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CONTENTS.

PREHISTORIC WEATHER AND VULCANICITY

Do Irish Bog Oaks Date the Shang Dynasty?

by Mike Baillie

In CA 111 Chris Scarre pointed out that the explosion of Thera could be dated to 1626 BC. This may, however, only be the beginning. There are at least 4 other prehistoric dates that the readers of CA should learn by heart. I believe that our work on tree-rings has revealed several major volcanic eruptions which may have caused climatic upset on a world wide basis. When these major eruptions occurred, the climate of the northern hemisphere may have been altered for several years. We have to envisage the possibility of failed harvests, famine – and no doubt plague and pestilence as well. Empires could have been destroyed and vast tracts of land rendered uninhabitable. In such circumstances the survivors would have been those who were more war-like than their neighbours – the consequences of such "aggression means survival" could have changed social behaviour for centuries. While such claims may seem like rather fanciful extrapolation from a few dated "events" in the Irish oak record, they need to be suggested in the form of an hypothesis for archaeological testing. The most dramatic claim, which can ultimately be tested, is that the dust veils recognised in the Irish tree-rings may date the start and end of the Chinese Shang Dynasty.

Before we begin let us recap on the methodology. There are three major lines of research which appear to provide evidence for environmental catastrophes. The earliest to surface was the work of Danish glaciologists led by Dr C U Hammer. They analysed long cores from the Greenland ice sheets and found layers of sulphuric acid deposited by volcanic eruptions. These could be broadly dated because the layers in the ice sheets are basically annual in character (see *Nature*, 288 [1980] and 328 [1987]).

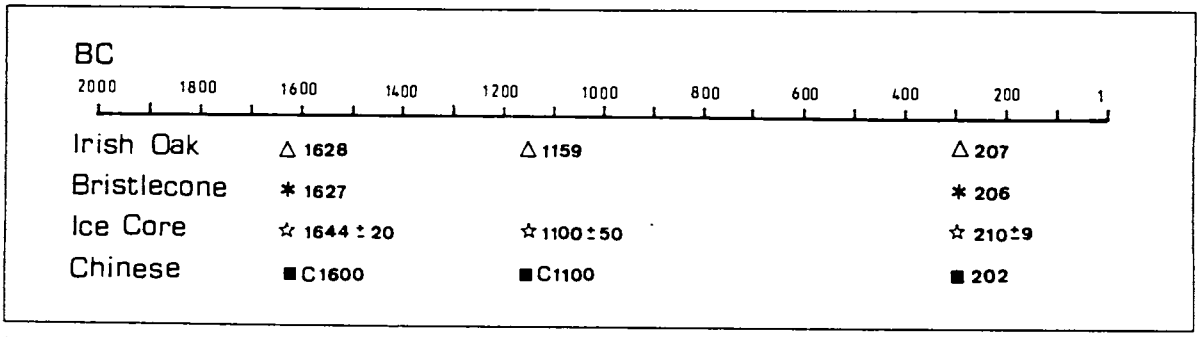
Then in 1984 an American dendrochronologist, Val Lamarche, who worked at the Tucson tree-ring laboratory, drew attention to frost damaged rings which occurred in a lot of his high-altitude bristlecone pines. In recent times such frost rings tended to occur in the years following major volcanic eruptions e.g. the eruption of Krakatoa was in 1883, the bristlecones showed frost damage in their growth rings for 1884. One notable frost-ring event occurred in 1626 BC and he suggested that this might well be the result of the explosion of Santorini – if so, the actual eruption would have occurred in 1626 BC "or one or two years earlier". To clarify one point at this stage, all the American prehistoric dates have to be moved back by one year. This was due to LaMarche's use of a "zero year" between AD 1 and 1BC. No such year is recognised in the historical calendar hence the frost ring actually relates to the growth ring for 1627 BC.

In Ireland our approach was rather different. One has to be clear that there are two populations of oaks. Archaeological timbers are assumed to be "land grown" oaks. The oaks we studied divided down fairly neatly by period – the last two millennia were mostly land-grown timbers while the prehistoric trees were almost exclusively bog oaks. These bog trees grew rooted on deep peat and hence their existence was marginal at best. It became clear that it was these trees which were sensitive recorders of worsening conditions. The starting point was the observation that some of our bog trees showed very narrow bands of rings in the decade of the 1620s BC – tending to support the notion that something big had happened at that time. When the effect was quantified it was discovered that seriously reduced growth episodes, indicated by the "widespread occurrence of *narrowest* growth rings", were infrequent *and* tended to occur at the same times as Hammer's ice-core acidity peaks i.e. clusters of narrowest rings in Irish bog oaks appeared to be related to large volcanic dust veil events (see *Nature* 332 [1988]). Interestingly recent climate research by the Climatic Research Unit at the University of East Anglia has shown that in the months following eruptions in recent times, we tend to get low pressure anomalies over Britain and Ireland. One can imagine that the more extreme conditions which might have been related to much larger eruptions in the past could have supplied just the right conditions to cause stress to the marginal bog trees. The diagram opposite lists the main events and these are discussed individually below.

1628 BC

The date of 1628 for Thera (or Santorini) presents a problem. Although it is backed up by the radiocarbon dates from Akrotiri on

Therefore BC1 ≡ AD1



Many different methods have been used to date the prehistoric catastrophes. Here we see four of them – the Irish bog oaks, the American bristlecone pine, the Greenland Ice cores, and the Chinese historical records. The results they give are plotted for three of the crucial dates.

Thera, the conventional dating – based on cross-links with Egypt – is around 1500 BC. One possible solution is to look closely at the conventional dates. It is tempting to see the Thera explosion in dramatic terms as marking the "end" of the Minoan civilisation. However the dates and the linkage has always been a problem ever since the connection between Thera and the Minoan civilisation was first mooted. The problem is that the Thera eruption is dated – by the pottery on Thera itself – to Late Minoan 1A, while the destructions on Crete are LM1B. In the original article in Antiquity in 1939 when Marinatos linked Thera with the end of Crete, he suggested in effect that LM1A and LM1B were merely two different styles and could be contemporaneous. However our date of 1628 for Thera would make this impossible.

The best current compromise – adopted as a resolution at the recent Thera Congress – is that Thera marks the end of LM1A, when there was considerable damage on Crete that was rapidly repaired. The "end" of the Minoan empire came at the end of LM1B, some time later. There are still chronological problems: the end of LM1B is still usually dated around 1500 BC by cross links with Egypt, and this is usually considered to be "a generation" later than LM1A. The problem undoubtedly needs further examination.

It is worth stressing the strength of our scientific case. We know that Santorini was massive – probably somewhere around 30-50 cubic kilometres were output – so how can it *not* show up, especially in the icecap? Also, none of the three lines of evidence shows anything else significant between 1628 BC and 1400 BC – a bracket which *must* encompass Santorini according to those who do not accept the 1628 BC dating. It should be mentioned that the vulcanologists say that Thera did not produce enough sulphur dioxide to account for the sulphuric acid found in Greenland. However it would be dangerous to assume that we can fully quantify an eruption of the magnitude of Thera. Undoubtedly we will hear lots more about this controversy. What is not in doubt is the fact that something hemispheric in scale took place in the 1620s BC!

1159 BC

In many ways, an even more interesting catastrophe was that of 1159 BC. Hammer had noted a significant ice signature at 1100 – 50 BC. The Irish trees show a very spectacular narrow band of rings beginning in 1159 BC – 43% of the trees from six sites have their narrowest rings during this period. The event is almost certainly the third great eruption of the Icelandic volcano Hekla. The case for something unpleasant happening in Britain has been noted by John Barber, director of the Scottish Central Excavation Unit. He has noted both extensive abandonment of upland sites and a decline in the numbers of "burnt mounds" – related to cooking at temporary hunting sites – in Scotland in the 12th century BC. This can be coupled with Andrew Dugmore's discovery of a fine layer of volcanic dust first in South Uist and the Shetlands and more recently in a wide area of northern Scotland. This tephra can be positively identified to the Hekla 3 eruption. So the evidence, precisely dated by the tree-rings, is accumulating to present a firm picture of effects on human populations. Colin Burgess has long argued (notably in his book "The Age of Stonehenge") that around this same date the Earlier Bronze Age came to a sudden end, and was replaced by a very different Later Bronze Age. It looks as if, for want of a better suggestion, that we may be able to argue that the Earlier Bronze Age ended, and the Later Bronze Age began in 1159 BC – precisely.

On a wider scale, this date also fits nicely in the Aegean, when a decade of famine may have pushed the tottering Mycenean civilization to its final collapse – it is only 25 years away from the traditional date for the fall of Troy! However, an even more dramatic use of these

dates may come from China. Kevin Pang and his colleagues have been scanning the masses of ancient Chinese texts which record all sorts of natural occurrences from floods to eclipses, from planetary conjunctions to crop failures and plagues. Of interest to us here, they have noted that the start and end of the Shang Dynasty seems to be associated with phenomena which suggest support for the long held concept of "Mandate from Heaven". These include dry fogs, dimmed sun, cold summers and famines which could easily be explained by the effects of severe dust veil events. Pang and Chou have noted that the tree-ring dates 1628 BC and 1159 BC would seem to fit admirably with the beginning and end of the Shang.

