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THE DISTRIBUTION OF ENERGY OVER THE SUN'S DISK

(With One Plate)

BY
C. G. ABBOT

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THE DISTRIBUTION OF ENERGY OVER THE SUN'S DISK

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Abstract.—Moll, Burger, and van der Bilt have attacked the accuracy and usefulness of Smithsonian solar disk drift curves. They describe them as omitting the region (from 95 to 100 per cent out on the solar radius) most important for solar theory; as affected by large instrumental error; and as subject to fatal systematic error due to tardy response of the bolometric apparatus.

Abbot states that since the Smithsonian observations were undertaken merely to test suspected variability of distribution from epoch to epoch, only differential accuracy was required. No attempt was made to carry the work over into the difficult region between 95 and 100 per cent of the solar radius, because it was unnecessary for this purpose. He fears that to reach demonstrable accuracy to 1 per cent in this region of the curves will meet insuperable difficulties.

Abbot points out that their statement regarding accidental error rests on one curve made at Washington City, sea-level, station, prior to 1908, though the work went on under highly satisfactory conditions for eight years afterwards at Mount Wilson. Photographs proving its general excellence are available.

He points out that the amount of systematic error claimed by the authors depends on the actual degree of quickness of response of the Smithsonian apparatus; that photographic evidence shows that this was 1.95 seconds; that such error tends to raise the following limb of the curve above the true values, though lowering the advancing limb beneath them, and thus tends to be eliminated in their mean; that where, as in Smithsonian observations, the measurements show negligible differences between the two limbs, the error is presumably negligible; that in different years, receiving instruments of unequal quickness of response were used without corresponding differences of result in the sense indicated by the Dutch authors' criticisms. He admits that very near the limbs the curves would not have furnished trustworthy indications. A determination of the error near the limbs is given. This indicates that at 95 and 92 per cent the

Smithsonian results would differ from the true curve on account of lag by 0.28 per cent, and 0.26 per cent, respectively.

On returning from a six months' expedition, I find the paper of Moll, Burger, and van der Bilt.[1] They take exception to the Smithsonian experiments on the distribution of energy over the sun's disk from three points of view. First, that our measurements were rarely extended beyond 95 per cent on the sun's radius. Second, that our findings were expressed with more places of figures than the experiments justified. This criticism they support by reproducing one of our early curves. Third, that owing to the tardiness of response of our bolometric apparatus, our curves differ very sensibly from true representations of the distribution sought.

That the reader may clearly understand what is in question, I recall for him that we formed a large image of the sun by a reflecting telescope. Stopping the clockwork, this image drifted its own diameter in about two minutes. The arrangements were such that this drifting solar image marched centrally and horizontally across a short vertical slit. From the ray which passed through the slit, a certain wavelength was selected by means of a spectroscope and brought to focus upon the strip of the bolometer of far more than hairlike thinness. The curve of bolometer temperatures corresponding to the intensities of the selected wave-length in the solar image was automatically recorded in the shape of an inverted U.

We observed on both solar limbs and took their mean values. We were accustomed to cut off the recording light from the galvanometer mirror at the instants when the sun's image visibly reached the slit and departed from it. At intervals we also inserted shutters which produced zeros of radiation on the records and permitted accurate examination of the behavior of the bolometer. The curves were inevitably a little wider than corresponded to the astronomical width of the sun in terms of the rate of motion of the plate. This is because of the time required for the galvanometer to descend to zero after the following limb of the sun had crossed the slit.

Our habit of measuring was to compute from astronomical and plate-speed data the widths corresponding to definite proportions of the solar radius; to adjust these places symmetrically to the central axis of the U-shaped curve; and to measure heights on both advancing and following limbs at these places. All results were finally compared to the mean form of distribution curve for the year 1913

[1] Bul. Astron. Inst. Netherlands, Vol. III, No. 91, Dec. 18, 1925.

as a standard. The curves, as I shall show, were symmetrical to within negligible limits up to 95 per cent of the solar radius.

We made our experiments at Washington prior to 1908, and at Mount Wilson from 1913 to 1920, every summer. Several different bolometers, several different galvanometers, and several different optical systems were used by us. Evidences of secular variations of distribution were found. Extensive discussion of the methods, sources of error and results are given in Volumes II, III, and IV of the Annals of the Astrophysical Observatory of the Smithsonian Institution.

