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A Statistical Examination of The Megalithic Sites in Britain

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Introduction

In the past twenty years I have visited some 250 megalithic sites in England and Scotland, and made accurate surveys where there appeared to be anything worthy of survey. The surveys were carefully made plans showing the position of every stone except those obviously loose. Particular attention was paid to the orientation of the plans, the azimuths being determined in nearly every case astronomically by theodolite observations of the sun. This mass of material provides data for a geometrical and statistical study of the sites. The sites examined consist mainly of circles of standing stones, of rows of standing stones, here called alignments, or of a combination of the two. Many of the circles have one or more outliers, i.e. single upright stones outside the ring.

Of the published plans of stone circles, some are lacking in accuracy, and only one or two contain determinations of azimuth and horizon altitudes. Thus, in a study of the possible astronomical significance of stone alignments, etc., they are quite useless. There is also the danger that if published surveys are used the data may be biased and unsuitable for statistical examination. Accordingly in the present paper the material is restricted to sites I have been able to visit, and strict attention has been paid to laying down and adhering to terms of reference in selection. In the analysis presented here, nothing has been excluded which has a bearing on the subject, except one or two sites which I am assured are fakes. A list of all the sites used in this paper, circles and alignments, is given in Table 1, with the latitude and longitude of each.

It is proposed to examine three different things:

(1) The geometry of the "circles".
(2) The possibility that a common unit of length was used.
(3) The possibility that the alignments and the outliers had an astronomical significance.

Each of these presents a different problem in statistical analysis. The data should be adequate, but completely rigid methods of attack are lacking.

The Geometry of the Circles

Of the circles surveyed, some forty seem to be true circles or rather were intended by their erectors to be true circles, two are egg-shaped, and about twelve are circles with part of the circumference flattened in a curious manner. I thought at first that this flattening was due either to accident or to earth movement, but when I surveyed the very large circle near Little Salkeld called *Long Meg and her Daughters*, I saw that here at least the slope of the ground was such that no likely earth movement could have produced the flattening. I accordingly tried to fit the

TABLE 1

List of Sites Used

H = Hebrides. P = Perth and district.
M = Mull and district. G = Galloway and Lowlands.
A = Argyllshire. L = Lake District and North England.
S = South England. D = Derbyshire and district.

No.	*Locality or Name*	*Latitude*	*Longitude*
H 1/1	Callanish I, Lewis	58° 12′	6° 45′
H 1/2	Callanish II, Lewis	58° 12′	6° 44
H 1/3	Callanish III, Lewis	58° 12′	6° 43′
H 1/10	"Steinacleit", Lewis	58° 24′	6° 27′
H 3/8	"Na Fir Bhreige", N. Uist	57° 38′	7° 13′
H 3/11	"Leacach an Tigh Chloiche"	57° 35′	7° 21′
H 3/17	"Pobull Fhinn", N. Uist	57° 34′	7° 17′
H 3/18	"Sornach Coir Fhinn", N. Uist	57° 33′	7° 18′
H 4/2	Gramisdale, Benbecula	57° 28′	7° 18′
H 7/9	Kirkibost, Skye	57° 11′	6° 4′
M 1/4	Dervaig A, Mull	56° 36′	6° 11′
M 1/5	Dervaig B, Mull	56° 35′	6° 10′
M 2/14	Lochbuie, Mull	56° 21′	5° 51′
A 1/2	Loch Nell, Lorne	56° 24′	5° 24′
A 1/4	Loch Seil, Lorne	56° 20′	5° 33′
A 2/6	Carnasserie, Kilmartin	56° 9′	5° 29′
A 2/8	Temple Wood, Kilmartin	56° 7′	5° 30′
A 2/12	Duncracaig, Kilmartin	56° 7′	5° 29′
A 2/14	Kilmichael Glassary, A	56° 5′	5° 27′
A 2/21	Kilmichael Glassary, B	56° 5′	5° 28′
A 3/4	Tayvallich, Knapdale	56° 1′	5° 39′
A 4/4	Ballochroy, Kintyre	55° 43′	5° 37′
A 5/8	Scalasaig, Colonsay	56° 4′	6° 12′
A 6/4	Knockrome, Jura	55° 52′	5° 55′
A 8/6	Machrie Moor, Arran	55° 32′	5° 19′
A 9/2	Etterick Bay, Bute	55° 51′	5° 7′
A 9/7	Kilchattan, Bute	55° 45′	5° 3′
A 11/1	Duntreath, Strathblane	56° 0′	4° 21′
G 3/7	Torhouse, Wigtown	54° 53′	4° 31′
G 3/12	Drumtrodden, Port William	54° 46′	4° 33′
G 4/1	Carsphairn	55° 13′	4° 18′
G 4/9	Loch Mannoch, Galloway	54° 55′	4° 5′
G 4/12	Cambret Moor (W), Creetown	54° 54′	4° 19′
G 4/14	Cambret Moor (E), Creetown	54° 53′	4° 18′
G 6/1	"Twelve Apostles", Dumfries	55° 6′	3° 39′
G 7/2	"Seven Brethren", Dumfriesshire	55° 8′	3° 13′
G 7/4	"Loupin Stanes", Eskdale	55° 15′	3° 10′
G 7/5	"Girdlestanes", Eskdale	55° 14′	3° 10′
G 8/2	"Ninestone Rig", Castleton	55° 16′	2° 45′
G 8/9	"Eleven Shearers", Hownam	55° 28′	2° 20′
P 1/1	Dalchirla, Muthill	56° 19′	3° 54′
P 1/2	Dunblane	56° 11′	4° 0′
L 1/1	"Castle Rigg", Keswick	54° 36′	3° 6′
L 1/2	Setmurthy, Cumberland	54° 40′	3° 17′
L 1/3	"Sunkenkirk", Millom	54° 17′	3° 16′
L 1/4	Boot, Cumberland	54° 25′	3° 16′
L 1/7	"Long Meg and her Daughters"	54° 44′	2° 40′
L 1/9	Glassonby, Cumberland	54° 45′	2° 40′
L 2/2	Tarn Moor, Helton	54° 35′	2° 48′
L 2/13	Oddendale, Shap	54° 31′	2° 38′
L 2/14	Orton, Westmorland	54° 2′	2° 33′
L 3/1	Felkington, Northumberland	55° 41′	2° 7′
L 6/1	"Devils Arrows", Boroughbridge	54° 6′	2° 23′
D 1/2	"Wet Withers", Derbyshire	55° 18′	1° 40′
S 2/1	"Grey Wethers", Dartmoor	50° 38′	3° 55′
S 2/2	Merrivale, Dartmoor	50° 33′	4° 3′
S 2/3	Brisworthy, Dartmoor	50° 28′	4° 1′
S 2/4	Ringmoor Down, Dartmoor	50° 28′	4° 2′
S 2/5	Trowlesworthy, Dartmoor	50° 27′	4° 0′
S 2/7	Lee Moor, Dartmoor	50° 31′	4° 1′
S 3/1	Stanton Drew, Somerset	51° 22′	2° 34′
S 4/1	Winterbourne Abbas, Dorset	50° 43′	2° 33′
S 4/2	Kingston Russell, Dorset	50° 42′	2° 35′
S 6/1	Rollright, Oxfordshire	51° 38′	1° 34′

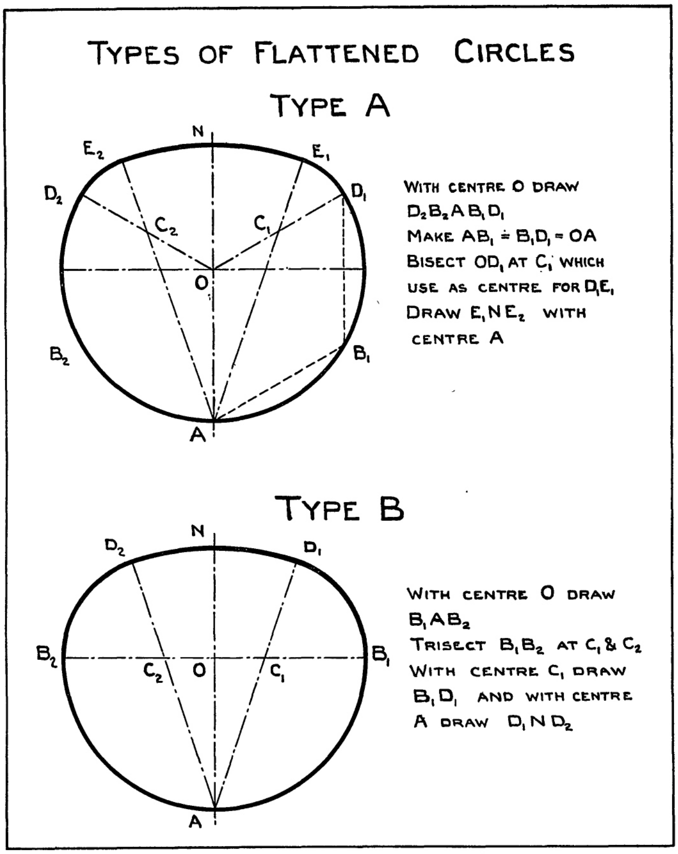
TYPES OF FLATTENED CIRCLES
TYPE A
WITH CENTRE O DRAW
D2 B2 A B1 D1
MAKE AB1 = B1D1 = OA
BISECT OD1 AT C1 WHICH
USE AS CENTRE FOR D1E1
DRAW E1 N E2 WITH
CENTRE A
TYPE B
WITH CENTRE O DRAW
B1 A B2
TRISECT B1 B2 AT C1 & C2
WITH CENTRE C1 DRAW
B1 D1 AND WITH CENTRE
A DRAW D1 N D2