It is also interesting that they have used key references in the texts, relating very rare astronomical configurations to actual *computed* occurrences, to give a date (1953 BC) in the 1st reign of the Xia Dynasty (Xia - Shang - Western Zhou) and to date the first year of the reign of the 7th Western Zhou king to 899 BC. Given these dates, and the known number of reigns in each dynasty, they note that by using linear interpolation between 1953 BC to 899 BC, they can place any of the 37 generations (of kings) at least approximately. Using this procedure they suggest the start of the Shang circa 1600 BC and the end circa 1100 BC. Their information, which appears to be based solely on independent Chinese records and astronomical calculations, raises the distinct possibility that the volcanic effects seen in the West are the same as those which provided the Mandate from Heaven for dynastic change in the East. It remains to be seen if ways can be found to verify the Chinese dynastic dates, but the very concept of the dust veil from Santorini also causing the fall of the Xia excites the imagination - anyway I like the concept of "Irish tree-rings date Chinese Shang Dynasty"!

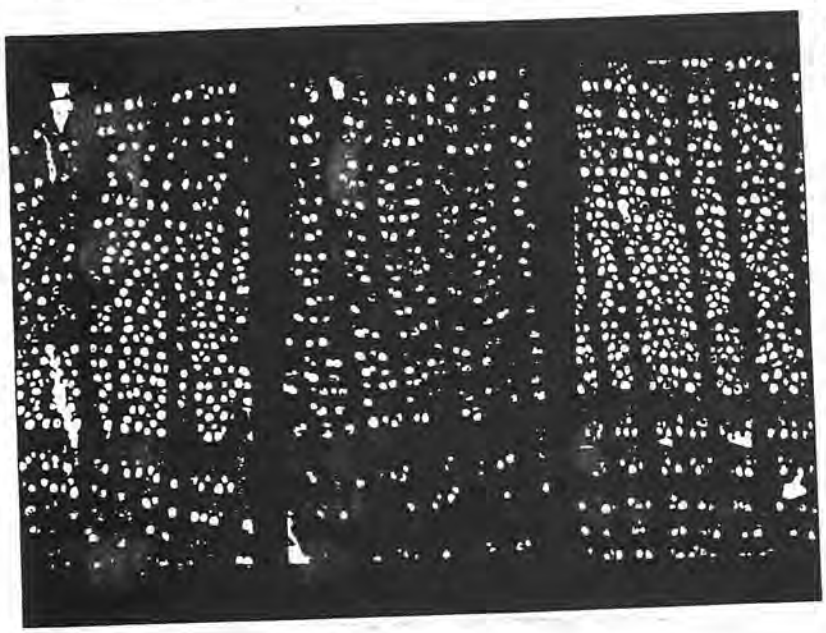
There are also a number of other narrow-tree-ring events. 207 BC shows up as a single year event in the Irish tree-rings and is only loosely dated by the Greenland ice-cores to 210 ± 30 BC. However, when this is seen in the context of another bristlecone frost ring in 206 BC and devastatingly bad conditions in China between 209 and 204 BC you can almost taste the ash. Again Pang and Chou tell of crop failures, cold and famine. They suggest that the evidence points to a possible halving of the population of China. It is the telling mention of "the stars being lost from view for three months in 208 BC" which tells us that it is a volcanic veil we are seeing. If this is indeed Laki in Iceland, then archaeologists in Britain and Ireland should be looking for signs in the record here, and classicists should start searching the classical authors.

AD 540

Coming forward, there is another major event in AD 540. The most obvious association with this date is the occurrence of the Justinian plague. If there really was a dust veil event at that time - and European dry fogs in 536 and 537 testify to that - then what better to follow any famine but plague.

The Neolithic and beyond

Earlier than 1628 BC there are two other events which may feature in the archaeological record. These are at 3195 BC and 4375 BC. It is



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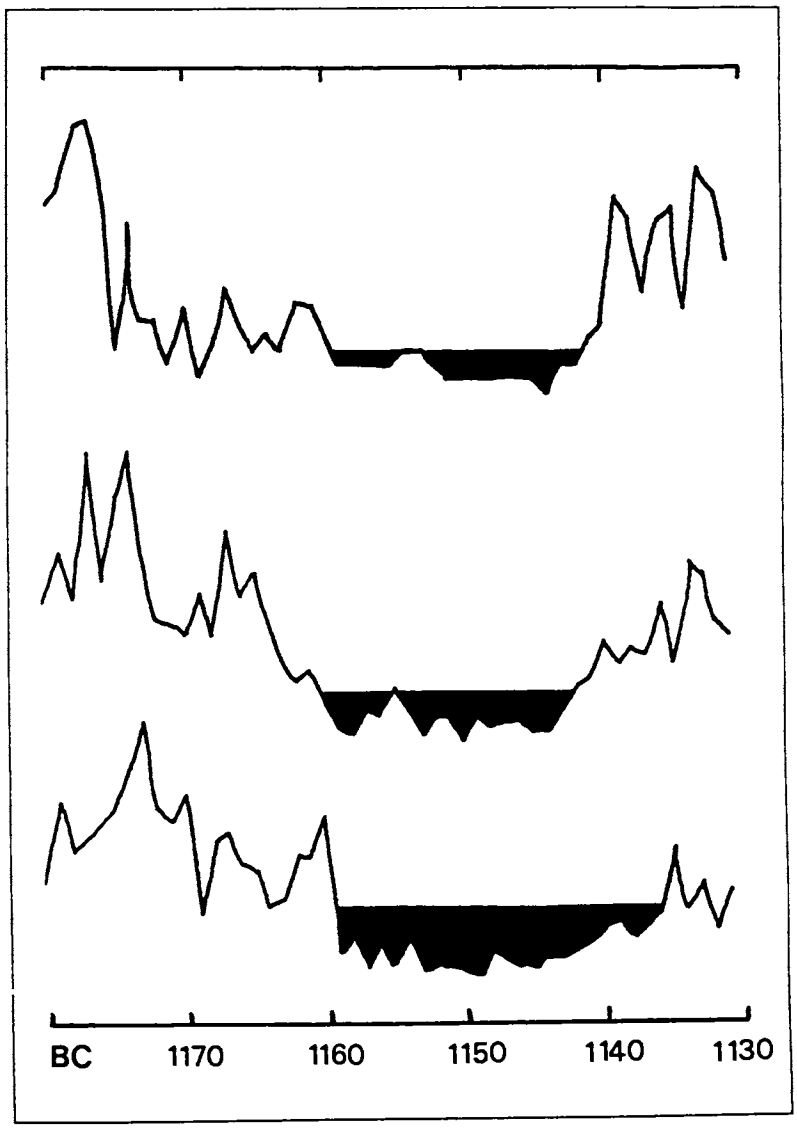
Sections of three bog oaks reveal the 1159 lacuna. The rings run across the picture, and at the bottom are the last half a dozen 'normal' tree rings, ending in the rather fat one of 1160 BC. There is then a central section where there appear to be no tree rings at all : in fact they are so narrow they scarcely show. It is only at the top of the picture that normal tree rings begin to show gain. The oak on the left, Q2211 is from Ballymacombs More, Q2243 in the centre is from Charlemont, and Q2248 (right) is from Tullyroan.



Right. The disaster of 1159 BC. Ring patterns from three sites, Gortgole, Toome and Tullyroan, showing the catastrophic reduction in ring widths in the 1150s and 1140s.

tempting at first to link the 3195 BC event to the elm decline, but unfortunately that doesn't work. Since 3195 BC is a tree-ring date, it has to be *de-calibrated* to convert it to the radiocarbon time scale. So this suggested dust veil event could be related to happenings around 2500 bc, to use the old lower case notation that everyone understands – distinctly too late for the elm decline. How about trying it out for the interface between the Early and Middle Neolithic – a nice catastrophe to end the causewayed camp/chambered tomb complex?

The same goes for 4375 BC which de-calibrates to something like 3600 bc in radiocarbon years. This could have made life difficult for Mesolithic people – or indeed for any really early farmers who had just sneaked in. Only time will tell how we interpret this date!

Further back still there is an event which should be noted even though there is no tree-ring evidence for it! Hammer's 5400 + 100 BC ice core date almost certainly related to Mt. Mazama better known as Crater Lake, Oregon. This was a very large eruption and it is interesting that the Irish bog oak sequence *begins* at 5289 BC. Again the de-calibrated date is somewhere around 4600 bc in radiocarbon years.

Having uncovered these events/dates in the Irish oak record and then having found the coincidence with the ice-core dates I have been amazed how additional information has tended to reinforce the catastrophic concept. I am convinced that these dates mean something in human terms. However I also recognise that the poor chronological control normally available in archaeology presents a problem. Any sloppily dated archaeological event, within a century or so, tends to be "sucked in" to the precisely dated tree-ring events. We all have to be on our guard against circular arguments. In reality I cannot use Pang and Chou's work, nor John Barber's, to argue about the magnitude of the environmental disasters brought about by Santorini or Hekla – for the simple reason that their work is not well enough dated to be compatible with the tree-ring dates. So if people really want their catastrophes they will have to work for them by refining *their* archaeological chronologies.

