) but might have been overlooked. What should not have been overlooked was the analogy drawn with a priestly caste much closer to hand — the orders of Druids which existed among the pre-Roman Iron Age tribes of Britain and Gaul. Here, it was suggested, was exactly the kind of non-literate¹ priesthood — divided into a number of specialities (including those skilled in astronomy and cosmology) and living in the same area two millennia later — which could fit the Neolithic evidence. Particularly interesting in this context is Caesar's information, first, that the most learned orders were in Britain where the Druids were thought to have originated and, second, that the influence of these 'wise men' was supra-tribal — they were entirely above local loyalties and sometimes acted as arbiters in inter-tribal disputes (MacKie 1977: 226–28; Piggott 1968).

However one must begin with essentials. To read Barclay's comments one would think that no-one except this author had ever suggested that a priesthood may have existed in Britain in the 3rd millennium BC. Yet it was Colin Renfrew, three years before the book in question, who first drew attention to the need to explain Neolithic Britain in terms of an analogy with the kind of societies known to anthropology as chiefdoms (1973: chapter 11). The primary purpose of *Before Civilisation* was of course to re-assess European prehistory in the light of the fact that radiocarbon dating was completely undermining the old view that advanced learning and technology had spread out from the Near Eastern civilisations into Europe; in that context it suddenly seemed reasonable to accept that by late Neolithic times chiefdoms were emerging in southern England and that the new order is likely to have included, not just a mechanism (under a chief) for collecting and re-distributing surpluses of economic goods, but also a class of professional priests with esoteric knowledge.

Although controversial to many archaeologists the basic findings of Alexander Thom, then recently published, seemed to fit well into this scenario and it was difficult to disagree with Renfrew's hint that opposition to it was based upon an essential unwillingness to accept that Neolithic 'barbarians' could have developed anything of the sort. In other words — and following the old-fashioned view — they would still have needed to be taught by more advanced cultures in the Mediterranean in the traditional diffusionist picture of *ex oriente lux*.

Renfrew also took up Geoffrey Wainwright's point that there were good analogies for the huge roundhouses at Durrington Walls in other parts of the

world — notably among the chiefdoms of the agricultural Indian tribes of the south-east USA in the 18th and 19th centuries. For example according to William Bartram's observations, each Cherokee 'town' (really a large village) then had about 200 people, supervised by a council of chiefs at the head of which was the local *mico* or ruler. The two other high officials of the council were the war chief and the chief priest. A large round wooden thatched house dominated the centre of each village and served not only for council meetings but as a storehouse and re-distributive centre. Bartram provides a plan of this structure which is remarkably similar to the large round-houses of the late Neolithic henge monuments (Renfrew 1973: figure 53; Bartram 1980; Hoffman 1996: 294–96).

Some decades later, in the early 1840s, Major E.A. Hitchcock described Tuckeebatchee Town, a similar settlement of the Creek Indians in Florida (Foreman 1930). By this time the square of the 'market' was formed of four rectangular houses but there was a round-house, or 'warm house', behind this containing the sacred fire. (Foreman 1930: figure facing p. 114). Hitchcock also learned from 'Tuckee-batchee Micco', the chief who was in charge of the roundhouse and the sacred implements of worship, something of the way that surpluses of corn and labour were used. There was also an individual called the 'prophet' of Tuckeebatchee who sounds like a chief priest.

Thus a picture can be built up of southern Britain in the 3rd millennium BC in which the local societies had advanced somehow beyond the level of the relatively simple tribal groupings of earlier Neolithic times, which had presumably been organised in basically egalitarian, familistic units without much social or economic specialisation (Service 1968: chapter 4). The new order saw the emergence of larger units, each dominated by a major chief capable of centralising authority, collecting surpluses of food for large public works, or sometimes for warfare, and thus in general encouraging the appearance of specialist occupations like skilled craftsmen, warriors and priests.

Indeed a major feature of chiefdoms according to Ellman Service (1968: chapter 5) was the elaboration of religious and ceremonial activity. In addition to the village shamans of the old tribal culture — mainly concerned with local life-cycle rituals and achieving their positions by personal qualities and reputations — there emerged a professional priesthood occupying a permanent position in society. 'Chieftainship and priesthood in fact seem to arise together as twin forms of authority' and the offices often descend in the same family lines. The emergence of such a priesthood would very probably result in the elaboration of religious ritual and the accumulation of esoteric knowledge. Renfrew pointed out that the unsuspected qualities of British stone circles then being unravelled by A. Thom fitted in well with the belief that these ceremonial sites appeared in later Neolithic times side by side with the new chiefdoms with their professional priesthoods.

It has long been clear to the author that he did not sufficiently acknowledge the debt that his book owed to *Before Civilisation*. He thought at the time that he had worked out the new picture of late Neolithic society as the text of his own work was being written; indeed he articulated this — and particularly the idea that Durrington Walls was an inhabited sacred site — earlier at a conference in 1975 (MacKie 1976). However Geoffery Wainwright had already written his excavation report on Durrington Walls in which the large roundhouses there were compared with those of the 18th century Creek Indian chiefdoms (Wainwright & Longworth 1971: 232–3) and Renfrew's book had constructed a broader scenario (1973: chapter 11). The author's book argued for a greater role for a more sophisticated priesthood but probably pushed the analogy with the Maya too far. At that time he had not really taken on board that professional priesthoods were commonly associated with chiefdoms and was still too much in thrall to the idea that some kind of primitive 'science'² was practised in Neolithic Britain.

Yet one must ask whether the quarrel which Barclay, and those who think like him, has with that book is not just with the Maya analogy but with the whole concept of a radically new form of society emerging in late Neolithic Britain as first postulated by Renfrew? Indeed does this school of thought even accept that, fully to understand what was going on five thousand years ago, analogies with recent simple societies have to be drawn? For enlightenment we must look briefly at some of the specific points Barclay has made.

Archaeological evidence for chiefdoms

The scale of the building projects.

The evidence for the emergence of chiefdoms in the third millennium BC is very varied and is quite independent of Alexander Thom's inferences. Indeed a much lower level of skill in sky watching — such as that postulated by Ruggles (below) — could fit the scenario just as well. Thus it is important to distinguish the various sub-hypotheses which form parts of the general ideas being debated here.

The first and most incontrovertible evidence that something had changed in late Neolithic society — at least in southern England and in Orkney — comes from the sheer size of the new monuments built then and the much greater manpower that must have been employed to build them. This is an old idea first quantified by Richard Atkinson (1974) in relation to Durrington Walls Avebury, Silbury Hill and other sites and summarised by the author (MacKie 1977: 136 ff). Atkinson drew a vivid comparison with the much lesser numbers of man hours required to build the earlier monuments like long barrows and causewayed camps, commenting that, in terms of the likely resources of Neolithic society, buildings like Silbury Hill and Avebury required an effort comparable to the then ongoing Apollo space programme in the 20th century USA.

Something similar, though not on quite such a vast scale, evidently happened in Orkney at about the same time and the author alluded to it recently (1997: 338–40). Renfrew conducted excavations on several Orcadian Neolithic sites in the 1970s specifically to investigate this problem and set out the case clearly, suggesting that again a more centralised authority emerged in late Neolithic times and organised the building of the larger monuments which were set up then (1979). The man hours involved in constructing some of these had already been estimated by Ralston (1979). Richards has offered a more speculative explanation of the motivation behind all this building activity (1996).

Thus it seems improbable that Barclay's objections can relate to the *genera. idea* of the rise of chiefdoms in at least two parts of the British Isles in late Neolithic times (though he never refers to the concept) but rather to some of the detailed evidence which can be interpreted as supporting this idea, and more particularly to that supporting the idea of a learned professional priesthood at that time. A brief examination of some of the specific objections may help.

Inhabited ceremonial centres?

Barclay objects strongly to the idea that the great timber circles at Durrington Walls (and by inference at neighbouring Woodenge and also at Moun Pleasant and Marden) could ever have been roofed roundhouses. If they were not then of course one major prop for the author's hypothesis — that we can identify the inhabited 'colleges' and training centres of the priesthood — is knocked away. First we can surely all agree that some, perhaps most, of the great timber circles may have been open air temples. The Sanctuary is an obvious possible case in which wooden circular settings were succeeded by a stone ring; thus interpreting the site as an open air temple throughout its history is not unreasonable (Musson 1971: but see below). The recently discovered post settings inside the unexcavated Stanton Drew stone circle could well be another (David 1998).