FIG. 1

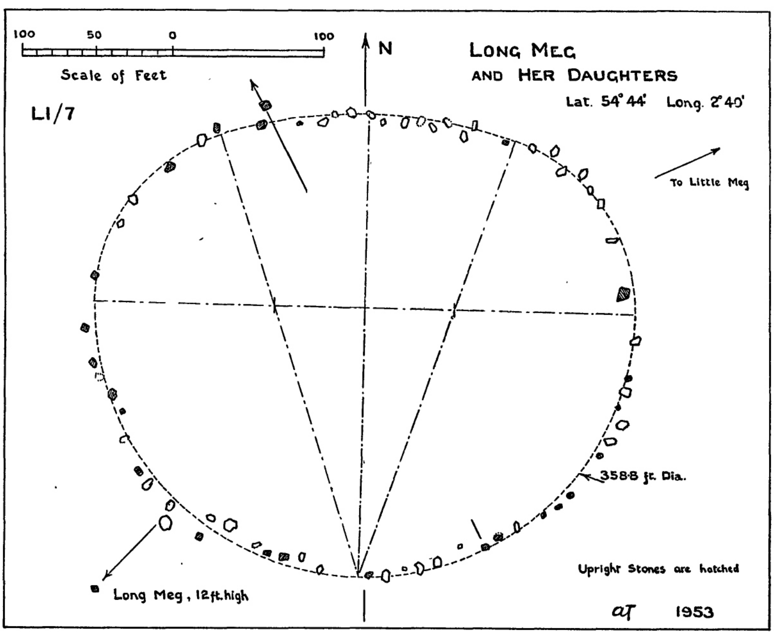
100 50 0 100
Scale of Feet
L1/7
N
LONG MEG
AND HER DAUGHTERS
Lat. 54° 44' Long. 2° 40'
To Little Meg
358·8 ft. Dia.
Long Meg, 12ft. high
Upright Stones are hatched
AT 1953

FIG. 2

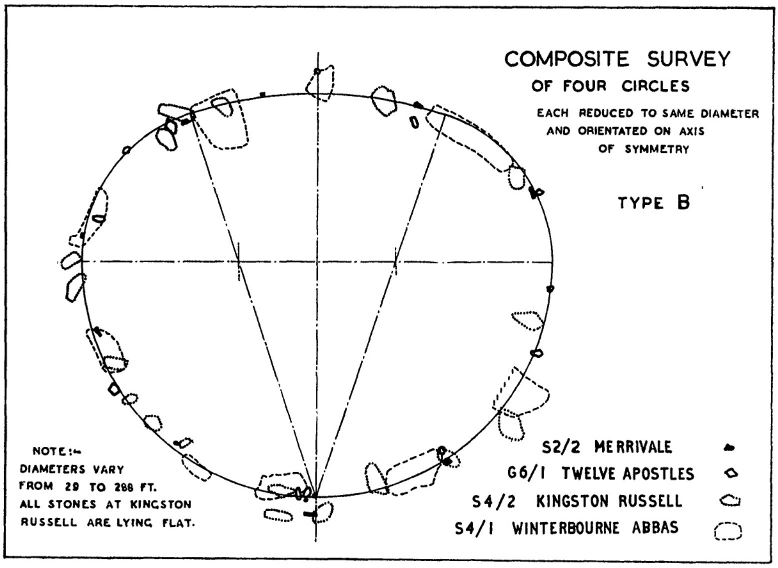
COMPOSITE SURVEY
OF FOUR CIRCLES
EACH REDUCED TO SAME DIAMETER
AND ORIENTATED ON AXIS
OF SYMMETRY
TYPE B
NOTE:–
DIAMETERS VARY
FROM 29 TO 288 FT.
ALL STONES AT KINGSTON
RUSSELL ARE LYING FLAT.
S2/2 MERRIVALE
G6/1 TWELVE APOSTLES
S4/2 KINGSTON RUSSELL
S4/1 WINTERBOURNE ABBAS

FIG. 3

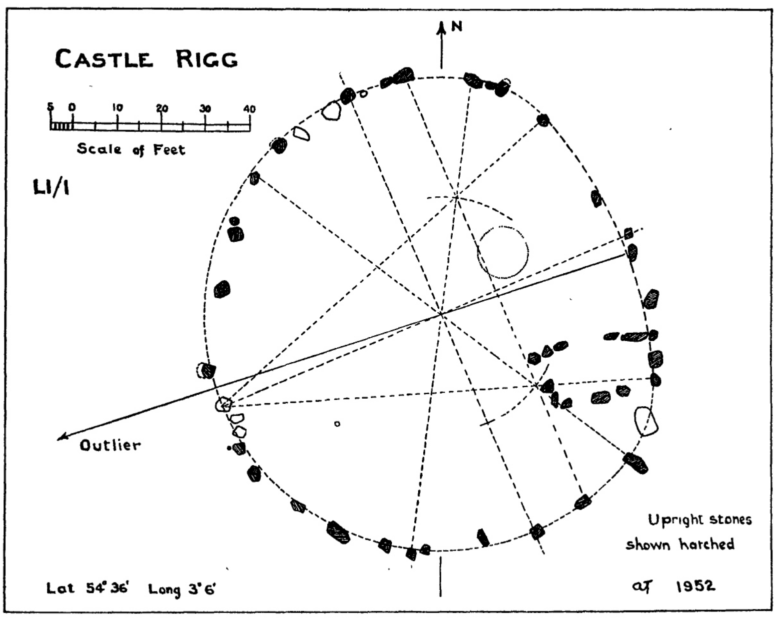
N
CASTLE RIGG
5 0 10 20 30 40
Scale of Feet
L1/1
Outlier
Upright stones
shown hatched
Lat 54° 36' Long 3° 6'
AT 1952

FIG. 4

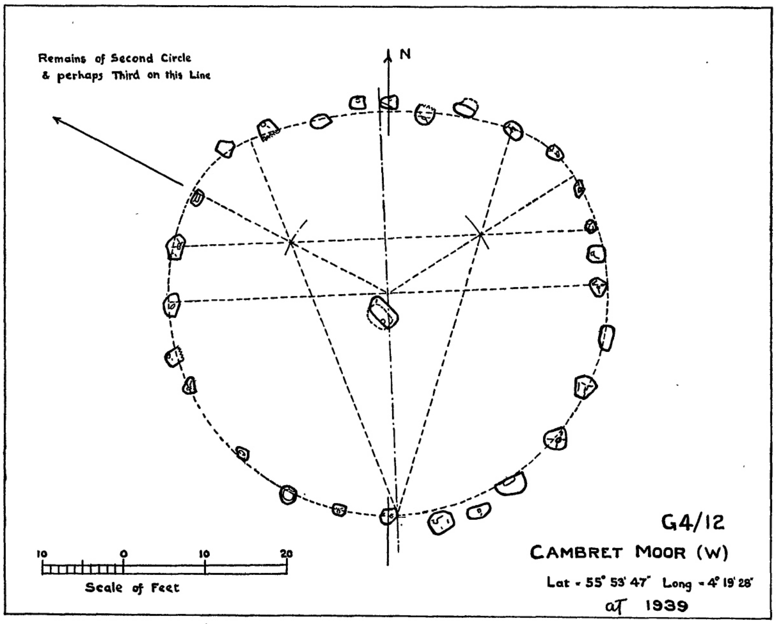
Remains of Second Circle
& perhaps Third on this Line
N
G4/12
CAMBRET MOOR (W)
Lat = 55° 53' 47" Long = 4° 19' 28"
aT 1939
10 0 10 20
Scale of Feet

FIG. 5

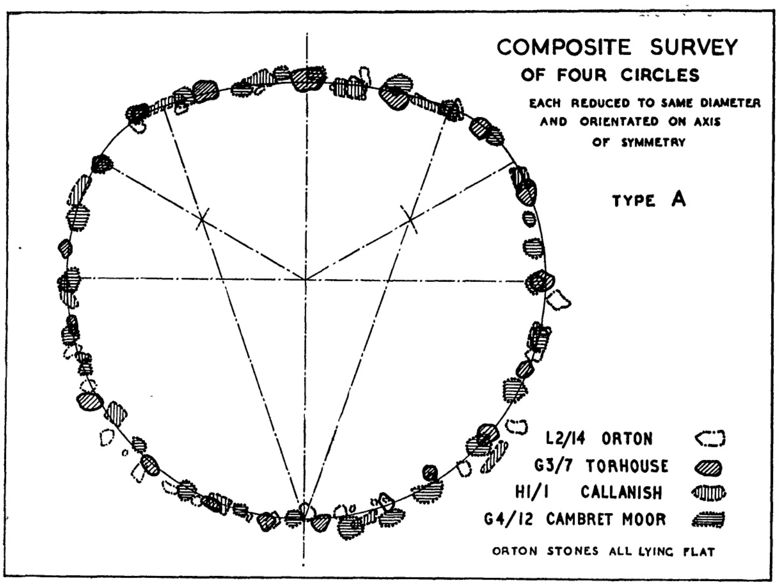
COMPOSITE SURVEY
OF FOUR CIRCLES
EACH REDUCED TO SAME DIAMETER
AND ORIENTATED ON AXIS
OF SYMMETRY
TYPE A
L2/14 ORTON
G3/7 TORHOUSE
H1/1 CALLANISH
G4/12 CAMBRET MOOR
ORTON STONES ALL LYING FLAT

FIG. 6

construction Type B in Fig. 1. It appears from Fig. 2 that the curve passes through the stones as closely as can be expected of any simple geometrical figure. As a start, I assumed a radius R of 180 ft. and further assumed that the axis of symmetry lay north and south. The distance of each stone was then measured from the assumed outline by taking the estimated centre of each upright stone, and the centre of the nearest end of the fallen stones. A least squares solution was then possible, which gave corrections (*a*) to the co-ordinates of the assumed centre, (*b*) to the radius R and (*c*) to the orientation of the axis. The new value of R is 179·4 ft., and the axis of symmetry need only be rotated 1·2 degrees. This geometrical construction, Type B, was found to apply to at least four other sites, all of which are shown in the composite diagram, Fig. 3. In this figure each circle has been reduced to the same diameter, and all superimposed on the same centre with the axes of symmetry in the same direction.

