On Megalithic Astronomy

by Gerald S. Hawkins
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Current Anthropology has performed a service for the new field of archaeoastronomy in publishing Ellegård's (CA 22: 99–125) paper with the comments of experts in astronomy, archaeology, and anthropology. The issue will provide a useful focus and reference for scholars. Because of this, I wish to offer a few minor corrections and clarifications for the benefit of future readers.

The book of mine that Ellegård refers to was published in 1965, not 1966, and he omits the publisher (Hawkins and White 1965). This book was not intended to be used as a scholarly reference, being nothing more than an expansion in layman's terms of the article "Stonehenge Decoded," published two years previously in *Nature.* It would have been more appropriate academically and more precise chronologically to have cited Hawkins (1963).

I am puzzled by Ellegård's statement that Hawkins and Hoyle were "more far-reaching" than Thom. On first perusal the words might read as though we took a lead from Thom's work and then extended it; yet Thom's work on precise megalithic lunar observatories was published eight years later. Nor has there ever been a Hoyle-Hawkins collaboration; Hoyle's work has always been entirely independent of mine. It seems to me that the situation, both in the published literature and in fact, was the other way round—Hoyle's and Thom's work was more far-reaching than that of Hawkins. My Stonehenge publication was limited to the astronomical alignments as shown by the structure. According to Burl (1976:305), "Hawkins made a significant step forward in a paper in *Nature,* 1963 . . . with his discovery of the alignments of post holes, stones and archways at Stonehenge to the extreme risings and settings of the sun and moon." Ironically, my work was criticized at the time because the anthropic alignments there were not precise, or not as precise as some archaeologists would have liked them to be.

Actually it was Thom who took the notion of alignments and extended it beyond the simple pattern first found at Stonehenge. It was he who claimed to find precision via natural notches on the horizon, the nine-minute moon wobble, the extrapolation grids, the precise megalithic yard, rod, fathom, and inch, the Pythagorean and other remarkable geometries, etc. It was Hoyle who took the notion of the Aubrey circle computer to the limits of calculating the motion of the sun, moon, and lunar nodes around the ecliptic, likening these to the Christian Trinity, and using the Aubrey circle to predict individual eclipses. His first article (Hoyle 1966) was entitled "Stonehenge—An Eclipse Predictor." This clearly was more far-reaching than my earlier suggestion, namely, that the circles at Stonehenge could have been used as a *Neolithic* computer (Hawkins 1964). My emphasis, of course, was on the adjective "Neolithic," not the noun "computer." As I said in the article (1964:1260) "I believe that the Aubrey holes provided a system for counting the years, one hole for each year, to aid in the predicting of the movement of the moon (the 18.6-year swing). Perhaps cremations were performed in that particular Aubrey hole during the course of the year, or perhaps the hole was marked by a moveable stone." The sun and moon alignments were a fact of the structure, but the counting aspects were speculative. That was why I separated the issues into two papers. The first (Hawkins 1963) dealt with the discovery of the alignments, the second (Hawkins 1964) with possible numerological aspects. However, I had reservations when I made the original suggestion of computer aspects, and even more reservation when Hoyle took the idea so far.

It would appear from Ellegård's article and the associated commentaries that the discovery of astronomical alignments in British megalithic structures is now accepted by prehistorians—or that a greater number of scholars accept it than reject it. What Ellegård cannot accept are the claims for precise alignments based on horizon foresights. Also he rejects the putative observations of the nine-minute wobble (Thom 1971) and the prediction of specific eclipses by numerical methods (Hoyle 1966). He does, however, accede to the possibility that megalithic people recognized the danger period for an eclipse, and he accepts a possible interest in the run of the seasons and the interplay of the orbs of the sun and the moon. I feel very comfortable with all this because, although not referred to directly by Ellegård, it is the essence of my original thesis (Hawkins 1963). As I said then, in commenting on the humanistic meaning of the two dozen sun-moon lines (p. 308): "To determine the anthropological reason for Stonehenge is impossible, and one can only speculate. The monument could certainly form a reliable calendar for predicting the seasons. It could also signal the danger periods for an eclipse of the sun or moon. It could have formed a dramatic backdrop for watching the interchange between the sun, which dominated the warmth of summer, and the moon, which dominated the cold of winter."