The question is however — were they *all* like this? The argument is about the function of such settings in the small number of quite different sites — namely three of the four giant henge monuments in southern England (excluding Avebury), and in the smaller Woodhenge adjacent to one of them (Durrington Walls). Excavation has revealed all of these to be different in one important way — in that unusually large quantities of what could be domestic rubbish were uncovered — namely animal bones, ashes, flints and potsherds. The vast majority of the rest of the excavated Neolithic circular sites have revealed very few finds.

Barclay implies that the author ignored vital evidence about the likely purpose of the Durrington Walls timber circles, supplied by the architect C.R. Musson (1973), but neglects to mention the views of Geoffrey Wainwright, the excavator of the site, who was the first to advance the 'inhabited roundhouse' hypothesis. He of course had to take into account not just the views of his architect adviser but also all the other diverse archaeological evidence he found in and around the 'Southern Circle' (Wainwright & Longworth 1971).

231–34). These views have all been set out again much more recently (Wainwright 1989: chapters 5 & 6) but none of this is referred to by Barclay.

As Barclay notes Musson himself ‘concluded that the patterns of posts at Durrington Walls, Woodhenge and The Sanctuary allowed equally well for unroofed as well as roofed interpretations’ (Ruggles & Barclay 2000: 64); yet he did not claim what Barclay implies — that they *disproved* the roofed interpretation. These conclusions took account only of the post-holes, not of the other archaeological evidence. Likewise later commentators like Barrett — whom Barclay cites as pointing out again the significance of Musson’s conclusions — also concentrated only on the evidence from the size and patterns of the post-holes and took little account of the debris in and around the rings (Barrett 1994: 20–4); his is essentially an exercise in deductive re-interpretation.³ The same applies to Wood who doesn’t even bother to explain why he is being deductive and offers an extensive discussion of these ‘treehenges’ based entirely on the assumption that they are open to the sky. In the section on Durrington Walls he fails to mention Wainwright’s interpretation of a roofed rotunda and ignores the archaeological evidence for that entirely (1996: 361–73).

One potentially important fragment of evidence about possible roofing has been ignored in most discussions about Neolithic timber circles; it was recognised by John Evans in shells of small riverine molluscs which were excavated earlier at The Sanctuary on Overton Hill, close to Avebury (Musson 1971: 371, footnote 2). Although proper stratigraphical information for the shell samples is lacking the presence of three freshwater species — far from the nearest pond or river — is most easily explained by supposing that they travelled in the bundles of reeds brought in to thatch a series of roundhouses constructed at that site. Reed matting is another possibility although even that would surely imply a roofed structure of some kind.

There is no need to pretend that the evidence about whether this sub-group of Neolithic timber circles were inhabited thatched roundhouses or not is unequivocal. It is not. However if one is trying to come to a sensible conclusion it is surely necessary to review *all* the evidence available, and not just the parts which suit one’s case — for example that from the patterns of the post-holes. Analysis of the latter could certainly set limits to the permissible inferences; the key post rings might be too slim to support a thatched building of the dimensions concerned, or the structure might be too large to have been roofed. However if there are no *disproofs* of this kind then other data — like the presence of hearths inside and domestic-looking debris round about — has to be taken into account and may well suggest that roofed structures are the likeliest explanation, as the excavator of Durrington Walls thought.

Sometimes it is useful to stand back and look at the general picture. Durrington Walls is one of the largest potentially inhabited ceremonial sites known (one could surely call it a small town like Tuckeebathee if the entire interior was covered with roundhouses, as it might well have been — Wainwright & Longworth 1971: 234). Moreover it is only 1.8 miles N of Stonehenge which is architecturally the most sophisticated stone circle, with the most complex history, in prehistoric Europe. Is it not likely that the greatest late Neolithic ‘monastery/college’ would have been close to the greatest circular temple of the age?

It may be that scepticism about inhabited Neolithic roundhouses is sometimes based on intellectual inertia. It has been stated for so long in textbooks that only open air circular temples existed in Britain in the third millennium BC that the idea seems like an obvious truth. Very similar Iron Age roundhouses have, by contrast, always been interpreted as inhabited dwellings.⁴

Grooved ware and regional Neolithic cultures

The author readily admits that time may well not have dealt kindly with his idea of an ‘expansion to Orkney’ of a sophisticated Grooved ware culture from southern England (MacKie 1977: chapter 9). That chapter title was a child of its time and, like the unrealistic idea of some kind of ‘science’ being practised in prehistoric times — now gladly abandoned (MacKie 1997: 341, footnote 3) — may well no longer be tenable. The author has no difficulty in abandoning hypotheses which have had their day when the evidence demands it. Doubtless too the radiocarbon dates can no longer bear the exact

synchronisms suggested twenty four years ago, though no-one would surely deny that there is a *broad* chronological correlation between the end of the late Neolithic and the start of the Beaker period throughout the British Isles.

Likewise it cannot be doubted that important regional cultures existed in the Neolithic period and can be defined by different kinds of stone circles and local pottery styles (Ruggles & Barclay 2000: figure 1). No-one has ever been rash enough to claim a nation-wide unity of all aspects of Neolithic archaeology! Indeed the flawed reasoning that has to be emphasised here is that which allows Barclay tacitly to assume that the existence of a clear regional Neolithic culture — for example among the Recumbent stone circles of Aberdeenshire (Ruggles & Barclay 2000: figure 1) — disproves the possibility of a nation wide element. This is logically indefensible.⁵

The point surely is that some national elements do seem to exist across this regional diversity even before we consider the less tangible and more controversial examples — like the ‘megalithic yard’ and the celestial alignments. The idea of the circular sacred site itself (stone circle and the henge monument) is an obvious case in point; sites like Ring of Brodgar and Stenness in Orkney — each a classic stone circle surrounded by a ditch with a causeway — are essentially the same kind of ditched temples as Stonehenge itself and Arbor Low (in Derbyshire) far to the south; Burl’s excellent book provides numerous other examples of structurally similar sites spread over wide areas (1976). It seems strange to have to point out such basic facts, and likewise to have to mention that some kind of mechanism must have existed to produce this widespread ceremonial uniformity.

Certain forms of Grooved ware provide another example of a nation-wide material culture and a new study of this pottery has provided a mass of new information (Cleal & MacSween (ed.) 1999). As Barclay says the distribution of the various styles of this pottery no longer show concentrations in the south and the extreme north as it still suggested in 1977; for example there is now plenty in northern England (Manby 1999: illustration 6.1) and a significant quantity in eastern Scotland (Cowie & MacSween 1999: illustration 5.2). The over simple interpretations put forward on the basis of the evidence of the early 1970s will of course have to be abandoned. However that is not really the point. Some of this pottery can still be interpreted as forming a national tradition, especially the flower-pot shaped vessel with finely grooved ornament originally named the Clacton style (Wainwright & Longworth 1971: 236 & figure 97). This material has been found associated with important ceremonial sites as far apart as the Hebrides, Orkney and southern and eastern England (Cleal & MacSween (ed.) 1999). A new example from Orkney of this type has recently been published; hitherto unrecognised it comes from Skara Brae (Shepherd 2000: figure 12.19), linking that site firmly with the great circular ceremonial centres. Another one came from the Stenness stone circle a few miles away (Ritchie 1976: figure 6, no. 16). There seems to be general agreement that Grooved Ware was important ritually.

In any case there must surely be some explanation for the relative uniformity of the first flat-based pottery in Neolithic times throughout the large areas of the British Isles in which it appears. Even leaving aside the more esoteric evidence for intellectual abilities at the time, if we assume that chiefdoms probably existed at least in Orkney and southern England, is it not perfectly reasonable to suppose that the chiefs and priests of the two areas had at the very least occasional contact with one another and took gifts with them?

Orkney Neolithic houses types

Barclay rejects the author’s view that the ‘Skara Brae’ type settlements in Orkney could be the residences of a priestly élite (Ruggles & Barclay 2000: 63); he writes elsewhere in rather vague terms that these settlements were ‘buildings of complex domestic, and perhaps ceremonial and religious function, constructed and used by a sophisticated society.’ as if this has always been understood by orthodox archaeologists and as if the author was defending some out-of-date view from the 1960s (Barclay 1997: 139).