The Dutch authors describe briefly their own experiments in which they used a thermopile whose time of lag in attaining thermal equilibrium is not stated. They, indeed, refer to a description in another paper in which several instruments of considerable quickness are described, but as one may infer from their figure 2, the sluggishness of the actual instrument used appears to have been very great. They employed a 3-centimeter solar image formed by a lens, a slit whose width in proportion to the image equalled ours, and a device adjusted to produce a uniform drift of the image across the slit in 14 minutes. They give no data as to the degree of uniformity of their galvanometer scale or the width of the curves compared with the computed width. They made observations during part of one month at the Gornergrat. Of the results, they say:

> We were able to get trustworthy values of the distribution of energy along about 99 per cent of the sun's radius, against Abbot's 95 per cent.
>
> Our measurements do not claim to give results of the highest precision obtainable. We think that our values of the energy are trustworthy to about one-hundredth part of their value at the sun's center.
>
> Our values for the common 95 per cent exceed those of Abbot. The differences attain a maximum amount of about 2½ per cent at a distance of about 8 per cent from the sun's limb. . . . . It is easy to explain this discrepancy in a satisfactory way as a consequence of the insufficient quickness of Abbot's instruments compared with the speed of the solar image. It is not so easy to explain the fact that the discrepancies are less at a distance of 5 per cent from the sun's limb. Probably Abbot has been under the influence of a preconceived opinion, viz.: That the energy at the sun's limb must, from a finite value, abruptly fall to zero.
>
> In looking at Abbot's curves, of which figure 1 is a specimen, a peculiarity attracts the attention: they show a certain skewness or absence of symmetry.
>
> A first glance at his curve shows that it is not smooth, a fact which we ascribe to disturbances, probably of the galvanometer. One wonders how it was possible to derive, from curves like this, reliable results, and to give the data in four figures.

Again they say, in regard to their figure 2, given to compare 14-minute drifts with 2-minute drifts:

Evidently the latter has been seriously affected by the slowness of our instruments. Now, since these were doubtlessly much quicker in response than Abbot's (which is evident from the absence of any perceptible skewness in our curves), his curves, which were all recorded with the sun's image at its normal speed, must have undergone a considerable deformation. No wonder that our final measurements led to data quite different from those given by Abbot.

I regard the authors' insinuations regarding our work as unfairly derogatory. Especially do I deprecate their implication that our results at 95 per cent were anything but direct computations from the measurements. That they are unbiased results from direct measurements the authors could have ascertained from Volumes III and IV of our Annals, but it will also appear plainly in certain illustrative examples below.

Again, one would hardly have expected that the authors would base a severe condemnation of our entire research on a single curve made prior to 1908 amid the murky atmosphere and rumbling vehicles of the city of Washington.[2] For we afterwards carried on the work for eight successive summers, 1913 to 1920, and made many thousand drift curves under fine conditions at Mount Wilson. Except when the sun itself presented irregularities of distribution, our curves are in general of great smoothness and symmetry. This prevails notwithstanding that our curves are on a higher scale of ordinates than those which the Dutch authors show. I am sending to the authors photographic prints which prove the prevailing smoothness and symmetry of our curves, and reproducing the same as the accompanying plate 1.

As to whether the Dutch authors have obtained or will obtain a higher degree of accuracy than we did in determining the distribution of energy over the sun's disk, we must await the more detailed experiments and descriptions which they promise before we can form a conclusion. The matter, indeed, will be exceedingly difficult to demonstrate. Certainly a degree of accuracy which they " think " extends to about 1 per cent cannot decide as between the results of two researches whose maximum discrepancy they inform us is $2\frac{1}{2}$ per cent.

This is the more obvious when we reflect that our results, to whose error they would attribute the whole discrepancy, were made during more than 10 different years, at two different stations, with four different bolometric outfits, and with three different optical systems.

[2] See plate XXVIII, Annals Smithsonian Astrophys. Obs., Vol. II, 1908.

They were made on solar images of 40 and 20 centimeters diameter, as compared with three centimeters used by the authors. They indicated differences from day to day and from year to year in the solar energy distribution. The authors, indeed, do not even state with which year of our observations they have made their comparison, and seem to suppose that the distribution of radiation over the sun's disk is invariable.