Ellegård mentions the effects of a Neolithic observer's missing a moonrise because of clouds on the horizon, pointing out that the observed turning points would therefore tend to fall short of the theoretical extreme. This is a valid comment and would indeed account for some of the offsets in the moon lines at Stonehenge. As I said originally (Hawkins 1963:307): "The moon was difficult to observe because of the variation from year to year. If the midwinter full moon was obscured by cloud, for example, when the declination was $+29°$, then the measured value in the preceding or following year would be $0.5°$ smaller."

Ellegård further mentions the paucity of data on horizon sightings. To that end it is useful to quote from a work that is not included in his list of references (Hawkins 1973:272):

> An ex-meteorological officer reported factually on dawn visibility conditions [at Stonehenge]. . . . The combined tally was:
>
> Sunrise at:

Midsummer	15 Perfect; clear cloudless sky.
	26 Partial; broken cloud, high nimbus.
	14 Poor; thick haze, faintly seen.
	27 Nil; rain, low cumulus.

Midwinter	26 Perfect; clear, striated alto-cumulus.
	9 Partial; broken cloud, fine.
	18 Poor; mist, or seen after rising.
	20 Nil; cloud, rain, fog, snow, mist.
Spring Equinox	36 Perfect; brilliant sky, high alto-cumulus.
	15 Poor; mist, squalls.
	8 Nil; rain, mist, low cloud.
Fall Equinox	48 Perfect; high-pressure clarity.
	11 Poor; partial, in cloud or haze.
	8 Nil; thick cloud, mist.

From this it can be deduced that the sun was visible on 78 percent of the occasions—a surprisingly high batting average—and on 125 dawns (45 percent) the visibility was perfect.

On the basis of these 281 dawns it would appear that the odds today for a successful sighting are better than even. In the Stonehenge era, climatological data indicate that the sky might have had greater clarity, approaching Mediterranean conditions, but it is difficult to tell whether the actual cloud cover was less.

For comparison I made a similar set of observations in the winter of 1966/67 during a sabbatical leave in Spain. It was, ostensibly, the place of best visibility on the southern shore, the Costa del Sol, looking out over a clear horizon. I observed the rising of the sun, the moon, and the brightest star in the sky, Sirius. The result:

Sunrise	15 Perfect; clear, complete sunrise seen from first gleam to disc standing tangent on the horizon. 4 occasions showed the "green flash."
	7 Poor; broken cloud.
	1 Nil; overcast.
Sirius rise	1 Perfect; on the horizon, apparent magnitude 4.
	13 Poor; not seen until about 1° above horizon, then scintillating to invisibility.
	6 Nil.

These observations were made in the months of November and December, probably the worst months on the Costa del Sol. It can be figured that the chances of seeing some portion of the sunrise are 95 percent, a factor of 2 better than Salisbury Plain. . . .

The moonrises were clearly seen, similar in visibility to the sun, but these have not been included in the tabulation because in December the moon in its swing had moved from over the Mediterranean to the distant outline of the Sierra Nevada, a mountain range with apparent elevation between 1° and $1\frac{1}{2}$°. I was interested to find that on the day before full, and on the evening of the full, the lunar disc was visible as it rose over the sea horizon. This visibility would be a critical factor at Stonehenge if, as I have suggested, the time of rising of the moon was used to give final warning of the imminence of an eclipse. The moonrise was clearly observable, even though it came up more than an hour before sunset.

As an astronomer I am naturally interested in possible thought processes of preliterate peoples that might show an awareness of the objects of the sky. To my mind it is an essential step in development, and there is surely a difference between a culture that has made this step and a culture that has not. Ellegård's paper and the associated comments would seem to provide consensus support for the existence of that awareness in prehistoric Britain based on the stark evidence of stones and postholes. A portion of the Neolithic awareness has been retrieved. At least a few people at that time were watching and marking on the ground the many turnings of the sun and moon, and for very little utilitarian benefit save the accumulation and recording of (what we today call) knowledge, or information.

Ellegård doubts that Neolithic people could or did count off the period of the swing of the moon. As a physical scientist I would urge a word of caution before closing the debate on this question. In talking about the moon cycle of 18.6 years we must at the outset clarify what it is not. It is not the 18-year 11-day Saros cycle. This is the period of exactly 223 lunations after which eclipses recur. Nor is it the 19-Julian-year Metonic cycle, after which period the full moon falls on the same calendar date. The moon cycle is distinct, simpler, and much more noticeable. It is the number of years required for the moon to return to being high in the winter sky (or low in the summer). Sometimes 18 years elapse, sometimes 19, depending on the fraction (0.6). The same interval governs the swing of the various moonrises and moonsets along the skyline as Ellegård has explained. This movement of the moon is the nearest analogy to the 12-month swing of the sun. If the Avenue at Stonehenge marked the year of the sun, then the stones on each side of the Avenue marked the "year" of the moon, averaging out to 18.6 years of the sun. This movement, which can be separated out entirely from any consideration of eclipses, is the most basic long-term phenomenon of the moon, and it should not lightly be discarded in the ongoing debate. In contemporary Ireland farmers call it the Duibhré (Barber 1973:37), that time when the moon stays below the mountains. Nor does a person have to watch for 18.6 years to be cognizant of the swing; the moon turns to retrace its path after only 9 or 10 years. The ambiguities (9 *or* 10, 18 *or* 19) do not disappear until an interval of 56 years has passed, which, coincidentally or otherwise, is the number of holes in the Aubrey circle.