This is more than a little unfair. It is the ‘orthodox’ view of the Orcadian villages he quotes (e.g. Parker Pearson & Richards 1994; Richards 1991) which has changed dramatically and which has moved, without much acknowledgement, several giant strides towards the author’s 1977 ideas. It has to be remembered that it was not until David Clarke re-excavated the site in 1972–73, and found barley grains associated with an earlier occupation of

similar houses (dated to about 3100 cal BC), that it was even realised that the Skara Brae community consisted of farmers (Clarke 1976). Up to that time the standard view was still that presented by Childe many years earlier (1940: 84–8) and more recently analysed in greater detail by Piggott (1954: 321–36). This was that it was an integrated cluster of houses of primitive herdsmen who lived in squalor under their own midden material.⁶ The author's view as set out in 1977 — that the village was the residence of an élite — seemed far more extreme than it does now.

One counter argument is that the number of known late Neolithic settlements in Orkney is increasing and that, since they are all of the Skara Brae type, they must be the standard dwellings of the population (Ruggles & Barclay 2000: 63). Skara Brae itself is the best preserved such site and all its various architectural features which suggest it was something unusual — such as a main drain under the village and an apparent cook-house and workshop serving the whole community — have already been discussed and need not be repeated (MacKie 1977: 184–203)

Yet the crucial question to ask surely is whether we can be certain that we have identified *any* stone dwellings of the ordinary farming population anywhere in the country at any period from the Neolithic to the Iron Age and beyond. Always worth remembering in this context is the complete absence of such dwellings from the recent archaeological record of the Scottish highlands before the middle of the 18th century. For example first hand accounts of the highland population near Inverness in the 1720s indicate that even the smallest single-storey stone cottages were occupied by the clan gentry. Ordinary people, even innkeepers, lived in flimsy structures of wood and thatch which would leave hardly any trace (Burt 1754). Samuel Johnson made similar observations in Skye in the 1770s (1985) and the author has discussed all of this in the context of the modernisation during the 18th century of single-storey cottages in northern Argyllshire (MacKie 1997b: 263–65). It is also the case that explanatory hypotheses are not necessarily the best available just because they have been unquestioned for decades. An example is the belief that the stone 'dressers' in the Skara Brae huts are imitations of wooden furniture. This is not proof that the buildings were domestic but derives from the assumption that they were; after all no such Neolithic wooden furniture has yet been found! Alternatively we could see those huts — each with its opposed pair of what look like single box beds — in a slightly different way, for example as the cells of something like a monastic settlement, each occupied by two priests (or a priest and a novice). In this case the 'dresser' could seem equally plausible as a safe place to keep delicate ritual and surveying equipment, or even as an altar. Some of the other well established traditional interpretations of this site are 'equally lightly anchored to the hard evidence' (MacKie 1977: 184–203).

The Skara Brae form of settlement at Barnhouse, not far from Maeshowe and the Stones of Stenness, was discovered and excavated by Colin Richards during the 1980s, and one of the more remarkable finds was that this site included, in addition to the standard small dwellings, two very large and presumably public buildings (Richards 1992: 1996). This seemed to the author to go a long way towards confirming his diagnosis that these sites were the residences of élite groups of some kind. Barclay however accuses the author of taking no account of the excavator's belief that the larger of the two big buildings belonged to the end of the site's history, or even after its abandonment.

This really is misleading. The other, better preserved large building *was* part of the main village and it looks like a monumental version of the small dwellings, with a massive central hearth. The excavator himself drew attention to the striking resemblance between its plan and that of the nearby Maeshowe chambered tomb and suggested a ceremonial function (Richards 1992). One accepts that the exact type of society which did all this is still not really clear, and that more than one view is possible, but we surely cannot doubt that there was also a strong ceremonial connection with some nearby chambered tombs, as is shown for example in the similarity between the bone pins from Quoyness and those from Skara Brae (Piggott 1954: figure 55).⁷ As noted the discovery of flowerpot-shaped 'Clacton' Grooved ware vessels at Stenness and Skara Brae also reinforces the link between these stone settlements and the stone circles.

In assessing the possible meaning of the new evidence from Neolithic times

that he and others had excavated in Orkney in the 1970s and 1980s Renfrew suggested that in the late Neolithic period Orkney may have been one of several centres in Britain 'of a remarkably powerful body of religious beliefs with accompanying ritual observances', and that Grooved ware may well be evidence of widespread travel — even pilgrimages — connected with these centres (Renfrew (ed.) 1990: 256). Presumably this is the picture that Barclay was supporting in the quotation given earlier, although he seems to have been excluding both the possibility of any 'intellectual' activities and the existence of professional priesthoods (which are never mentioned).

3. THE SKILLS OF THE PRIESTHOOD

Introduction

We have seen that — using only the evidence from 'traditional' archaeological investigation, though helped with the judicious use of analogy — a perfectly good case has long been made out for the chiefdom form of society existing in certain parts of late Neolithic Britain, presumably evolving from earlier in that period. As already described such recent chiefdoms which have been directly studied are usually found to have a professional priesthood of some kind. However what seems to stir up vehement opposition in this context is the attribution to this hypothetical Neolithic priesthood of the kind of arcane intellectual skills suggested by the discoveries of Alexander Thom and it is to this topic — discussed by Clive Ruggles both in this journal and much more extensively, in his book (Ruggles & Barclay: 67 ff.; Ruggles 1999) — that we must now turn.

Space does not permit yet more detailed discussions of the individual sites the author has examined, and sometimes excavated with the aim of testing Thom's ideas, and the diverse results from which have been uniformly rejected by Ruggles. Brief comments on these criticisms will be made later in the hope of showing that the evidence in favour of Thom still stands up. It seems more important to ask why Ruggles — Britain's only Professor of Archaeoastronomy and an experienced field-worker — finds all this data so completely unconvincing. Could it be because the results *are* all equivocal, or because Ruggles is sceptical *a priori*, or both?

In it has long been clear from his own statements that the larger picture of 'prehistoric astronomy' he has constructed from his own work finds no place for the kind of systematically deployed, accurate long celestial alignments claimed by Thom. Is this why the new ones which have been identified by the writer, most recently at Midhowe and Howe in Orkney, seem implausible? As we shall see this does seem to be the case and an important section of the second part of this paper must be devoted to analysing this firmly deductive approach.

It must be fairly well known by now that Alexander Thom claimed that he could detect three different aspects of the intellectual activities of an élite⁸ in his surveys of standing stone sites. The first is the that a standard unit of length was widely used; he believed that his accurate surveys of scores of stone circles revealed that a high proportion had once been exactly circular and that the diameters of these had been measured out with this *megalithic fathom* of 5.44 ft (1.658 m), made up of two *megalithic yards*(MY).

The second is the idea that those stone rings which were not true circles were actually laid out on geometrical principles as ellipses, flattened circles and egg-shapes. It is claimed that this was done using basic field geometry and surveying — including a knowledge of the properties of right-angled triangles — and of course using the MY to establish the dimensions.

The third idea originated long before Alexander Thom but he refined it by undertaking a nation-wide survey of standing stones and stone circles, noting particularly those parts of the local horizon which seem to be pointed at by straight elements of the 'back-sights' — such as pairs of standing stones, stone circles with outlying stones, elements of the assumed geometry of the circles (such as the long axis of an ellipse) and the straight passages of chambered tombs. Of course such straight lines could have come about for many reasons — not least by chance — but Thom argued that, when these lines were projected to the horizon, they peaked so often around celestial latitudes that there must have been a clear intention of systematically marking the rising and setting points of the Sun, the Moon and some bright stars (1967: chapter 8). If only the terrestrial azimuths of these points are plotted sharp peaks rarely appear.

The point about these long alignments is that, if they indicate the Sun, they are theoretically capable of defining the length of the year exactly, and of subdivisions within it, to make an accurate calendar.

Thom also claimed that the sophisticated lunar alignments could help predict eclipses. However the lunar lines are omitted from the present study because there are potentially so many of them that they are difficult to verify individually.⁹ As noted Ruggles has made a special study of these (1981, 1982a & 1983) which the author has yet to consider in detail. By contrast the annual movement of the Sun is much simpler and slower so solar lines should therefore be easier to detect, longer (and hence more accurate) and therefore less controversial. If these turn out to be undoubtedly genuine then clearly they would support the case that the Moon's movements were also tracked.