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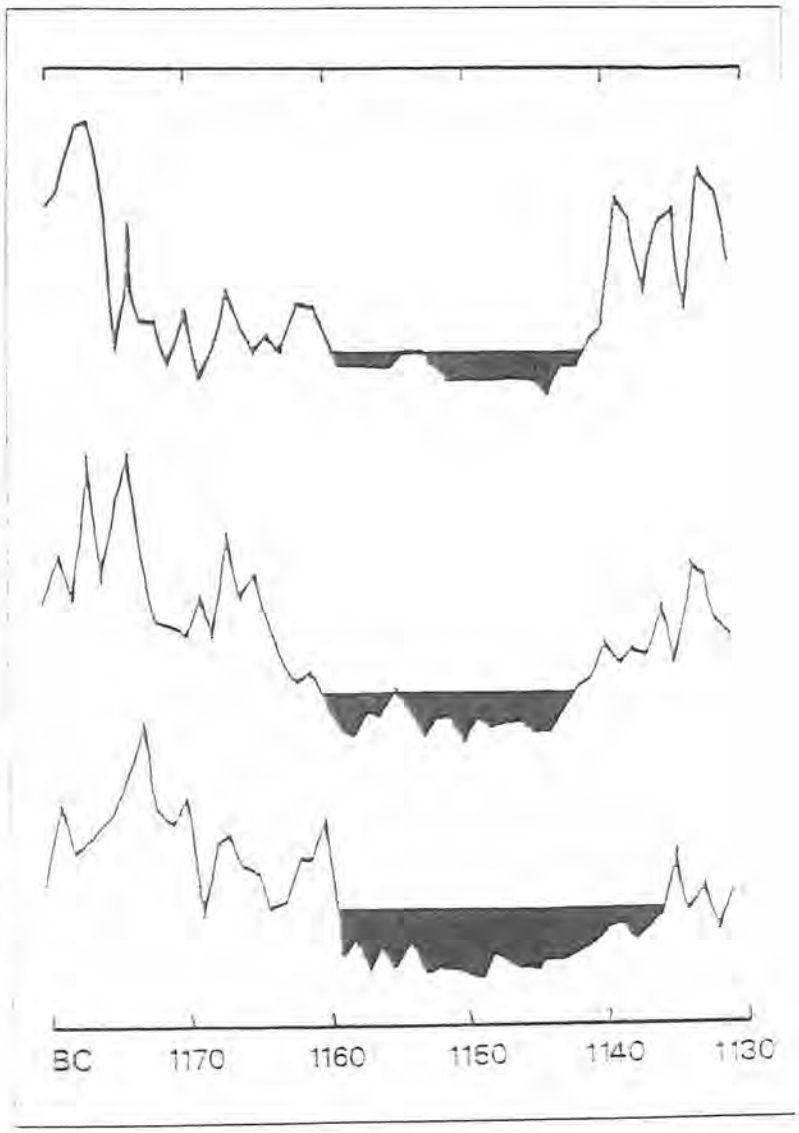

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precisely. However there were over thirty Roman brooches, predominant among which are the Colchester B and BB types, dating to between 40 and 60 AD. Other finds include iron reaping knives, spindle whorls and loom weights, an important group of Dressel 1 amphorae, and a variety of bronze objects, including Roman tweezers.

To what extent can we interpret this structure, lying at the centre of a late Iron Age "village" as a shrine or temple? In the first place there is the size of the structure: with the exception of the six-poster "granary" on the northern edge of the enclosed area (and the adjacent, curiously small circular gully) it was the smallest structure on the site. Purely on the basis of floor-space, therefore, neither "chief's house", nor "village meeting place" is a convincing explanation.

The second reason is based on the finds: the onyx ring stone, the brooches, and the fact that the gully around the building contained many bone fragments but hardly any pottery. This must mean that pottery was not generally used in the vicinity, and therefore the building was probably not domestic.

Thus a "shrine" is surely the most attractive interpretation of a structure which occupied such a prominent position in the village. At present, such a shrine within a village is unique: will it remain so, or will the Stansted village become the typesite for the typical Iron age village whose central shrine has everywhere else been missed?

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Volcanoes, Catastrophe and the Global Crisis of the Late Second Millennium BC

by Colin Burgess

When Andrew Selkirk asked me to append some comments to Mike Baillie's piece on volcanic "events", it prompted the notion that I had written on catastrophes in *Current Archaeology* some years ago. It proved after a long search to be exactly ten years ago (CA 67) and to be a paragraph entitled "Catastrophe?". I postulated then a wide-spread collapse of established systems in Britain in the 12th-11th Centuries BC; everything from settlement and agriculture to burials and domestic pottery.

I suggested that around this time there occurred one of the most fundamental breaks in the whole prehistory of these islands. What came after, in the early First Millennium, bore no resemblance to what had gone before in the Second. I have encountered nothing since 1979 to shake my belief in the concept of a late Second Millennium catastrophe. On the contrary, the evidence for crisis has continued to accumulate. In 1985 I developed the theme, adding a collapse of population to the general breakdown of systems already postulated (BAR 143).

"My first attempt to introduce archaeologists to volcanoes drew mirth"

It was only subsequently that I began to come across papers on the "volcano effect", which seemed to support this thesis. My first attempt to introduce archaeologists to volcanoes at the 1987 Newcastle Later Bronze Age conference drew a mixture of mirth and non-comprehension, so I am delighted that reputable scientists such as Mike Baillie are now publishing so extensively on these catastrophic events; for archaeologists are wont to pay attention to scientists. The time has come to draw together evidence from the whole of Europe, and indeed the Mediterranean lands, for this breakdown in the late Second Millennium. In doing so, one is keenly aware of the danger of 'sucking in' dates for unrelated phenomena arrived at by different methods, to create a false horizon (Baillie, Endeavour 13, 1989, 81).

"A global environmental deterioration set in in the 13th century"

The breakdown of the existing order in the East Mediterranean c.1250-1150 BC has long been a familiar concept. The Aegean world, Mycenae, the Hittite empire, and the great Late Bronze Age cities of Cyprus and Syria, all came to an inglorious end, and even Egypt was so exhausted by the struggle as to be finished as a

world power. A cyclical global environmental deterioration seems to have set in in the 13th century (see Bryson in Antiquity, 98, 1974). In some areas the problem was too much moisture, while in others it was drought and desiccation. In the Near East the Egyptian pharaohs (Seti I, Ramesses II) had to sink wells deeper and deeper to keep Nubian caravan routes open, and had to open their borders to drought stricken bedouin; and eventually Merneptah and Ramesses III had to fight to keep desperate Libyans out of the Delta in the late 13th and early 12th centuries. These problems are dealt with fully in Nancy Sandars' admirable *Sea Peoples*. "Peoples of the Sea" became an increasing problem in the late 13th century, coming, Sandars has persuasively argued, from the Anatolian littoral. Merneptah had to dispatch large shipments of

"A catastrophe resembling a 'nuclear winter'"

grain to Anatolia to relieve famine, while the archives of Ugarit around 1200 BC confirm that a terrible famine was raging in Hittite lands, and even Cyprus was appealing for food relief. The famine appears to have been so widespread and severe in Anatolia that memory of it lasted for centuries afterwards, eventually to be commented on by Greek writers such as Herodotus.

It is clear, then that the late Second Millennium climatic downturn had already started in the Mediterranean by the early 13th century. In this case Baillie's volcanic 'event' of the 1150s and 1140s will have had an exacerbating effect.

What happened in Britain in the late Second Millennium? In the *Independent* for 16.8.88, David Keys suggested that 'Most of northern Britain appears to have been rendered uninhabitable by a catastrophe resembling the "nuclear winter" that some scientists believe would follow a nuclear war'. While comparison to a nuclear winter and population losses over 90% in north Britain may be judged over-dramatic, there was undeniably a remarkable contraction of settlement and agriculture throughout Britain and Ireland after 1200 BC.

At the opening of the conventional Late Bronze Age in Britain, in the 10th century BC, signs of this shrunken scale of activity and settlement are still everywhere. There are still extensive tracts of the country for which there is little or no settlement evidence between the 12th and 8th centuries BC, for example the whole of the North between the Tees and the approaches to Edinburgh, and much of Wales. Perhaps the most telling signs of this slump are those Late Bronze Age settlements – of new forms – such as the Mucking ringworks (BAR, 83) which overlie abandoned Second Millennium field systems, on lands which previously had been intensively farmed and settled.

The settlement abandonment and population loss will have been greatest on the marginal lands, but many more favourable areas, such as the chalk and the Fen Edge, did not escape. I have argued that there

"No settlement between the 12th and 8th centuries BC"

is some evidence of a flight of population and settlement to river margins. This is reflected in the Thames Valley by the appearance of riverside and island settlements such as Runnymede (there are none pre-1200 BC). There is also a shift in axe distributions to the river and its margins, and a rise in the number of axes known from the 12th-11th centuries, against a marked axe slump for this period in most areas. In my 1985 BAR paper, I linked a severe dip in the numbers of axes known for the 12-11th centuries to this decline: a reflection of a greatly reduced number of users. But at the same time the range of weapons increases dramatically, as might be expected of a time of fierce competition for land and resources.

Clearly this was a crisis which affected lowland and upland, if not so severely in the case of the former. But recovery had to come. The demographic curve following the 14th century AD population collapse typically shows a centuries-long trough followed by a sudden upswing; a population in such circumstances may multiply several-fold in a century. Exactly this seems to have happened in the Iron Age Aegean, where the demographic surge in the late 9th and 8th centuries was such, according to the Greek writers, as to prompt the colonisations which began in the

"The general assumption that as the Bronze Age progressed so everything got bigger and better"

8th century. A similar curve seems to be indicated for Britain, with the period from the 12th-8th centuries (most of the "later" and Late Bronze in effect) a time of shrunken population and settlement, with recovery coming only at the end of the period. Throughout Britain settlements of the 8th-7th centuries seem much more numerous than in the preceding few centuries. Everywhere settlement appears again on lands for which there is little or no Late Bronze Age evidence, from Fengate (BAR 83) to the Cornish Moors (Arch J 1957). The most

impressive sign of this population rise is the reappearance of settlements, in the form of ring-ditch house farmsteads and stockades, in the archaeological record of the uplands of north Britain; vast tracts which appear to all intents and purposes empty of human activity between the 12th and 7th centuries BC. (Burgess, 1984).

