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SOLAR VARIATION AND FORECASTING

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By C. G. ABBOT

Inasmuch as there are probably still many persons both in Europe and America who doubt the reality of the variation of the sun and, much more, the possibility of applying it to the study of the weather, it has seemed desirable to sum up some of the principal objections which remain in the minds of such persons, to answer these objections, and after that to state some of the principal grounds of a belief in the existence of solar variation.

In order to treat the subject more definitely, I have ventured to assume a personality to represent those who entertain these doubts of the solar variability, and will, in what follows, speak of this personified doubter as "the critic."

The Smithsonian Institution is publishing a group of three papers, Nos. 5, 6, and 7 of Volume 77 of the Smithsonian Miscellaneous Collections, of which this, the first, deals with the objections to the variability of the sun and the principal indications which lead us to believe in it. There are a vast number of straws all of which point in this direction and, combined, make up a very stiff bundle of evidence, but in the limits of a paper of reasonable length, it is not possible to include all of these minor indications, however interesting they may be.

The second paper, by Mr. H. H. Clayton, gives the major results of his investigations of the past two years on the weather conditions of North America in their relations to the variation of the sun.

The third paper, by Mr. G. Hoxmark, to which I have ventured to prefix a short introduction pointing out what seemed to me very interesting features of his results, gives an account of the applications of solar variation to the forecasting of the temperature and rainfall of Buenos Aires, Argentina, for the years 1922, 1923, and 1924.

This group of three papers have such a close connection that I have brought them together in these short paragraphs of introduction.

Although very kind expressions in regard to the accuracy of our work on the solar constant of radiation come to us from all parts of the world, that does not imply universal belief in solar variability.

Professor Eddington, everywhere recognized as one of the foremost astronomers of the world, prepared the article "Astronomy" for the recent supplement of the Encyclopaedia Britannica. Although he has perhaps made more use of our results in his researches than anybody in the world except Clayton, he indicates in that article that appreciable solar variability probably does not exist. Dr. Exner, a leading meteorologist of central Europe, in a recent letter tells me that he and his colleagues are unconvinced. Dr. Linke, in a recent article, paints a vivid picture of the difficulties in measuring solar radiation, and concludes that only an independent investigaton, entirely divorced from the Smithsonian Institution, if it should confirm our results, would justify confidence in the variability of the sun. The summary of astronomical progress for the year 1924, published by the Royal Astronomical Society, mentions several papers adverse to solar variation, and leads the reader to conclude that scientific opinion generally, if not actually in opposition, is still doubtful of solar variation.

We are at the very great advantage compared to our critics that we know all about the work. We are aware of a great many circumstances that disarm criticism, and promote belief. It will be impossible to enumerate all of these here, but I hope to present so strong a case as to fully justify the investigations of Mr. Clayton, who has adopted solar variation as a working hypothesis and sought to see what comes of it. He reports these studies in the next succeeding paper of these Miscellaneous Collections.

Before proceeding, let me state one illuminating consideration. Some writers mention our data for the past 10 or 15 years as if all were of equal value. Really, to speak in a figure, the Washington data of 1902 to 1907 were Prehistoric. As for Mount Wilson results of 1905 to 1908, inclusive, before the invention of the silver disk pyrheliometer, or Fowle's method for estimating total atmospheric humidity, and while we yet used a flint glass prism limiting our spectrum at the H and K lines in the violet—this work is Ancient. Excluding altogether July and August, 1912, the year of the eruption of the Katmai volcano, all Mount Wilson work of 1909 to 1920 can be classed as Medieval. We had then but one station, operating only in summer. We obtained only one determination per day, subject to error from changes of sky transparency and also to errors of computing in the enormous multiplicity of computations used in the reductions of results by Langley's fundamental method. The period from January, 1919, to the present is of another order of accuracy, and represents the Modern period.

All of the Mount Wilson work, excluding altogether July and August, 1912, is useful in the form of averages. It is only since January, 1919, when we have had several determinations each day by a method which avoids errors from the variability of the sky, and much of the time have received results from two stations, that individual values have begun to deserve some confidence. Even yet, they are not up to the class which we hope they will reach within one or two years more. They are still most useful in the form of mean values. This, indeed, is a major reason why correlation coefficients reported by Clayton and by Hoxmark, in discussing their solar forecasts of daily weather conditions, are still low. Very accurate solar radiation data are necessary for that purpose, and we cannot yet quite reach the required degree of accuracy. The methods of reduction of observations for the station at Montezuma are being improved, the Harqua Hala station is being transferred to Table Mountain, California, 2,000 feet higher, and the National Geographic Society is installing, in cooperation with the Smithsonian Institution, a new station in the Eastern Hemisphere. We believe that these improvements will in about two years largely better the results.

DEFENSIVE ARGUMENTS

1. Our critic and I approach this matter from opposite points of view. He has felt that it is necessary to be sure that our solar observations are sufficiently impeccable before he can use them. I was convinced five years ago by figure 1 that one can use them, and having, in cooperation with the Argentine Weather Bureau and with Mr. Clayton, tried experiments in using them, every month reveals new evidences that they can be used. Consider figure 1. The high reputation of Mr. Clayton, whose results are here shown, forbids us to doubt that the march of the curves is real.

What then? Certain observations made on Mount Wilson, California, in the years 1913, 1914, 1915 and 1918 were definitely associated with temperature differences of 6° F. at Buenos Aires, Argentina, 10 days after the event.[1] But, says our critic, this is not a solar but a terrestrial correlation. I am so constituted that, if I

[1] Owing to errors often occurring at Mount Wilson because of increasing or decreasing haziness during an observation (errors nowadays eliminated in our "new method") no doubt some values in the high and the low groups of solar constants used by Clayton were extreme because erroneous. Thus the range he finds of 5 per cent, he and I now agree was probably not over 2½ per cent in reality. As will be seen in his present paper, such a range of solar constant is large enough to produce notable effects.