The construction assumed for the second type of flattened circle is shown in Type A, Fig. 1. It will be seen (Fig. 4) that at Castle Rigg, near Keswick, all the necessary construction points, changes of radius and 60° arcs are marked by stones. It is difficult to believe that this could occur by chance, but no method of rigid proof can be produced to show that this construction was actually used by the erectors. In my collection four other circles fall into this group (e.g. see Fig. 5) and these are brought together in Fig. 6. It will be seen that both constructions A and B are such that they could easily be made in the field with no other equipment than a piece of rope and a few stakes. In the surveys so far made there is no definite sign of a flattening intermediate between Type A and Type B.

The Circle Diameters

Tables 2 and 3 contain particulars for all the circles of which I have made a proper survey and for which I can approximate to a diameter. Even with an accurate survey it is sometimes difficult to estimate the diameter to which the circle was originally constructed. Often many of the stones have fallen and it must be remembered that trees may have grown over the site at some time and caused distortion. Unfortunately, there is no sure method of knowing if the circle has been tampered with in recent times by well-meaning persons. Thus, for the two circles at *The Grey Wethers* on Dartmoor, Worth (1953) gives a survey made last century by Lukis, which shows many of the stones fallen, whereas all are now upright. I have used this small copy of Lukis's survey to identify, as far as possible, the stones upright in 1879 and these only were taken into consideration for the estimates of diameter in this paper, but some doubt exists about the south circle.

In nearly all cases the method used to obtain an objective value for the circle diameter was as follows: Having drawn on the survey what appeared to be the best circle (or flattened circle of the appropriate type), the azimuth and distance of the stone centre from the outline were noted for each upright stone. Where fallen stones had to be used a point midway between the stone's centre (as it lies) and the end nearest the assumed outline was taken. The most likely diameter was then calculated by minimizing the sum of the squares of the distances of the stones from the outline (see Appendix II). In some cases where the stones were more or less uniformly distributed round the circumference the process was shortened by taking the mean displacement in each quadrant; then the mean of the four was assumed to be the correction to the radius. Where all stones have fallen the above methods possibly bias the result one way or another, depending on whether the majority of the stones have fallen in or out; otherwise the error should be random. The uncertainty attached to each diameter in the tables is an *estimate* of the standard deviation based principally on the scatter about the outline.

The results from both the English and Scottish circles are shown pictorially in Fig. 7. Each circle is represented by a small Gaussian curve or distribution centred on the diameter. Where two or more overlap, the ordinates are added. There seems to be some advantage in this form of diagram when the data are scanty. It evidently produces a peak approximately over the mean of the values within a group. In the simple case of two equal adjacent values, there will be a single maximum if the distance between the centres of the curves is less than twice the standard deviation, and two peaks with a hollow between if it is greater. The actual curves used had a standard deviation twice as large as that in the tables.

There are four main peaks in the figure near 22, 44, 55 and 66 ft. If the radii were made multiples of a man's height, or a man's span (c.f. the fathom) or something like the Roman double

TABLE 2

Circle Diameters—England

D = Diameter, m = multiplier, z = | D—5·435 m |
Type A and B—see Fig. 1; Type C—Circle

Site	*Assumed Type*	*D (feet)*	*m*	*5·435 m*	*z*	*Remarks*
S 2/4	C	11·9±0·3	2	10·87	1·03	Circle
S 2/7	C	16·0±0·3	3	16·30	0·30	closing
S 2/5	C	22·3±0·3	4	21·74	0·56	avenue
L 2/13	C	24·0±0·5	4	21·74	2·26	Concentric
S 4/1	B	29·7±0·3	5	27·17	2·53	Fair
L 3/1	C	31·5±0·3	6	32·61	1·11	Good
S 2/4	C	41·2±0·3	8	43·48	2·28	—
L 1/9	C	47·7±0·5	9	48·91	1·21	Poor
L 1/4	C	51·7±0·5	10	54·35	2·65	,,
L 1/4	C	53·3±0·5	10	54·35	1·05	,,
L 1/4	C	54·9±0·5	10	54·35	0·55	,,
L 1/4	C	66·3±0·5	12	65·22	1·08	,,
S 2/2	B	67·4±0·3	12	65·22	2·18	? Type
S 2/8	C	81·4±0·3	15	81·52	0·12	Good
L 2/13	C	86·0±0·5	16	86·96	0·96	Concentric
S 4/2	B	91·1±1·0	17	92·39	1·29	All fallen
L 1/3	C	93·7±0·3	17	92·39	1·31	Good
D 1/2	C	96·9±0·5	18	97·83	0·93	With bank
S 3/1	C	103·1±0·5	19	103·26	0·16	Fair
S 6/1	C	103·6±0·3	19	103·26	0·34	Good
S 2/1	C	104·5±0·3	19	103·26	1·24	Re-erected
L 1/1	A	107·0±0·3	20	108·70	1·70	Mostly erect
S 2/1	C	108·5±0·3	20	108·70	0·20	Re-erected
L 1/2	C	113·4±0·5	21	114·13	0·73	Poor
L 2/14	A	145·9±1·0	27	146·74	0·84	All fallen
L 1/7	B	358·8±0·5	66	358·71	0·09	Many fallen
S 3/1	C	370·5±1·0	68	369·58	0·92	,, ,,

TABLE 3

Circle Diameters—Scotland

D = Diameter, m = multiplier, z = | D—5·435 m |
Type A & B—see Fig. 1; Type C—Circle

Site	*Assumed Type*	*D (feet)*	*m*	*5·435 m*	*z*	*Remarks*
A 2/8	C	11·2±0·3	2	10·87	0·33	Good
A 1/2	C	14·0±0·3	3	16·30	2·30	Poor
G 4/9	C	20·9±0·3	4	21·74	0·84	Good
H 7/9	C	21·0±1·0	4	21·74	0·74	Part buried
A 2/12	C	21·0±0·5	4	21·74	0·74	Poor
M 2/14	C	21·8±0·3	4	21·74	0·06	Good
G 8/2	C	23·2±0·5	4	21·74	1·46	Poor
H 1/1	C	24·0±0·5	4	21·74	2·26	Half circle
G 7/4	A	38·7±0·5	7	38·05	0·65	Fair
H 1/1	A	43·3±0·3	8	43·48	0·18	Good
M 2/14	C	44·1±0·3	8	43·48	0·62	,,
A 2/8	C	44·2±0·3	8	43·48	0·72	,,
G 4/12	A	54·5±0·3	10	54·35	0·15	Fair
A 8/6	C	54·9±0·5	10	54·35	0·55	,,
H 1/3	C	55·0±0·5	10	54·35	0·65	Poor
A 1/2	C	65·1±0·3	12	65·22	0·12	,,
G 7/2	A	66·4±0·3	12	65·22	1·18	Fair
G 3/7	A	69·3±0·3	13	70·65	1·35	Good
H 1/2	C	70·0±0·3	13	70·65	0·65	Poor
G 4/14	C	82·1±0·3	15	81·53	0·57	Good
H 4/2	C	87·4±0·3	16	86·96	0·44	Fair
H 3/17		124·0±1·0	23	125·01	1·01	Circle arc
G 7/5	C	130·9±1·0	24	130·44	0·46	Part circle
H 3/18	C	139·0±1·0	26	141·31	2·31	,, ,,
G 6/1	B	288·4±1·0	53	288·06	0·34	? Type

pace, the diameters would be multiples of two of these units, or about 11 ft. In the first place, the Scottish and English values were examined separately to find what value near to 5½ ft. best suited the *diameters.* It appears that a value of 5·435 ft. closely represents both sets. The diameters (D) and the necessary multipliers (m) are shown in Tables 2 and 3. The value of $\Sigma D \div \Sigma m$ is found to be 5·436 for Scotland and 5·434 for England. We now propose to test the significance of the English value when applied to Scottish diameters. It is found that twenty of the twenty-five values fall in the ranges 5·434 ($m \pm \frac{1}{4}$). As the probability with a random distribution is ½, the deviation from the expected value is about three times the standard error. Reversing the procedure, and testing the Scottish value on the English diameters gives about the same result. The latter calculation was, however, sensitive to the exact value of the unit used and it would seem much better to use the method given by Broadbent (1955). To apply either Broadbent's tables or the binomial distribution method to our problem is perhaps not logically defensible, since the unit being tested is derived from the data itself whereas strictly speaking it ought to come from some outside source. An attempt to get over this difficulty has been made by dividing the data into two groups and taking the value obtained in the one group as the value to be tested by the second.

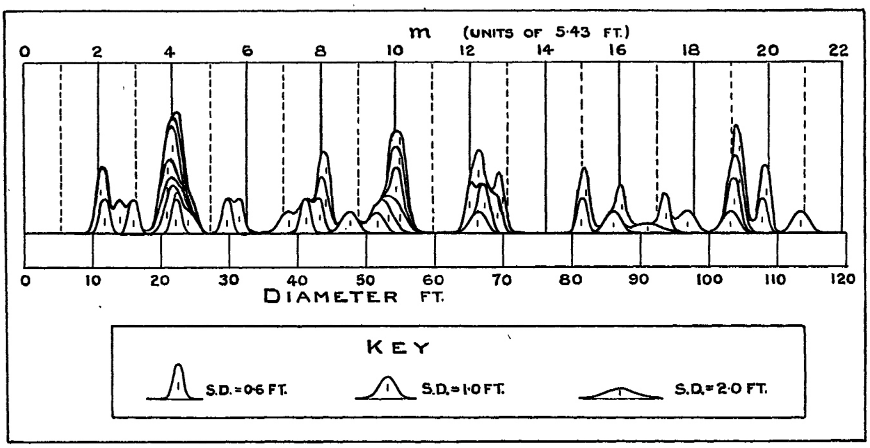

FIG. 7

Broadbent finds the variance of the difference between the quantity and the nearest multiple of the assumed unit. The tables he quotes then give the probability level, allowing for the fact that we may have associated any particular value with the wrong multiple. Thus writing 2δ for the assumed unit of length (5·435 ft. in our case) we put $ns^2 = \Sigma z^2$ where

$$z = | D - 2m\delta |$$

We then enter the table with s^2/δ^2 and deduce the probability level. The results for various groupings of the diameters are given in Table 4. Fig. 7 suggests that these favourable values

TABLE 4

Probability of Obtaining the Observed Groupings if the Circle Diameters had been Random

Group	$s^2 = \frac{\Sigma (D - 2m\delta)^2}{n}$	$\left(\frac{s}{\delta}\right)^2$	*Probability Level*
All English circles	1·744	0·236	<0·05
All Scottish circles	1·097	0·149	0·001
All with D < 80 ft.	1·732	0·235	<0·05
All with D > 80 ft.	0·954	0·129	0·001

D = Diameter; 2δ = assumed unit = 5·435 ft.; m = multiplier.

are obtained because there are a number of the smaller circles with diameters near to 22, 44 and 55 ft. and these were perhaps simply popular sizes. But when the last two groups in Table 4 are compared it appears that a very much better agreement is found for the twenty circles larger than 80 ft. than for those smaller. Thus it appears that a unit of about 5·43 ft., or a sub-multiple thereof, was in common use throughout Britain.

