Archaeo-astronomical Cushites: Some Comments

Robert Soper

This article examines the interpretation of some stone monuments in north-eastern Kenya put forward by Dr M. Lynch. Mr Soper is now with the Institute of African Studies at the University of Nairobi. The article is followed by Dr. Lynch's observations.

In a series of recent articles and a PhD thesis, Lynch and Robbins have described a series of three stone monuments in Turkana District of north-western Kenya (fig. 1), all known to the Turkana as 'Namoratung'a', and ascribed them to ancestral Eastern Cushitic speaking people around the fourth century BC.[1]

The first two sites are close to the Kerio river near Lokori in southern Turkana and are cemeteries characterised by circles of vertical slabs with pecked artistic designs; one cemetery contains one hundred and sixty two graves and the other eleven graves. Three of the graves were excavated by myself in 1969 and forty more by Lynch in 1975/76. Lynch has recorded and analysed a considerable amount of data both on the burials and the rock art and presents the analysis and conclusions in his PhD thesis (Lynch, 1978) and in Lynch and Donahue (1980).

The third site (Pl. I) is some 160 km to the north (not 210 km as given in e.g. Lynch and Robbins, 1979) near Kalokol on Ferguson's Gulf, and consists of a series of nineteen basalt pillars (a twentieth is displaced and lies some distance away), some of them having petroglyphs similar to the other sites; there is also a feature with vertical flat stones which Lynch equates with the graves at the other sites and a few other features to be described below. This site is also described and interpreted in Lynch's thesis, though in much less detail than the other sites. Alignments of certain of the stones were found to correlate with the rising points of seven stars and constellations currently used by Borana Eastern Cushitic speakers of southern Ethiopia to calculate their annual calendar, as these rising points would have been in around 300 BC (but see below).

Although the sites are of this rather different character and are not associated with any settlement sites, Lynch and Robbins regard them as being approximately contemporaneous and culturally related, the work of an Eastern Cushitic-speaking population.

I have recently had occasion to resurvey the Kalokol site and to look in detail at Lynch's data and conclusions. The present article presents a certain amount of additional information on the Kalokol site to amplify Lynch's descriptions, corrects a serious survey error and raises some questions and anomalies on Lynch and Robbins' interpretations.[2]

---

1 Lynch and Robbins (1977, 1978, 1979), Lynch (1978), Lynch and Donahue (1980). Soper and Lynch (1977) and Paul (1979) are also relevant. Lynch's thesis (1978) provides most of the data to support what often appear as unsubstantiated statements in the more synthetic articles.

2 I should like to say at the outset that I am highly appreciative of Lynch's presentation of the cemetery data which is analysed in a variety of imaginative and fruitful ways in his thesis; in most cases the data support certain statements and conclusions which on the face of it appear dogmatic or unjustified in the more general synthetic articles. However it is only to be expected that a number of loose ends and anomalies should remain. A brief description of my survey methods is given in the appendix. In addition to my thanks to Lynch, I am grateful to Peter Robertshaw and David Collet for suggestions on earlier drafts of this paper, and to Robert Caukwell for advice on surveying and loan of equipment. The Norwegian Agency for International Development generously facilitated my second visit to Kalokol.