The Smithsonian observers approached this research from a totally different point of view than the Dutch authors. With us it was incidental to our general study of the variation of the sun. We supposed that the sun's variations of short-interval and of long-interval might be associated with changes of the transparency or of temperature of his outer envelopes. Such alterations might, as we thought, reveal themselves by modifications of the distribution of intensity of radiation along the east and west diameter. We even hoped that the correlation of change of solar-constant with change of distribution would prove so close that we could substitute for the (at that time) tedious solar-constant observations the easy drift observations. Accordingly, we sandwiched in between solar-constant bolographs, on nearly every day of observation at Washington and at Mount Wilson, sets of drift curves at several wave-lengths. At Washington we made three drifts for each wave-length on each day. At Mount Wilson the conditions were so much better that we contented ourselves, except in the year 1920, with two.

The reasons which induced us to limit our measurements to 95 per cent out on the sun's radius were that we did not need to go farther out to show secular changes in distribution, and that we conceived that the boiling of the atmosphere, the intensity of sky light, and the extremely rapid change of intensity at the sun's limbs introduced factors of such uncertainty that the measurements farther out would be of little value for indicating such small changes from day to day and year to year as we were searching for.

In short, we did not undertake to test theories of the sun's constitution by distribution experiments, or try to obtain results suitable for that purpose, though we were, of course, glad if the measurements later proved adaptable to that inquiry. This is the problem which the Dutch authors set for themselves. For its solution they desire accurate values out to 99 per cent of the sun's radius. I am tempted to refer them to the words of Ahab:[3] "And the King of Israel answered and said, Tell him, Let not him that girdeth on his harness

[3] I Kings 20, 11.

boast himself as he that putteth it off." There are yet great difficulties before them in arriving at 1 per cent demonstrable accuracy out to 99 per cent of the sun's radius. Full details, quantitative investigation of errors, the effect of altering the experimental means and an investigation of solar variability will be demanded to support such claims.

In the remainder of my remarks, I wish to defend our results from the theoretical objection made by the authors that, owing to the quick march of the solar drift and the slow response of the bolometric outfit, our curves are sensibly deformed as far back as 95 per cent and even 92 per cent of the sun's radius. They support this objection by printing [as their fig. 2] two curves taken with their own apparatus on drifts respectively of 2-minute and 14-minute speeds. They do not show these curves on both advancing and following limbs. Yet they seem to leave their readers to infer that the *quick* drift curve is the *lower* on *both* sides of the sun. This is, of course, not so. If the receiving instrument lags behind in attaining thermal equilibrium, it will be *below* the true curve on the advancing limb, and *above* the true curve on the *following* limb. Hence, taking the mean of measurements on the two limbs tends to eliminate the error.

The elimination of error by this device cannot be perfect and it is highly desirable to use apparatus acting so quickly that the difference between the two limbs is negligible. I give in illustration a number of sets of measurements of our curves on the two limbs for different wave-lengths, different years, and different bolometers. These values are exactly as obtained and measured many years ago in our Mount Wilson work. The results are neither better nor worse than hundreds of others which I might have quoted.

It is possible to determine approximately the magnitude of the error which the Dutch authors fasten upon our results. For this purpose, consider first the effect of inserting the shutter before the slit as photographically recorded on all of our plates. I find by measurements of several such records that the trace starts to fall very steeply without preliminary slow gathering of motion, and runs to zero in 1.95 seconds according to the following schedule:

Fraction of whole time of falling	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1.0
Fraction of whole fall	0	.05	.12	.20	.31	.44	.61	.74	.85	.92	1.0

The upward march when the shutter is removed is substantially identical.