It should be mentioned that the value of the unit (i.e. $\Sigma D/\Sigma m$) is slightly larger for the circles under 80 ft. than for the others. This may be an indication that some of the circles were measured to the inside of the stones instead of the centre, but the evidence does not show that this was universal.

The Astronomical Significance of the Megalithic Remains

There is little doubt that certain of these sites contain a pointer to the rising or setting points of the sun at the solstices and equinoxes, and indeed this is borne out by the results to be given presently. There is a strong suspicion that others contain a similar indication for the sun at May Day, Candlemas, Martinmas and Lammas—days which are midway in time between the equinox and the solstice. Some of the evidence for the eight dates is contained in a recent paper (Thom, 1954). But as it might be argued that the evidence for the four intermediate dates (declination $\pm$ 16°) is not conclusive, it is proposed to ignore the possibility that they were used. The present investigation is mainly a study of the evidence for the hypothesis that many of the sites contain pointers to the rising or setting points of the first magnitude stars. All pointers found which do not indicate the solstice or equinox are grouped together and examined as indicators for these stars.

The argument is frequently put forward that there are so many bright stars that one or another is certain to rise or set on any chosen point on the horizon. The weakness of this contention becomes apparent when it is realized that in our latitudes only about a dozen first magnitude stars are so placed that, on a level horizon, they do in fact rise or set. The others are always either above or below the horizon. It will also be apparent that, within reason, it is not very important what magnitude we decide to take as the lower limit, as the use of a larger number will increase the number of "successes" to be expected as well as the actual number of successes. So I have, arbitrarily, used only first magnitude stars, i.e. all stars brighter than mag. 1·5.

The Pointers

For present purposes a direction in the horizontal plane (i.e. an azimuth) can be specified by a pointing consisting of:

(*a*) An outlier to a circle.
(*b*) The line joining two stones.
(*c*) The line joining the centres of two circles or sites.
(*d*) Two vertical slabs in line, thus — —.
(*e*) A row of three or more stones.
(*f*) A prominent natural feature, e.g. a mountain top indicated by (*b*), (*c*), (*d*) or (*e*) or more simply by the long edges of a single vertical slab.

To restrict the material to a manageable amount and to make the terms of reference more definite, it is proposed here to deal only with (*a*), (*d*) and (*e*).

In listing examples according to the above definitions it is still difficult to know where to draw the line, e.g. should a group of stones like *The Whispering Knights* at the Rollright Circle (S6/1) be treated as an outlier? I decided to do so because of the "hole stone" in the circle periphery on the line from the centre to *The Whispering Knights*, and if *The Whispering Knights* why not also include *Little Meg* as an outlier to the circle at *Long Meg and her Daughters*? Other borderline cases have been excluded as being more properly classified under (*c*) above. None of the Dartmoor double rows is included.

A histogram of the azimuthal distribution shows very little of interest except a group of pointers near the true north direction. It is proposed to accept these as intentional and to ignore them.

Further progress can only be made by converting the azimuths to declinations. For this we use the expression:

$$\sin \delta = \sin h \sin \lambda + \cos h \cos \lambda \cos A \qquad . \quad . \quad . \quad . \quad (1)$$

where h is the observed angular altitude of the hill or sea horizon corrected for astronomical refraction, A the azimuth, δ the declination and λ the latitude. Each point on the horizon corresponds to a definite value of the declination and so may perhaps be associated with a particular heavenly body, e.g. a star, since a star's declination changes only slightly in a century.

TABLE 5

Outliers

Site	*Azimuth*	*Altitude*	*Declination*	*Site*	*Azimuth*	*Altitude*	*Declination*
H 1/1	See refs		+ 0·3°	G 4/1	100·4°	3·1°	− 3·5°
H 1/1	8 and 10		+ 7·0°	G 4/12	289·5°	0	+10·7°
H 1/1	9·9°	1·6°	+32·5°	G 7/2	228·0°	1·0°	−21·9°
H 1/10	88·5°	0·7°	+ 1·0°	G 7/2	236·1°	0·7°	−18·4°
H 3/17	288·9°	0·7°	+10·2°	G 7/2	251·7°	0·2°	−10·6°
H 3/18	313·1°	0·6°	+21·6°	G 7/2	46·6°	1·3°	+23·9°
H 3/18	318·5°	0·7°	+24·0°	G 7/2	334·3°	0·6°	+31·2°
M 2/14	223·6°	0·4°	−23·7°	L 1/1	252·1°	3·2°	− 7·8°
M 2/14	237·0°	2·1°	−16·0°	L 1/7	223·4°	1·1°	−24·2°
M 2/14	123·4°	6·8°	−12·0°	L 1/7	65·1°	3·4°	+16·7°
A 1/2	147·5°	6·6°	−21·8°	L 1/7	333·6°	0·3°	+31·1°
A 2/8	141·2°	1·8°	−24·4°	L 2/2	201·3°	3·6°	−29·4°
A 2/8	135·0°	3·0°	−20·8°	L 2/13	252·0°	1·7°	− 9·2°
A 2/8	115·7°	7·1°	− 8·2°	L 2/13	12·5°	0·7°	+34·7°
A 3/4	32·8°	1·9°	+29·5°	S 3/1	94·9°	1·5°	− 2·1°
A 8/6	282·2°	0·9°	+ 7·3°	S 3/1	72·3°	0·7°	+11·2°
A 9/2	77·5°	1·4°	+ 7·8°	S 3/1	63·5°	0·8°	+16·5°
G 3/7	89·6°	1·0°	+ 0·5°	S 6/1	94·9°	−0·2°	− 3·7°
G 3/7	81·2°	1·5°	+ 6·0°	S 6/1	28·7°	−0·1°	+32·0°

TABLE 6

Alignments

Site	*Azimuth*	*Altitude*	*Declination*	*Site*	*Azimuth*	*Altitude*	*Declination*
H 3/8	288·9°	2·3°	+11·7°	A 2/21	166·1°	1·9°	−31·2°
M 1/4	341·1°	0·0°	+30·8°	A 4/4	225·0°	0·0°	−24·0°
M 1/5	154·0°	1·6°	−28·4°		44·2°	6·6°	+29·6°
	334·0°	0·8°	+30·3°	A 5/8	222·7°	0·7°	−24·0°
A 1/4	146·8°	6·9°	−21·3°	A 6/4	73·7°	1·9°	+10·4°
	326·1°	5·3°	+32·1°	A 9/7	310·9°	0·7°	+21·9°
A 2/6	168·7°	2·4°	−30·9°		134·5°	2·5°	−21·3°
A 2/8	203·5°	0·3°	−30·9°	A 11/1	56·7°	7·2°	+24·0°
	23·5°	2·2°	+32·5°	G 2/4	240·5°	2·4°	−14·8°
A 2/8	149·6°	2·0°	−27·1°	G 3/12	43·3°	0·4°	+24·8°
	329·6°	5·8°	+34·0°	G 8/9	94·7°	4·1°	+ 0·5°
A 2/12	151·9°	1·1°	−28·8°	P 1/1	237·3°	5·6°	−12·7°
	331·9°	3·1°	+32·2°		57·3°	1·8°	+18·7°
A 2/12	140·7°	2·3°	−23·7°	P 1/2	195·0°	1·5°	−31·5°
	320·7°	3·1°	+28·2°		13·5°	0·5°	+32·7°
A 2/14	138·2°	3·4°	−21·7°	L 6/1	150·9°	0·4°	−30·9°
	318·2°	1·9°	+26·0°		330·9°	0·7°	+31·0°

All the available data for pointers giving declinations between $-35°$ and $+ 35°$ (the limits for latitude 55° with a flat horizon) are contained in Tables 5 and 6, where there are particulars for thirty-eight outliers and twenty-two alignments. The distribution in declination of all pointers is shown in Fig. 8, where each observed declination is shown, as for the circle diameters, by a small Gaussian curve placed with its centre over the observed declination. The width of the curve used corresponds to a standard deviation of about $\pm 0{\cdot}25°$ which is roughly the uncertainty given by an outlier 100 ft. distant.

A difficulty arises with the alignments, in that we do not know which way to look. For ten of these the direction pointed has, for one reason or another, been assumed; for the other twelve it seemed best to use both directions, adjusted slightly to any natural feature which seemed to be indicated. In the two-directional alignments one pointing will be spurious* and allowance will

* That is unless we assume that the erectors were ingenious enough to make use of both directions by taking advantage of varying hill altitudes.

have to be made for this in the subsequent analysis. An alternative procedure was considered, namely, to assume that we should always look along the line from the lowest stone to the highest. This procedure would tend to make Fig. 8 still more convincing, but it seems more logical at this stage to sight from both ends. Those pointings which at the site seemed more impressive have been shaded, and it appears that nearly all the shaded areas lie in one or other of the concentrations. On the figure the declinations of all first magnitude stars in the range are shown plotted for dates between 2400 and 1800 B.C. (Neugebauer, 1912). The declination of the sun, calculated from Newcomb's constants (Newcomb, 1895), at important days of the year has also been shown. The reason for expecting the sun's declination from the equinoctial sites to differ from zero is given fully in a recent paper (Thom, 1954). It depends on the fact that the erectors had no astronomical instruments as we know them, but could only define the equinox as that date on which the sun set over a mark which had been established as marking the setting point exactly half a year before; half a year later it would be back at the same mark. On these dates the sun's declination (2000 B.C.) would be not zero but, owing to the ellipticity of the earth's orbit, $+ 0 \cdot 5°$.