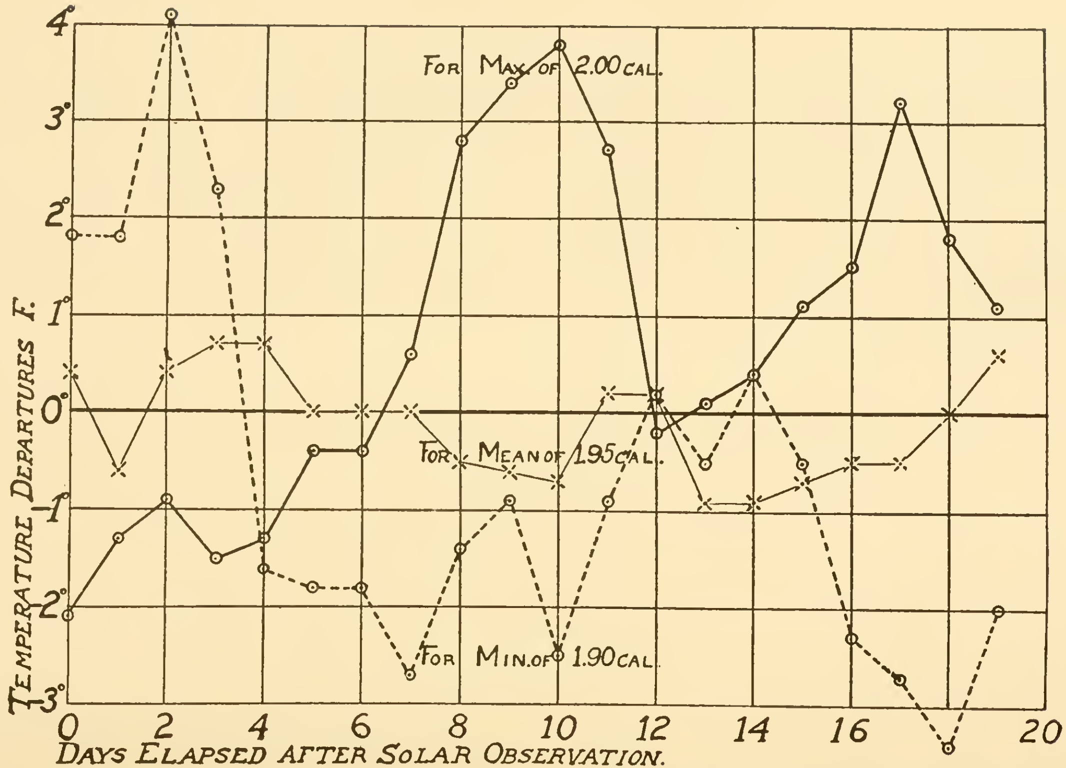


Fig. 1.—Prolonged influence of solar changes on terrestrial temperatures. The three curves each represent mean results for the years 1913, 1914, 1915, and 1918 on the variation of the sun (determined at Mount Wilson) and the temperature at Buenos Aires. The solar values are arranged in three groups, those exceeding 2.00 calories, those below 1.90 calories, and those between 1.94 and 1.96 calories, respectively. The corresponding mean departures of temperature for Buenos Aires from 0 to 20 days after the event are plotted in the diagram.

were a meteorologist, I would not care whether the correlation was through our air, through the sun, or through the star Arcturus; I would try to see whether so fair an opportunity to predict weather 10 days in advance could be reduced to a working basis, not only in Buenos Aires but elsewhere.

Mr. Clayton's paper describes the wealth of interesting results for North America which he has lately obtained in this way. I shall only give two or three examples of Argentine results. Figure 2[1] shows the solar variation of April, 1920, observed at Calama, Chile,

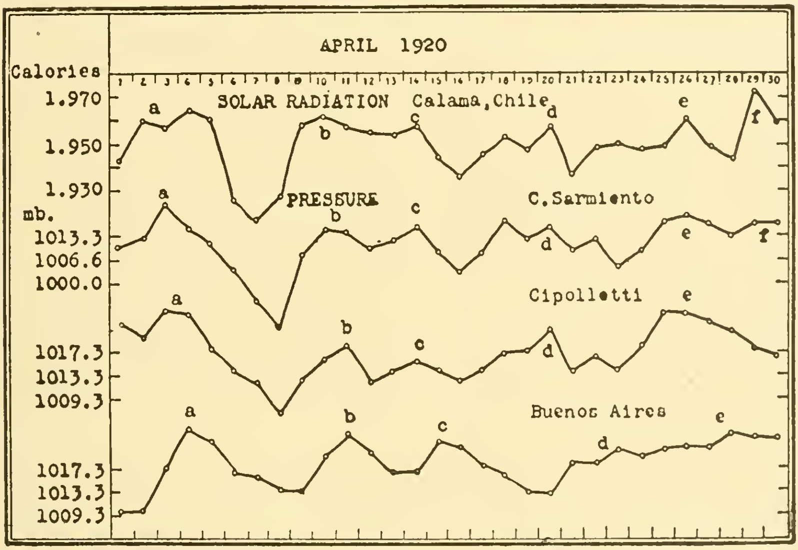


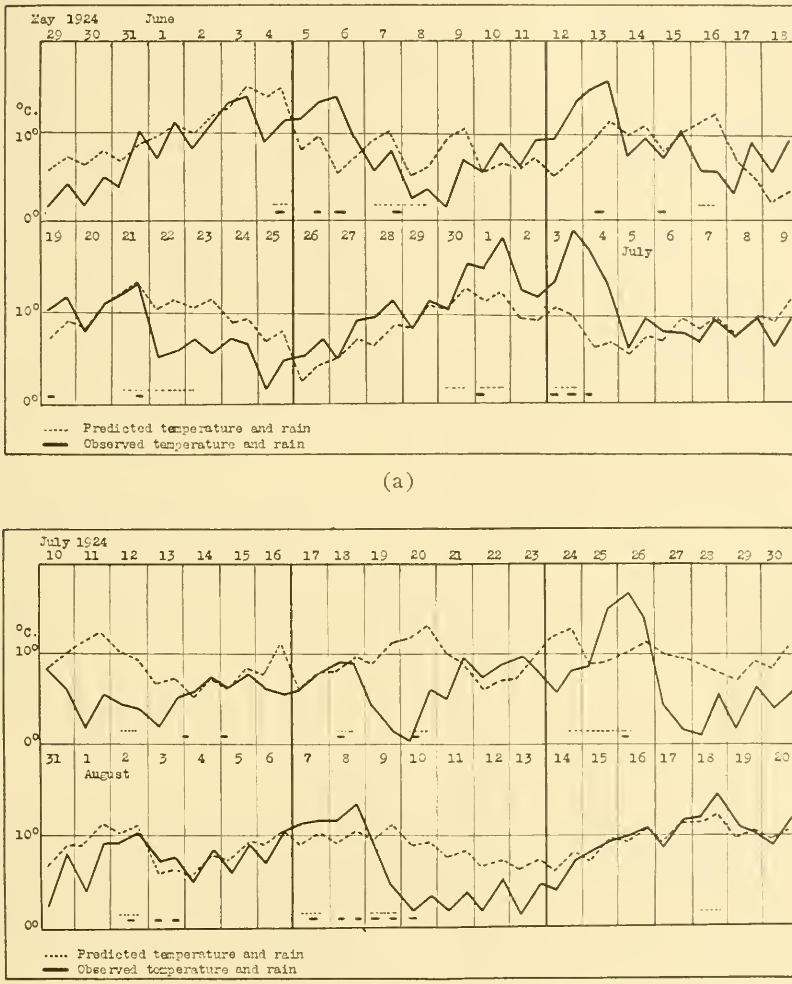
FIG. 2.—Solar variation and atmospheric pressure. Solar-constant values obtained in Calama, Chile, in 1920 are compared with the atmospheric pressure at three Argentine stations.