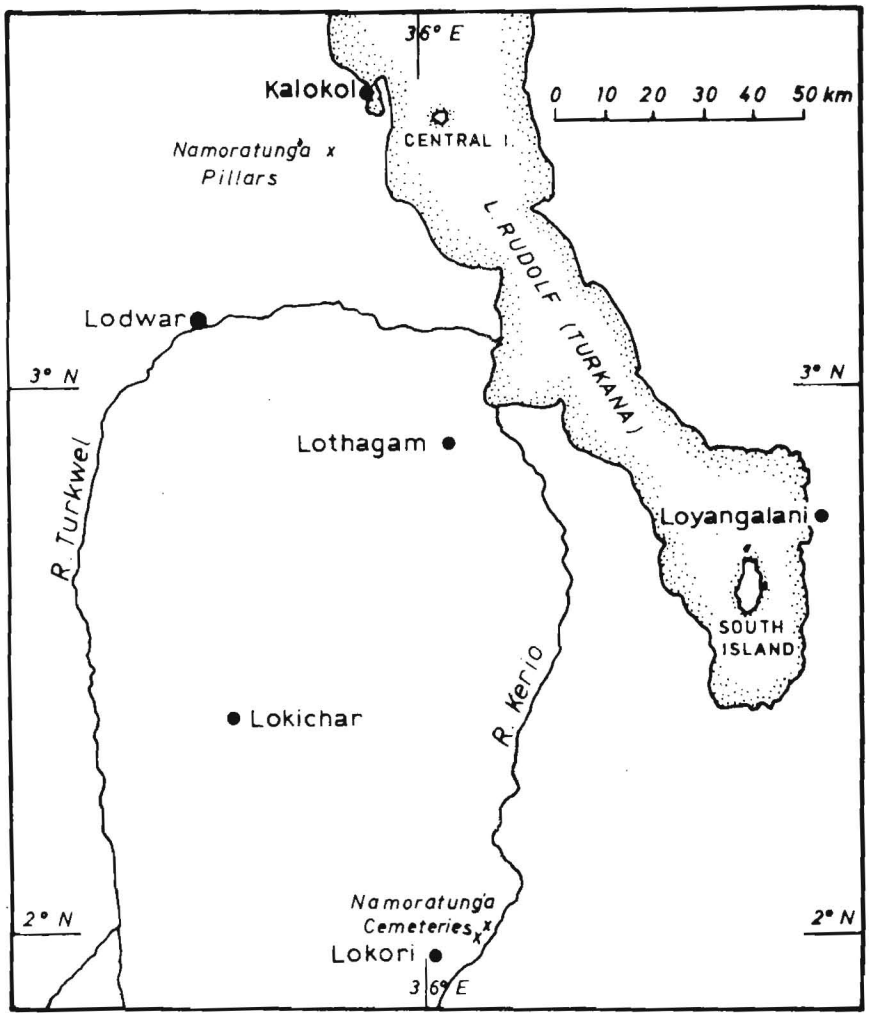


Fig. 1. Location of the Namoratung'a sites.

The Kalokol Site

Survey and astronomic function

The Kalokol monument was resurveyed in April 1982 after earlier observations had suggested errors in Lynch's quoted alignments. The results of this new survey are given here in Table 1 and figure 2. It would appear that Lynch's survey was distorted, perhaps by magnetic anomalies or instrumental error, and that the alignments he gives are in error by amounts ranging from 1^0 to nearly 17^0. (In fact he does not anywhere give his own bearings for the stone alignments, merely quoting the azimuths of the relevant stars now and in 300 BC and whether his alignments correspond; a more objective comparison would have been more convincing.) In view of these gross errors the archaeoastronomical hypothesis as presented in Lynch and Robbins (1978) can no longer be maintained.

Table 1

NAMORATUNG'A – KALOKOL

Bearings between highest points of stones (from magnetic north)
14th April 1982. Magnetic variation 33' W

	from Stone 1	from Stone 5	from Stone 18	Direction of lean	Angle from vertical	Max. ht above present surface
Stone 1		166°25'	158°13'	250°	25°	45 cm
Stone 2	76°26' (81°)*	125 31	144 21	285	8	45
Stone 3	79 28 (88)	113 33	137 11	105	12	40
Stone 4	79 32 (88°)	100 47	125 34	115	7	100
Stone 5	346 26 (360°)		154 45	340	25	40
Stone 6	37 18	90 52 (103/107°)	137 31	55	57	50
Stone 7	54 51 (67°)	95 29 (103°)	133 6	70	40	50
Stone 8	69 19 (76°)	99 –	129 24	25	33	42
Stone 9	73 32 (81°)	98 26 (107°)	126 32	–	–	25
Stone 10	64 59	89 1 (c.100°)	121 35	105	28	85
Stone 11**	41 44	66 42	116 5	15	90	–
Stone 12	74 52 (76°)	88 42	111 30	65	47	60
Stone 13	60 25	75 49 (81°)	105 29	335	57	50
Stone 14	55 13	69 12	99 29 (103°)	–	–	35
Stone 15	52 51	68 19	101 42 (107°)	360	9	35
Stone 16	50 1	63 32	95 38 (c.100°)	95	c.25	70
Stone 17	41 43	53 11	85 37 (88°)	215	62	30
Stone 18	338 13	334 44		280	c.12	20
Stone 19	5 42	155 35	154 56	165	?	15
Central I. (S. edge of S. peak)	73 9 (90°)	73 7	obscured by rain			

* Figures in brackets are Lynch's from Lynch and Robbins (1978).
** Bearings not significant — this stone is lying flat on the ground.

It may therefore be superfluous to comment further on the astronomical theory but a number of other general points may be made.

The alignments as given by Lynch, and as measured in my survey for direct comparison and given in Table 1, are taken through the highest points of the stones in their present attitudes, the latter varying widely in their direction and angle of lean. The significance of any such bearings thus depends on the stones having been deliberately set at these attitudes and not having shifted or settled since. To support this point, Lynch quotes instances of Eastern Cushites deliberately setting wooden or stone pillars at angles. In the Kalokol case, if the astronomical hypothesis were correct, a functional reason for inclining the stones might be to make minor adjustments to lines of sight; since these lines are broadly east/west one would then expect the inclinations to be roughly north/south but only six out of the seventeen