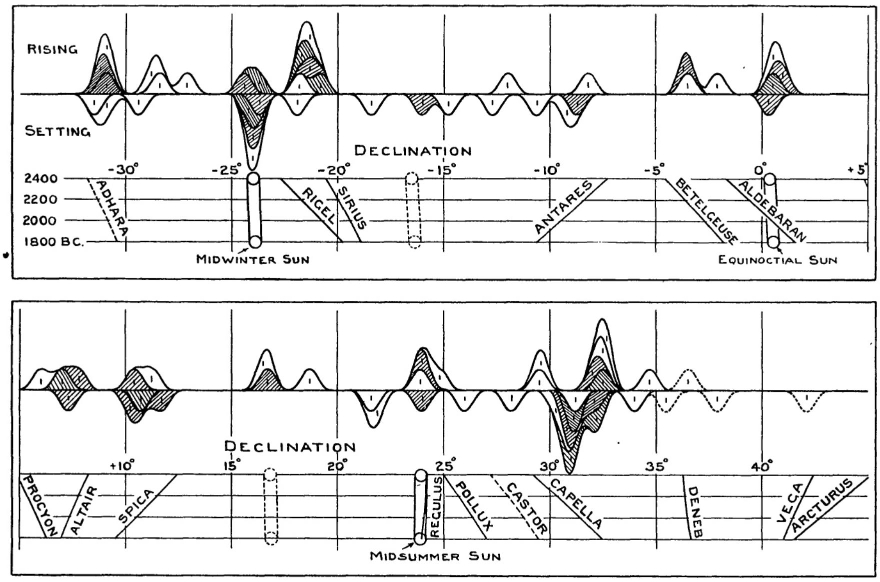

FIG. 8

It will be seen that many of the observed values agree with the sun or star declinations for a date around 2000 B.C. The agreement is more impressive when it is recalled that at least twelve of the values are, as explained above, spurious.

To examine more closely the significance of the agreements the following procedure was adopted: Eight periods, of 200 years, were each treated in the same way. The range of declination of the star in the 200 years was widened, arbitrarily, by $0 \cdot 2°$ at each end. For the sun the widening was $0 \cdot 5°$ since there is uncertainty as to which edge of the sun the builders observed, and the semi-diameter is about $0 \cdot 3°$. It is proposed to call these widened intervals "pockets" and the number of pointers falling into the pockets the "catch". The catch by the sun at the solstices and equinoxes is definitely significant (see later). Accordingly these pockets and all observed pointers caught in them were removed before the data was further examined.

A difficulty now appears. If the distribution of pointers is random in azimuth it will not be so in declination. A greater concentration will be obtained in the higher part of the declination range. In fact declination is not linear with azimuth and therefore logically we should convert the declination pockets into azimuth pockets. Each pocket in the declination range was multiplied by the factor $dA/d\delta$ as calculated from (1) for zero horizon altitude and for a mean latitude of 55°.

Evidently the integral of this expression over the mean declination range (which for $\lambda = 55°$ is $\delta = \pm 35°$) is 180°. We have in fact made the analysis at least approximately linear in azimuth. It may seem strange that we first reduced the azimuths to declinations and now seek to revert to azimuth; but we had to convert to declination first in order to determine the catch accurately. If all the sites were in a flat plain and all in the same latitude this procedure would, of course, be unnecessary. When the analysis is made entirely on a declination basis the results are very little different, indicating that we are making a sufficiently close approximation by using the method described. The labour involved in any rigid method would be prohibitive, necessitating the measurement of horizon altitudes over a range of azimuth at every site.

Sun catch.—The sun, in latitude 55° at the equinoxes and solstices, provides round the horizon six pockets, with a total width of about 12·9°; so $p_s = 12{\cdot}9/360 = 0{\cdot}036$. The number of pointers falling into the sun pockets from outliers and alignments is twelve or thirteen (depending on the date) out of a total of seventy-two. On any method of reduction this is significant, so it is proposed to proceed as mentioned above and remove the sun pocket and the sun catch before analysing the star catch.

Star catch—outliers.—Let us illustrate first by the simple case of the outliers, since these are essentially one-directional. From the total azimuth range of 360° remove the sun pockets leaving 347·1° (see Table 7). The total width of the pockets covered by the first magnitude stars, allowing for overlap, was carefully computed for each 200 years' interval and expressed as a fraction p of 347·1°. The actual catch minus the expected (on a random distribution) enables the value of the deviation to be expressed as a fraction of the standard error $\sqrt{Np(1-p)}$. It will be seen (Table 8) that the mid-dates 2100 and 1900 B.C. stand out clearly with a high significance level; all the other dates show a value below significance.

TABLE 7

Open Pockets

Mid-date B.C.	2700	2500	2300	2100	1900	1700	1500	1300
S = Sun pocket	12·9°	12·9°	12·9°	12·9°	12·9°	12·9°	12·9°	12·9°
A = Total arc to be used = (360° − S)	347·1°	347·1°	347·1°	347·1°	347·1°	347·1°	347·1°	347·1°
a = Total star pocket	39·5°	47·5°	50·8°	48·7°	48·3°	47·4°	51·8°	48·9°
a_0 = Total pocket arc facing pocket arc	2·5°	2·2°	3·7°	0·0°	5·5°	2·3°	0·0°	0·0°
$a - a_0$	37·0°	45·3°	47·1°	48·7°	42·8°	45·1°	51·8°	48·9°
$p_1 = (a - a_0)/A$	0·107°	0·131°	0·136°	0·140°	0·123°	0·130°	0·149°	0·141°
$p_2 = a_0/A$	0·007°	0·006°	0·011°	0·0°	0·016°	0·007°	0·0°	0·0°
$p = p_1 + p_2$	0·114°	0·137°	0·147°	0·140°	0·139°	0·137°	0·149°	0·141°

TABLE 8

Outliers Only

Mid-date B.C.	2700	2500	2300	2100	1900	1700	1500	1300
Total number	38	38	38	38	38	38	38	38
Sun catch	7	7	7	7	8	8	7	7
N = difference	31	31	31	31	30	30	31	31
Star catch	4	2	7	14	10	4	6	4
Np = expected catch	3·5	4·2	4·6	4·3	4·2	4·1	4·6	4·4
Deviation	0·5	2·2	2·4	9·7	5·8	0·1	1·4	0·4
$\sigma = \sqrt{\{Np(1-p)\}}$	1·77	1·91	1·97	1·93	1·90	1·88	1·98	1·94
Deviation/σ	0·28	1·15	1·22	5·02	3·05	0·05	0·71	0·21

Star catch—alignments.—The alignments need more careful handling. For all the sites (or for any selected group of sites) let n_1 = number of alignments for which the direction of sighting is assumed known, plus the number of pointers given by outliers, n_2 = number of two-directional alignments, then $N = n_1 + 2n_2$ = number of pointers.

We must introduce a factor to allow for the fact that a two-directional alignment may pick up an open pocket at each end. Thus, let p_2 = proportion of catching pockets or parts of pockets which fall on the horizon opposite other catching pockets expressed as a fraction of 347·1°, and let p_1 = proportion of pockets not opposite another pocket, then it appears* that the expected catch is given by

$$c_e = (n_1 + 2n_2)(p_1 + p_2) = Np \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and the variance of the catch is

$$\sigma^2 = n_1 p(1 - p) + 2n_2\{p_1(1 - 2p_1) + 2p_2(1 - p_2) - 4 p_1 p_2\}. \quad . \quad . \quad (3)$$

It is impossible to calculate p_2 accurately. The value given (Table 7) is for latitude 55° and assumes a horizontal horizon all round. It is relatively so small that any error here would seem to be negligible.

The results for the alignments alone are shown in Table 9. It will be seen that the significance again attains a maximum for the mid-date 2100 B.C., but that all other dates fall below significance.

TABLE 9

Alignments Only

Mid-date B.C.	2700	2500	2300	2100	1900	1700	1500	1300
Total number	34	34	34	34	34	34	34	34
Sun catch	5	5	5	5	5	5	5	5
$N = n_1 + 2n_2$	29	29	29	29	29	29	29	29
$2n_2$	22	22	22	22	22	22	22	22
n_1	7	7	7	7	7	7	7	7
Star catch	4	4	4	8	4	2	3	2
Np = expected catch	3·3	4·0	4·3	4·1	4·0	4·0	4·3	4·1
Deviation	0·7	0	0·3	3·9	0	2·0	1·3	2·1
σ (see Eq. 3)	1·67	1·77	1·84	1·75	1·84	1·78	1·79	1·7
Deviation/σ	0·4	0	0·2	2·2	0	1·1	0·7	1·2

Outliers and Alignments

Since strong doubts have been expressed about the astronomical explanation of alignments, etc., it is safer to assume that there is no *a priori* reason for testing the theory. Hence, as in the case of the diameters, I have divided the data into two geographical groups:

(*a*) All North-West of the River Clyde (Table 10) and
(*b*) All South-East of the River Clyde (Table 11).