The essential point to keep in mind is the basic difference between *orientations* and *alignments* already explained (MacKie 1997: 340–1). The former can be built anywhere; no useful horizon marks need be involved, and nothing need be concluded about the interests of builders except that such an orientation was ideologically important. Alignments on the other hand involve selecting the position of a back-sight (a standing stone or stone circle) *specifically in relation* to a distant horizon foresight so that a celestial body will rise or set at that mark on a specific day. Done systematically and on a large scale this surely carries all kinds of implications about the detailed interest of the builders in the sky, in a solar calendar and perhaps also in the complex movements of the Moon.

However it is possible that this mass of Neolithic standing stone sites may still have many astronomically significant lines built into them, but at a much lower level of accuracy — that these may in effect be orientations rather than alignments. In other words the standing stone erectors may have been concerned only with *approximate* dates of celestial events rather than with marking them exactly and accumulating knowledge about them. Such a discovery would not necessarily disprove the existence of a priesthood (Renfrew 1974: figure 53) but it would certainly suggest that, outside the major ceremonial centres, there were probably only village shaman figures involved, concerned with marking the seasonal rituals of agriculture and stock breeding and with ancestral funeral rites at the appropriate times. This is the picture which, broadly, Ruggles believes that his own work supports. By contrast undoubtedly genuine, carefully constructed long alignments — together with the sophisticated surveying and geometrical techniques — must surely be indicating to us the existence of, in addition to and *not* instead of this shaman class, another group of highly skilled astronomer priests which existed on a national basis and which kept some kind of records of their observations. It is this picture which the author believes to be more likely to be correct.

He also believes that statistics alone cannot provide a final answer to the validity or otherwise of these three concepts if the numerical data is itself not conclusive. In that case supplementary evidence is needed, preferably obtained by testing the three ideas in different ways that will reveal tangible evidence. This is what he has been trying to do for three decades now, in relation to the claimed accurate long alignments, and some examples of these 'experiments' are briefly discussed later.

Neolithic metrology

There is no space here for a systematic discussion of Thom's hypotheses about the geometry and measuring system inherent in the layout of the Neolithic stone circles. The most recent thorough re-assessment in an archaeological journal of Thom's own survey data is that of Barnatt and Moir (1984) and their conclusions seem eminently sensible. Their data was confined to genuine stone circles in a reasonably good state of preservation and which were not known to have been extensively reconstructed. Broadly these conclusions are that one sub-set of the data (that collected by Thom up to 1955) gave excellent support to a unit of length of 5.44 ft (2 MY) as did the 21 clearly non-circular sites. On the other hand the 40 well preserved circles surveyed later, and the 26 sites with 3 or 4 erect stones, gave no support at all to any unit of length. In statistical terms this means that the stone circles *as a whole* fail to reveal convincing evidence of a megalithic fathom (or yard). Admittedly it is hard to see why circles measured before 1955 should be different; might the explanation be that a standard measuring rod was used, but only in a proportion of the stone circles? A reason for this

is suggested below.

Likewise a significant proportion of the rings are irregular in plan and are consistent with having been laid out by eye rather than systematically with pegs and rope. Unless one assumes *a priori* that all stone rings were laid out geometrically with rope, pegs and a megalithic fathom rod — and that serious deviations from the ideal are the result of subsequent damage — this is the most economical hypothesis. It seems that nearly half of the systematically laid out circular rings are concentrated in southern and south-western England. Barnatt and Moir point out that, in the far north, only Ring of Brodgar has shown this highly accurate circular layout (with a 1.5% deviation from the true circle) and wonder whether an affinity with southern England is implied (1984: 212).

Barnatt and Moir also suggest this interesting possibility (1984: 212). ‘The distinction drawn between accurately laid out circular sites and those laid out by eye may reflect differences in social structure. The use of a rope and central peg is a simple method of design, but implies a desire to incorporate an accuracy beyond what is visually apparent.’ This observation fits well with the notion that the members of any professional priesthood of late Neolithic times would not necessarily have been found all over the country, that its arcane rituals were often copied by local groups, far from its main centres of influence, which did not really understand them. Such a phenomenon is clearly seen in a late Classic burial at Barton Ramie in Belize in which was a stone axe incised with a crude copy of the ‘Ahau’ glyph of the Maya writing system (Willey 1956, 779). ¹⁰

Cultoan stone circle, Islay

It is clear from the re-analysis of Thom’s evidence for the megalithic yard, and for a systematic geometry to go with it, that there may not be enough well preserved sites to come to an unequivocal statistical conclusion. Thus some independent evidence is needed. The Cultoon stone circle in Islay was excavated by the author in 1974–75 and some unique evidence was uncovered which bears directly on the problem being discussed (MacKie 1977: 92–4; MacKie 1981: 116–28). Barnatt and Moir do not refer to this site.

Cultoan is on open moorland, now peat-covered, but had been set up on the solid old ground surface. Only two stones were standing at the start of the work but the stump of a third was found during excavation; eleven more stones were prone. All these stones formed a distinct oval but the best-fitting ellipse missed many stones by a metre or more. After the peat was cleared 13 empty sockets appeared in the old ground surface which had never held stones, showing that the circle was unfinished. The estimated centres of the 16 reliable positions (the three standing stones and the thirteen sockets) fitted well to an ellipse measuring 40.716m by 35.310 m with an eccentricity of almost exactly one half (0.498). The standard deviation of these positions from this ‘ideal’ perimeter was ± 29.4 cm or 7.2% of the long axis (some of the stone sockets were quite large, more than a metre across; MacKie 1981: figure 3.5).

It might be argued that an elliptical ring of this size could be laid out by eye to this degree of accuracy; a practical experiment would probably decide the matter. However one may doubt that the eye alone would produce the eccentricity of one half; three pegs at the points of a *measured* equilateral triangle together with a rope loop would be needed for this. Of course there is no way of knowing from this one site if a measuring rod was used and, if so, of what length but if such a triangle had sides of 25 units (and the ellipse thus a long axis of 50 units) the unit would have been some 0.814m, or about 15mm shorter than the alleged national standard.

The important point about Cultoon is that, because the stone circle was never finished, its exact plan was only revealed after excavation. The site was in effect a test of the ‘accurate geometry’ hypothesis (and indeed of the long alignment hypothesis — below). Moreover, this discovery means that it is no longer possible to argue that a ruined ring cannot preserve its builders’ intentions sufficiently accurately.

Cup-and-ring rock carvings

Rarely mentioned in this kind of discussion is the fact that Thom studied the patterns of these late Neolithic and early Bronze Age rock carvings, using

numerous rubbings provided by Ronald Morris ¹¹ (Thom & Thom 1978: chapter 5 & references). The carvings of course consist of pecked grooves so the mid-lines of these have to be estimated. However the measured diameters of the concentric rings did indeed fall into distinct groups with peaks about 0.816 ins. (20.73mm) apart. This is almost exactly 1/40th of the megalithic yard and there is a pronounced peak at five of these 'megalithic inches' (103.7 mm) (*Ibid.*: figure 5.1). Moreover Thom thought he could apply some of the geometrical constructions he detected in the stone circles to the non-circular rock carvings.

Davis has undertaken a study of the dimensions of a group of north English rock carvings in order to test this hypothesis independently (1988). No universal unit of length was found which was sufficiently plausible to pass the statistical test applied, although a quantum of 5 MI was clearly detected at several sites, especially among the motifs known as ringed cups (1988: 413). With the same end in view he also examined the Greenland Farm rock carvings just north of the river Clyde in West Dunbartonshire which the author had just cleaned and recorded (MacKie & Davis 1991). Thom had never studied this site. The results here were different and the use of multiples of the MI seemed highly probable in the 74 diameters analysed at this two period site. It may be that, as with the stone circles, a plausible explanation of these differing results is that some rock carvings were initially scratched on the rock by highly skilled members of a learned order while others were geometrically unsophisticated copies; both could have been finalised by equally skilled rock carvers.