Two major problems hinder an objective review of settlement and demography in the 13th-11th centuries: firstly the general assumption that as the Bronze Age progressed so everything got bigger and better. There is no room for blips and reverses in such attitudes. Secondly there is the present dislike of simple explanations. Catastrophist theories are widely held to be too black and white. Considering how difficult it has been to persuade colleagues of a general contraction of settlement and population in Britain, which has the most thoroughly explored and complete Bronze Age settlement sequence in Europe, it is going to be no easy matter to make the point for other regions, especially those, such as most of Atlantic Europe, where the settlement record is patchy at best. But the pointers to 12th century problems can be found in many parts of Europe.

Firstly, France: considerable areas of the west show a slump in 12th-11th century axe numbers similar to that observed in Britain. In a colloquium at Nemours in 1986 (published in 1988 by Brun and Mordant in the Memoires du Musee de Prehistoire d'Ile de France) I calculated the figures for the Paris Basin and Picardy and compared the fall of more than 50% with that for northern England, Wales and the Marches. Unfortunately the settlement record in these areas of France is too fragmentary to help much. At the colloquium, author after author used the words "Les habitats sont mal connues", or their equivalent. In eastern and central France, however, several writers pointed to a marked *rupture* or *cesure* in the archaeological record between Bronze Final IIa and IIb (ie between Hallstatt A1 and A2 in more familiar terms: c.1100BC in conventional dates: Brun, Petrequin, Villes). Unfortunately this break has been explored mostly as it relates to burials and associated artefacts; known settlements are simply too few to draw demographic conclusions, though Petrequin pointed out that the *rupture* is marked in some regions by a dramatic increase in the use of caves as temporary shelters.

For Franche-Comte, Petrequin (ibid, 223.fig 8) has a particularly telling chart making clear the *rupture* in every aspect of life at this time, with settlement pulling out of the uplands and suddenly focusing on the lowlands; in the 11th century settlement appears spectacularly on the lake margins. In other areas, too, such as Alsace, Piningre argued that there was some withdrawal of settlement from the uplands in the 12th century in favour of valley/plain locations. However Primas envisaged a contrary picture for Switzerland, where she claimed that there was an increase in the number of upland and lowland settlements alike.

In Spain, especially the thoroughly searched south, the evidence for crisis seems clear at first glance. There is an obvious *rupture* with the end of the Argaric culture (and neighbouring equivalents), so rich in its evidence for every aspect of life. The contrast with what came next is startling. "What *did* come next?" is a more appropriate response. The traditional end date for the Argaric, c.1200 BC, has recently been reiterated both by Gilman (Gilman and Thornes, 1985) and Harrison (1988, 25-8). Many Argaric sites ended in violent conflagration; in very few cases is there any occupation immediately after the Argaric. Most sites were abandoned, and if reoccupation came at all it was only after an interval of unknown duration, and is then "Late Bronze Age". An end of the Argaric at c.1200 BC would fit perfectly with the catastrophe hypothesis advanced here.

"Les habitats sont mal connues"

Unfortunately, it simply will not do. Some Spanish writers have realised that this is much too late (cf, for example, Almagro Gorbea, 1986, 343 chronological table; here the Argaric is followed by a Bronce Tardio filling the centuries c.1500-1200 BC). To the outside observer the Argaric has all the appearance of an Early Bronze Age phenomenon, a southern Spanish equivalent of the Wessex Culture or "Les tumulus d'Armorique", complete even with faience beads and dagger graves. This would assign it to the first half of the Second Millennium BC. If we reject the vague parallels with Aegean material on which the traditional end-date depends, there is nothing in the Argaric which would prolong it beyond 1500 BC. This becomes ever clearer as more and more C14 dates for the Iberian Bronze Age come to hand. The large number of C14 dates now published from the deeply stratified site of Fuente Alamo (which does have occupation following the Argaric) seems to put the matter beyond dispute: eighteen dates range from c.2020-1340 bc. The post-Argaric Bronce Tardio levels which immediately follow have four dates c.1450-1210 bc (Schubart, in Historia de Cartegena, II, 1986).

What fills the period after the mid-Second Millennium is a problem over much of Iberia. Late Bronze Age settlements in profusion do eventually appear, but are difficult to date, for they begin practically everywhere after a Middle Bronze Age hiatus of unknown duration (Chamorro, AJA, 1987)

On the southern Meseta the situation parallels that in the South quite closely. There is a remarkable earlier Bronze Age development represented by the motillas (Molina and Najera, Madrider Mitt., 1978). These end, often amidst violent destruction, and are abandoned, and there is then little or nothing to fill an unknown span of time, likely to be centuries long, until Late Bronze Age settlements re-emerge in the archaeological record at new sites. There is thus major dislocation between earlier and later Bronze Age in Iberia. At present, though, it appears to fall nearer 1500 than the 12th century BC.

In Italy the evidence is variable. In *Italy before the Romans* (ed Ridgway and Ridgway, 1979) Peroni pointed to a widespread, often sudden and violent disruption in settlement development between the Recent and Final Bronze Ages, ie c.12th century BC. The *terremare* in particular come to an abrupt and dramatic end, and the great plains of the Middle Po then seem to have been abandoned until the Iron Age. On the other hand there may have been a flight to peripheral areas, especially northwards. The Lake settlements of the north were also given up about this time,

"In Etruria, almost total discontinuity between Bronze and Iron Age settlement"

though whether as suddenly and dramatically is less clear. Over the north as a whole there seem to have been far fewer settlements after this period of disruption than before (Peroni, ibid). In central Italy the evidence at present appears more ambiguous: in the south-centre as many sites remained in occupation as were abandoned (Peroni, ibid).

In Etruria, however, almost total discontinuity between Bronze and Iron Age settlement has been claimed (cf Delpino, 1979, 39), yet the current view is that population continued to rise into the Iron Age In the south the evidence is again patchy. Some Apulian sites were abandoned (La Geniere, 1979), but others apparently remained in occupation into the Iron Age. On the other hand it is clear that throughout the south Iron Age settlements and cemeteries occupy new sites, without Bronze Age precursors, and that there must have been some sort of late Second Millennium dislocation.

The aim of these notes is to persuade colleagues to examine, dispassionately, the settlement records of their own regions. Clearly dislocation, hiatus, *rupture, cesure* – all these terms have been used – occurred in many parts of the Old World in the late Second Millennium BC. In some regions this took the form of a collapse of settlement and population. Was this a Pan-european disaster, in population terms akin to those of the 6th and 14th centuries AD, and in cultural and political terms on a par with the disasters which overtook Europe in the 4th-6th centuries AD?

Volcanoes and Population

My studies of this Bronze age disaster led me to look at other examples of population collapse, and in 1985 I presented a population graph for Britain extending from the Mesolithic to recent times, which was characterised by periodic ups and downs, the lows being the result of catastrophic processes in which an overall loss of the order of 50% in a century was envisaged. This was on the level of the historical population disasters of the 6th and 14th centuries AD. Just such a population collapse, I suggested should accompany the contraction and dislocation of settlement in the 12th century BC.

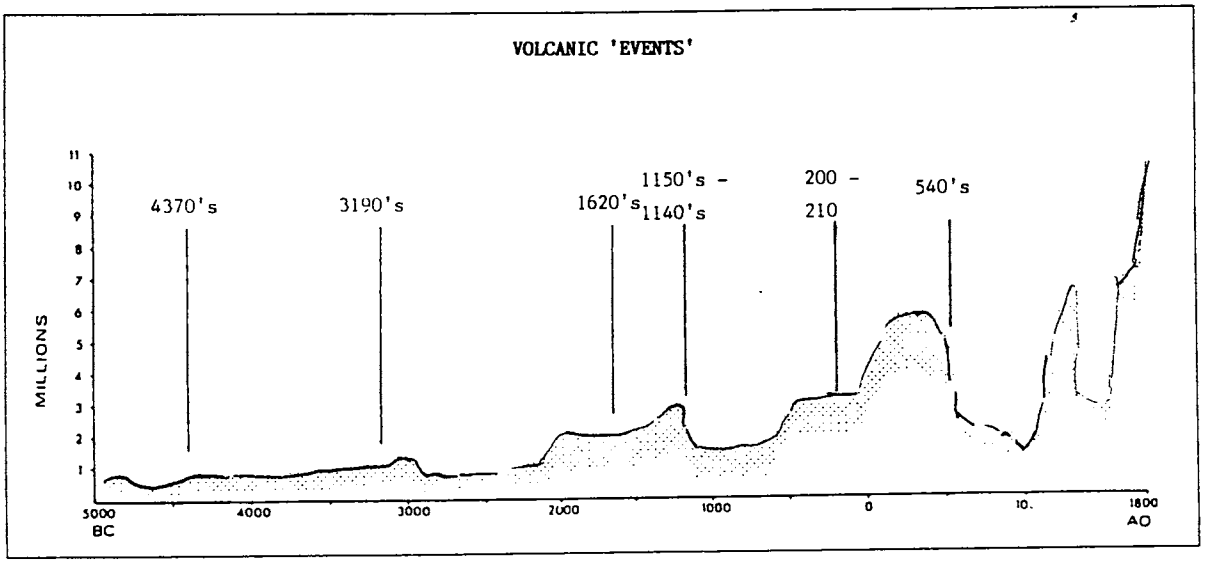
What struck me immediately on reading Baillie's 1988 account of his volcanic "events" was that so many of them matched, more or less, the ups and downs on my 1985 population graph. I have plotted the two together on the accompanying figure and offer some comments on their possible significance.