I published the Stonehenge alignments in 1963, and now, close on 18.6 years later, the moon has been through one complete cycle. During that time considerable advances have been achieved in the understanding of megalithic monuments. Whether or not the builders of Stonehenge did actually count off 18- and 19-year intervals is still debatable, but a significant step forward has been made in the understanding of what might have been involved in those seemingly numeric circles. Let us hope that similar advances in understanding will be made during the next 18.6-year moon cycle.

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Ellegård's paper starts off with a good description of the spherical astronomy necessary for an understanding of the subject. Thereafter a great deal of what he writes is directed to his conclusion that there was no science in Bronze Age Britain. He arrives at this decision by simply ignoring everything we have done which appears to disagree with it.

He says there was no standard megalithic yard, the yard being established, when required, by pacing. We hold the only accurate survey of Stonehenge. This is at a scale of 1/84. When we draw on tracing paper a circle of exactly 45 megalithic rods in circumference (i.e., 30 sarsens $\times$ $1\frac{1}{2}$ rods) and place it on the survey, it fits the inside of the sarsen circle stones *exactly* (Thom and Thom 1978:para. 11.2), and when we draw an ellipse 27×17 megalithic yards it touches the inside of the trilithons *exactly* (para. 11.3). How could these have been so exact without a much better standard than that obtained in the way Ellegård suggests? The value of the yard accurately determined from the Carnac alignments is 2.724 feet, practically identical with the British value (see Thom and Thom 1978:table 4.4). Much more information is given in our chap. 4. All this was available to Ellegård, but he has ignored it. Similarly, he has ignored the value of the megalithic inch as given by the histogram in our figure 5.1. These values come from measurements of the diameters of circles carved on rock by the people who made the cup-and-ring marks, diameters measured not by us but by R. W. B. Morris and D. C. Baillie. In table 5.2 we give some diameters provided by E. Haddingham from rubbings made in Yorkshire. Using Broadbent's method, these both yield the same value for the quantum, 0.816 inches, and 1/40 of a megalithic yard is 0.817 inches. In our chap. 5 we present evidence that the people who carved the cup-and-ring marks also carved the spirals and eggs, based on right-angled triangles with the megalithic inch as a unit. Ian Orkney has suggested that these designs were drawn on hide which was then cut to form a template which could be placed on the rock.

We want to know if the small circles are earlier than the large ones. The cult extended over more than 1,000 years, and in that time there must have been changes in method and changes in ideas. If we knew that the large circles (Brogar, Avebury, Stonehenge, etc.) were all later, then we could base our estimate of the value of the megalithic yard on these circles and neglect the smaller and presumably earlier ones. What we really want is more surveying. We need more accurate—and I mean *accurate* —surveys of the known sites and surveys of new ones. Why cannot people spend a month or two learning to use a theodolite properly and go into the field themselves and make measurements? It seems to me that a great many people accept my surveys but do not accept my explanations.

Ellegård attacks the work we have done on the perturbation, making much of its very small value. Of course it is small, and that is why we went back time after time to many of the sites to make sure that our measurements were correct. Over the years we have many times plotted histograms of which those in our 1978 book, figure 10.9, may be taken as typical. Can anyone look at one of these and understand it and then say that this is merely the result of chance? L. V. Morrison of the Royal Greenwich Observatory wrote us to point out how much more satisfactory it would be if we divided the histogram into four parts, and we have recently done this (Thom and Thom 1980: fig. 2). (Morrison [1980] has recently reported on megalithic lunar lines in Scotland.) Thom (1981:fig. 1.7) has presented the histogram with the introduction of the concept of the "lunar band." In a conference to be held in Oxford later this year, we shall offer an entirely different form of "proof" that the perturbation was observed (Thom and Thom 1981).