Two other finds may also be relevant. The first is a group of five cup marks forming a right-angled triangle above Gourock golf course in Renfrewshire; the centres of the cups can form quite accurately two classic superimposed Pythagorean triangles with sides of 3, 4 and 5 and 6, 8 and 10 MI respectively (Thom & Thom 1978: figure 5.2). The second is a rectangular piece of bone with a square cross-section found in a deposit with Beaker material at Dalmore near Callanish in Lewis; it is marked on adjacent faces with two regular zig-zag patterns, the points of each set meeting at one edge (Ponting 1988: 432 & figure 19.8). The mean distances between the five 'points' of the two sets of zig-zags at the edge are 5.106 + 0.411 mm and 5.102 + 0.541 mm respectively; four of these units are thus almost exactly one MI (20.75mm). ¹²

The large gold lozenge from Bush Barrow

An ideal test for Thom's metrological hypothesis would be a contemporary artefact which bore an accurate design which could be analysed to discover if the craftsman concerned used a unit of length to set it out. The early Bronze Age gold lozenge is mentioned here because it bears what is surely the finest, most delicately and accurately engraved geometrical design known from prehistoric Britain (Taylor 1980; plates 24 & 25: Kinnes *et al.* 1988) the analysis of which should surely provide a test case for the existence of the megalithic inch. Unfortunately Ruggles dismisses this possible aspect of the lozenge without any proper discussion (Ruggles 1999: 139 & figure 8.10).

A claim has already been made that units of 5 MI appear in the design, and also that the design itself could be a small version of a template for experts laying out calendric alignments (A S Thom *et al.* 1988). Precise measurements of the elements of the geometric pattern were taken by A S Thom but are not published as far as the author knows. It seems that North has also taken measurements but the details of these are not published either, although he draws an important distinction between lines which had to be physically measured on the gold during the construction of the geometrical pattern by the engraver and those which did not but appeared automatically because of the design. Among the former 'There are . . . distinct traces of sub-multiples of 1 MY. There are nine compartments to the central rhombus, each itself a rhombus, and each has a side almost exactly one hundredth part of Thom's Megalithic Yard. Furthermore, the shorter sides of the 36 right-angled triangles in the zig-zag all approximate even more closely to exactly two such units.' (North 1996, 511). It is surely time that an impartial group studied the dimensions on the lozenge pattern statistically to find if any unit of length was employed by the master craftsman concerned and, if it was, whether it fits with Thom's system. The Bush barrow lozenge seems at present to be the only high quality direct evidence available for any measuring system employed in Wessex in prehistory. Although it dates from a few centuries after the late Neolithic period its archaeological context surely implies that its owner was a member of the south English élite of his

day.

Neolithic sky watching

Alexander Thom's second major claim was that large scale and systematic observations of the heavens — particularly of the movements of the Sun and the Moon — took place at stone circles and standing stones. The existence of an accurate solar calendar, with the year divided into sixteen parts, is perhaps the least dramatic of the hypotheses which followed from this, but everything hinges on the genuineness of the accurate long alignments he identified.

In the late 1960s the implications of these claims seemed potentially overwhelming for British prehistory; for example Richard Atkinson, although he did not specify the possibility of priesthoods and chiefdoms, wrote, 'It seldom happens that a single book, by an author who makes no claim to be an archaeologist, compels archaeologists themselves to re-examine their assumptions about a whole section of the past.' (1968).

It is hardly surprising then that opposition soon arose to accepting Thom's evidence and that doubts about the validity of some of it emerged. Among the few who attempted to evaluate data and conclusions reasonably objectively over a long period of time were the author and Clive Ruggles, the former trying the experimental method (that is, testing predictions made about individual sites through fieldwork and excavation) and the latter undertaking a wholesale re-survey of the supposed astronomical sites in one large area of the country — western Scotland (Ruggles 1984a).

Ruggles' critique of Thom.

It is clear from his book (Ruggles 1999) and his joint paper with Barclay (Ruggles & Barclay 2000) that Ruggles believes that the new statistical data he himself collected is so overwhelmingly against the idea of long accurate celestial alignments that he has no doubt that any evidence which seems to point to the contrary conclusion — particularly that recovered by the author — can easily be shown to be invalid. The hypothesis of an intellectually skilled elite is therefore made redundant. It seems only fair therefore that in turn Ruggles' own fieldwork, and the conclusions he draws from it, should be critically assessed. The author did this in 1984 but the review appeared in a journal which has little or no impact on British archaeology; most of those interested in the UK have tended to take Ruggles' work at its author's valuation, most recently Ashmore (2000; 2001). However the author believes that the situation is nothing like as straightforward as this and that it is necessary to repeat here a few of his 1984 criticisms to show just how far Clive Ruggles actually is from 'disproving' Thom, despite his recent book (Ruggles 1999).

There is no doubt that by the mid 1970s a re-assessment of Thom's claimed alignment sites was urgently necessary. Incorrect archaeological identifications by Thom and other discrepancies had been pointed to, for example in North Uist and Caithness (Moir, Ruggles & Norris 1980). Also doubts had been raised about the objectivity with which Thom had selected some of the horizon markers of his long alignments; if there was any suggestion that they had been selected because they fitted a preconceived idea (a charge that both Thom's always strenuously denied) then clearly the general alignment hypothesis would become, if not worthless, then of much less value. Ruggles deserves great credit for completing his arduous and necessary study and for publishing it in full (Ruggles 1984a). Those who wish to follow the author's assessment of this work in detail may consult the review concerned.¹³ The crucial point concerns the criteria — both archaeological and in terms of in-built direction indicators — which Ruggles used to select which of Thom's alignments should be re-measured and included in the statistical study and which should be rejected. These criteria seem impeccable and Ruggles explains his ground rules very well; he also took pains to weed out sites which were not genuine standing stones, or not prehistoric at all. The core of the work is a list of the acceptable sites in six regions in the west — namely Lewis and Harris, the Uists, northern Argyll, Lorn and mid Argyll, Jura and Islay, and Kintyre — together with brief descriptions of each and scale drawings of the horizons indicated by artificial features in them.

It is necessary to understand that on each horizon profile¹⁴ is marked the section of skyline which in various ways is actually pointed at by the standing stone back-sight; the stone is rarely regular so usually two

horizontal arrows mark the minimum and maximum angular distances indicated (Figure 3). This 'accuracy' of indication was established by standing 2m behind each back-sight and looking along the oriented feature (usually the flat side of the stone or past an unique outlying stone in front¹⁵); the outer limits of the indicated zone were then marked by eye. The pairs of declination of the parts of the horizons so indicated (one at 180° to the other) were then established with theodolite readings and the scale of these is marked with curved lines in the sky on the profiles.

This procedure seems entirely rational and objective, especially when doubt has been cast on aspects of Thom's procedure when doing the same thing. It was essential to design the fieldwork to avoid falling into the trap of looking even unconsciously, for 'suitable' foresights and then accepting them, and from a praiseworthy desire to regard the long alignment hypothesis as *no proven* and requiring testing.

However the first point to note is that a clear basic assumption is made that the part of the horizon indicated (as opposed to the physical horizon itself) will not be measurable to an accuracy of better than plus or minus 0.1°, or 6 minutes of arc, because of the irregularity of the stones; this is 'the greatest precision in which we are interested here.' (Ruggles 1984a: 65). This seems reasonable but it does mean that the procedure rules out the existence of, or at least the detectability of, the kind of distant notch or mountain slope measurable to a minute of arc in good conditions, and which Thom firmly believed were used to define to the exact day of important calendar dates like the solstices.

This leads on to the second point which it is essential to grasp before one decides what can and cannot be inferred from Ruggles' mass of fieldwork data. At each re-examined site the basic unit of his analysis is the backsight only in its various direction-indicating combinations. As noted the parts of the horizon pointed at were defined objectively by looking along the orientated artificial features from a position 2m behind them and marking on the horizon profile the approximate limits of the zone which could be indicated — usually from 0.25° – 1.0° wide. Thus all the configurations of the horizon itself except its altitude, are ignored and the assumption effectively is that only approximately orientated stones are being dealt with, not precise long alignments. Thus the methodology allows only each appropriately defined broad horizon target to be counted for the analysis.

Sometimes the results of this process can be faintly comical, as with the orientation of the Stiaraval stone (in North Uist) towards the island of Boreray near St. Kilda 52 miles away (Ruggles 1984a: profile 96, 116) (fig. 1); the minimum range of possible azimuths defined by the face of the stone is 303.6° to 304.4°, or about 40' of arc (Ruggles 1994a: Table 6.1 & 103), yet there is only a tiny island peak there, on a flat sea horizon (Figure 1, B)! If it is an indicated foresight the upper limb of the Sun *has* to set exactly with its right edge at azimuth 304.0° and the indicated declination of +16° 47' at the centre of the disc *has* to be equally precise (Thom 1967: figure 11.5 d, (Figure 1, A)! In view of what is described in Section 4 it is important to note that this is a very long, potentially very accurate indicated sight-line to the second and third Quarter Day sunsets, at the beginnings of May and August respectively.