In radiocarbon terms the event of the 4370's BC corresponds with the period around 3500 bc when the first Neolithic colonists arrived from the Continent; hence the rise in my curve. Recent fashion has been to play down the immigrant contribution to the British Neolithic, but someone has still to bring the sheep/goats and seedcorn; and medically, what would be the impact of outsiders from the Continent on an indigenous Mesolithic population which shows such scanty signs of having had any contact with the Continent since 6500 bc? The historical analogies suggest disaster for the indigenes. One question which has

"Someone has still to bring the sheep and the seedcorn"

received scant attention is why Britain and Ireland were only settled from c.3500 bc, when there were farming groups on the French and Dutch coasts for some centuries before this. Baillie's "event" perhaps provides the catalyst, the final straw which launched the boats – not necessarily in vast numbers.

The event of the 3190s BC coincides neatly with the point around 2500 bc at which the Late Neolithic has for some time been divided from the earlier Neolithic. No more causewayed enclosures, long tombs and the like, and instead henges, passage tombs, Peterborough and



Correlation of Baillie's volcanic 'events' and a population graph for Britain

Grooved wares, and all that goes with them. Just how a volcanically-influenced climatic downturn could contribute to such changes I leave to others. The answer presumably lies in knowing more about Neolithic settlement and agriculture. The match between "event" and fall on my graph should have been closer; my intention in 1985 was to dip the curve at a point equivalent to a calibrated date of c.2500 bc. I erred.

"When an economy is expanding, it can survive a disaster."

The event of the 1620s BC is the one that got away. I still cannot see any firm reflection of this in our Early Bronze Age record. There may have been a hiccup in the expansion of settlement in the marginal lands, especially the uplands, which had been gathering strength since c1800/1700 bc. A temporary setback or retreat would be very difficult to spot given the stratigraphic and artefactual exiguities of our upland sites. But when an economy is expanding, it can survive a disaster.

We have already discussed the event of the 1150s and 1140s; the next event is that of 207 BC. Somewhere about this point my graph begins to show the ever rising population curve which characterises the late Iron Age and Romano-British period. For long, Iron Age specialists saw the 3rd century as a time of trouble, characterised by Marnian invaders and defence building and refurbishment; concepts which seem now to belong to another age. Yet in my own area, North-East England, there is good evidence for 3rd century attention to defence building; and by the 2nd century settlement had spread to marginal areas which had apparently been neglected for more than a millennium. Colleagues may care to scrutinise their own regions with a 207 BC event in mind.

"If society is at the margins, a volcanic 'event' may trigger complete collapse"

This leaves the final event of the 540s AD. As Baillie himself has noted, the 540's were a time of cold and plague. In particular this decade saw the pandemic of Justinian, with an effect on world population as catastrophic as the better known Black Death of the 1340s (see Russell, in *Demography*, 1968).

That volcanic "events" could be linked both to contractions and expansions of settlement and population might seem like having one's cake and eating it. The explanation is perhaps that it depends on the level of population and settlement at the time. If there is room for manoeuvre, as in the 4370's, 1620's and c.207 BC, then settlement can be shifted and there is opportunity for expansion both of settlement and population. But if society is already at the margins, with nowhere else to go, as in the 12th century BC, then the volcanic "event" may well trigger complete collapse.

Acknowledgements

My thanks to Mike Baillie and Victor Clube for answering my queries; to Peter Topping for his comments on this paper; and especially to Doris Davies, for coping with my scrawl.

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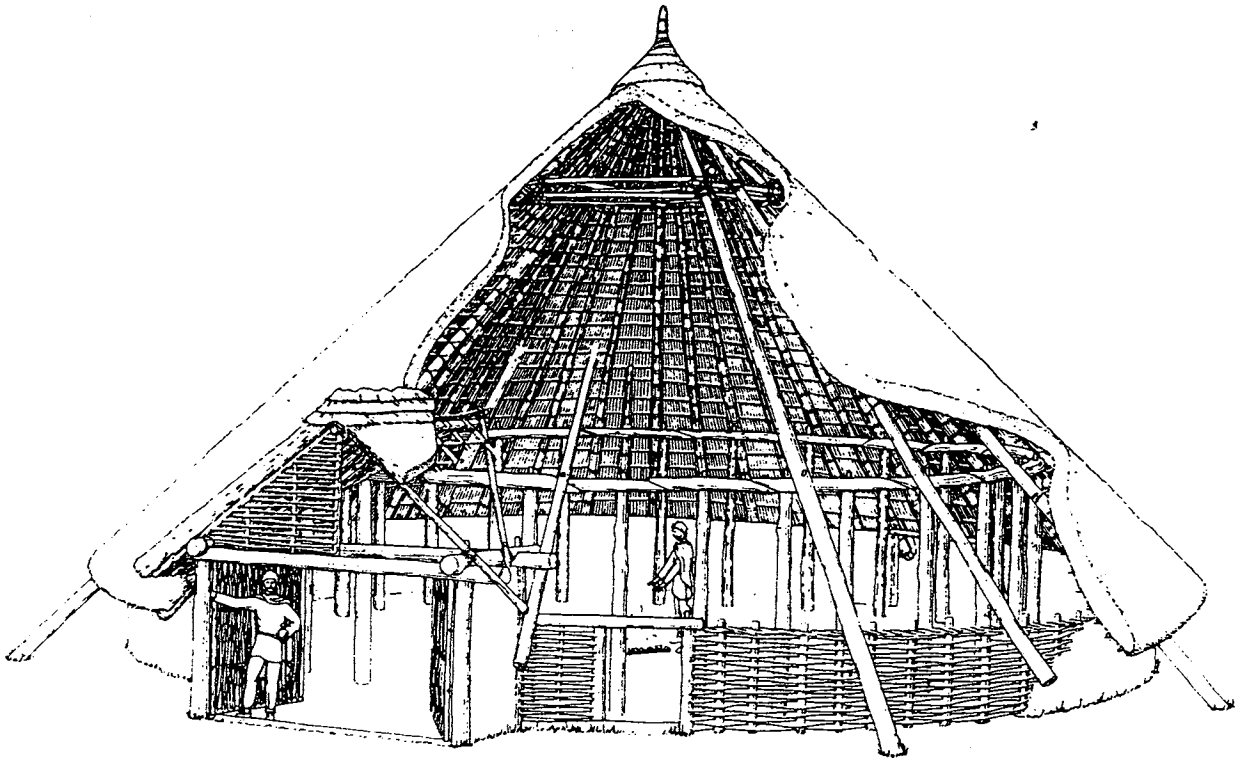
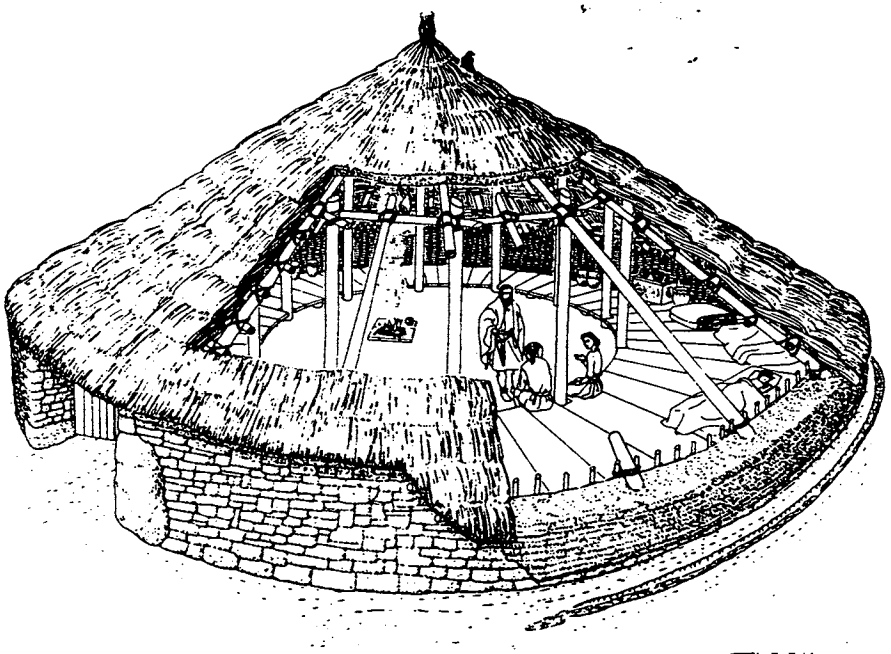


Reconstruction of a timber roundhouse with wattle and daub wall (Longworth,I & Cherry, J (eds) *Archaeology in Britain since 1945*, London, 1986, 62)



Reconstruction of a bronze age roundhouse at LAIRG, Sutherland, using timber, stone and turf for the wall (McCullagh, R *Lairg: the archaeology of a changing landscape*, Edinburgh, 1992, 9)

Proc Soc Antiq Scot, 120 (1990), 55–60

Agricola in the Highlands?