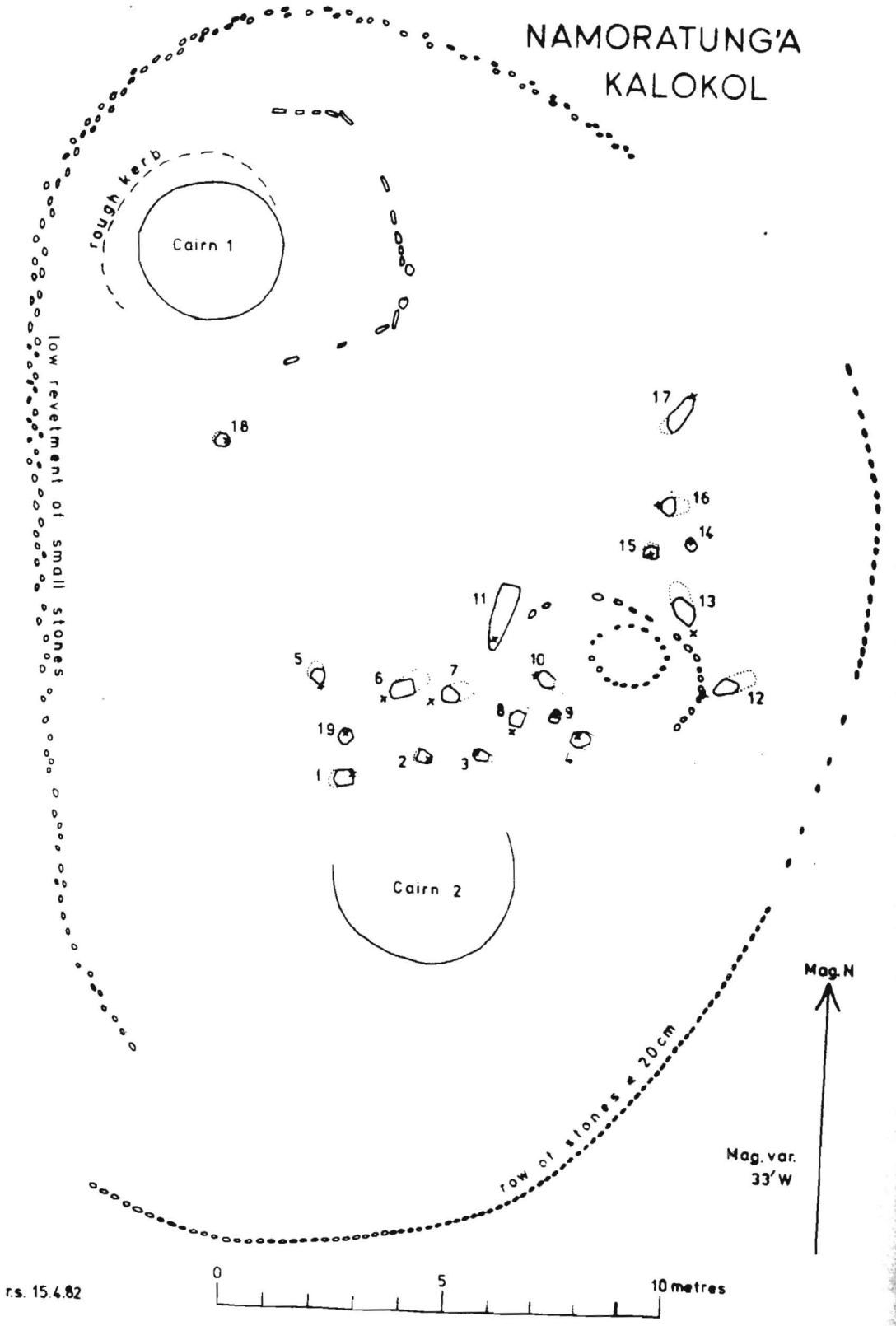


Fig. 2. The Kalokol Site.

inclined stones lie this way, one of which (Stone 5) Lynch regards as a back-sight with multiple foresights. However, some of the Kalokol stones are at angles of around 60° from the vertical, hardly a stable angle of rest for 2000 years or more, while one is flat on the ground; where one has fallen completely, others are likely to have settled. I would not therefore regard my bearings given in Table 1 as much more significant than Lynch's incorrect bearings. For the sake of argument however, I have compared the bearings in Table 1 with the azimuths of the seven stars over the past 3000 years as shown in Paul (1979, fig. 1) and at no point could all seven azimuths be matched at an accuracy of ± 1°. Correspondence was closest through the first millennium AD, for most of which six out of seven could be matched. Paul's figure admittedly does not allow the azimuths to be estimated to nearer than half a degree.

An alternative approach to the arrangement of the stones is to assume that they were originally upright and to estimate their positions. On figure 2, the crosses indicate the estimated original centre of each stone based on the arbitrary assumptions a) that there was originally 50 cm length of stone beneath the level of the present surface in each case and b) that as the stone declined the base would remain in position up to an angle of 45° after which it would rise more or less vertically; there would thus be a maximum horizontal displacement of the centre point by 50 cm. It may be borne in mind that few of the stones have, or would have had, a sharply defined top, so that precise alignments would be hard to define. (My own table gives a false impression of precision, which would not be obtainable without benefit of spirit level and chalk to define the highest point). Examination of figure 2 shows a number of possible alignments of these crosses such as 5/6/(7)/8/4 on *c.* 100°, 1/2/3/4 on *c.* 79°, (12)/13/14/17 on *c.* 2°, 11/9/4 on *c.* 136° and 6/11/15/14 on *c.* 61°. Pairs of stones would of course offer a wide variety of bearings. I do not propose to speculate on the possible significance, if any, of these alignments.

As a final comment on Lynch's astronomical hypothesis, one may note that among the modern Borana there is no record of pillars being used to mark alignments and that in fact it is the *time* of star rising in conjunction with the moon that is significant, not the position of rising as it might be in the case of the sun or moon.

Other features of the site

(a) The surface of the ridge on which the monument stands is a stone-mantle of generally small stones and gravel, with rock exposures in the form of either basalt boulder outcrops or planed-off crumbly bedrock. The surface around the pillars themselves consists of smallish loose cobbles derived from the practice of the Turkana of placing stones on top of the pillars. There may now be an appreciable depth of these and they would seem to have obscured the northern half of Cairn 2.