and compared to the barometric pressure at Sarmiento, Patagonia. Figure 3[1] shows 3 consecutive weeks of Argentine official forecasts by Mr. Clayton. Figure 4 shows 12 consecutive weeks of Argentine official forecasts by Mr. Hoxmark, Mr. Clayton's successor. All of these forecasts, based on solar variation, are exactly stated numerical predictions of the temperature of Buenos Aires, and are compared to the temperatures afterwards actually observed. The Argentine official forecast is prepared each Wednesday to cover the week beginning Thursday morning. Mr. Hoxmark writes that this solar forecasting is based on our daily observations at Montezuma supple-

[1] From Clayton's "World Weather," Macmillan Co., New York, 1923.

FIG. 3.—Weekly solar forecasts and verifications. The full curves give temperatures at Buenos Aires predicted each Wednesday for the ensuing week beginning Thursday. The actual observed temperatures are given by the dotted curves.

mented by visual observations of the sun in Argentina. It is prepared in a separate branch of the Argentine Weather Service from the ordinary daily forecasts, and independently of them.



(a)

(b)

FIG. 4.—Weekly solar forecasts and verifications. Weekly predicted temperatures and rainfall for Buenos Aires are indicated by the dotted lines, and observed temperature and rainfall by the full lines.

It may seem to some readers that the close agreement shown in figure 2 between the barometer at Sarmiento and solar variation observed at Calama ought not to be expected. They will say that

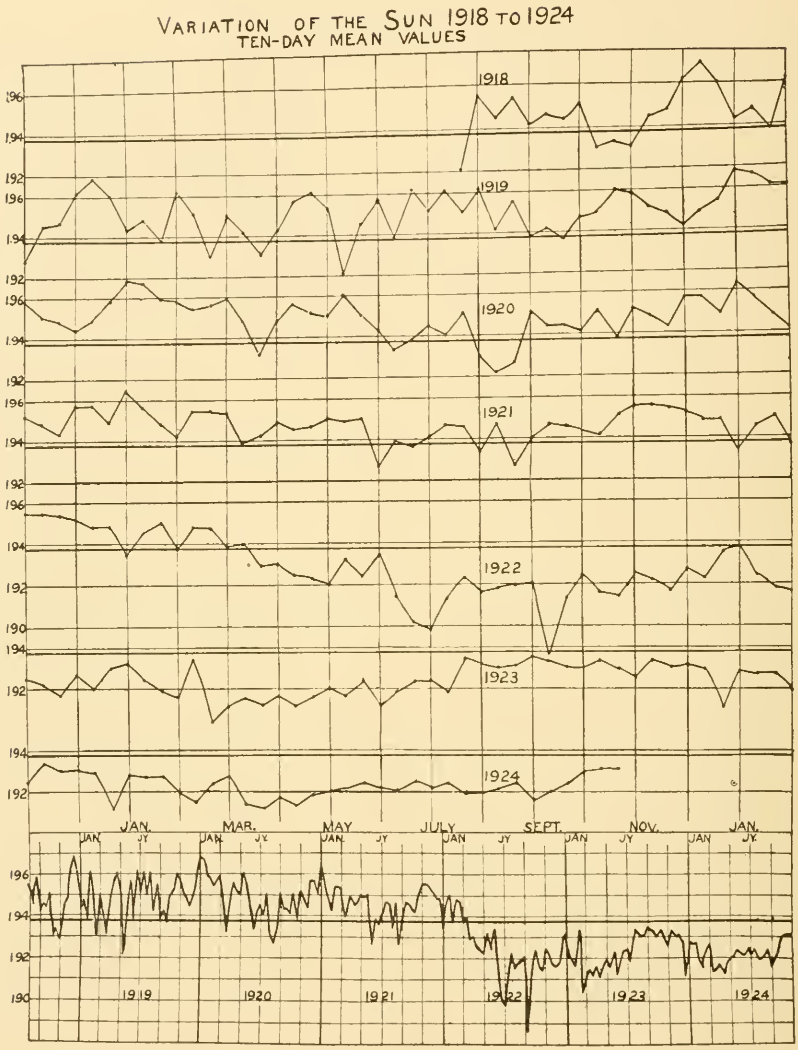


FIG. 5.—Ten-day mean values of the solar constant of radiation, 1918-1924. The seven upper curves give the march of values on a more extended scale, which is condensed into the single lower line of the diagram.

of course temperature varies if the sun does, but that the barometer can only follow temperature, and must lag behind. Please to reflect that, depending on locality, from 10 to 50 per cent of solar radiation outside the atmosphere is absorbed in the atmosphere by smoke, haze, water vapor, and clouds. Since the atmosphere has a very small capacity for heat, the heating effect of this tremendous energy absorption is very quick in the atmosphere, compared to what it would be in the ocean or on the solid earth. Sarmiento is about 100 miles from the Atlantic Ocean, in a very dry, clear region. Rainfall increases in every direction from it except the north. Suppose solar radiation increases. The surrounding air and the air at Sarmiento both immediately grow warmer and expand, but the effect is much greater over the cloudy regions than at clear Sarmiento. Hence, air flows from all around to Sarmiento and raises the barometer there. Similar action centers exist all over the world. They vary in position with annual change of cloudiness and from other causes, which Mr. Clayton's paper discusses.

I now pass to consider certain other criticisms before taking up reasons why we are convinced that the sun varies, and that our observations give substantially the true picture.

2. It has been pointed out by the late Professor Newcomb and by Professor Marvin that there is no evidence of a permanent hot or cold side of the sun. This accords with our results. Such a condition sometimes exists for a few revolutions of the sun, but not permanently. Hence, the solar rotation can be used only with greatest circumspection as a period to forecast by.

3. Professor Marvin has suggested that our recent observations are badly prejudiced by a terrestrial 12-month periodicity. I will not say that there was absolutely nothing of the kind in Mount Wilson observations, but I regard it as nearly or quite nonexistent in later work. He has mistaken a real 11-month periodicity in recent years for a 12-month periodicity. Mr. Clayton discovered the 11-month periodicity over a year ago and reported it to me. Figure 5 shows maxima in January, 1920, and September, 1923, an advance of 4 months in 4 periods. Figure 6, which Mr. Clayton prepared, shows the matter still clearer, because the short period solar fluctuations have been removed by a usual process of smoothing, and we see clearly that the maxima and the minima succeed one another by 11-month intervals. Additional minima occurred in April, 1924, and March, 1925, so that the 11-monthly depression has clearly shown, excepting in May, 1923, ever since the year 1918.