There are other similar examples; Camus an Geall in north Argyll is evidently a fairly rough standing stone as the target azimuth to the NW cannot be defined better than 3° — from 328° – 331° (Ruggles 1984a: table 7.1, 124) However there is a clear notch 1.3 km away at 327° 40' which could well be the target but it, like many others like it, is lost in the statistical mist produced by Ruggles' ground rules.¹⁶—Thus the methodology actually *prevents* deliberately arranged, long and potentially accurate celestial alignments from being recorded and can therefore hardly be a test for their genuineness.

Despite this initially necessary filtering process the statistical analysis of the data obtained (from the 276 indicated declinations found in 189 sites, weeded out from an original 322) did reveal clear evidence of astronomical interest among their builders (Ruggles 1984a: 303 ff; Norris 1988: 273). Briefly, there was a marked preference for declinations which could be marking the Moon rising or setting at its monthly southerly limits over a cycle of 19 years.

At the most precise level there is marginal preference for six particular

declination values (to within a precision of one or two degrees), which include the winter solstice (at -25° , but to no other solar events) and three (-30° , $+18^\circ$ and $+27^\circ$) which could imply an interest in the lunar ‘standstills’ which ‘would imply that organised observations were undertaken over periods of at least twenty years.’ This completely independent evidence for a strong and long term concern with the Moon’s movements is surely striking and important.

Have long alignments been refuted by Ruggles research?

What about the question — fundamental to the present discussion — of the existence of long, potentially accurate alignments? In his 1984 discussion Ruggles is slightly contradictory about this point. In summarising the main results of the study he says ‘We find no evidence of astronomical orientations of a precision greater than about one degree.’ (Ruggles 1984a: 304) which seems to rule out all accurate ‘observatories’. Similarly, and comparing his own work with that of the Thoms, he writes ‘... the results of this project ... strongly suggest that any claimed astronomical sight lines of a precision of 0.5° ¹⁷ and better can be completely explained away as chance occurrences emphasised purely by the process by which they were selected for analysis in the first place.’ (Ruggles 1984a: 306).

However a little later it is stated that ‘We have not examined our data for very high precision indications using distant horizon features such as notches, on the grounds that there is no motivation from our data at lower precisions to do so.’ and he goes on to say that anyone can investigate this point themselves from the information presented (Ruggles 1984a: 308). As noted such notches can be spotted in many of the horizon profiles ([Figure 1](#)). However it is really not possible to agree with Ruggles that his conclusions about the general low precision of the measured orientations make such a search superfluous; as described earlier the methodology used in the survey actually forced such a conclusion on him. The logical conclusion must be something like the following.

The methodology devised by Ruggles to re-examine Thom’s claimed standing stone ‘observatories’ was essential if the problem of bias in the selection of long alignments was to be overcome. However no long alignments could be seen in the data because they were not looked for nor measured as such. One cannot therefore say that they did not exist among the sites examined, although Ruggles’ other work on the claimed high precision lunar alignments may have raised grave doubts about accurate Moon observatories (1981, 1982a, 1983).

By contrast a keen interest in some lunar and solar lines still clearly shows in the data examined in western Scotland, despite the ruling out *a priori* of high precision. Therefore it seems perfectly reasonable to suggest that some of these are likely to have been primary investigation sites, positioned carefully in relation to a distant horizon marker and therefore capable of accurate observation. To establish the genuineness or otherwise of these different approaches — actual archaeological tests at specified sites — are needed (see Section 4 below).

It is fair to add that Ruggles himself has addressed this very question of the potential conflict between the ‘statistical’ and the ‘interpretive’ approach to identifying or discarding observing sites, though not in quite the same way as the author (1988). Norris also noted how potentially important was this independent demonstration of a fairly sophisticated interest in the sky in Neolithic times (1988: 273).

Specific controversial evidence

The author has carried out several such tests, on one of Thom’s claimed accurate solar sites and on two others independently identified. Ruggles, in his joint *Antiquity* article and in his book (Ruggles 1999) spends some time attempting to demolish the relevant evidence for each of these. This is perhaps not the place to respond to all the arguments in detail; interested readers can go back and assess the material themselves. Such might also ask themselves this question.

Is it likely that such investigations into primarily ceremonial sites are ever going to produce unequivocal hard evidence in favour of or against the ‘observatory hypothesis’, or is it more probable that any conclusions

(avoiding excessive deductive thinking) will have to depend on weighing up probabilities? Those who are familiar with the incomplete and equivocal nature of much archaeological evidence, and who are not necessarily mesmerised by the apparent infallibility of statistics, may see the point more clearly. It seems to the author this his profession is engaged in a difficult search to recover and interpret sadly incomplete data, and that it is much harder to unravel the real meaning of this data than to impose general positive or negative explanations on to it.

A few comments follow on the individual sites, and on the criticisms Ruggles has made of them.

Kintraw standing stone and hill platform.

The author's work at this site in 1970 and 1971 was designed as a scientific test of Thom's long alignment hypothesis (Norris 1988: 271). In Ruggles' words "The story of Kintraw is one of the best known in the entire debate about 'megalithic astronomy'" and he gives a good account of the research at the site, and of the resulting controversies, with many helpful photographs and diagrams (Ruggles 1999: 26–9). It is only necessary to mention here that most of the objections in principle to the hill platform being the backsight for a long midwinter sunset alignment depend on pointing to the topographical difficulties which hinder establishing the site from scratch.¹⁸ On the other hand if the organisation which set up Kintraw already had a functioning solar calendar and the date of midwinter was already exactly known, most of these objections become irrelevant. The site would then be an accurate local calendar site designed to keep track of the year for local purposes but integrated into a much wider system.¹⁹

Cultoan stone circle, Islay.

It is pleasant to record that Ruggles finds the long alignment at Cultoon reasonably plausible. 'An important example (of an elliptical ring with a solstitial alignment) is . . . Cultoon . . . whose major axis is aligned upon the midwinter sunset; and it may not be a coincidence that it is also aligned on a distant peak in Ireland.' (Ruggles 1999: 133). During the 1975 excavations this quality was predicted and tested for. During a spell of hazy weather the computed dimensions of the best-fitting ellipse were received over the telephone from Glasgow, and the positions of the two foci were measured out on the ground and marked with ranging poles. When after a day or two the weather cleared the poles were found to be pointing very close to Slieve Snaght in Co. Donegal (MacKie 1981: figure 3.6).²⁰

Brainport Bay, Loch Fyne, Argyllshire

This complex site has been described several times and Ruggles gives a lengthy account of it (Ruggles 1999: 29–34). It has the advantage over Kintraw that the possible solar alignment includes undoubtedly artificial elements, that flint flakes were found in the soil horizon stratigraphically associated with these artificial features and that the earliest ¹⁴C date takes the occupation of the site at least as far back as the late 2nd millennium BC (MacKie 1988: 246 ff).

Ruggles dismisses the value all the work done there as follows. Acknowledging that the fieldwork was guided by the principle of constructing hypotheses, testing them on the ground and then abandoning or modifying them as necessary he nevertheless says "...the problem is that while the archaeological evidence is allowed to modify the specific predictions, for example by adding more alignments, it is never allowed to influence the more fundamental hypothesis that Brainport Bay was a high precision 'calendric' site. Thus, as contradictory data confront each suggested alignment, more are suggested in an attempt to bolster the calendric idea, and the structure of 'supporting' evidence becomes steadily more cumbersome. Yet the increasingly attractive alternative, that the astronomy of the main alignment was of lower precision and all other alignments were fortuitous, is never considered. Instead the idea of archaeological verification has been turned into mere *post hoc* justification, which in this case becomes less and less viable as the weight of the evidence builds up against the hypothesis being proposed." (Ruggles 1999: 34)

This is not at all a fair summary of the situation. The most recent account of Brainport Bay by the author explains the methodology of the research (MacKie 1988). He argued that the 'prediction and test' method as described there is perfectly valid; there was a sense of being led by the refuted

predictions to the discovery of the cup-and-line rock carvings on Oak Bank one of which (after some tree clearance) was found to point directly to a notch suitable for marking the equinoctial sunsets. The plethora of extra alignments which he is supposed to have conjured up in his apparently desperate desire to keep his idiosyncratic interpretation of the site going in fact totals two (in addition to the main alignment). One is the indicated equinoctial notch just mentioned (MacKie 1988: figure 1)²¹ while the other is the midwinter sunset notch on Oak Bank. The latter is admittedly less plausible because it is short it was discovered by chance and nothing depends on it.