(b) The stone pillars are polygonal basalt columns of varying sizes from *c.* 25 to 40 cm in thickness; Stone 11, apparently one of the largest, is *c.* 155 cm long, while Stone 20, displaced and lying loose to NNE of the site, is 82 cm long and 20 x 25 cm in section diameter. No source for these columns is known. Casual enquiries from local Turkana elicited only vague references to 'the hills' and clearly did not evoke thoughts of any specific site near or far from which the pillars might have been brought. (If such a site existed one would expect it to be sufficiently noteworthy to be fairly well known). Other boulder outcrops within a few hundred metres up the same ridge appear to be of similar stone but without the characteristic columnar formation. Perhaps the latter is a localised

phenomenon on the site of the monument itself, though this is not to suggest that the pillars were not artificially erected.

(c) *Cairns.* As Lynch notes, there are numerous stone cairns on the ridge on which the monument stands. He does not however mention the presence of two, or possibly three, of these within the confines of the site itself; presumably he regarded them as 'clearly Turkana in origin' like the others. While there is nothing to suggest whether or not these cairns are contemporary with the pillars, I would question whether all of them are as clearly Turkana as he says. Some are much larger and more regular than any Turkana graves I have seen, being up to 7 m in diameter and 75 cm high and some having a well-defined edging or kerb of neatly positioned stones, totally patinated. Modern Turkana graves which I have seen are simple piles of stones, often oval rather than circular and rarely more than two or three metres in diameter. One such recent-looking cairn exists some 60 m north of the site. Cairns 1 and 2 shown on figure 2 are low heaps of smallish stones; the 'rough revetment' of cairn 1 could be an original edging or kerb, this side of the cairn being somewhat eroded as it borders the slope.

(d) The arrangement of vertical stones which Lynch calls the 'grave' at the northern end of the site (fig. 2) consists of flattish stones of varying rock-type protruding to a maximum height of *c.* 20 cm, though mostly lower. The westerly stones are flush with the present surface and it is not known if the feature continues to the west. The layout and size are as shown and not, as Lynch indicates, semicircular and 'about 3 m in diameter'. The rounded stones at the SE corner are small basalt pillars, suggesting an entrance, and the whole feature is rather suggestive of a 'fore-court' to cairn 1, though whether it is really associated can only be clarified by excavation.

(e) The surrounding feature or features are more extensive than Lynch shows on his plan. On the south and east sides where the ground is more or less level, there is a single row of rounded stones up to 20 cm long. To the north and west, where the ground falls away, steepest to the north-west, there is a less distinct stone arrangement, not apparently more than one course deep, serving to reinforce the edge of the slope. The two features are not continuous; to the south-west the alignments are different and to the north-east there is a gap of *c.* 6.5 m with no indication that they ever met. If the features represented the base of some kind of fence then perhaps one could see the SW gap as an entrance, but the south-eastern feature looks too evenly laid to suggest such a fence-base.

(f) *Petroglyphs.* On my original visit I examined the stones and was only able to find two designs on Stone 2. On my second visit, with Lynch's figure 27 in my hand, I could still find only nine out of the twenty-three designs which he lists and for some of those I could not follow his interpretation. The light however was not good and I have no great confidence in my own observations.

(g) *Connections with southern Namoratung'a sites.* The connection of the Kalokol site with the two cemetery sites near Lokori rests on the presence of the 'grave' and the admitted similarity of the petroglyphs at all the sites, as well as on the 'Eastern Cushitic connection'. In the absence of excavation, the similarity of the 'grave' remains inconclusive: the nature of the slabs used is certainly different but this is not significant since good slab material is not available near the site and any flattish stones have been used. The size is rather larger than the maximum diameter of 4.7 m recorded at Lokori, though not by much. The sub-rectangular shape would however appear to be a significant difference. The stone cairn, if in fact part of the same feature, might also replace the horizontal

slab filling of the Lokori sites. The petroglyphs, while suggestive of a connection, are, like all rock art, not directly datable and could have been executed at any time after the erection of the pillars. The firmest part of the Eastern Cushitic ascription was the supposed calendrical function of the stones and if this is taken away we are left only with the practice of some Eastern Cushitic speakers in southern Ethiopia of erecting stone pillars, which is hardly conclusive. The common name is given by the Turkana who may be supposed to have arrived long after the construction of all the monuments if the proposed dating is correct. Thus there remains no very positive link between the Kalokol and Lokori sites, though such may yet be established, especially if they could be associated with any other type of archaeological evidence such as settlement sites.

Dating the Sites

Radiocarbon

The larger Lokori cemetery has provided two radiocarbon dates on human bone, the first on bone apatite of 330±165 bc (GK-5042a) and the second on bone collagen of ad 750±100 (UCLA-21240). The latter date is rejected by Lynch on the grounds that apatite dates are more reliable and that it does not fit the linguistic dating. The first claim is not in fact substantiated (see Collett and Robertshaw, in press) while the second is dangerously close to a circular argument.[3] There is thus no objective reason for accepting one date in preference to the other or, in view of the vagaries of bone dating, for accepting either on its own.

Linguistic dating

Lynch accepts without question Ehret's reconstruction and dating of linguistic developments affecting Cushitic and Nilotic speaking peoples in northern Kenya/southern Ethiopia. The linguistic picture and the Eastern Cushitic connection are discussed further below, where it will be pointed out that no linguistic reconstruction places Eastern Cushites as far south as Lokori at any time on this side of Lake Turkana (though I would not regard this as significant negative evidence). Here it may simply be pointed out that linguistic reconstructions can provide no evidence of absolute dating (though they may give relative dating) and that geographical locations of ancient linguistic developments must necessarily be rather hazy in view of the time lag between the events reconstructed and the data base in present day languages — especially considering the wholesale nature of the migrations suggested.