This relates to the past seven years. I am not prepared to insist that it runs back of 1918. Possibly, like the hot and cold side of the sun, which holds sometimes for several revolutions, it may have disappeared in process of time.

In Professor Marvin's 12-month curves from Mount Wilson observations of from 3 to 7 months duration, it is necessary for him to exterpolate over half the year. He combines 15 years of observing. Surely he should have omitted July and August of the year 1912 when the sky was so very turbid from the Katmai eruption that its skylight reaching the pyrheliometer may very likely have led to higher values than its relation to sun-spot minimum would have led us to expect.

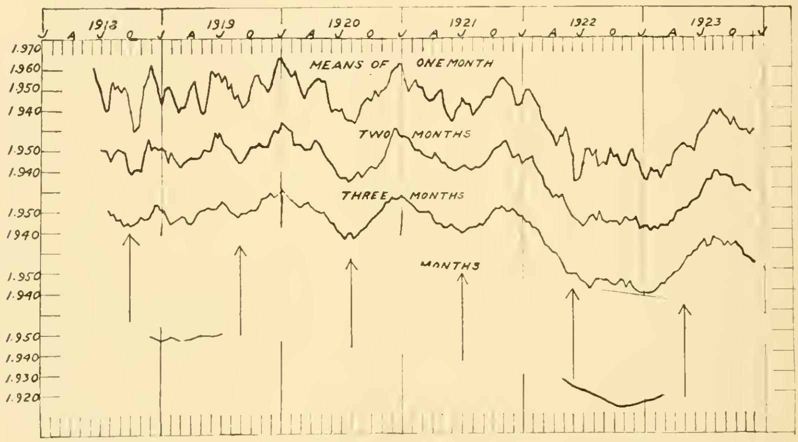


FIG. 6.—Eleven-month periodicity in solar variation. The monthly means for solar radiation given in the upper curves are smoothed by a usual process, and show minima eleven months apart, as indicated by the arrows.

The Mount Wilson values of the years 1918, 1919, and 1920 agree in detail with Calama, as I shall show directly, so that they need not be considered as indicating spuriously high summer values. The monthly mean values for the other 11 years are plotted in figure 7. Maxima occur in every month observed except May, and minima occur in every month observed except August. The run of the curves is so varied that one cannot safely conclude how the other unobserved months of the year would have turned out. Professor Marvin has thought that their maxima agree with minima of Chile, northern summer agreeing with southern summer, and northern winter with southern winter. His Mount Wilson and Chile data refer to different years. There is no fair comparison of one year with another when the hypothesis of solar variation is in the field. When, however, we take Mount Wilson and Calama data of identical dates, as in figure 8, they show no such variance. Figure 8 gives all comparable daily values of 1918, 1919, and 1920. Except for the greater number

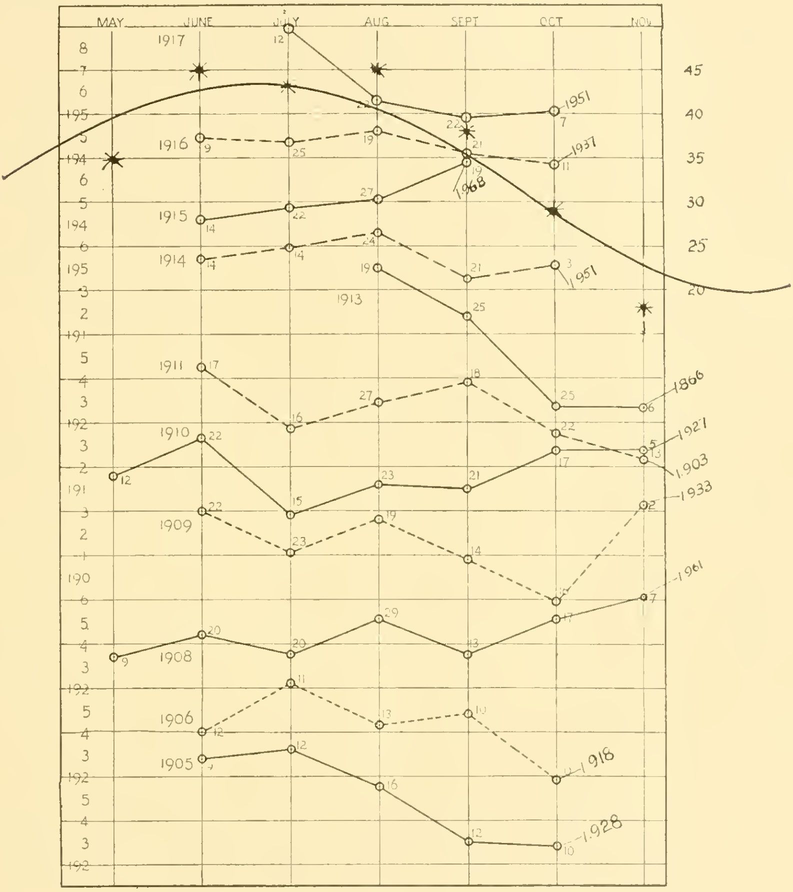


FIG. 7.—Monthly mean observations at Mount Wilson, 1905-1917. The heavy smoothed curve and stars near it represent Prof. Marvin's determination of the yearly periodicity in solar radiation, as indicated by the scale at the upper right-hand corner of the diagram.

of poor results at Mount Wilson, the agreement is good and yields a correlation coefficient of about 50 per cent.

There is another objection to using the mean results of the 15 years as Professor Marvin has done. The numerous values of the months June, July, August, and September, which have fixed the high part of his data, relate to two sun-spot maxima and only one sun-spot minimum, so that they tend to a high level on account of great solar activity. The points for October and November, which are so very intrumental in leading him to his conclusion that winter months would run lower, are brought down by the disproportionate number of very low results of 1911 and 1913, years of sun-spot minimum and little solar activity.