Moreover the author's interpretation of the main alignment — that it was a partly fortuitous linear assemblage of natural features which pointed by chance to the midsummer sunrise and was modified by man in ancient times to make it more impressive ritually (MacKie 1988: 229 ff) — is surely exactly that advocated by Ruggles. It seems strange that he could have edited the book mentioned while apparently not picking up this basic point.

Equally difficult to understand from someone well versed in practical astronomy is the discussion of the equinoctial line (Ruggles 1999: 33–4). Firstly this is unlikely to be fortuitous as its situation, if not its nature, was predicted and discovered as explained. Also it is unfair to mention as an objection that the second cup-and-line rock carving does not point to any obvious horizon marker without also mentioning that the *two cup-marks themselves* also point (along the pecked line through the first) to the equinoctial notch in Siaradh Druim, the 'western ridge' (MacKie, Gladwin & Roy 1985: figure 1).

The comments on the nature of the equinoctial foresight are also surprising (Ruggles 1999: 34). For example Ruggles finds unconvincing the idea that the whole Sun was visible in the notch at the 'megalithic equinox' as shown in the original publication (*Ibid*). Yet this is inevitable for two reasons, first because the daily movement of the Sun along the horizon at the time of the equinoxes is so rapid, and second because sunrise or sunset at that date cannot recur in the same place two years running because of the extra quarter of a day in each year. Thus in any given equinox notch the Sun's disc must in different years be either in full or in partial view on the established dates in March and September. This is fairly elementary astronomical knowledge.

One does sometimes get the impression that any stick will do to beat the author with. For example Ruggles' second objection to the genuineness of the western alignment at Brainport Bay is a general one, and a classic example of the use of deduction to oppose a fragment of concrete evidence. 'Finally there is a deeper problem in relation to supposed alignments upon the equinox which is a concept not necessarily meaningful outside the Western scientific tradition (see Chapter 9 and Astronomy Box 8)' (Ruggles 1999: 34). This Box (Ruggles 1999: 150) clearly explains the point just made about equinoctial sunrises and sunsets in successive years but even there Ruggles mixes up the two concepts of the 'megalithic equinox' and the modern astronomical equinox and implies that they are interlinked. They are not of course. The former is simply a slightly variable date arrived at by subdividing the total days in the year; because of the irregularity of the Earth's orbit, the Sun will arrive at this 'calendar equinox' slightly later than the true (or astronomical) one in spring and slightly earlier than it in the autumn, as Thom explained many years ago (1967: 107 & figure 9.2).

The latter concept depends on understanding that the Earth hangs in space,²² that its axis is tilted in relation to the plane of its orbit, and that the Sun therefore twice a year must reach declination 0° as it crosses the celestial equator against the background of the fixed stars. As far as the author is aware not even the most zealous advocate of prehistoric wisdom has maintained that Neolithic man in Britain knew all that!

One piece of evidence from Brainport Bay has recently been re-assessed. During the first phase of excavations at the site, in the mid 1970s, Peter Gladwin and members of the mid Argyll Archaeological Society found a cache of 33 quartz pebbles buried in a shallow pit on the main alignment. Many such pebbles had been found scattered around all over the site but the vast majority of these were broken (Gladwin 1985: 28, appendix III). The cache is of smooth, whole pebbles which 'were packed tightly together as though they had formerly been contained in a bag?' (Gladwin 1985: 14). A fine colour photograph of the group has been published (Butter 1999: 17

plate) and it is now in the Kilmartin House Museum, Argyll, with the rest of the finds.

When he was shown the cache in the late 1970s the author could not think of any calendric significance for the number 33. The possibility that the distant horizon notch pointed at by the long alignment marked a period of time before and after the summer solstice sunrise in *c.* 2000 BC — giving advance warning of the longest day as well as being able to define it exactly by the ‘splitting the difference’ method — was considered, but this interval is about 16 days.

Very recently he read Heath’s account of the 33 year solar cycle and its possible mythological connections (Heath 1998: 227–29). It is such a simple concept that it seems surprising that it is not better known in the archaeo-astronomical literature. It depends on the fact that the year is just under 365.25 days in length (actually 365.242199) and that after 33 years this cycle repeats almost exactly (to within just under 11 minutes). By contrast when eight normal leap years have been inserted into the calendar of 365 days (that is, after 32 years), this adjusted calendar will still be running almost exactly six hours ahead of the real year. To any society accustomed to checking its calendar against horizon notches this would surely be obvious.

With a good clear equinox notch to hand it might just be possible (because of the rapid daily changes in declination mentioned earlier) for naked eye observers (no doubt with smoke arranged to rise up between them and the Sun at sunset) to detect the fact that after 33 years the disc has returned exactly to the notch.²³ Only a long term practical experiment could verify this. Yet it seems unlikely to be a coincidence that an obviously important cache of 33 white pebbles was hidden on the main alignment with a plausible equinoctial alignment close by.

Maeshowe and Howe in Orkney

There seems no good reason to enter into a further debate about the alignments suggested by the author at these sites. The reasons for suggesting them were set out in detail, together with possible difficulties, in the papers concerned and Ruggles’ comments add little to what was said then (MacKie 1997; 1998). All such suggestions are intended to be tests of the basic Thom alignment hypothesis — in other words if there is something in the idea then it should be possible to interpret many major late Neolithic structures as having significant solar calendar lines incorporated in their design. However one of Ruggles’ counter arguments cannot be ignored — his objection to Axis A at Maeshowe.

He accepts that this is a classic Thomian long alignment in which a straight line — formed by the longer inner part of the tomb passage, and by the Barnhouse standing stone 400m in front — points towards a spot on the summit of Ward Hill on Hoy; here the Sun sets one sixteenth of the year before and after midwinter (Ruggles & Barclay 2000: 70). However a cloud of doubt is then thrown over it by implying that this alignment points 1.5° (actually more like 1.7°) to the left of the clear notch (between the base of the right slope of Ward Hill and the almost level local horizon) where sunset on the ‘sixteenth’ finally takes place, so it is really not very accurate at all.

As the author’s drawing makes clear (MacKie 1997: figure 9) the rounded shape of the right end of Ward Hill means that the Sun can set twice here for several days twice a year — disappearing behind the flattish summit and then reappearing for a short time at the base of the right slope. Thus when it comes into view and sets again, as a brief flash, with its upper edge at the base of the slope — presumably on the ‘sixteenth’ date being indicated — it had already set a few minutes earlier on the summit ridge to the left, the point indicated by ‘Axis A’. As far as the position in the sky is concerned it does not matter which of these two azimuths the Maeshowe alignment points at; they have the same declination, as Ruggles must surely know.

A calendar stone at Knowth?

There is a possibility that there exists at Knowth in Ireland independent evidence in a rock carving for the reality of the sixteen ‘month’ solar calendar worked out by Thom from alignments found in standing stone sites (Thom 1967: chapter 9). Kerbstone SE4 — illustrated but not discussed by Ruggles (Ruggles 1999: 129 & figure 8.5 a) — bears a curious rayed pattern which has been interpreted as this (Thomas 1988: 42 & 44–7). Not having seen the stone first hand the author is reluctant to comment on the reliability

of this idea;²⁴ for example Thomas' diagram suggests the pattern is complete but the photograph shows that a small section seems to be missing. Here again an independent study of the idea would seem to be advisable.

4. A NEW DISCOVERY IN NORTH UIST

The last part of this paper contains a brief description of a new discovery on North Uist which shows in rather a striking manner just how unwise is Ruggles' continued unwillingness to accept long alignments. The standing stone sites in the Long Island provided Thom with some of his most intriguing material (Thom 1967: chapter 11)

Introduction

In late August 2001 he was on holiday in the Outer Isles with a friend who had not been there before and decided on impulse to visit a well sign-posted stone circle in North Uist which he was unfamiliar with, having been primarily concerned with brochs on previous visits. This is *Sornach Coir Fhinn*²⁵ and it stands on a fairly steep slope immediately to the north of Loch Langass (NF 843650); Burl says that it 'occupies one of the loveliest situations for a stone circle in the British Isles.' (Thom, Thom & Burl 1980: 311). It also has the rare quality of having been built on an artificial platform: the stone ring is set on a level bay cut out of the hillside and the material excavated in this way was spread around the circle as a broad bank (*Ibid* 311). The local tourist board has prepared a path linking the circle with the nearby Barpa Langass chambered cairn (NF 838657) and there is an excellent notice board a short distance from the site, next to the hotel.