ORIGINAL MEASUREMENTS ON SMITHSONIAN DRIFT CURVES

VARIOUS YEARS, BOLOMETERS, AND WAVE-LENGTHS

Measurements in Centimeters

Solar Limb	Date	Bolometer	Wave Length	Per cent of radius from center										
				0	20	40	55	65	75	82.5	87.5	92	95	97
Preceding........	1914, Aug. 11	Fine strip in air	0.4265	7.03	6.98	6.70	6.11	5.71	5.17	4.61	4.24	3.60	3.12	2.71
Following........	" "	"	"	—	6.90	6.53	6.10	5.73	5.27	4.65	4.19	3.60	3.13	2.63
F minus P.......	" "	"	"	—	—0.08	—0.17	—0.01	+0.02	+0.10	+0.04	—0.05	0.00	+0.01	—0.08
Per cent (F—P).	" "	"	"	—	—1.1	—2.5	—0.2	+0.3	+1.9	+0.9	—1.2	0.0	+0.3	—3.0
Preceding........	" "	"	0.5955	14.48	14.24	13.83	13.21	12.59	11.76	10.92	10.20	9.30	8.60	7.80
Following........	" "	"	"	—	14.28	13.78	13.22	12.66	11.83	11.00	10.29	9.30	8.53	7.73
F minus P.......	" "	"	"	—	+0.04	—0.05	+0.01	+0.07	+0.07	+0.08	+0.09	0.00	—0.07	—0.07
Per cent (F—P).	" "	"	"	—	+0.3	—0.4	+0.1	+0.6	+0.6	+0.7	+0.9	0.0	—0.8	—0.9
Preceding........	1916, July 28	Coarse strip in vacuo	0.4265	13.78	13.52	12.90	12.07	11.32	10.19	9.02	8.00	6.90	6.00	—
Following........	" "	"	"	—	13.52	12.84	11.97	11.12	10.02	8.95	8.00	6.90	6.00	—
F minus P.......	" "	"	"	—	0.00	—0.06	—0.10	—0.20	—0.17	—0.07	0.00	0.00	0.00	—
Per cent (F—P).	" "	"	"	—	0.0	—0.5	—0.9	—1.7	—1.7	—0.8	0.0	0.0	0.0	—
Preceding........	" "	"	0.5955	16.30	16.02	15.49	14.82	14.12	13.22	12.28	11.49	10.39	9.38	—
Following........	" "	"	"	—	16.04	15.50	14.80	14.12	13.21	12.23	11.42	10.32	9.32	—
F minus P.......	" "	"	"	—	+0.02	+0.01	—0.02	0.00	—0.01	—0.05	—0.07	—0.07	—0.06	—
Per cent (F—P).	" "	"	"	—	+0.1	+0.1	—0.2	0.0	—0.1	—0.4	—0.6	—0.7	—0.6	—
Preceding........	1920, Aug. 30	Fine strip in vacuo	0.4265	16.07	15.83	15.08	14.00	13.00	11.73	10.49	9.40	8.20	7.15	—
Following........	" "	"	"	—	15.83	14.94	13.90	13.00	11.67	10.40	9.38	8.20	7.10	—
F minus P.......	" "	"	"	—	0.00	—0.14	—0.10	0.00	—0.06	—0.09	—0.02	0.00	—0.05	—
Per cent (F—P).	" "	"	"	—	0.0	—0.9	—0.7	0.0	—0.5	—0.9	—0.2	0.0	—0.7	—
Preceding........	" "	"	0.5955	11.25	11.15	10.85	10.30	9.90	9.21	8.60	7.90	7.30	6.70	—
Following........	" "	"	"	—	11.11	10.78	10.36	9.92	9.20	8.60	7.97	7.33	6.72	—
F minus P.......	" "	"	"	—	—0.04	—0.07	+0.06	+0.02	—0.01	0.00	+0.07	+0.03	+0.02	—
Per cent (F—P).	" "	"	"	—	—0.4	—0.6	+0.6	+0.2	—0.1	0.0	+0.9	+0.3	+0.3	—

As the time of complete fall or rise is about 1/65 of the time required for a complete drift on the day I investigated, namely, August 21, 1920, I gave the Dutch authors a slight advantage by calling the time of full bolographic response to a new stimulus 1/60 of the time for a complete solar drift.

Our next object is to estimate the effect of this degree of sluggishness upon the true drift curve. Not knowing positively the true curve near the limb of the sun, I have used the Dutch authors' preliminary result as our best approximation for it. As a sample, I have chosen their curve for wave-length 0.5 microns. This I plotted on our great sheets of millimeter cross section paper, on a scale of 12,000 millimeters of abscissae to the solar diameter, and 500 millimeters corresponding to the ordinate of the Dutch authors' curve at the sun's center, taken as 1,000 in what follows.