A great deal of Ellegård's opposition comes from the fact that he tries to take the weather into consideration. I do not believe that we know what the weather was like in Britain at the beginning of the 2d millennium, but I have pointed out how a missed observation could be replaced on the ground.

I have several times attempted to attach a probability level to our hypothesis. Some of these were frankly approximations, but they nevertheless cannot be very far wrong and every one of them indicated a high degree of probability that we were discovering genuine lunar observatories. I do not mind hearing a good reason these probabilities are wrong, but to have them simply ignored is almost insulting.

I have gone back time after time to Brogar and measured the important foresights several times. I have been over the ground in detail, and naturally I know much more about the site than can be obtained on a casual visit. For instance, did Gingerich really see the ridge on the ground, near the Comet Stone, which we show in Thom and Thom 1978:fig. 10.7? This shows unequivocally the notch on Mid Hill, as do the mounds along the top of the main ridge at Brogar. There is no argument against this, because the notch on Mid Hill is the only notch on the horizon in the neighbourhood of the lunar band (see Thom and Thom 1975, Thom 1981). Did Gingerich, and for that matter Ellegård, consider a point I had made already—that from the centre of the main ring the Mid Hill and the cliffs at Hellia were in the correct position for around 3000 B.C., but by the beginning of the 2d millennium the drop in the obliquity of the ecliptic made these foresights in the wrong position, and, as they could not be moved, the observers proceeded to make the mounds, etc., to the south of the ring? This idea suits our measurements perfectly. Perhaps one day Gingerich will go back with a theodolite and make his own survey. He will then see how well Mounds A and B provide a stance for a warner whose duty it was to tell the observers that the moon was about to appear, and he will see how Salt Knowe provided a stance for a warner watching for moonrise on Kame of Corrigal. Most of the important lines at Brogar are not obvious on the ground, but only become obvious when the whole site is plotted carefully.

Ruggles asks how it is that I appear to obtain overwhelming evidence in support of direct observation and recording of the nine-minute wobble. The answer is very clear: it is because they are real. The lunar band is a band in the sky which covers all possible positions of the upper or lower limb of the moon at the stand-stills; I of course am not interested in any foresights which fall outside this band. If there were nothing in my hypothesis, then the points of a histogram would be uniformly scattered over the lunar band. Why, then, do we find them clustering?

In addition to this, there are of course a number of foresights which are indicated at the backsight. Perhaps the best is the one to Mid Hill which I have just mentioned. One might also mention Dunskeig, where the two stones point unequivocally to the foresight.

On p. 106 Ellegård discusses briefly the importance to megalithic man of being able to extrapolate, but I have shown how they did this. The sectors necessary are still on the ground in several places; the extrapolation method that used them has been entirely overlooked by Ellegård, who gives no explanation whatever for these sectors.

Ellegård cannot be aware of the many carved stone balls found in Scotland that bear representations of the five Platonic solids. There are only five of these solids possible, and the stones carry all five (Critchlow 1979:chap. 7). It is at present impossible to date these, but from what other culture could they have come? If these are not scientific objects, then what are they?

To sum up, it seems to me that Ellegård was a sceptic from the beginning and that the apparent neutrality of his paper on casual reading is quite misleading.

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In response to some of the comments on Ellegård's paper, I should like to point out that a preliminary study of the ethnographic and historical data concerning the practice of astronomy and its importance to society as a whole has already been undertaken.[1] The results of these investigations do not, however, lead to any desire to seek to praise archaeoastronomy; rather, we should seek to bury it as a study conducted, as it has been up to now, separately from consideration of the society in which it operated. The evidence (for a preliminary report see Thorpe 1981) shows that some interest in the heavenly bodies is almost universal and that the attainment of a highly accurate *observational* astronomy is not uncommon. This observational astronomy is conducted by trained specialists, but they are not full-time astronomers and, more surprisingly, they do not always display a high level of competence (e.g., Parsons 1925:75; Rubenstein 1978:341). It is clear that even those societies which place considerable emphasis on astronomical observations can in no way be described as theocracies, either in its generally accepted use (Webster 1976) or in MacKie's (1977) very particular definition. The main conclusion I would draw from the ethnographic and historical record is that astronomy is just one among many facets of socially complex sedentary societies; it certainly does not merit being concentrated on to the exclusion of all other aspects of society.