Astronomical dating

The correlation of Lynch's alignments with the position of rising around 300 BC of the specific stars relevant to the Eastern Cushitic calendar was a strong support both for his dating and for the Eastern Cushitic ascription. While the latter may still perhaps stand without astronomical support, the former now collapses.

Patination of petroglyphs and duration of use

Lynch in all the publications avoids the question of how long the cemeteries and Kalokol monument may have been in use. The patination of the petroglyphs ranges

---

3 See e.g. Lynch (1978, p. 222) '. . . radiocarbon dates which could definitely link early Cushites to known archaeological assemblages have not been available' [before the present work].

Kalokol Namoratunga site from the south. Stone 1 at left, Stone 13 at right. Cairn 2 in the foreground.

from completely weathered to the same state as the parent rock surface, to apparently completely fresh. Lynch divides this range into three broad categories which he includes as a factor in his extensive analysis of the rock art. Since there is no significant difference in the distribution or frequency of the motifs between the patination categories, he accepts them as having all been executed by the same community and he uses them as a *relative* dating method to draw interesting conclusions on social developments within and between the social groups using the two cemetery areas.

There would appear to be a serious anomaly here. According to Lynch and Robbins' historical hypothesis, the Eastern Cushitic inhabitants would have been replaced by Eastern Nilotic makers of Turkwel pottery by AD 450 at the latest. If the apparently fresh petroglyphs have not become patinated at all in 1500 years and the patination process was constant, the completely patinated ones must be of extreme and unacceptable antiquity. We must therefore conclude either that the patination process was completely arrested sometime in the early first millennium AD , which seems improbable, or that the fresh petroglyphs are in fact really quite recent. The latter conclusion seems to me to be far the most likely but raises more questions.

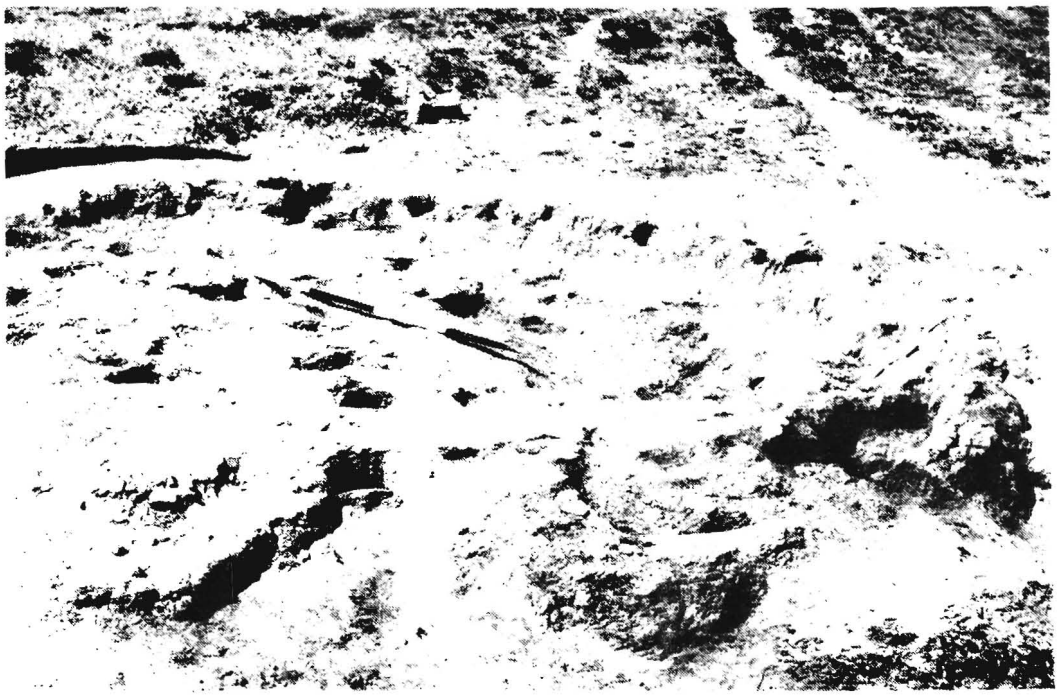
Was all, or even any, of the art in fact executed by the builders of the graves? Theoretically it could have been executed at any time after the graves were erected but Lynch shows convincingly that art is consistently and significantly associated only with *male* graves and the occupant of a particular grave could only be known to those who buried him or her. Unfortunately Lynch does not record or analyse the relative patination of the designs on the actual gravestones, only the much more numerous designs on the surrounding rocks. We do not thus know how many, if any, of the grave petroglyphs may be fresh as opposed to patinated and whether there are sufficient fresh designs to present a significant association with the male graves. In view of the relatively few grave petroglyphs it seems unlikely that such an association could be shown, and if the fresh designs follow the distribution of the patinated ones there could still be biasing factors at work, such as the recent copying of old designs on the same grave tending to perpetuate an original distribution.

At the time of erection, one or both surfaces of the great majority of the grave slabs must have been more or less freshly exposed to the elements, and petroglyphs executed at that time would weather with the rock. Thus one would expect the range of patination of the gravestones themselves to coincide with that of the petroglyphs in general, if their time range is the same. Lynch does not mention this factor but so far as I remember there was no noticeable variation in surface patina between different graves. Thus there is at least a suggestion that the execution of the art has extended over a long period subsequent to the erection of the gravestones.