4. In the next place, our critics have argued, from the steadily decreasing average scatter of the solar constant observations, as we have been getting better and better observing conditions, that if

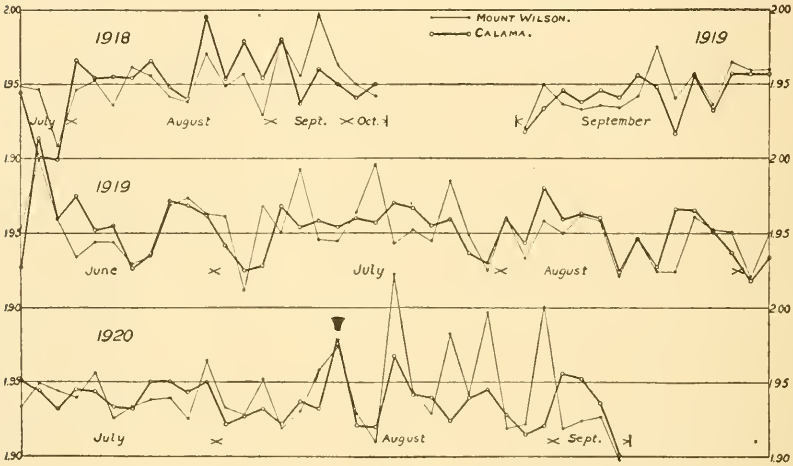


Fig. 8.—Simultaneous daily solar-radiation values at Mount Wilson and Calama, Chile. The days not observed simultaneously are omitted and the curves brought together into continuous broken lines.

we got perfect results no fluctuations would be left. Indeed, according to them, in recent years there is no room left, after allowing for reasonable error, for any appreciable solar variation. With apologies for being a little playful, I would like to put my reply in the form of a short parable.

I meet our critic and say, "Have you noticed, Sir, those tall objects in that field?" "No," he says, "How tall are they?" "Why, Sir, I measured them," I reply, "and the measurements are all on this paper." "Let me take it," says he, "and I will look it over and perhaps I will be able to go down and see those objects."

The next time I meet him, I say, "Well, Sir, have you seen those objects which I mentioned to you the other day?" "No," he answers.

"To tell the truth, I have been examining your paper and I fear you are mistaken." "How so?" I ask. "I have taken the mean value of the heights of the objects as you measured them," he replies, "and find that it is but 4 inches. And, therefore, according to the theory of probability it is excessively unlikely that there are tall objects in the field." "But, Sir," I say, "there are flowering shrubs there at least 6 or 8 feet high, and trees which look at least 30 or 40 feet high." "I regret to differ with you," he answers, "but I am sure my averages are right, and so the mathematics are against you." "But, really, Sir," I reply, "the Washington Monument is in that field. The fact that there are also 17 million blades of grass there cannot shorten it any, though it brings down your average to 4 inches."

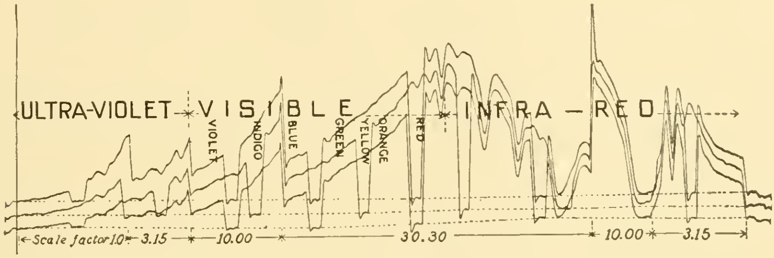


FIG. 9.—Bolographs of the solar spectrum energy distribution.

Similarly, as I see it, the small average scatter of our solar radiation values of recent years about their mean does not preclude us from admitting that some even of the larger deviations are really of solar origin.

5. This leads to our critic's most serious charge. It is suggested that Harqua Hala and Montezuma have lost their characters as independent solar observing stations owing to our methods of removing systematic errors from their data.

My colleague, Mr. Fowle, and I have devoted a great deal of thought and time to a conscientious effort to free the observations of both stations from all terrestrial errors.

Referring to figure 9, our trouble is this: Owing to the greatness of the water-vapor absorption bands in the infra-red spectrum whose areas we have to determine, it is not possible to know just exactly how to draw smooth curves over the bands in that region. We draw as best we can, but we expect to find, and do find, when we examine a large number of solar-constant values, that the results are not independent of the quantity of water vapor prevailing.

What is to be done? Certainly not to leave a known source of error without attempting to remove it. We proceed as astronomers do in correcting star observations for known defects. We separate the results into groups with steadily mounting values of water vapor, plot them, and determine the best corrections for the water vapor effect. That was all we did or could do to correct such systematic errors in the Mount Wilson data.

This would be satisfactory if it were not for the sun's variation at the same time. If we had 50 years of homogeneous observations made at one station to discuss, solar variation could be neglected. It would fall out in the means. But we cannot wait 50 years.

Now comes the part that our critic objects to. We make only one assumption. It is this: A series of observations taken with identical water vapor, identical sky brightness, and all at one observatory, are comparable without any corrections at all. Suppose we take all the observations of one observatory and divide them into such groups, each including only a very narrow range of humidity and sky brightness. Each group of days indicates the solar variation in that group. But there is no way to pass from one group to another, so long as we have only one observatory.

But arrange the values similarly for the other observatory. Again we shall have the variation of the sun indicated strictly within each group, but have no means to pass from group to group. But stay! The days comparable at one observatory fall in various groups at the other. Thus, we find a great many independent determinations, sometimes as many as 20, of each crossing-over factor from one group to another. We take their mean indications, and so are able at length to put all of the observations at each observatory on a comparable footing. Then we compare all the days which are common to both stations and we find that a small, uniform, constant correction, which, of course, does not affect variability at all, is needed to bring them to a common scale.

In all this there is nothing that I can see to make Harqua Hala variations dependent at all on Montezuma variations. After thus getting all *past* observations to a comparable status, we can now go back to eliminate solar variation from the original observations. Having done this, we can go on with each station independently by the usual method of grouping, already explained, so as to get a separate formula for each station, by which all *future* observations of that station are corrected. This also introduces no dependence of one station on the other.

CONSTRUCTIVE ARGUMENTS

Having considered the objections: (1) That it is futile to seek meteorological correlations with imperfect solar observations; (2) that the most naturally to be expected solar variation does not appear; (3) that terrestrial sources of error are obviously still in evidence; (4) that for the past six years our results have shown so small a scatter about the mean that there is no room for solar variation; and (5) that our two stations, intended to check each other's findings, are not really independent, I am ready to take up the constructive part of my paper.

Thesis (*a*).—The theory of probability admits of the belief in the real existence of short-period solar variations, some of which exceed 2 per cent in amplitude.

It is not material to this argument to prove that the scale of Smithsonian measurements is exactly in terms of the 15° calorie. If the average value of the solar constant which we find to be 1.94 calories is really as little as 1.90, or as great as 1.98 calories, it matters not. By expanding or contracting the true calorie slightly, the mean solar constant can be expressed as 1.94. The only question at issue is whether, after this adjustment is made, there are real fluctuations of short period as large as 2 per cent in this conventional value.

Conceive, if you please, an angel to have brought us from heaven the true curve of solar variation covering the period 1920 to 1924, expressed on the same scale as our determinations. We are to inquire, first of all: What will be found to be the average deviation and probable error of our observed curve from the angel's curve?