One of the problems bedevilling archaeoastronomy has been its insistence that the phenomena with which it deals are extraordinary, a tendency encouraged by initial overestimates of the labour investment in Later Neolithic ceremonial centres (e.g., Renfrew 1973), only now being revised (Startin and Bradley 1981). This view of Later Neolithic astronomy as being extraordinarily sophisticated has, naturally enough, led to the proposal of extraordinary solutions to resolve the supposed problem. MacKie's (1977) proposal of an elite of full-time professional orders of priests and wise men directing the activities of Later Neolithic Britain has been criticised on archaeological grounds on numerous occasions (most recently in Whittle 1981),

[1] I should like to thank Richard Bradley for reading an earlier draft of this comment.

but it is perhaps also necessary to point out that his analogy with the Maya is a false and misleading one. The ethnographically and historically recorded societies in which observational astronomy was practised to a standard as high as that which it could reasonably be suggested was practised in Later Neolithic Britain are not as highly stratified as the Maya, are not theocracies, and do not contain an elite of astronomers maintained by tribute from the mass of the people.

Once the misconceptions that "megalithic astronomy" is qualitatively different from that practised at other times and places is disposed of, we may better see it in its true light. That observational astronomy played a part in Later Neolithic society is in no way surprising. The part it played was almost certainly a minor but not insignificant one. The value of archaeoastronomy is thus better seen in terms of the light it can throw on the development of specialisation (especially since there is a clear division between those societies which restrict astronomical knowledge and those in which astronomical knowledge is widely disseminated) and social stratification and on the use of ritual to legitimise existing social arrangements (Webster 1976), rather than as a false mystery to be solved.

Reply

by Alvar Ellegård

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Neither in my article nor in my reply to the comments have I questioned the *accuracy* of Thom's estimates of the "megalithic yard." The point that I wished to make (and had to repeat) is that, while an *average* for the whole of England, Britain, or Western Europe can certainly be determined with great accuracy, this does not imply that the *individual measures* used were at all close to that average. Thom's own data indicate that the range of variation is quite consistent with the use of pacing or, as I also wrote, with rods made at each site, based on the pace. I certainly never thought that a monumental place like Stonehenge, for instance, would have been built without the help of measuring rods, poles, or ropes. The only question concerns the method of *standardization* of the rods. The data, as far as I can see, are not inconsistent with the very simple hypothesis that the human pace provided the ultimate standard. This does not exclude the possibility—indeed, the probability—that the same rods were used over a longish period (after all, some sites must have taken many years to build) and perhaps at more than one site. All this would be well in accord with what we know of nonscientific, nontechnological societies in the more recent past.

There are other statistical questions involved in the megalithic-yard question, e.g., the selection of the sites to be measured, the possibility of fitting other measures to the layouts, etc. I did not go into these, since they seem to me to have been thoroughly dealt with by others (see Burl 1980, Freeman and Elmore 1979, Hadingham 1981, Moir 1979, Moir, Ruggles, and Norris 1980, Ruggles and Whittle 1981). Nor did I go into the statistical problems involved in the interpretation of the astronomical sightlines. To attach probability figures to the data adduced by Thom would be a tremendous undertaking, requiring either an exhaustive search of all possible sightlines (surely thousands, if not tens or hundreds of thousands) or of a truly representative sample of these.

Burl's analysis of recumbent stone circles in northeastern Scotland and Cooke et al.'s work on Callanish are examples of the kind of spadework that is needed. Some of the statistical problems involved have been discussed recently by Heggie (1981). Important references (some of which I have cited above) are given by Ruggles in his comments on my paper. When Thom complains about my "ignoring" his calculations, he apparently does not realize that the doubts I and others have about his interpretations have very much less to do with the "formal" probability levels or the accuracy of the measurements than with the altogether fundamental question of the selection of the basic data. There is no doubt that the Stone Age observers could have determined the exact rising positions in the way assumed by Thom, but just marking the extremes does not make scientists of the observers. They would deserve that name only if we could show, or find it probable, that they marked the extremes of the lunistices for another purpose, e.g., establishing the exact periodicity of the "swing," the periodicity of the wobble, or the connection of the wobble with eclipses. It is claims such as these that I think have been refuted.

About the necessary "extrapolation" from observed to unobserved data, Thom says that he has "shown how they did this." I disagree: he has only shown how it would be *possible* for a naked-eye observer to extrapolate. I did not go into a discussion of the stone rows described by Thom in *Megalithic Lunar Observatories*, since my argument was directed at the possibility of discovering the wobble at all (even given unbelievably good weather conditions), let alone connecting it with eclipses. Naturally Thom's interpretation of the stone rows is subject to the same uncertainties as his interpretation of the construction methods used for stone rings.