Next, recalling that Mr. Fowle, who measured all of our drift curves, was accustomed to place the curve symmetrically, and to measure to the computed abscissae corresponding to astronomical and plate-speed data, we must consider where his measurements really lay with respect to the true curve. From a number of our drift curves of August 21, 1920, I find the bolographic width from zero to zero of ordinates to have been 131.0 millimeters,[4] but the visually observed width, as indicated by the instantaneous cutting off of the record-light at ingress and emergence from the slit, was 128.4 millimeters.[4] The excess, 2.6 millimeters, was, we may suppose, symmetrically divided in Mr. Fowle's placement of the curve for measuring. Hence, he measured $\frac{2.6}{2 \times 131}$ of the solar diameter away from the orientation of the true curve. Therefore, in terms of the orientation of the true curve, he measured for the place 95 per cent out on the solar radius, for example, at 93 on the preceding and 97 on the following limb, or at places very close thereto, depending on accidental differences of individual curves. These very slight accidental second order shiftings might, of course, lead to changes of the order of a per cent or so between the advancing and following limbs in his measurements of individual curves, but, since the lowering of the one must produce the lifting of the other, these slight changes would be closely eliminated in his mean values. In what follows we shall assume that Mr. Fowle exactly bisected the bolographic curve by his zero setting of the plate for measurement.

[4] Comparable because measured on same plotting paper. Fowle's computed value, 129.02, on slightly different scale.

I then assume that at 95 per cent out on the radius, as indicated by the large scale plot above described, the preceding limb was measured at −2 per cent or −12 cm., and the following limb at +12 cm. from the 95 per cent place on the true curve. At the first named place it will be obvious that the bolographic trace was not only as high as the true curve had been at 20 cm. nearer the limb of the sun, but higher. For 20 cm. corresponds to the interval of time required for full response, and during all that interval the stimulus had equalled or exceeded the stimulus of the true curve at the place just mentioned. Similarly, for Mr. Fowle's place of measurement on the following limb, the ordinate must be inferior to the ordinate upon the true curve at 20 cm. nearer the sun's center. Thus we have a first approximation.

Our next inquiry is to find the effect of the excess of radiation persisting over the aforesaid 20 cm. interval for the preceding limb, and the defect of radiation persisting over the equal interval for the following limb. For this purpose I read the ordinates of the true curve at places 2, 4, 6, etc., to 20 centimeters towards the limb, counting from Mr. Fowle's place upon the preceding limb, and correspondingly towards the center upon the following limb. The differences of readings of ordinates corresponding to these 2-centimeter intervals were then obtained. We are now ready to proceed. For instance, the stimulus at Mr. Fowle's observed place on the preceding limb had exceeded that at 20 cm. back on the true curve by an amount corresponding to the first of the aforesaid differences active during 9/10 of a response interval, 8/10 for the second, etc.

Proceeding in this way, we find the following numerical values at place 95, preceding limb:

Places, cm	−20	−18	−16	−14	−12	−10	−8	−6	−4	−2	0
Ordinates	493	505	516	526	536	545	554	563	571	580	588
Differences	12	11	10	10	9	9	9	8	9	8	—
Fractional Response	.92	.85	.74	.61	.44	.31	.20	.12	.05	00	—
Products	11.04	9.35	7.40	6.10	3.96	2.79	1.80	1.08	.40	00	—

The sum of these products is 43.9. Adding it to the value at −20 cm., we obtain, as the second approximation to Mr. Fowle's reading, 536.9. It would have been possible by dividing the interval into 100 parts instead of 10 to get a very slightly higher result, but the difference surely for our inquiry is negligible. Proceeding similarly for the following limb, the correction becomes −38.9 and the second approximation there is 532.1. The difference between Mr. Fowle's readings on the two limbs in the sense preceding minus following would therefore be expected to be 4.8, or 0.9 per cent. Their mean is 534.5, which, as the Dutch authors claim it should be, is lower than 536.0, the true curve value at place 95, but by only 0.28 per cent. I have performed a similar analysis at place 92. It indicates a correction in the same sense of only 0.26 per cent there. Farther towards the center the correction sensibly vanishes.