To determine this question, we have 327 differences between independent daily solar-constant determinations of good character, made at Harqua Hala and Montezuma.[1] I may remark, in passing, that since there is almost three hours difference in longitude, these daily differences are greater, owing to solar variations occurring between measurements, than they would be if the stations observed simultaneously. So our investigation is too liberal to our critics, but I am willing to grant them this advantage.

The average daily difference, Harqua Hala minus Montezuma, is ±0.011 calorie. This is the average daily difference between two series of measurements both affected by accidental errors, and, let us assume, equally affected thereby. Evidently, therefore, the average deviation of either station from the angel's curve is less than 0.011 calorie. It is, in fact, $\frac{0.011}{\sqrt{2}}$ as I have demonstrated both theo-

[1] See Smithsonian Misc. Coll., Vol. 77, No. 3, table 4.

retically and by the expedient of drawing several hundred numbers from a bag containing positive and negative numbers equally, and arranged in magnitudes in accord with the probability curve.

So the average deviation of either station from the angel's curve is 0.0078 calorie. The probable error, therefore, of the daily measurement of either station alone is 0.845×0.0078 or 0.0065 calorie. But there are many cases available where both stations observed on the same day. In these cases the probable error of the general mean is $\frac{0.0065}{\sqrt{2}}$ or 0.0046 calorie.

I wish it to be realized fully that we do not have to *guess* at the probable error of our determination of the solar constant. Our 327 observations at two stations give abundant material to determine it accurately. As remarked above, we do not pretend to claim that constant errors of scale are included in these small probable errors, which are respectively 0.0046 calorie for general mean results of both stations, and 0.0065 calorie for mean results of a single station. But the possibility of systematic errors of scale has nothing to do with the question of short-period variability.

Having thus obtained the probable error values, we next inquire whether the results, when the long swings of the solar constant which critics have sometimes admitted may be probably real are shut out, still exhibit solar fluctuations. To eliminate the long swings, I make use of the monthly mean values, and take daily departures therefrom. I separate these daily departures from the monthly means into two series, the first containing days observed at both stations, the second containing days observed at one station only. The numbers of observations are 398 and 744, respectively, in the two series.

Taking the first series of 398 days observed at both stations, the numbers of departures from the monthly means, grouped in magnitudes, are as follows:

TABLE 1.—*First Series. Departures in ten thousandths of a calorie.*

Departures	Magnitude ...	0–.0015	.0015–.0030	.0030–.0055	.0055–.0075
Number	Observed	84	69	59	49
	Computed ...	69	68	94	56
Departures	Magnitude ...	.0075–.0095	.0095–.0115	.0115–.0145	.0145–.0175
Number	Observed	48	41	14	12
	Computed ...	46	28	20	13
Departures	Magnitude ...	.0175–.0205	.0205–.0245	.0245–.0295	
Number	Observed	11	5	6	
	Computed ...	3	0.9	0.1	

The values marked "computed" are obtained as follows: Taking the value of the probable error as 0.0046 calorie, table 25 of the Smithsonian Physical Tables enables us to compute at once how many departures there should be according to the theory of accidental errors up to the limits ±0.0015, ±0.0030, and so on up, in a series of 398 observations. By subtraction, we obtain the numbers between intervals ±0.0015 and ±0.0030, between ±0.0030 and ±0.0055, *et cetera.* These are the values given in the lines marked "computed" in the table.

It will readily be seen that there is a goodly number of considerable departures, even exceeding ±1 per cent of the solar constant, where the theory of accidental error indicates that should be none at all. No less than 18 of these extra, unexpected, values exceed

TABLE 2.—*Second Series. Departures in ten thousandths of a calorie.*

Departures	Magnitude ...	0–.0025	.0025–.0055	0055–.0085	.0085–.0115
Number	Observed	156	136	136	121
	Computed ...	152	169	143	107
Departures	Magnitude ...	.0115–.0145	.0145–.0185	.0185–.0225	.0225–.0270
Number	Observed	74	53	23	18
	Computed ...	74	58	26	10.5
Departures	Magnitude ...	.0270–.0310	.0310–.0350	.0350–.0390	.0390–.0430
Number	Observed	15	4	4	4
	Computed ...	3.0	1.3	0.2	0.0

± about 1 per cent. This tends to prove that short-period solar variation cannot be denied a real standing in court. We shall treat long-period changes separately.

We are at less advantage when we take the second series, because the probable error is now 0.0065 instead of 0.0046 calorie. Nevertheless, the conclusions are much the same, as shown in the table. There were here 744 cases in all, of which 30 more than the expectancy exhibit departures exceeding ± about 1 per cent, and 4 exceed ± about 2 per cent.

Figure 10, based on table 1, shows strikingly the excess of large observed departures over those predicted by the theory of accidental errors.

Thesis (*b*).—Consistent evidence of solar variation is found in the results of both stations.

In figure 11, I show monthly mean values of both stations for the years 1920 to 1922. The curves cover one of the most interesting periods of solar change which we have discovered. It will be seen that not only the long enduring downward march, but the temporary recoveries of solar radiation are often duplicated in the results of Montezuma and Harqua Hala.

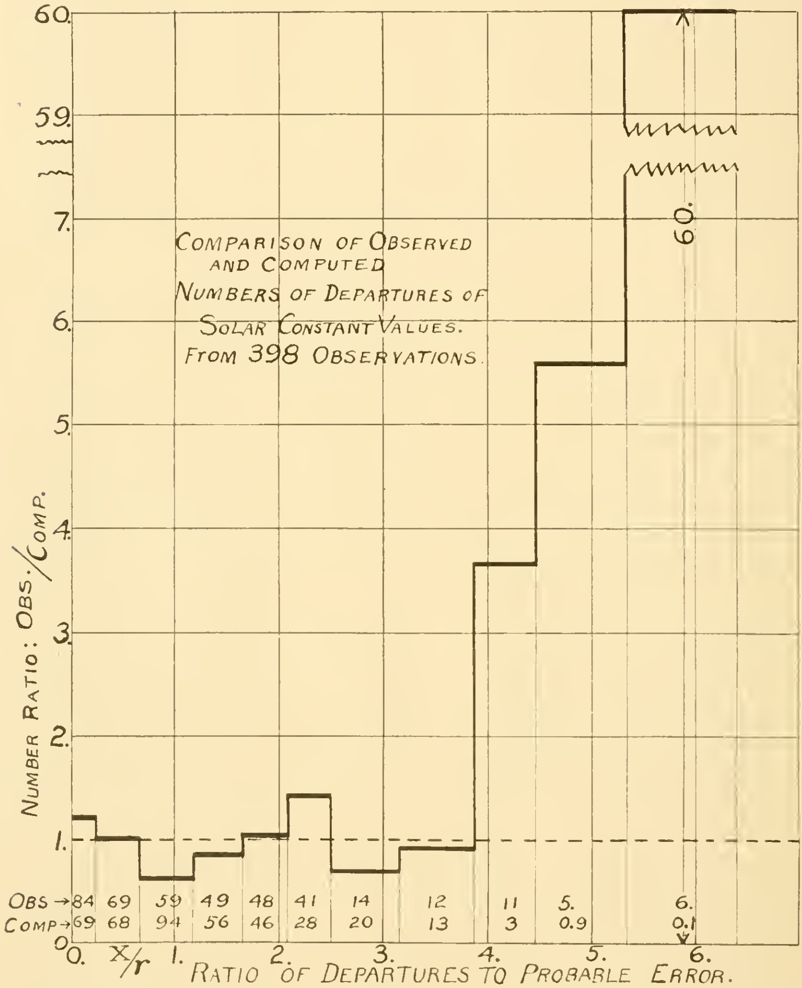


Fig. 10.—Theory of probability indicates possible solar variations of short period.