Whether I started out as a sceptic or not seems to me irrelevant. Scepticism is a necessary ingredient of scientific work. Science needs enthusiasm and imagination to formulate hypotheses like Thom's, but it also needs scepticism to knock down hypotheses that do not stand up to stringent tests.

References Cited

Barber, J. 1973. The orientation of recumbent stone circles of SW Ireland. *Journal of Kerry Archaeological and Historical Society* 6:26–39.

Burl, Aubrey. 1976. Revised edition. *The stone circles of the British Isles.* New Haven: Yale University Press.

———. 1980. Science or symbolism: Archaeo-astronomy today. *Antiquity* 54:191–200.

Critchlow, Keith. 1979. *Time stands still.* London: Gordon Fraser.

Freeman, P. R., and W. Elmore. 1979. A test for significance of astronomical alignments. *Journal for the History of Astronomy* 10:S86–96.

Hadingham, Evan. 1981. The lunar observatory hypothesis at Carnac. *Antiquity* 55:35–42.

Hawkins, Gerald S. 1963. Stonehenge decoded. *Nature* (London) 200:306–8.

———. 1965. Stonehenge: A Neolithic computer. *Nature* 202:1258–61.

———. 1973. *Beyond Stonehenge.* New York: Harper and Row.

Hawkins, Gerald S., and John B. White. 1965. *Stonehenge decoded.* New York: Doubleday.

Heggie, Douglas. 1981. Highlights and problems of megalithic astronomy. *Journal for the History of Astronomy* 12:S17–37.

Hoyle, Fred. 1966. Stonehenge—an eclipse predictor. *Nature* 211: 454–56.

MacKie, E. W. 1977. *Science and society in prehistoric Britain.* London: Elek.

Moir, G. 1979. Hoyle on Stonehenge. *Antiquity* 53:124–29.

Moir, G., C. L. N. Ruggles, and R. Norris. 1980. Megalithic science and some Scottish site plans. *Antiquity* 54:37–43.

Morrison, L. V. 1980. On the analysis of megalithic lunar sightlines in Scotland. *Journal for the History of Astronomy* 11:S65–77.

Parsons, E. C. 1925. *A Pueblo Indian journal 1920–1921.* Memoirs of the American Anthropological Association 32.

Renfrew, A. C. 1973. "Monuments, mobilisation, and social organisation in Neolithic Wessex," in *The explanation of culture change.* Edited by A. C. Renfrew, pp. 539–58. London: Duckworth.

Rubenstein, D. H. 1978. Fixing flaws in a cognitive model: A note on calendrical knowledge in the Caroline Islands. *Journal of the Polynesian Society* 87:339–42.

Ruggles, C. L. N., and A. W. R. Whittle. Editors. 1981. *Megalithic astronomy and society.* Oxford: British Archaeological Reports. In press.

Startin, D. W. A., and R. J. Bradley. 1981. "Some notes on work organisation and society in prehistoric Wessex," in *Megalithic*

astronomy and society. Edited by C. L. N. Ruggles and A. W. R. Whittle. Oxford: British Archaeological Reports. In press.
THOM, ALEXANDER. 1971. *Megalithic lunar observatories*. Oxford: Clarendon Press.
THOM, ALEXANDER, and A. S. THOM. 1975. Further work on the Brogar lunar observatory. *Journal for the History of Astronomy* 6:100–14.
———. 1980. A new study of all megalithic lunar lines. *Journal for the History of Astronomy* 11:S78–89.
———. 1981. Statistical and philosophical arguments for the astronomical significance of standing stones. Paper prepared for the Archaeoastronomy Conference, Oxford, England.
THOM, A. S. 1981. "Megalithic lunar observatories: An assessment of 42 lunar alignments," in *Megalithic astronomy and society*. Edited by C. L. N. Ruggles and A. W. R. Whittle. Oxford: British Archaeological Reports. In press.
THORPE, I. J. 1981. "Ethnoastronomy: Its patterns and archaeological implications," in *Megalithic astronomy and society*. Edited by C. L. N. Ruggles and A. W. R. Whittle. Oxford: British Archaeological Reports. In press.
WEBSTER, D. L. 1976. On theocracies. *American Anthropologist* 78: 812–28.
WHITTLE, A. W. R. 1981. "Later Neolithic society in Britain: A realignment," in *Megalithic astronomy and society*. Edited by C. L. N. Ruggles and A. W. R. Whittle. Oxford: British Archaeological Reports. In press.