The Kalokol petroglyphs are similar in style and execution to those of the Lokori sites and are completely patinated. Since they may have been executed at any time after the erection of the pillars, we can say that the Kalokol site is at least as old as the Lokori burials, assuming a comparable patination rate in this rather different and harder rock type.

Plate II

Ngamuriak and the Lemek valley looking S.W



Ngamuriak: the hut floor. Scale is in 50cm units.

Ngamuriak: post-holes at the edge of the hut floor. Scale is in 10cm units.

Plate IV

Material exposed during excavation at Ngamuriak. The scale is in 10 cm units.

Bao game site near Sigor in Northern Kenya

Petroglyphs and the Turkana

Another anomaly arises from the claimed recognition by the present day Turkana of 77 per cent of the 142 different design motifs as cattle brands used by different Turkana lineages (actually clans), when the graves and petroglyphs are claimed to be pre-Turkana and even non-Nilotic. While some of the motifs are elaborations on circles and spirals which could be duplicated by chance, others are of considerable complexity. The function of *Turkana* cattle brands as patrilineal lineage markers is then used to interpret the distribution of petroglyph designs in and around the cemeteries in terms of social organisation of the allegedly *Eastern Cushitic* inmates; this hardly seems appropriate.

If the artistic classification is valid and such a high percentage of the motifs are really used by the Turkana, there must surely be a significant relationship of some sort between the Turkana and the cemeteries, demanding some explanation which Lynch fails to provide. Possible explanations might be suggested:

a) The cemeteries are much more recent than proposed, or

b) The Turkana have been there much longer than proposed.

Neither of these appears very likely and in any case the Turkana had no idea of the funeral function of the stone circles and deny having done the art.

c) The Turkana have copied their cattle brands from the rock art.

This seems improbable in view of the wide clan distribution of the symbols claimed to be recognised.

d) The owners of the cemetery were absorbed by the Turkana and their symbols adopted as cattle brands.

This would imply a much more recent use of the cemetery than Lynch proposes since the Turkana are unlikely to have been there for more than two or three centuries.

e) There is a common 'ancestral pool' of cattle brands in the relatively distant past, common to Cushitic and Nilotic speakers if Lynch is right, of which the Turkana at least have preserved a high proportion. If this is the case, then many other pastoral peoples, both Cushitic and Nilotic, should share most of the brands. Information is not unfortunately available from other pastoralists to check this, the lack of data from modern Eastern Cushites being especially regrettable.

However, before proceeding to undue consideration of this curious point, one might query what exactly constitutes 'recognition' in this case. Lynch's procedure was to assemble a group of elders, draw the symbols on the ground and then ask the elders if they recognised them as being Turkana — perhaps a rather leading question. As a control, the test was applied to different groups of elders with apparently consistent results. This would seem to have been a practical *ad hoc* test to apply, given the time constraints of fieldwork, and a truly objective assessment of the Turkana conception of similarity would probably be a complete research project in itself, but one would be happier to see the designs compared with actual photographs of beasts in order to have a more objective (or at least independent, non-Turkana) assessment of the similarity.

One may note that of the ninety-nine motifs recognised by the Turkana, sixty-three are represented by only one example at the sites, though of these only eight are in fresh condition (eighty-eight of the total 142 motifs are represented by a single example and two by no examples at all so far as I can see).

The Eastern Cushitic Connection

The Namoratung'a sites are attributed to Cushitic speakers, especially Eastern Cushites, 'on the basis of similar mortuary practices, the use of stone pillars and the astronomical nature of the Kalokol site' (Lynch, 1979, p. 222). The last factor may now be rejected and the first two relate to two different sites separated by 160 km whose cultural connection is by no means certain.

First one may consider the linguistic evidence for a past Eastern Cushitic presence in the area in question. Ehret (1974) quoted by Lynch, has pointed to the very strong Eastern Cushitic influences in Southern Nilotic in the form of loanwords and some cultural practices, and postulated extended and intimate contact between the groups in the southern fringes of the Ethiopian highlands in the second and first millennia BC. He does show Eastern Cushites extending across northern Turkana District into NW Uganda in the first millennium BC but the only basis for this would seem to be the presence of two Eastern Cushitic loanwords in Nangiya languages of NW Uganda such as Tepeth (Ehret, 1974, p. 52). Heine *et al* (1979) however provide rather firmer support. They postulate a close interaction between early Southern Nilotes and a 'Proto-Baz' Eastern Cushitic group in the area north-west of Lake Turkana; the Proto-Baz language is now extinct and its speakers may have been absorbed in the Southern Nilotes. From their map, the distribution of proto-Baz might have reached Kalokol, though not Lokori; however as pointed out above, past linguistic distributions are necessarily vague to the point of guesswork, so a hundred miles or so either way is hardly significant in the absence of marked ecological barriers. Finally, the Galbo phratry of the Gabbra now to the east of Lake Turkana have oral traditions of migration from the west side of the lake in the relatively recent past (D. Stiles, pers. comm.). Thus the presence of Eastern Cushites in the relevant area is at least plausible if not specifically proven.