Figure 5 gives 10-day mean values of solar radiation from 1918 to 1924. This indicates that the low period of solar radiation still continues, although recently with a rising tendency.

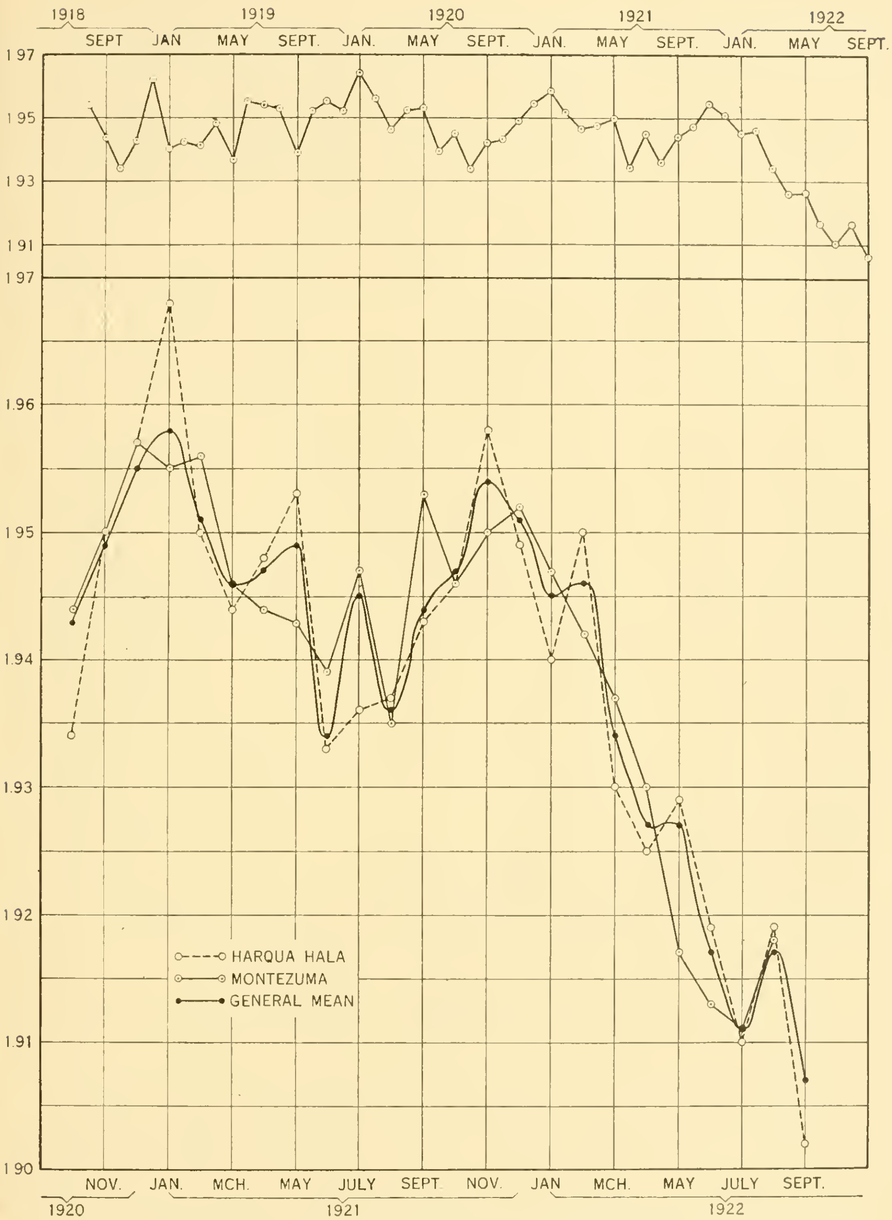


FIG. 11.—Monthly mean solar-constant values and individual results of 1920-1922 at Harqua Hala and Montezuma. The curve shows the great fall of solar radiation beginning February, 1921.

Figure 12 shows a direct comparison of Montezuma and Harqua Hala from 1920 to 1924. Over 300 days common to both stations have been arranged in 16 groups of gradually increasing mean solar-constant values, as indicated by Montezuma observations. These identical groups of days' results were also averaged for Harqua Hala. Of course, in this way the range shown at Harqua Hala must

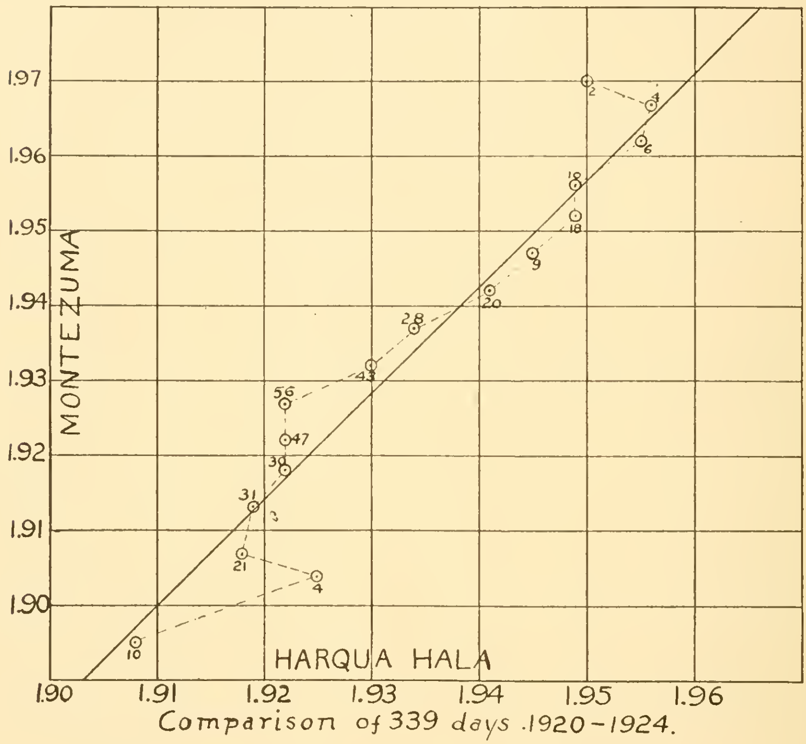


FIG. 12.—Correlation between Montezuma and Harqua Hala on solar variation.

necessarily be less than that shown at Montezuma, because some of the extreme Montezuma values will be extreme on account of error of observation, and will not be extreme at Harqua Hala. I have, therefore, given Harqua Hala a more open scale so as to incline the line at 45°. The correlation is obvious.