TABLE 10

Outliers and Alignments N.W. of Clyde

Mid-date B.C.	2700	2500	2300	2100	1900	1700	1500	1300
Total number	46	46	46	46	46	46	46	46
Sun catch	8	8	8	8	9	9	8	8
$2n_2$	20	20	20	20	20	20	20	20
n_1	18	18	18	18	17	17	18	18
Star catch	5	4	8	12	9	4	7	4
Deviation/σ	0·4	0·6	1·1	3·3	1·8	0·6	0·6	0·7

TABLE 11

Outliers and Alignments S.E. of Clyde

Mid-date B.C.	2700	2500	2300	2100	1900	1700	1500	1300
Total number	26	26	26	26	26	26	26	26
Sun catch	4	4	4	4	4	4	4	4
$2n_2$	2	2	2	2	2	2	2	2
n_1	20	20	20	20	20	20	20	20
Star catch	3	2	3	10	5	2	2	2
Deviation/σ	0·3	0·6	0·1	4·3	1·2	0·6	0·8	0·7

* The author is indebted to Mr. J. M. Hammersley for the necessary analysis.

The results show the same characteristics as before. It might be argued from the figures that there is a tendency for the northern group to be a little later than the southern.

Taking all pointers together (Table 12) the value of deviation/standard error is 5·2 for the date 2100 B.C., indicating a very high significance.

TABLE 12

All Outliers and Alignments

Mid-date B.C.	2700	2500	2300	2100	1900	1700	1500	1300
Total number	72	72	72	72	72	72	72	72
Sun catch	12	12	12	12	13	13	12	12
$2n_2$	22	22	22	22	22	22	22	22
n_1	38	38	38	38	37	37	38	38
Star catch	8	6	11	22	14	6	9	6
Deviation/σ	0·5	0·8	0·8	5·2	2·2	0·9	0·0	0·8

Effect of Extending the Magnitude Range

An examination of Fig. 8 shows that there is a group of pointers over the second magnitude star Adhara. Although perhaps we should not change the terms of reference after starting the analysis, it is interesting to see what happens when we extend the range from magnitude 1·5 to magnitude 1·6. Only two more stars now come in, namely Adhara and Castor. Repeating the whole calculation exactly as before we find the results shown in Table 13. The significance for the dates 2100 and 1900 B.C. has increased still further, but, as before, no significance emerges for the other dates.

TABLE 13

All Outliers and Alignments (with Adhara and Castor)

Mid-date B.C.	2700	2500	2300	2100	1900	1700	1500	1300
Total number	72	72	72	72	72	72	72	72
Sun catch	12	12	12	12	13	13	12	12
Star catch	9	6	14	28	19	9	11	11
$p_1 = (a - a_0)/A$	0·148	0·170	0·173	0·158	0·157	0·158	0·179	0·173
$p_2 = a_0/A$	0·007	0·006	0·011	0·017	0·016	0·016	0·005	0·0
Deviation/σ	0·1	1·6	1·1	6·1	3·1	0·5	0·0	0·2

Other possible Explanations of the Azimuths

To be logical we must examine all imaginable hypotheses. Since significant grouping does not take place unless the azimuths are converted into declinations, we need only consider astronomical explanations, but there may be others than that already examined. For declinations in the range $\pm 24°$ one can always explain any declination by associating it with a particular day in the solar year. For declinations $\pm 28°$ other writers have pointed out that this is the maximum possible lunar declination, and in Fig. 8 we do seem to have one or two pointers near to these values. Finally, the group of pointers near $\pm 32°$ might have been erected to show the most northerly and southerly setting points of the planet Venus. But none of these alternative explanations gives as convincing a picture of the whole range as that provided by the sun and first magnitude stars.

Future Work

If the geometrical constructions suggested in Fig. 1 were not actually employed by the builders, then one would expect that the estimates of diameters obtained in Tables 2 and 3 by the use of these constructions would be at variance with those obtained from the true circles; but they appear to show significant agreement. Were the number larger more reliance could be placed on this method of assessing the probability that these patterns were actually used. This indicates the importance of obtaining many more accurate surveys.

The validity of the astronomical hypothesis having been established, the terms of reference might be widened to include more pointers by accepting the definitions (*b*), (*c*) and (*f*). This increases the data to such an extent that the country can be examined district by district. At the same time, it would be an advantage to use a date interval smaller than 200 years in order to get a more precise value for the mean date in an area.

It will be appreciated that in building up the data, much ultimately depends on the investigator and therefore the validity of his conclusions depends upon his judgment and honesty. Were it possible to publish all the observed data, with surveys where necessary, the whole case could be better established. Until a method of publishing in considerable detail can be found, all that can be done is to say that the stated terms of reference have been adhered to, and that nothing which can affect the conclusions has been suppressed.

Conclusion

The study of a single megalithic site can only provide a limited amount of information, more especially as it is unlikely that there is such a thing to-day as a completely undisturbed example. We must examine many, and this immediately makes it necessary to introduce statistical methods of analysis. There are a number of problems to be solved and some of these are presented in such a form that they seem to call for special treatment and methods.

From an examination of three of these problems it appears that many of the circles follow certain geometrical designs; that a universal unit of length was used in setting these out on the ground and that many of the constructions carry indications of astronomical uses beyond those associated with the sun. Existing statistical theory allows us to be reasonably certain about the last two statements, but there seems to exist no method of attaching a value to the probability that the geometrical designs suggested were intended.

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Appendix I

Other Information on Diameters

All other workers' measurements of diameters, known to me at the time of writing, are contained in Table 14. Analysis by Broadbent's method, using the unit (5·435 ft.) derived from my own measurements gives a probability level between 0·01 and 0·001. It thus appears that these values lend strong support to the hypothesis of a universal unit of measurement.

Combining *all* the data an interesting point arises. Putting, as before,

$$z = | D - 5{\cdot}435\, m |$$

z ranges from 0 to 2·7175.

Dividing this range into four equal parts, we find the numbers in each to be: 39, 29, 6 and 12. Does the high value (namely 12) in the upper range indicate that some of the circle diameters were made multiples of half a unit?

The very high proportion of even values of m in all three Tables 2, 3 and 14 will also be noticed. In fact, out of a total of eighty-six circles fifty-six have an even value of m, a very unlikely proportion in a random distribution.

TABLE 14

Diameters from Other Sources

Site	*Diameter*	5·435 *m*	*m*	*Ref.*	*Notes*
Stonehenge, Blue Stone	78·0	76·1	14	3	—
„ Trilithons	103·0	103·3	19	4	Outside 108, inside 98.
„ Z holes	130·0	130·4	24	3	—
„ Y holes	180·0	179·4	33	3	—
„ Aubrey holes . .	288·0	288·1	53	3	—
Egryn Abbey	111·0	108·7	20	3	—
„ „	159·0	157·6	29	3	—
Image Wood	11·3	10·9	2	2	—
Cairnwell	28·0	27·2	5	2	—
Binghill	33·6	32·6	6	2	—
Raes of Clune	54·2	54·4	10	2	—
Aquorthies	72·0	70·7	13	2	—
Auld Kirk o' Tough	102·7	103·3	19	2	—
Fernworthy	65·5	65·2	12	1	One foot added to reduce to stone centres.
Langstone Moor	58·0	59·8	11	1	
Down Ridge	82·0	81·5	15	1	
Buttern Hill	82·0	81·5	15	1	
Scorhill	89·0	87·0	16	1	
Sherberton	97·0	97·8	18	1	
Cordon Whitemoor	67·0	65·2	12	1	
Crick Barrow	92·0	92·4	17	6	—
Rempstone	76·0?	76·1	14	7	Scaled.
Overton	130·0	130·4	24	3	—
„	65·0	65·2	12	3	—
„	46·0	43·5	8		Scaled from small plan.
„	32·0	32·6	6		
„	12·0	10·9	2		
Drannandow	89·0	87·0	16	5	Scaled.
Claughreid	34·0	32·6	6	5	Scaled, Type B.
Clava Circles	53·0	54·4	10		Communicated.
„ „	22·0	21·7	4		
„ „	108·0	108·7	20		
Bar Brook	43·0	43·5	8		Communicated by K. Shaw.
Ashopton	50·0	48·9	9		

APPENDIX II

Least Squares Solution for "Circle" Diameters

Let ACB Fig. 9 be the assumed outline the size being specified by $R = OB$

let $\Delta =$ distance of a stone S outside the ring
$\rho = OC$
$\beta =$ angle between OC and the normal to the outline

let R increase by a small amount r then ρ increases by $\rho r/R$ and so Δ decreases by $(\rho r \cos \beta)/R$.

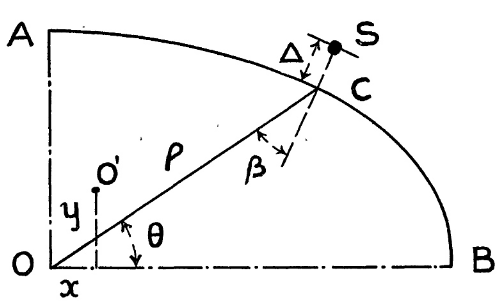

FIG. 9

Now let the assumed centre *O* move to x, y and the whole assumed outline *OAB* rotate through φ then Δ becomes approximately

$$\Delta - (\rho r \cos \beta)/R - x \cos (\theta + \beta) - y \sin (\theta + \beta) - \varphi\rho \sin \beta$$

Equating this to zero gives the necessary relation which can be used for the least squares solution to determine r, φ, x and y. For all the flattened circles except L 1/7 the process was shortened by assuming φ to be zero.

Discussion on Professor Thom's Paper

Professor M. G. Kendall: I move the vote of thanks to Professor Thom with great pleasure. We always give a special welcome to the application of statistical methods to a new field and Professor Thom has provided us with a paper which is not only of great intrinsic interest but raises some fresh problems for the theoretical statistician. One must admire the immense amount of field work which lies behind the paper as well as the moderate and careful way in which the conclusions are stated.