David J Breeze*

ABSTRACT

The lack of place-names in Ptolemy's Geography *north and west of the Great Glen suggests that Agricola did not penetrate into these areas. The location of the place-names in the territory of the Vacomagi suggests that he may have marched back from Mons Graupius through the Spey and Tay valleys.*

In a recent volume of the *Deeside Field* the late A A R Henderson suggested that we should consider the possibility that the battle of Mons Graupius took place north of Inverness in Sutherland or Caithness (Henderson 1984). His argument was based primarily upon the statements in the *Agricola* (30, 32 and 33: cf 27), in the speeches Tacitus put into the mouths of Calgacus and Agricola, that Mons Graupius was fought at the end of the island. This was supported by interpretation of other information. Mons Graupius ought to have been fought in the Highlands, which is where the Caledonians lived; the Boresti ought to have lived beyond Mons Graupius as they had not previously had contact with the Romans while their name may indicate that they were the people who lived at the end of the island: Agricola cannot simply have retraced his steps from Mons Graupius as he marched through the territory of new tribes and these tribes may have been those recorded by Ptolemy as living in the far north and west of the island. Henderson suggested that Agricola 'returned to the south of Scotland by the one major route through the Highlands, namely the Great Glen'. There is evidence, however, to suggest that Mons Graupius was not fought either north or west of the Great Glen.

Ptolemy published his *Geography* in the mid-second century (Rivet & Smith 1979, 103–47). When his co-ordinates are used to draw a map (illus 1), Britain north of the Solway Estuary is seen to be turned at right-angles to its true position. A convincing reason for this eastwards turning of north Britain through 90° has recently been suggested (Smith 1987, 47–8). This is that Ptolemy had determined that the absolute northern limit of the known world should be set at 63° N and thus Scotland had to be turned through 90° in order that it did not extend to 66° N, which is where the north point of the island would have lain if the map had been drawn correctly. Mann (1990) suggests that Ptolemy's determination of the northern limit of Britain related to the Greeks' belief that life was not possible north of that latitude, thus rendering the existence of land unnecessary. In the illustration and in the discussion below Ptolemy's map of north Britain has been realigned by turning it northwards through 90°.

There is general agreement that Agricola's campaigns – and presumably their aftermath – were Ptolemy's main source of information for geographical, tribal and place-names in north Britain.

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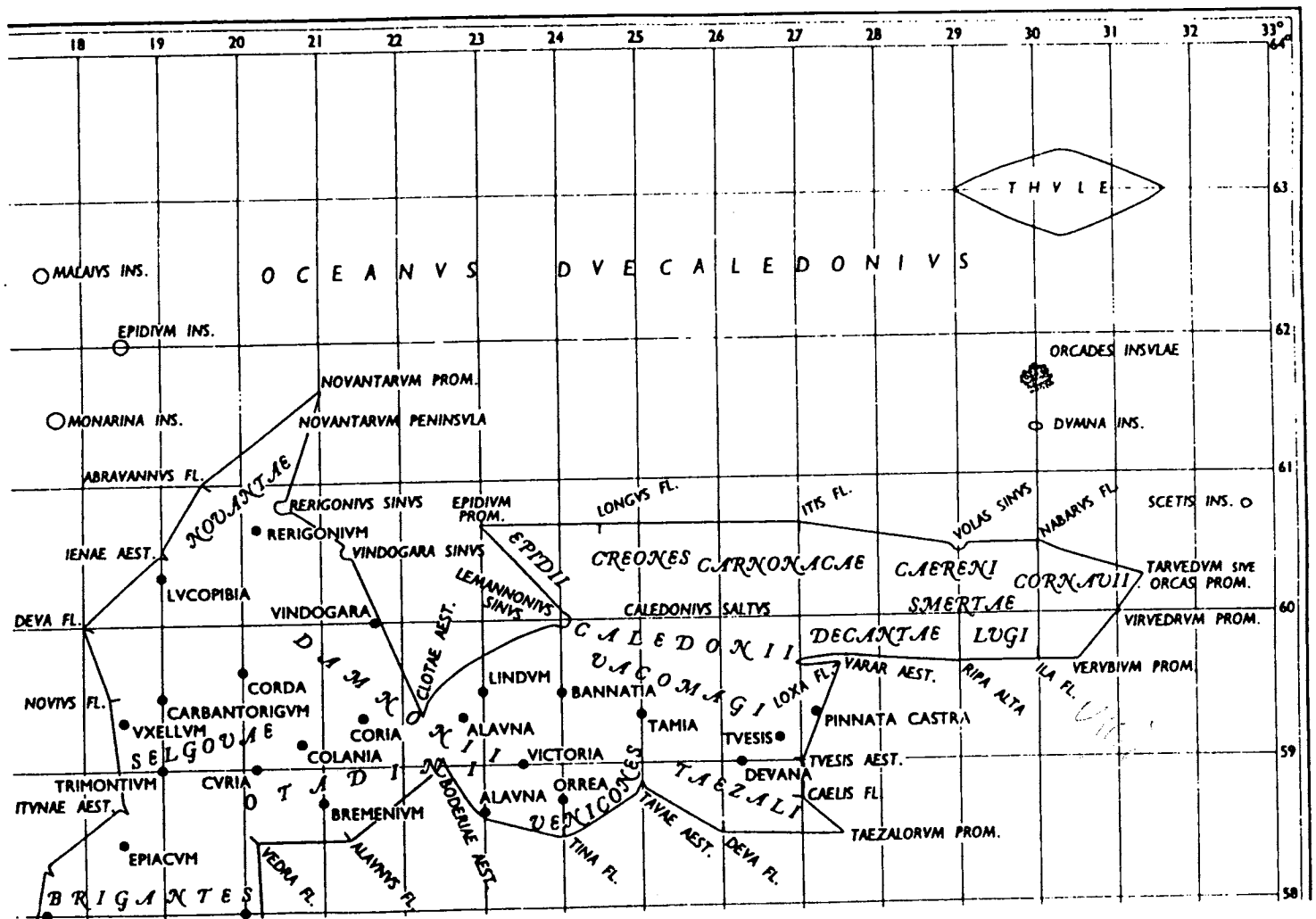
Indeed Smith (1987, 18) has argued that as Ptolemy did not list all the first-century forts north of the Tay then the 'map is wholly Agricolan in origin. Perhaps recording Agricola's great achievement on his return to Rome', omitting the forts built under his successor. Sixteen tribes are named north of the Tyne-Solway isthmus, and their locations given.

Within the tribal territories south and east of the Great Glen at least one place-name is given within each tribal territory, and more often several. Some of these place-names are likely to have been native sites rather than Roman forts (Richmond 1958, 139; Mann & Breeze 1987). Some lie deep inland and must reflect information gathered by Agricola's army. They contrast with the geographical names, which are all coastal in distribution, and the tribal names north and west of the Great Glen, which also could all have been obtained as a result of maritime activity. Agricola's fleet is known to have sailed round Britain and is the most likely primary sources for these names, though Henderson argued that some tribal names could have been recorded by the army after Mons Graupius.

The lack of any place-names within, north and west of the Great Glen strongly suggests that this area was not penetrated by Agricola's army and that therefore Mons Graupius was not fought beyond Inverness. If Agricola had marched through this country it might be expected that at least one place-name would have found its way on to Ptolemy's map and to argue that such place-names had been recorded but subsequently lost is to push special pleading too far. The simplest solution is to assume that no known place-names means no army presence.

Ptolemy does record place-names inland, for example, in the territory of the Vacomagi. The location of this tribe has long caused problems. Two of their place-names, *Pinnata Castra* and *Tuesis*, Ptolemy locates on or near the Moray coast, while a third, *Bannatia*, appears to be in Strathmore. Watson (1926, 22) accepted that the tribe straddled the Mounth, but Richmond (1958, 142) rejected this, locating it in Strathmore. This was undoubtedly connected with his belief that *Pinnata Castra* 'ought to refer to Inchtuthil', a conclusion he had reached many years before (Richmond 1922, 299). Mann (1968, 307) and Rivet and Smith (1979, 128, 141, 485) reject this suggestion, firmly placing *Pinnata Castra* on the Moray coast. The place-name *Tuesis* is tied to the river *Tuesis*, normally regarded as the Spey: *Pinnata Castra* lies on the coast to the west. These two place-names locate at least part of the tribe in the lower reaches of the Spey.