Other linguistic groups possibly associated with the area are Southern Cushitic, perhaps around the second millennium BC (i.e. before Eastern Cushitic); Southern Nilotic which it is suggested spread down the western side of the lake from the Sudan lowlands in the first millennium BC; and the proto-Ongamo-Maa group of Eastern Nilotic around the middle of the first millennium AD. The present Turkana language belongs to the Teso-Karimojong group of Eastern Nilotic. In view of the uncertainty of both archaeological and linguistic dating, there is nothing to link the monuments directly with any of these groups.

To return to the stone pillars and mortuary practices, the closest parallels to the former would indeed seem to be among Eastern Cushitic speakers such as the Konso in southern Ethiopia, though of course the mere use of stone pillars does not necessarily imply a cultural connection and there is little here to suggest any specific relationship apart from the negative evidence given by the apparent rarity or absence of similar pillars elsewhere in Kenya. Essentially here we are back to Murdock's 'Megalithic Cushites'. Similarly the Lokori graves would seem to have at least some superficial affinities to those of the Arussi of southern Ethiopia illustrated by Haberland (1963, Plates 78, 79), which have large elaborately decorated stone slabs surrounding a stone cairn, and again we are short of parallels further south. The absence of more sites of both types in Turkana or elsewhere might be partially explained by geological factors — the lack of other columnar basalt occurrences or of large slab material for such graves; elsewhere their function may have been filled by less permanent materials or less distinctive monuments, such as one or more of the variety of cairn types.

In fact one could regard the Lokori graves as simply an idiosyncratic expression of a more common stone cairn type found in Turkana land which has a well-defined and carefully laid outer edge or kerb supporting the low heap of stones which covers an oval or circular grave; a number of these occur in the vicinity of the Kalokol site as mentioned above. If the kerb were translated into vertical slabs and the filling into horizontal slabs the result could be the Lokori type of grave.

What is perhaps curious if these monuments are really Eastern Cushitic, is their apparent absence to the east of Lake Turkana where an Eastern Cushitic presence is much better attested, perhaps as early as the second millennium BC with early Yaaku speakers, and continues to modern times with speakers of Galaboid languages such as Dassenech and Elmolo.

Other aspects of mortuary practices analysed by Lynch may suggest that they are closer to Konso than to present-day Turkana, Pokot, Nuer or Masai, but this hardly constitutes convincing proof that they were built by Eastern Cushites; the 'social distinctions symbolized in the mortuary ritual' are sufficiently ambiguous or general and the details of burial and grave construction insufficiently close to eradicate doubt. Burial in cemeteries is, as Lynch notes, not characteristic of modern Nilotic speakers in general, but it does occur elsewhere in Kenya around the first millennium BC (e.g. Hyrax Hill, Njoro River Cave and other 'Pastoral Neolithic' sites) among communities who might have been Southern Cushitic but certainly not Eastern Cushitic.

Alternative Hypotheses

A seriously limiting factor in assessing the significance and affiliation of these sites is the apparent absence of associated settlement sites to provide any sort of cultural background. Lynch's survey of 190 km^2 around the Lokori sites located hardly any sites at all and none which offered a likely association with the burials. Neither he nor Robbins attempted any correlation with sites further afield, the implied conclusion being that the Eastern Cushites responsible were archaeologically invisible in life and only materialised in death, following the parallel of modern Turkana who leave little or no imperishable diagnostic material culture behind on their temporary habitation sites (Robbins, 1973).

There is however a fair variety of pottery from Turkana district, especially in the vicinity of the lake, of which the two more or less formally recognised types are Nderit and Turkwel, though other types probably remain to be properly defined, especially around Eliye Springs. Nderit pottery occurs on both sides of Lake Turkana and far south down the Rift Valley as far as northern Tanzania, being associated with part of the 'Pastoral Neolithic'. It appears to be one of the earlier neolithic wares, dating to at least the first millennium BC and probably as early as 2000 BC on the eastern side of the lake. It might justifiably be attributed to early *Southern* Cushitic speakers on grounds of the distribution and possible date of the latter,[4] but certainly not to *Eastern* Cushites who are not suggested ever to have occupied the central Rift of Kenya. In Turkana the known distribution of Nderit follows the old lake shore and it has not yet been found anywhere near Lokori, though in view of the scale of research this is not strong negative evidence. It has

4 It is surprising that Lynch and Robbins (1979) make no mention of this Nderit pottery in an article which specifically deals with archaeological evidence of Cushitic and Nilotic prehistory in this area.

been found in the immediate vicinity of the Kalokol site (but for that matter so have modern beer bottles and tin cans). Since the dating for all the Namoratung'a sites may still be regarded as more or less open, the possible association of one or both Namoratung'a phenomena with Nderit ware and Southern Cushites, perhaps early in the first millennium BC or before, may be borne in mind as a possible hypothesis, not markedly less probable than that of Lynch and Robbins.

The Turkwel pottery, tentatively dated to the second half of the first millennium AD, is attributed to Eastern Nilotic speakers on the grounds that its known distribution and age coincide with the geographical homeland and date proposed by Ehret for the Eastern Nilotes, that the economy is consistent with that probably practised by early Nilotes, and that it could not have been made by Cushites since the latter do not eat fish[5] (fish bones are numerous on some Turkwel sites). While not perhaps conclusive, the Eastern Nilotic ascription would seem to be a reasonable working hypothesis, though Southern Nilotic might also be considered.