Of the three hypotheses which Professor Thom propounds, the first, as he says, does not seem amenable to a probabilistic treatment but his diagrams seem to me very convincing. His second hypothesis also is very plausible. One would suppose that the unit of length, if there was one, would be in terms of paces rather than heights. One can easily suppose that distances were paced out but if the individuals who constructed these circles were laid end-to-end in a long line one would, to misquote Dorothy Parker, be rather surprised. But if the measurement was done by pacing, the error of a given length would depend on two factors, the number of paces and the deviation from the average of the individual who was making the paces. The quantum hypothesis under test would then be that the variance of an observed length was of the form $an + b$, where n is the number of paces, a is a component representing the error variance of the individual and b is a component representing the variance between individuals. I am not sure whether this is in fact the test which Professor Thom has applied.

I approach the third hypothesis with some prejudice. We are asked to suppose that the peoples who erected these circles had common shapes of construction and a common unit of measurement; but that when it came to alignment they chose the rising of any star as long as it was a bright one. I suppose that such a thing is possible but for me (whose ignorance, I may say in passing, is literally both monumental and astronomical) the prior probability of the hypothesis is lowered. Secondly, it appears that the azimuths are more or less irregularly distributed but the declinations, to which they are simply related by equation (1), are not, Thirdly, Professor Thom uses two arguments which strike me as dubious. The first, on p. 283, is that "it is not very important what magnitude we decide to take as the lower limit, as the use of a larger number will increase the number of 'successes' to be expected as well as the actual number of successes." It seems to me that if one took enough stars one would get a perfect catch, which would prove nothing at all. Again, on p. 285, Professor Thom says "the agreement is more impressive when it is recalled that at least twelve values are spurious". But may this not be begging the question? We do not know that the spurious values are the ones not recorded among the catch.

This catching of stars bears a strong resemblance to one of my favourite forms of skittles, the detection of periodicities in time-series. I therefore constructed from tables of random numbers a set of random azimuths, matching them against the values of h and λ given by Professor Thom, and calculating the declinations. On this basis I get, analogously to Professor Thom's Table 13 (all outliers and alignments, with Adhara and Castor)

Mid-date B.C.	2700	2500	2300	2100	1900	1700	1500	1300
Sun Catch . . .	2	2	2	2	2	2	3	3
Star Catch . . .	5	9	10	9	15	13	13	16

I am far from sure, even with Professor Thom's assistance, that I have done exactly the same thing as he did (apart from the azimuth values) and the experiment would bear repetition. But so far as it goes, it confirms his findings. My values are more or less uniform, whereas his show a peak at 2100 B.C. It would seem that there really is something to be explained here. Whether the orientation by star rising is the correct one is more arguable. Even the maximum catch, sun and stars together, account for only about half the total orientations. Nor are the brightest stars (and Sirius in particular) caught the most frequently. I hope Professor Thom will forgive me if I say that I still have some doubts; but I nevertheless wish to congratulate him on a very stimulating paper which opens up many new lines of thought both in archaeology and statistics.

Mr. Hammersley (in seconding the vote of thanks): I am very glad to have this opportunity of paying tribute to Professor Thom's work. What impresses me most about his paper is the breadth of scholarship involved and the wide range of talents necessary to execute the work. Clearly with archaeological work of this character, it is not enough to be a mere archaeologist: one must also be a local historian, a surveyor, an astronomer, an engineer, a statistician, a yachtsman, and a hiker all rolled into one. In these days, when we are apt to pay too much lip-service to narrow specialization and routine research by battalions of doctors of philosophy, it is pleasant to recollect that savants are not yet an extinct species and that those who prospect the no-man's-land between two or more branches of science are very often the real instigators of progress. Not only do such people raise new problems; but they raise them in refreshingly unorthodox ways, which cause us to re-examine in a fresh and critical light the existing foundations of the orthodox structure.

There are two features of Professor Thom's data which are quite obvious and yet worth stressing. In the first place none of his data is replicable; and in the second place they are man-made. Those who work in physical or biological sciences usually have the opportunity of testing a hypothesis by collecting a completely fresh set of data and seeing whether or not it obeys the hypothesis. When they do this, they are testing how well the hypothesis fulfils the essential role it is designed to fulfil, namely that of forecasting data in general as opposed to describing data collected in a single experiment. This is not possible here; there may perhaps be a few undiscovered megalithic sites in the country, but there will certainly not be enough to provide an independent test of Professor Thom's conclusions. Since there is thus no question of forecasting future data, let us examine the character of Professor Thom's hypotheses. They relate, on the one hand, to the dates of construction of these Druid circles, and, on the other hand, to the methods of constructing them. This brings me to the question of the data being man-made. Man is an inveterate pattern-maker; and what Professor Thom is doing in assigning a geometry or a unit of length to these circles is choosing a pattern which seems to him aesthetically satisfying, just as his predecessors forty centuries back chose a pattern aesthetically satisfying to them. I am here using the word "aesthetically" in a suitably wide sense; and one should not overlook the fact that its use must then be wide enough to encompass the two *different* senses employed by modern and ancient man respectively. In the present context, I suspect that modern man employs it in some sense related to Occam's razor: he wants a relatively simple construction, which despite its simplicity nevertheless portrays the main features of the megalithic site. In putting forward some alternative hypothesis, he ought therefore to take account of some measure of the simplicity of the alternative. The issue appears essentially epistemological and one which, despite its importance and wide occurrence, has not been treated much in previous statistical theory. I was puzzled by this issue when treating a somewhat similar though much easier problem (the molecular weight of insulin) at a meeting of the Society four years ago (*J.R. Statist. Soc.* B, **12** (1950) 192–240); but I have not since then been able to contribute towards solving it. I hope however that Professor Thom's paper will encourage others to study the question.

Finally I should like to call attention to the magnificent way in which Professor Thom has condensed some twenty years' work into a paper which is as short as it is lucid. This is an object lesson to all of us, and I have much pleasure in congratulating Professor Thom and seconding the vote of thanks.

Mr. S. R. Broadbent: I should like to say a few words on the statistical problems raised by the common unit of length for the circle diameters. First the question of estimation. Professor Thom has calculated $\Sigma D/\Sigma m$; there are actually better estimators, which give in this case almost the same numerical result. The least squares estimator (which is the maximum likelihood estimator in the case of normal error) is $\Sigma mD/\Sigma m^2$; the estimator which minimizes the criterion s^2/δ^2 is $\Sigma D^2/\Sigma mD$. Both these give on these data the unit 5·434 ft. The reason all three estimations agree so closely is that the main work of estimation is completed when we guess the m corresponding to each D. Unfortunately the three estimators are equally sensitive to the choice of m.

There are logical difficulties raised in the testing of the reality of this common unit. Perhaps I should describe the test briefly. On the hypothesis that the unit of length tested is not in fact obeyed by the data (for example if the distribution of circle diameters were unimodal) it is possible to show that $z = | D - 2m\delta |$ is distributed approximately rectangularly between 0 and δ. Then $s^2/\delta^2 = \Sigma z^2/n\delta^2$ is distributed approximately normally with mean $\frac{1}{3}$ and variance $4/45\, n$. In answer to Professor Kendall's question, it is the alternative or rectangular hypothesis which is tested.

I am not convinced that the separation into English and Scottish circles provides independent sets of data. Once it is decided that the unit is about $5\frac{1}{2}$ ft., the estimates from Tables 2 and 3 are certain to agree closely, and s^2/δ^2 for each is sure to be low. I would therefore regard Table 4, which purports to give the probabilities of the observed groupings, as a *prima facie* significance test only.

The test described in Appendix I, on other measurements of diameters, is, however, a strong argument for the common unit of length, as long as these data did not influence the initial choice of $5\frac{1}{2}$ ft. My remarks in fact concern Professor Thom's method, but not his conclusion.

My reasons for being sceptical about Table 4 are as follows. The 52 diameters could be used to test various proposed units of length, from tens of feet to a single foot. For each unit the criterion s^2/δ^2 could be calculated. Its value, graphed against 2δ, the unit tested, would be a continuous oscillating curve. For any 2δ given from an outside source the curve has the expected value $\frac{1}{3}$ and a known standard deviation on the rectangular hypothesis, and so the significance of its value can be tested. If it is small we accept the hypothesis of grouping. But no matter what the real distribution of diameters, the curve will fall below any significance level in some parts of its range, and about these units of length the data will appear to be grouped.

Some sampling experiments are at present being done at the National Physical Laboratory to find where the minima of such curves actually occur. For example, we have taken two samples of 50 observations each, from a uniform distribution between 0 and 1. Any grouping that appears is purely fortuitous. But when testing units of length down to $0 \cdot 24$, both samples admitted units significant at the 5 per cent. level, testing units down to $0 \cdot 16$, both admitted units significant at the 1 per cent. level. This means that if we are free to choose our unit of length after inspecting the data, conventional significance levels have little use. When the National Physical Laboratory calculations are complete we shall have some grounds for such *a priori* testing.

A similar point arises in an elementary way with Professor Thom's (properly guarded) remark that for 56 out of 86 diameters, m is even. Sampling from a binomial population with $p = \frac{1}{2}$ we should obtain this number of successes or more once in 400 trials. Do we therefore reject $p = \frac{1}{2}$ at this level? The point is that this is one of many hypotheses which we might have chosen to test. It is rather like being asked to give prices for the 3.30 race when you know which horse won the race. In such circumstances I think the statistician should hedge about his statements on significance levels with a great deal of caution.

Professor GORDON CHILDE: While archaeologists greatly appreciate the immense amount of work, learning and technical skill which Professor Thom has put into his paper, many of them, I must confess, when faced with mathematical symbols which they do not understand have aroused in them severe emotions which are somewhat unbalanced; some of us think that sigmas and symbols of that kind are "words of power" and any result from their use must have a very high degree of truth. Others are liable to feel that they are deadly weapons which are being used against them to put over a "fast one", and it is only fair to state that this is the attitude which archaeologists are likely to display at the start.