Though we have no direct statement of the time required for complete response in the case of the Dutch authors, their figure 2 enables us to know that their instrument was far more sluggish than ours. For, as noted above, the sluggishness of a receiving instrument must cause the results on the sun's *following limb* to show *less contrast* than the *true curve* of solar drift. Hence, if we admit that P in their figure is the true curve, the following limb, had they published it, must have shown a continuation of the curve Q, higher than the continuation of the curve P. In other words, the difference between the two limbs indicated by a full curve Q taken with their apparatus would have been greater than that between P and Q in their figure 2. But this difference is actually no less than 8 per cent at 95 per cent out on the radius in their figure 2. In our work no systematic difference between the two limbs as great as this appears.

I suspect that the Dutch authors, being accustomed to the thermopile, have underestimated the quickness with which our bolometers respond. We have abundant evidence that our bolometers usually attained thermal equilibrium indefinitely sooner than our galvanometer could make its first swing, which usually occupied only 1.7 to 1.9 seconds.[5] But we have used bolometers of three different degrees of quickness of response. Prior to 1916, we used bolometers in air, which are quickest. In 1916, we used comparatively very coarse bolometer threads in vacuo, which made a far more sluggish instrument, almost indeed as sluggish as the most delicate of thermopiles. Since 1917, we have used finer threads again, but in vacuo, and therefore intermediate in quickness between those of years prior to 1916 and that of 1916 itself.

If, then, the Dutch authors were right in their criticism, our drift curves of 1916 ought to show lower " shoulders," or in other words greater contrast, than those of later years; and these, in turn, greater contrast than those of years prior to 1916. It needed only to have examined tables 68 and 74 of our Annals[6] to be convinced that no

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[5] See also our Annals, Vol. II, p. 218.

[6] I draw attention here to a misprint throughout tables 73 and 74. For 1.0035 in the place headings read 0.92.

certain evidence of the kind appears. The accompanying figure 1 shows this. In order to save myself a future note, I admit that the *two* curves of *one* day of 1916 given in the table in this present paper do show lower values. But I hope readers will be fair enough to form their judgments from the *mean* results of many days and *many* wave-lengths given in the Annals.

As I have said, we did not take up drift-curve work for the sake of getting the most accurate distribution tables for the use of solar theorists. We were concerned only with relative measurements to compare distributions from day to day and from year to year. Hence, we

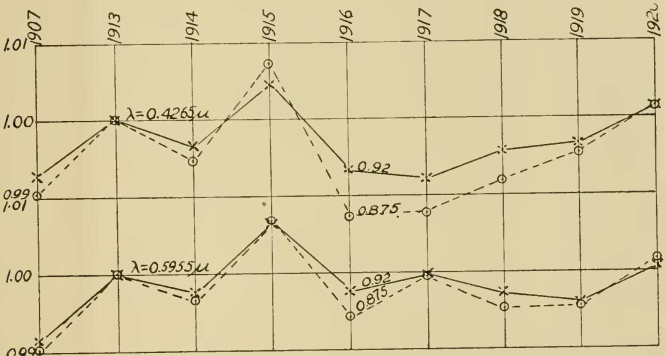

FIG. 1—Comparison of drift results of different years for two different wave-lengths and two different places on the solar radius. Taken with four different bolometers and three different optical systems.

did not try to attack the difficult region near the sun's limbs, nor did we seek to produce absolute distribution curves of the highest attainable freedom from systematic errors. We were concerned with relative values. Nevertheless, I could not let pass without reply so hasty and unjust an attack on what, after all, was work of a pretty high order of accuracy.

From this investigation I see no ground for admitting that the defect of ordinates attributed by the Dutch authors to our results as a consequence of sluggishness of response is of much consequence. The main part of the difference between their results and ours must be due to other causes. Such may be:

1. Too hasty a conclusion. Further experiments proposed by the Dutch authors may not indicate such a discrepancy.

2. Too small a solar image. Possibly with a different optical outfit the results would differ. Perhaps, too, there is error on account of stray light from other spectral regions.

3. Error in wave-length. The change in form of distribution curve with wave-length is quite rapid.

4. Error in determining places of measurement. The ordinates of distribution curves vary rapidly along the radius.

5. Difference due to alteration in the distribution in the sun itself. See the accompanying curve where a range of over 1 per cent is shown independently by two wave-lengths.

6. Non-uniformity of galvanometer scale. We were accustomed to test this frequently and reduced it to negligible dimensions.

I am by no means prepared either to admit that our work is wrong or, on the other hand, to deny catagorically that it has appreciable error. I await with much interest, therefore, the further investigations which the Dutch observers promise.