Could the Namoratung'a sites be associated with the makers of Turkwel pottery? The dating, being uncertain, provides no obstacle. The main objection to such a link is thus the Cushitic ascription of the Namoratung'a sites and the lack of close parallels for them among modern Nilotic speakers. However, one may note the occurrence of a potsherd with parallel grooved decoration in the fill of one of the Namoratung'a graves (variously grave 77 or 79: Lynch, 1978, p. 340 and 270) which might well be of Turkwel type. Curiously Lynch fails to remark on this, the only decorated and possibly diagnostic sherd from the site. It is clearly very important to know more about this sherd — is it really likely to be Turkwel and, if so, whereabouts in the grave fill was it found? From the bare description it would appear to occur with tooth fragments which Lynch regards as contemporary with the graves, so that if it is Turkwel, it would put this grave, and presumably the cemetery, at least as late as the arrival of makers of Turkwel pottery in this area. The sherd itself is unfortunately not to be found in the National Museum with other (plain) sherds from the graves.

Conclusions

The above points may be summarised as follows:

1. The astronomical function of the Kalokol site rests on faulty survey data and must be rejected, at least in its present form and specific Eastern Cushitic parallel.
2. While the Lokori and Kalokol sites may possibly, even probably, be contemporaneous and the work of the same people, this remains to be proved.
3. The dating of all three sites may still be regarded as open.
4. The *fresher* examples of rock art were not necessarily or even probably executed by the grave builders and the remarkable coincidence of motifs with Turkana animal brands remains to be accounted for or discounted.
5. The specific affinities of the sites with modern Eastern Cushites in Ethiopia are suggestive but hardly conclusive. An obvious lacuna to be filled by further work is the comparison of the petroglyph motifs with Eastern Cushitic animal brands or other art forms; comparable ancient petroglyphs in southern Ethiopia would be an even stronger link.

---

5 With the notable exception of the Eastern Cushitic Elmolo. Heine (1982) on linguistic grounds actually suggests that recent traditional fishing practices on Lake Turkana are likely to go back to Eastern Cushites originating from the Ethiopian highlands who had given up the widespread Cushitic taboo on fish — and hence that the possibility of a Turkwel/E. Cushitic correlation should be re-examined.

6. The past presence of Eastern Cushites in this area is not certain on linguistic grounds.
7. Possible alternative hypotheses need to be considered more objectively, for example association with Nderit ware/Southern Cushites, Turkwel ware/Eastern Nilotes or other groups such as Western or Southern Nilotes.

This paper has not set out to disprove the historical reconstructions proposed by Lynch and Robbins for the Namoratung'a sites, but it does point to some anomalies and uncertainties in the evidence which would make their conclusions a good deal more tentative than is implied in their presentation. The Eastern Cushitic hypothesis remains perhaps the least improbable explanation, but it would not seem justifiable to put its level of confidence any higher than that.

APPENDIX: Survey Procedure

The highest point of each pillar was ascertained with a spirit level and marked with chalk. To avoid magnetic anomalies associated with the stones, two reference pegs were set up some 80 m from the site, the triangle so formed with Stone 1 closing to less than 1^0 on the basis of the compass bearings from the pegs. A theodolite (Kern DKM 1) was set up over Stone 1 and zeroed on the back bearings from the pegs, after which bearings to all the other stones and to Central Island in Lake Turkana were taken twice, circle left and circle right, to the nearest 20″ or less. The instrument was then set up over Stones 5 and 18 successively, zeroed to Stone 1, and bearings again taken to all the other stones. (Stones 1, 5 and 18 are the westernmost stones, regarded by Lynch and Robbins as back-sights). Out of 54 double readings, the maximum difference was two minutes and only four exceeded one minute. Plotting the bearings at a scale of 1:50 shows them to be internally consistent. Table 1 gives the average of each pair of readings rounded to the nearest minute and gives Lynch's published bearings for comparison. The absolute bearings depend on the accuracy of the compass used (4-inch dry prismatic) so a factor of say ±10′ may be allowed for, constant for all the bearings. Magnetic variation should be 33′W. Position of the sites is approximately $35^0\,47'\,34''$E, $3^0\,24'\,44''$N.

Fig. 2 is based on a separate survey using an east-west base-line and off-sets. It shows the outline of the pillars and grave stones at ground level, the dotted line indicating the overhanging top; the crosses are estimates of the original centre point of each stone if they had been set upright. The stones of the surrounding features and those between Stones 11 and 12 were not individually surveyed and are shown conventionally. The approximate angle of lean given in Table 1 was measured with spirit level and protractor.

B. Mark Lynch makes the following comments on the above:

I would like to begin this discussion by thanking Mr. Soper for his thoughtful and meticulous reevaluation of the Namoratunga sites. He raises a number of issues which warrant comment. This provides an opportunity to discuss several issues which have not been addressed in earlier reports on the Namoratunga sites. This report is organized so as to parallel as much as possible Soper's comments

Kalokol Site

Soper's new alignments for the stone 'pillars' at Kalokol differ significantly from the initial measurements taken in 1975 and call for a reevaluation of the calendric reckoning hypothesis, based upon a map which was kindly provided by Mr. Soper. I have recently discussed these new alignments at some length elsewhere (Lynch, 1982, in press), so just a few comments will suffice here. I have no reason to believe the transit utilized in the initial mapping of the site was in error. This transit has