I think also an archaeologist should emphasize the condition of the monuments. Most of the monuments are in a rather dilapidated condition, many of the stones are fallen and some are known to have been tampered with. In the Castle Rigg circle, for instance, there is a rectangular structure which the Prehistoric Society three years ago decided was the foundation of a mediaeval cow-house and not an original part of the circle. At the time of its construction the circle was liable to considerable disturbance. Most of the stones are a very irregular shape, very few of them are squared or even quarried. In most cases I think the builders picked up suitable stones lying about. At Stonehenge the sarsen are dressed, but the stones of most circles are very irregular, and, even when they are standing, it is not at all easy to decide what the centre of the stone is or what it was intended to be.

It is important to remember when considering the question of azimuths and declinations that the landscapes in which the monuments are situated were probably very different at the time they were erected. A great deal of forest has been destroyed since Roman times, some before.

Bearing these limitations in mind let me turn to the results which Professor Thom has laid before us. We archaeologists can admit perfectly freely that prehistoric men were capable of laying out circles with complete accuracy. Stonehenge I and Stonehenge II are certainly strung out from a centre. In some circles one can work out the methods by which the circumference has been sub-divided. Traces of the peg holes for the subsidiary constructions have been observed both in Holland and in this country. These are the sort of operations that have been documented by archaeologists employing the latest excavation techniques.

Whether the diameters so carefully measured by Professor Thom provide statistically valid data for standard measures is beyond my competence to discuss. But if they do what are the inferences which we must draw, if any? I would suggest that we are not dealing here with any conventional standardized unit, it bears no relation to the well-known ancient conventional units which have been worked out by Petrie and others. Professor Thom has not mentioned the Aubrey circle, the figure for which works at $65 \cdot 22$ in. I do not know that we need infer from such figures the existence of a conventional unit. It is only an average obtained from a number of measurements which have diverged more or less from it, and I do suggest that it is the average of the *actual* fathom and not necessarily a conventional fathom. In Egypt they had a conventional fathom which was four times the cubit. In this country we have obviously not that fathom, but what we have would work out pretty well judging by the known stature of the people of the period.

Turning to the astronomical values, here again I speak with extreme hesitancy and, in fact, would prefer to say nothing at all except to point out that the dates to which Professor Thom referred are different from those which archaeologists have reached by other methods. It is true that we have no real evidence of the dates of the great majority of the stone circles, but we have good evidence for our dates of Stonehenge I and Stonehenge II. Stonehenge I, which contained no stone whatever, is dated for us by radio-carbon methods at 1850 B.C., which fits in very nicely with the archeological evidence from the encircling ditch. Stonehenge II cannot be dated by the radio-carbon method but from archaeological data, particularly from the type of axe recently discovered curved upon the uprights, the date is somewhere between 1600 B.C. and 1500 B.C. The present tendency of archaeological thought in this country would be very much against putting any stone circle earlier than Stonehenge I.

We think that the stone circles in Scotland are likely to be later than those in England and we would not put any English circle earlier than 1850 B.C. Archaeological data on the whole fit in with the radio-carbon dates.

I should perhaps question the hypothesis that these circles all fulfil the same function. It would probably be a mistake to assume that they did. The Clava circles are sepulchral; the two inner ones are the bounding kerb of a cairn and the kerb of the chamber under the cairn. On the other hand, we have now in Southern England an extensive group of circular monuments, some of them fairly regular, of the same type as Stonehenge I. Penannular ditches enclose rings of holes which contained (although not dug for that purpose) cremated human bones and had apparently never held any other timber posts or stones, so that they could not be used for sighting. One feels that these earthen monuments of Lowland Britain cannot be separated functionally from the stone monuments erected in more stony and rocky country.

We archaeologists are always grateful to collaborators from other disciplines and I am extremely grateful to the Society for giving me the privilege of hearing this paper.

Dr. IRWIN: I should like to ask two questions, one of a statistical and the other of a non-statistical nature. I think I understood Fig. 7 very well, but I was just a little puzzled about the rising and setting sections of the diagram. I can understand that in the case of the sun or any star the declination corresponding to its direction varies according as it points to the rising or to the setting sun or star; but for some of these observations which did not come opposite any star at all, how did Professor Thom decide whether to plot them as rising or as setting?

The other question I ask with caution. Supposing this astronomical theory is in fact true, how did these people round about 1800 B.C. get their astronomical knowledge? Did the Chaldeans know astronomy at that time? Was there any contact with the east? Could their knowledge have spread across the seas? It would be extremely interesting if somebody could tell us whether anything is known of such movements.

Professor THOM subsequently replied in writing as follows:

Professor Kendall raises an interesting point when he suggests that the variance should increase with the diameter. Does the fact that better agreement is found with the Quantum Hypothesis for circles over 80 ft. diameter than for circles under 80 ft. indicate that in fact the variance does *not* increase? Should this be substantiated by further evidence it seems to me to show that the erectors actually used measuring rods and did not depend on "laying individuals end to end" or simple pacing. I was interested in Professor Kendall's experiment showing the "catches" obtained by taking random azimuths and converting to declinations. I believe that otherwise his calculation followed mine and as he used the same pockets the results should be comparable. If, as my results suggest, outliers and alignments were erected to indicate the rising points of certain first magnitude stars, then one should try to find a reason. There is perhaps an *a priori* reason in that there are a great number of records from the Mediterranean region of the year being dated by the heliacal and antiheliacal rising or setting of certain stars and it would seem reasonable to mark the rising points in some way. Accordingly I made a study of the conditions which would make a star suitable for dating by this means. But I did not find a very good correlation with the stars most frequently indicated by the stones. We are thus thrown back on the explanation that the stars were used as timekeepers throughout the night. This seems a more reasonable explanation because in northern latitudes the day of the year is much more accurately indicated by the setting point of the sun on the horizon than by heliacal risings. Need we be surprised that Sirius did not have many indicators? Its rising point is sufficiently well indicated by Orion's belt which rose some time before Sirius itself. I sympathize with Professor Kendall in still having doubts. All I will say is that I think there is sufficient evidence in favour of an astronomical hypothesis to warrant further investigation. I think my use of the words "within reason" excludes Professor Kendall's suggestion that "if one took enough stars one would get a perfect catch".

I agree with Mr. Hammersley's remark that the data are definitely limited. Nevertheless, I hope to be able to survey a good many more sites; there are, I understand, many in Ireland. I feel that perhaps one should try to evolve a method whereby one could examine and compare with the site-plans every reasonable geometrical construction which could have been carried out on the ground with stakes and rope. In this connection I should perhaps mention that since the paper was written I found a circle on the moor above Boot which is practically identical in size and shape with the prototype at Castle Rigg. Here, again, many of the construction points are marked. Unfortunately this circle is less well preserved than Castle Rigg.

At the time I analysed the Quantum Hypothesis for the diameters I realized to some extent the dangers to which Mr. Broadbent draws attention, but I could see no alternative to dividing the data into two by some arbitrary method. Since I had already noticed the $5\frac{1}{2}$-ft. unit, any method of division at this stage is open to criticism. There is, however, no objection on this ground to the data given in the Appendix since nearly all these were collected after I had analysed my own data. I gather from Mr. Broadbent's remarks that a good deal of work is proceeding on the methods by which the Quantum Hypothesis can be more rigidly examined in a given case. In the meantime I am surveying more circles and so hope to produce an independent check.

I am glad to see that Professor Childe mentions the possibility that the circles were not all built to fulfil the same function; to one who has visited as many as I have, it becomes apparent that the circles themselves differ in more ways than can be described simply in terms of diameters, stone sizes, etc. Thus I think I am right in saying that what I would call the smaller or poorer circles seldom have outliers. Professor Childe's point answers to some extent the difficulty raised earlier by Professor Kendall, i.e. that *all* the pointers are not explained by the rising points of first magnitude stars. In fact some of the stones which I have assumed to be outliers may be the remnants of some entirely different construction, or three stones in line may have been originally part of a circle. I agree with Professor Childe that many of the monuments are so badly knocked about that it is impossible to determine an accurate diameter. It was for this reason that I resorted to a statistical method of calculating the diameters. I wished to obtain an objective value which would be unaffected by my preconceived ideas. The fact that I state the diameters to 1/10 ft. does not mean that I believe that I know them individually to this accuracy; I do not. I mostly used a linen tape which was liable to stretch or shrink. Other observers may find slightly different diameters, but I cannot see how my results can be biased in favour of a quantum hypothesis. As for the possibility of changes in the contours of the countryside, I do not think this likely to bias the astronomical arguments one way or the other. Geologists do not claim any serious movements, and the elevation of a horizon by trees will only affect the results seriously if the horizon is near. Many of these sites are on elevated plateaus where the horizon is distant. It will be observed that I have assumed several lines to be one-directional; in most cases the reason was that in the other direction the horizon was too close. A more serious factor in my opinion is the possible local distortion of the alignments by earth movements. I think that at present it is not possible to decide on an exact date by radio-carbon methods. The date of 1850 B.C. obtained from charcoal samples from Stonehenge has an uncertainty of $\pm$ 275 years, according to W. F. Libby in the January, 1954, number of *Endeavour*.

The answer to Dr. Irwin's question about the rising and setting is simply that for azimuths from 0–180 the alignment indicates a rising star and for those from 180–360, a setting. I am quite unable to deal with the contacts which these people may have had with the East. I would point out that for a people living without artificial illumination the face of the sky is a much more important factor than for modern man, especially during the long winter nights of these latitudes. To a people who were evidently developing the rudiments of geometry, the movements of the stars must have presented a continual challenge.

As a result of the ballot taken during the meeting the candidates named below were elected Fellows of the Society:

Eric Broughton.
Shriniwas Bhamburkar.
Stanley Frank Burgin.
Harold Park Cooper.
Desmond Alfred Dean.
Jacques-Paul Desabie.
Bryan Douglas Haig.
Henry William Haskey.
Ivor Hodges.
Douglas Hugh Johnston.
Faiz Shukri al Khuri.
Geoffrey Maynard Pacey.
Indrakenty Rao.
Norman James Royce.
Kenneth Sweeting.
Graham Turner.

Corporate Representatives

Harold Nabb, *representing* the South Western Gas Board, Bath.
Paul Newman, *representing* Aspro Ltd.