Ptolemy lists four places within the territory of the Vacomagi (*Geography* II, 3, 8–9). We have already seen that *Pinnata Castra* lies on the coast west of the mouth of the Spey. The other three places, *Tuesis*, *Tameia* and *Bannatia*, lie inland. When the alignment of Scotland is righted by turning it through 90°, it becomes apparent that these three places lie along a north–north-east/south–south-west axis. The river Spey, the most substantial river cutting through the north Highlands, flows roughly north-east from its source. These lines are close enough to suggest that coincidence is not involved and that *Tuesis*, *Bannatia* and *Tameia* lie in Strathspey, and that this is the location of the tribe. Thus the Vacomagi can be pulled wholly north of (or into) the Grampians, leaving Strathmore to the Venicones (Rivet & Smith 1979, 141, 491) or the Caledones (Watson 1926, 21; Wainwright 1955, 52; Hind 1983, 375; Barrow 1989, 163). Interpreting Ptolemy's description of the Caledones as stretching from the Lemannonian Gulf on the west coast (probably Loch Long, Loch Fyne or Loch Linnhe) to the Varar estuary on the east (Beauly Firth) as indicating a distribution along the southern and eastern edges of the Highland massif (where the modern place-names of Dunkeld, Schiehallion and Rohallion occur), rather than through the Great Glen (Richmond 1958, 141; Rivet & Smith 1979, 141; 291), would entail their territory crossing that of the Vacomagi, as Hind (1984, 375) realized, but Barrow (1989, 163) ignored. Hind dealt with this problem by arguing that the Vacomagi, Taexali and Venicones were all part of a Caledonian grouping of tribes, placing greater weight on Tacitus's reference to the region known as Caledonia over Ptolemy's recording of the Caledones as a separate



ILLUS 1. Ptolemy's map of north Britain. (Based on the *Map of Roman Britain*, reproduced by kind permission of the Ordnance Survey)

tribe. Mann (1974, 36; cf Mann & Breeze 1987, 90) accepted the difference in usage, arguing that the formerly powerful tribe of the Caledones had given its name to the country, rather as the otherwise obscure Graei had given their name to Greece, but had subsequently been pushed back into the Great Glen and possibly the upper glens (cf Richmond 1958, 142) by incomers – the Vacomagi, Venicones and Taexali – leaving relics of their former extent in the place-names of Dunkeld, Schiehallion and Rohallion an argument which Barrow (1989, 163) finds unbelievable. Clearly the problem of Caledonia and the Caledones has not yet been solved.

As Rivet and Smith note (1979, 141) it is impossible to reconcile the placing of the Vacomagi in Strathspey with the normal identification of *Bannatia* with Dalginross in Perthshire or their own suggested identification of *Tameia* with Cardean, or, we may now add, Maxwell's proposed identification of *Bannatia* with Bochastle and *Tameia* with Doune (Maxwell 1984, 221). The identification of *Bannatia* as Dalginross resulted from its position on Ptolemy's map and on its relationship to other places (Richmond 1922, 295). If, however, its location within the territory of the Vacomagi is regarded as paramount, then the identification with Dalginross cannot stand.

There is little point in speculation about the location of the places in the territory of the Vacomagi. No Roman forts are known to lie in Strathspey, but in any case it is more likely that these are native place-names. No large hillforts are known in Strathspey (cf Feachem 1966, 59–87, and end map) but there is no need for these place-names to refer to hillforts as opposed to other settlements. The name, *Pinnata Castra*, suggests a Roman place, but, if not Roman, Burghead must be the most likely candidate. Although no pre-Pictish material has been found there, a radiocarbon date of 260 ad, centred on AD 290 after calibration, may hint at pre-Pictish occupation (Edwards & Ralston 1978, 206–7).

The information about these places, wherever and whatever they were, presumably had been gathered by Agricola's army, the only Roman force known to have penetrated into north-east Scotland before the time of Ptolemy. This army must have either reconnoitred Strathspey, or perhaps even marched through it: Rivet and Smith (1979, 141) accept that Agricola must have invaded the territory of the Vacomagi.

Roman marching camps have been located in eastern Scotland (St Joseph 1973, 214–46; 1976, 1–28). Disagreement surrounds the date of some of these camps, but all known discoveries are restricted to the areas east of the Grampians: the valleys of Strathallan, Strathearn and Strathmore and the more broken country of Aberdeenshire. No camps are known within the Highland massif though it is not clear how far they have been sought (Frere 1980, 95). In any case these camps reflect no more than the state of today's knowledge. New camps are being found, almost on an annual basis, and new discoveries could considerably alter our appreciation of the location of the Roman campaigns. It might be argued that, so far as eastern Scotland is concerned, since the 18th century we have been doing no more than infilling the map whose outline was drawn by William Roy, for there are still only two or three camps known beyond his most northerly camp, Glenmailen or Ythan Walls (Roy 1793, pls I & LI; cf St Joseph 1977, 142, fig 11). Yet new discoveries in south-western Scotland, the forts and fortlets in the 1930s and 1940s and the camp at Girvan in 1976 (St Joseph 1976, 6–12; 1978, 387–400), have led to a radical re-appraisal of our view of the relationship between the Roman army and this area: the same could happen *vis-a-vis* the Highlands. Strathspey may be the best place to start the hunt for Roman marching camps in the Highlands.

Is it possible to determine whether Agricola's army merely reconnoitred the territory of the Vacomagi in Strathspey, presumably before Mons Graupius, or used it as a through route after the battle? The size of camps, if found, might help – so would sure knowledge of the site of the battle! There is, however a hint ready to hand.

After Mons Graupius Agricola led his army into the territory of the Boresti (*Agricola* 38). This

tribe must have lived by the coast because while there Agricola ordered his admiral to circumnavigate Britain. The army marched slowly back to winter quarters through the territory of new tribes. It seems not impossible that part of this route lay through Strathspey, one of the main routes into the Highlands from the north, the line of march continuing perhaps down Glen Garry and Strathtay, the army emerging from the Highlands at Dunkeld, near the (?later) site of the legionary fortress at Inchtuthil. It might be argued that the Roman army could not find its way around the Highlands, but that would be to ignore the geographical sophistication of the Romans and their use of intelligence in planning their route when on campaign (Breeze 1967, 14–15). Several sources refer to the use of merchants, travellers, refugees and other natives, prisoners, deserters and reconnaissance parties to gather intelligence. Agricola certainly reconnoitred and used merchants, refugees and prisoners to gain intelligence (*Agricola* 20, 24 & 25). Moreover, the Highlands were not *terra incognita* to the natives. People had lived in the area for several thousand years: routes will have been known and the Romans could easily have gathered information about these routes. There is no reason to believe that the Romans would have experienced any greater difficulty finding their way round the Highlands of Scotland than any other hostile part of the world.

The location of Mons Graupius at either the Pass of Grange (Ogilvie & Richmond 1967, 65) or the Culloden area (Breeze 1982, 50) would suit this interpretation. A battle at either site would have allowed Agricola subsequently to move into the lands of the Boresti, who would accordingly be placed on the Moray coast, and then march through the territory of new tribes, in Strathspey and the Highland massif.

In summary, it is suggested that Agricola may have marched back from Mons Graupius through the Highlands, utilizing the Spey and Tay valleys, and that Roman marching camps might with profit be sought there.

ACKNOWLEDGEMENTS

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The early peoples	7000-2000 BC.
Tombs	4000 -2000 BC
Ceremonial Centres	3000 - 2000 BC
The First Metal Workers	2000 - 1000 BC
Climate & Social Change	100BC - AD 79
Celts & Romans	AD 79 - 563
Picts & Scots	AD 563 - 795
The Viking Age in Scotland	AD 795 - 1100

HEKLA — 1169.

192 ALASDAIR WHITTLE.

Climate, grazing and man: notes towards the definition of a relationship.
[HARDING, A (Ed) Climatic Change in Later Prehistory : 1982 : Edinburgh].

192 The reality of climatic change in most of the British Isles spanning the transition from the second to first millennium b.c. can hardly be doubted. The bulk of British studies have been concerned with increased peat growth both in lowland raised bogs and in upland blanket bogs (reviewed recently by Turner (1981) and Tinsley (1981) suggesting both decrease in temperature and increase in rainfall. Isotope studies on lake sediments in Gotland in the Baltic (Mörner & Wallin 1977) and on polar ice cores in Greenland (Dansgaard et al 1969) seem to confirm the same general trend and there is a general consensus that a drop of up to 2°C in summer temperatures and an increase in annual rainfall (generally unspecified) may be in order (eg Lamb 1977, Taylor 1980). It is worth noting that this picture is imprecise. There may be regional west-east variation starting with evidence for wetter conditions by 3950 BP (2000 bc) in Ireland, but not elsewhere, and perhaps wisely later in different dates for renewed peat growth on either side of the Pennines (Tinsley 1981). This provides a long period, from 3350 to 2550 BP. (1400 to 600 bc) over which changes were spread, and isotope studies also support this. Nor is the seasonal distribution of changes yet explained, though attempted in some detail for Wales by Taylor (1980) Despite such imprecision, the prehistorian is confronted with the problem of accommodating changes of this nature into wider explanations of contemporary change in the archaeological record. The temptation — as in much else — has been

to react with simple rather than complex hypotheses, and in exploring contemporary social and economic change to invoke a rather crude environmental determinism. In this way conditions have been widely seen as automatically more difficult for a variety of activities such as cereal cultivation and animal husbandry (Fleming 1976; Coles & Harding 1979 pp 475, 493; Burgess 1974 p 195; 1980 p 157). cross-Channel sea crossings (Piggott 1979-3) and even astronomical observation and dry-land religion (Burgess 1974). Perhaps because it has seemed so clearly to have changed, climate has then been used in a variety of ways to 'explain' the nature of the later Bronze Age (from the Acton Park phase in Burgess 1974, but only from the Penard phase in Burgess 1980) shifts in economy and settlement, or more specifically reduced cross-Channel contact from the Hallstatt D phase onwards. Sometimes straightforward muddy catastrophe seems to be envisaged (Burgess 1974) sometimes a significant tipping of the scales is suggested (Burgess 1980) but there has been a general tendency in accounts of this kind to give observed climatic change
192 a decisive role in all activities, at all seasons, and over the country as a whole.
193 This paper proposes instead that prehistorians should investigate change of this kind with far greater subtlety and discrimination, by actually exploring the effects of climatic shift on the economic activities involved rather than merely assuming them, and by giving due consideration to the place of these activities in contemporary society