Thom (1967) described the stone circles and standing stones of the Outer Isles and made some detailed inferences about the way they were interlinked by celestial long alignments (Thom 1967: 130–33). He also made an accurate plan of *Sornach Coir Fhinn* (Thom, Thom & Burl 1980: 310–11: site H/17). It seems to be partly built as a true circle with a diameter of 125 ft.²⁶ but the arc on the southern (downhill) side is flattened; there are possible entrances at the ends of this arc. Though the alignments are not discussed in the text of *Megalithic Sites in Britain* his plan shows that Thom evidently believed that the stone circle was positioned in order to indicate two such from the centre of the ring — one towards standing stones on the summit of Cringaval not far to the west (Thom; Thom & Burl 1980: 310–11). This gives a good calendar declination for sunset on the third 'sixteenth' before and after midwinter.

The other suggested alignment is to the large standing stone next to the excavated chambered cairn at *Leacach an Tigh Cloiche*,²⁷ known as 'Uneval' or 'Unival' in the literature, but this does not fit easily as a solar calendar marker (Scott 1929, 1930; Henshall 1972: [UST34]; Ruggles 1980: 102, site UI28) .

Ruggles also surveyed the indicated horizons at both circles and discusses them briefly (Ruggles 1984a: 102 & 106, 114, & 116–17: figures 6.11 & 6.12: sites UI33 & UI37). In the case of *Sornach Coir Fhinn* he suggests in addition (since it is visible) a line to the other circle *Sornach a'Phobuill* and while accepting Thom's line to *Leacach an Tigh Cloiche*, does not mention the one to Cringaval.

One can hardly doubt that the position of *Sornach Coir Fhinn* was carefully chosen; evidently it had to be at a particular place on the slope above Loch Langass and a flat platform was prepared for it. Looking at Thom's plan it seems that another line is clearly indicated by the plan of the stone ring itself: the diameter of the true circle which seems to mark the boundary with the flattened part was marked by eye on the plan in the book and then measured with a protractor; the angle was 117/297°. It has already been noted that Ruggles excluded such alignments from his survey *a priori* so could not discuss such a one here even if it was found.

A long alignment discovered

None of this was known to the author when he visited the site in August 2001; he had not briefed himself on any of the Neolithic sites in the Long Island before going on holiday, nor taken any relevant books. By chance the weather was fine and the air clear so he looked round the horizon in search of distant peaks to see whether there were any feasible long alignments. Most of the horizon is only a few miles away but there is one group of distant peaks in South Uist, visible to the south so clearly irrelevant as solar markers.

However in the south-east the landscape is particularly striking; the low hills Eaval and South Lee to its left stand out, framing a stretch of local low ground between them. In that gap showed faintly two much more distant flat-topped mountains which he was able to recognise, having spent a month excavating near them in 1965; they are MacLeod's Tables on Skye — *Healabhal Beag* and (to its left) *Healabhal Mhor*, about 27 miles away. Just to the left of the latter, and showing even more dimly (almost vanishing before he left the site), was *Glamaig*, a conical peak also on Skye, just south-west of Raasay island and almost exactly 47 miles away.

The azimuth from the stone circle to the top of *Glamaig* is very close to 122°. and it seems reasonable to suggest, first, that the diameter of the true circle which forms the boundary with the flattened part was actually aimed at this remote peak (so that it is a genuine indicated alignment) and that, second, the reason for digging out the platform for the circle at this point on the hillside was to keep the top of *Glamaig* in view, together with whatever other nearer horizon targets, if any, were being aimed at. From the OS map data the declination of the peak proved to be — 16.5°. Thus when the Sun rises with its upper edge at the peak ([Figure 2](#)) its centre has a declination of — 16.7 – 16.8°, very suitable for marking in prehistoric times the first and last Quarter Days of the year, at the beginnings of February and November respectively.²⁸

It has been suggested several times by the author and by others (MacKie 1997: 358) that these Neolithic solar calendar dates explain the origin of the much later Celtic feasts — Imbolc in the spring and Samhain in the autumn ²⁹ — which divided the old Celtic year in half. They appear to be very prominently marked at Maeshowe itself (MacKie 1997: figure 5). The idea has been strongly opposed by Ruggles (Ruggles & Barclay 2000: 68–9) so the chance discovery of a very long potentially very accurate alignment to exactly the same dates is particularly interesting. If long alignments are figments of the author's imagination what are the odds against finding such a precise and significant indicated example, by chance during a half-hour holiday visit to a randomly chosen stone circle?

Moreover if the argument offered earlier is correct — that Ruggles' fieldwork has not disproved accurate long alignments, only suppressed them — then one could predict from the evidence from *Sornach Coir Fhinn* that more examples of clearly indicated Quarter Day alignments will be visible in his own data. This turns out to be the case, and the best examples are shown in [Figure 1](#).

Discussion

Some useful points emerge from this new discovery at *Sornach Coir Fhinn*. First, how many more such long alignments, visible only in clear weather, await discovery? Thom and Ruggles between them have surveyed more British standing stone sites than anyone else but they rarely comment on the weather at the time of the site visits. Obviously if a distant peak is recorded it must have been clear but it is unlikely to have been equally good when all the sites shown having only near horizons were surveyed.³⁰

With present climatic conditions in Scotland it must often be a matter of luck whether a vital distant peak is seen during a short visit³¹ and the situation cannot improve much until more archaeologists take an informed interest in this aspect of standing stone sites, visit them systematically in fine weather and carefully record what they see. The basics of this aspect of Neolithic landscape studies should surely be part of every undergraduate archaeology course.

The second point is that investigations of the astronomical qualities of standing stone sites should be a little more flexible. The irreducible requirement must remain of course: to be plausible a celestial alignment must include a direction indicator of some kind in the backsight which points at the horizon foresight. However as with *Sornach Coir Fhinn* the landscape itself could often be giving us important clues about what the circle builders and stone erectors intended. There the distant mountains in Skye, framed between local hills, seemed an obvious potential sunrise foresight and an examination of the site plan showed that this direction was indeed marked. Moreover if plausible indicated alignments have already been identified at a site it is surely reasonable to accept an additional distant foresight which marks a solar calendar position even if no indicator can now be identified.

Maeshowe again is a good example; the vital first and last Quarter Day sunset marker, at the right end of Cuilags, is not now specifically marked but other solar calendar dates are and it therefore seems quite arbitrary to ignore the Cuilags line, especially as the tomb itself must have been carefully positioned in the landscape in relation to various sunrises over Hoy (MacKie 1997: 357 & figure 5). The main need is to have a set of clear rules which prevents one from choosing foresights at random, and then claiming them as significant, and to follow these carefully. It should be possible to draw up such a set with which one could try predicting the existence of long celestial alignments through landscape analysis.³²

The third point is that the discovery of the rarely visible yet indicated Glamaig sight-line surely points again to a better climate and clearer air in Neolithic times. Other rarely seen long sight-lines suggest the same, like those to Boreray. The literature on the climatic deterioration in Britain between the middle Bronze and the Iron Ages is large but an essential point is that peat is repeatedly found to have grown over Highland archaeological sites of these earlier periods, marking the onset of wetter and cooler weather. A local example is Callanish in Lewis the stones of which were originally standing in 5 ft of peat. It simply will not do to argue from present atmospheric conditions that long alignments are implausible.

CONCLUSIONS

Reconstructing late Neolithic society, and the activities of any élites of that time, is evidently not simply matter of refuting or confirming this or that piece of specific evidence; it also requires the drawing together of a great variety of evidence (some of it not too familiar to archaeologists), as well as the recognition that the use of analogy, involving the findings of anthropology and ethnography, is a vital step in the process. It also involves recognising that archaeological and statistical data are rarely unequivocal and that coming to a firm conclusion over its meaning often involves balancing various probabilities. However much of the evidence for the intellectual skills current in Neolithic times is quantifiable and has to be understood and accommodated. In the author's view, when one takes all this into account, the general argument for the existence of chiefdoms with professional priesthoods in late Neolithic times is soundly based on traditional evidence. It is the level of expertise of those priestly classes in arcane intellectual activities that is still debatable, as also of course is the wider significance of those activities.

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