My main interest is in the uplands of the country, here defined as over about 210 m above sea level, a substantial part of the British land-mass (eg Pearsall 1971 fig 12). I have made the working assumption — to be justified later in the paper — that the lowlands, including therefore much of the Wessex chalk downland, were not radically affected by either temperature drop or rainfall increase of the magnitude discussed since economic activities potentially at risk would not have been near critical environmental thresholds. There is considerable evidence that cereal cultivation is not likely to have been affected: field systems and cereal remains can be documented on various areas of the chalk downland more or less continuously from the beginning of the later Bronze Age right on into the Iron Age (eg Bowen & Fowler 1978), and linear boundaries, which have been connected with pastoral activities because they disrupt previously established field systems, do not in fact provide unambiguous evidence in a late phase of the Bronze Age for a pastoral interlude, apart from being datable to a wide timespan (Bowen 1978). Pasture may have been improved in the lowlands by an increase in rainfall, though initial growth would have been slowed by a decrease in temperature (see below). There is evidence from contemporary studies that
193 grass-productivity can be increased by as much as a factor of 2·5 by increased rainfall

193. (Evans 1960) and even now in part of eastern England there is a 'soil moisture deficit' for grasses, which are shallower-rooted than cereals (Jenkinson 1978). For well watered attractions such as Romney marsh, figures of 10-12 sheep per acre all year round have been quoted (Hopkins 1978). Increased rainfall may thus have been welcome. Explanations have been proposed as over the East Anglian fen edge around 2980 BP (1000 BC) where it has been suggested that a rising water table affected not so much the summer grazing in the fens themselves but the winter grazing or holding land on its edge (Pryor 1980). Even here one could suggest the likelihood of a retreating fen edge rather than the total collapse of the fen land grazing system, and for each possible loss of this land there were gains to be made from new coastal grazings (eg Cunliffe 1980) themselves with rich pasture (van Zeist 1974).

Turning to the uplands themselves, difficulties for cereal cultivation through shorter growing seasons, colder and wetter conditions and lower altitudinal limits have been outlined by Fleming (1976). It is possible that there was recent abandonment of upland agriculture though this has to be proved rather than assumed. Johnson (1980) has noted a quantity of sites in S-W England above 180 m with continuity well into the first millennium BC, and upland sites on the W Yorkshire limestone pavement are not certainly pre Iron Age (Fleming 1976). It may anyway be argued that

p193 agriculture was a minor component of upland exploitation.

BURNT MOUNDS.

Buckley, V. (Ed.) Burnt Offerings: Dublin 1990.

Syntheses C J Russell-Wilcock Size, Shape & Context. p87

The Scottish sites vary greatly in size. Largest known Jarlshof, or Fair Isle – c 40m across At the other end of the scale sites like Kebister – less than 2m across. The bulk of the sites lie between 5 m and 15m across and are less than 1m high

As much variability in shape. Classic Burnt Mound is said to be crescentic or kidney shaped, but of 40 sites in SW Scotland only 15, roughly 1/3 had this shape. Halliday notes (supra) that 75 ex 110 sites recorded by the RCAHMS in Scotland are kidney shaped whilst Hunter notes that a proportion of the Fair Isle sites are crescentic in form.
The others range throughout Scotland, from amorphous mounds, or groups of mounds to multi-pealed annular settings with central or peripheral hollows.

Cooking troughs found within them – sometimes outside – connect place between the type of the barns. A trough was found at only one of the 2 sites excavated at Kilcarran Hill. None of these sites revealed hearths

The classical Welsh shaped site normally has a trough between the open arms. Unless excavation is extensive, the troughs will not be revealed. In the case of settlement-associated material troughs may lie some distance from the deposits of burnt stone

Scottish Burnt Mounds consist of burnt stone set in a dark soil matrix which also contains charcoal and ash. This material is so distinctive that is termed "Burnt Mound Material" even when found in other contexts Hard, unweathered and relatively heat-resistant stones were selected by the burnt mound users, the most common being hard sandstones. In the East Rhins area the locally available greywackes were used, while on Arran metamorphic types were employed.

Lewisian gneiss which is reduced to a coarse granular mass by heating and quenching (outer Hebrides)

Wood seems to have been the preferred fuel, but peat was extensively used in the Northern Isles.

Routine analysis of the soils that form the matrix of the burnt mounds in the East Rhins area revealed the soils were uniformly acidic. The relative phosphate levels were higher for the burnt mound deposits than for the local topsoils or subsoils except for the two medieval mounds. The quantities of organic matter present in the soils were also high, as revealed by the "Loss on Ignition" (LOI) tests. High phosphates are normally indicative of the presence of decayed bone, while the high LOI values indicate that the soils of the deposits have been enriched by the addition of organic material. These tests seem at face value to support the contention that the burnt mounds were used for cooking. However the high phosphate values might have been explained by the presence of wood ash from burnt fuel were it not for the two later mounds. It may therefore have some reflection upon the use. The high LOI values could result from the presence of the decay products of unburnt fuel. In the presence of an alternative explanation neither the high phosphate levels nor the high LOI values can be accepted as firm indications that the sites were used for cooking.

That burnt mounds were used for cooking has been disputed but there seems to be no disagreement that they represent the debris created as the result of heating water by the immersion of heated stones.

Buckley suggests that metamorphic or igneous rocks survived repeated quenching to a greater degree than sandstone and thus may generate less than 0·5 m^3 per boiling.

This is less significant in Scotland than in Ireland because most of the Scottish mounds are of sandstone. Furthermore it does not alter the basic observation that these are small and very large burnt mounds with relatively few of intermediate size.

Burnt mounds are invariably located in the immediate vicinity of a source of water. This is all but inevitable given their primary purpose, which is the heating of water.

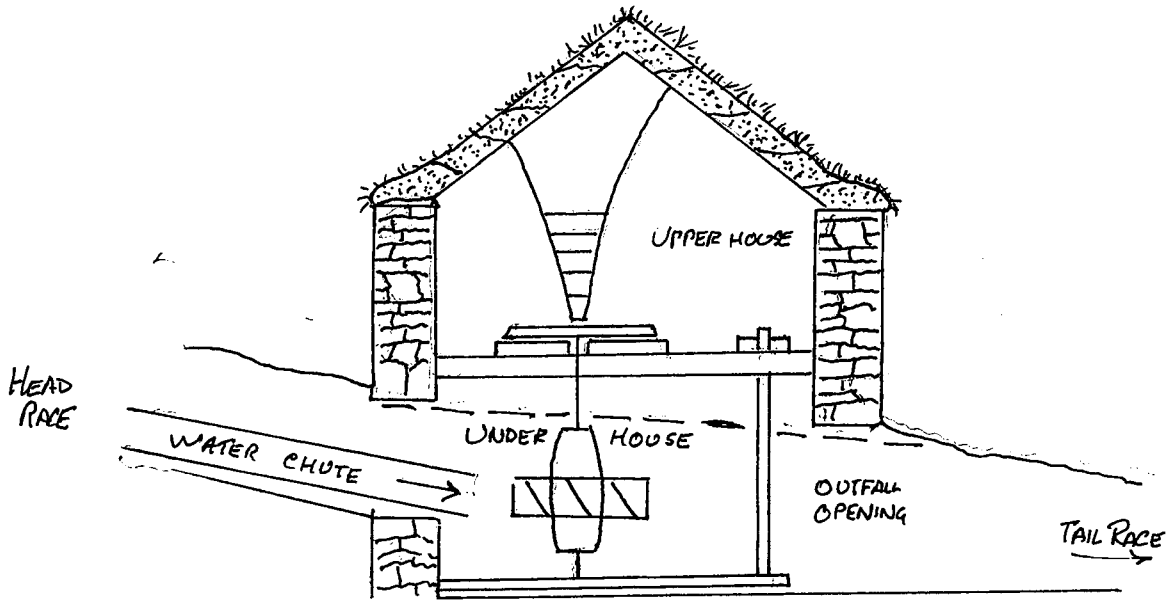
Analysis of data from surveys at Loch and Kildonan will continue to explore the hypothesis that burnt mounds "service" monuments of some other type, from which they are set apart- [? Sauna baths ??].

There are both radiocarbon and Thermoluminescence (TL) dates for burnt mounds sites in Scotland. The radiocarbon dates are most numerous for the southern area, 9 from the East Plains, 4 from Arran and are from Islay. There are also dates from the Strath of Kildonan, Sutherland, from 2 sites (Kildonan 15, and Kildonan 28) The use of peat as fuel in the north has meant that most of the dating there has been TL.

The Northern Isles dates are generally Early Iron Age, with one Late Bronze Age. Two radiocarbon dates from Liddle place it roughly in the tenth century BC (Hedges 1975) The Arran mounds are all Early Bronze Age. In the East Plains a less uniform picture is apparent. Most of the burnt mounds from which samples were taken were on the east side of the Water of Luce. These were all Bronze Age. The 2 mounds excavated on the W side of the river were dated from the 11th & 12th C AD. The Kildonan site was also of the 10th C. AD.

The available radiocarbon date distribution therefore gives the main body of dates between the 20th and 8th C's, BC with an outlier of 5 dates from the Middle Ages. No burnt mounds as such fall into the 17th century gap between these. However it is not devoid of dates associated with burnt stone deposits.

The archaeological record for the first millennium BC is dominated by domestic settlements and fortifications rather than burials. The origins of the Brochs continue to be the subject of controversy.



Section across width of Mill (length of underhouse) based on Hunter example.

0 1 2m