Thesis (c).—Observed changes in solar radiation are clearly associated with visible changes in the sun.

Figure 13 shows a comparison of Wolf sun-spot numbers with all of our thousands of solar-constant values obtained from 1905 to

1924. Mean Mount Wilson values, grouped with gradually increasing spot numbers, are given by crosses. Modern values of 1918 to 1924 from Montezuma and Harqua Hala observations are indicated by circles. Evidently higher solar constants are associated with greater solar activity. Some of the irregularities of the data are probably due to the counteracting tendency associated with crossing of the sun's central meridian by spots, as will be mentioned below.

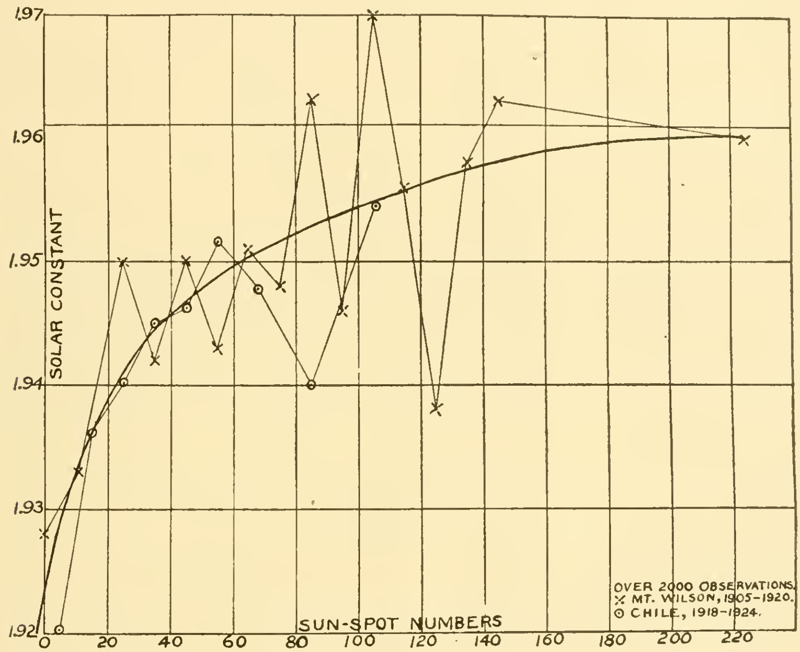


FIG. 13.—Increased sun-spot activity brings higher solar-constant values.

My colleague, Mr. Fowle, has compared the results published by the Observatory of Ebro on areas of sun spots and of flocculi with our solar-constant values of 1921-1923. Figure 14 shows these relations. It is clear that a fairly close connection appears between flocculi and solar constants, closer than prevails between sun spots and solar constants. The extraordinary drop from 1921 is confirmed.

Solar changes of *short* period also accompany observed changes in the sun's visible appearance.

In the summer of 1923, being at the Mount Wilson Observatory, Director W. S. Adams and I took all the simple photographs of the sun which had been made there from August, 1918, to July, 1920, and compared them with the solar-constant values secured by Smithsonian observers at Calama, Chile. We were soon perfectly agreed that we perceived the following relation: When a sun spot, or group of sun spots, crosses the central diameter of the solar disk, in course of the solar rotation, the next following day almost invariably shows a minimum value of the solar constant. We perceived this to hold in so very large a proportion of cases that all doubt of it was dispelled.

FIG. 14.—Comparison of solar variation with variation of visible phenomena on the sun from results of the observatory at Ebro.

A conspicuous case occurred in March, 1920, as shown in figure 15. More recently, Mr. Clayton has made a quantitative examination of this relation extended over several years of observation. His result entirely confirms ours. Still more recently, my colleague, Mr. Fowle, has taken the quantitative data of the Observatory of Ebro in Spain, where they give sun-spot areas within 15° of the sun's center. He finds a plain correlation of the same sort. Some examples of it are shown in figure 16. Large spotted areas near the sun's center are nearly always associated with lower solar constants.

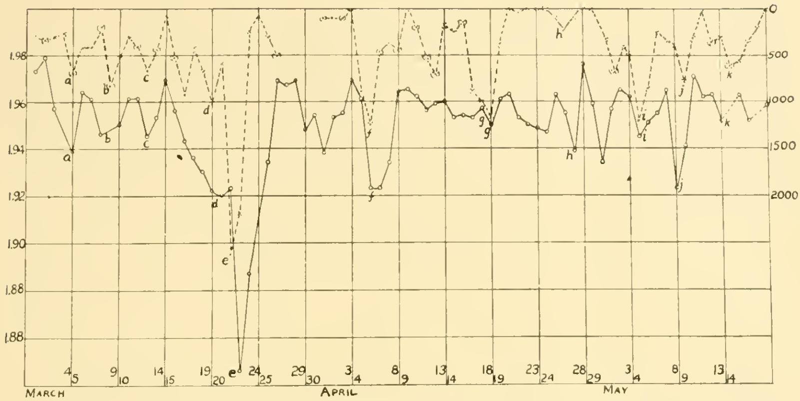


FIG. 15.—Variation of the sun, March to May, 1920. The lower curve (scale at the left) gives values of the "solar constant of radiation" observed by Smithsonian men at Calama, Chile. The upper curve (scale at the right) gives areas, in millionths of solar hemisphere, of calcium flocculi, measured at the Observatory of Ebro in Spain. Only flocculi within ±15° of the central solar meridian are included. Two scales of days are given, as the upper curve is displaced 1 day forward. Coincident depressions are indicated by letters.

FIG. 16.—Central sun spots and solar radiation. The lower curve is from publications of the observatory at Ebro.

Another relation has been found by Mr. Clayton. He speaks of it in his paper. When faculae are conspicuous, high solar-constant values may be predicted. Mr. Clayton has gone further. He has made visual solar observations daily with a telescope in Canton, Mass., on every available day for nearly a year, and has sent me a letter the same afternoon in which he has predicted what the solar constant would be 5 days after. After these predictions had been maintained for 7 months, I compared them with our observations.

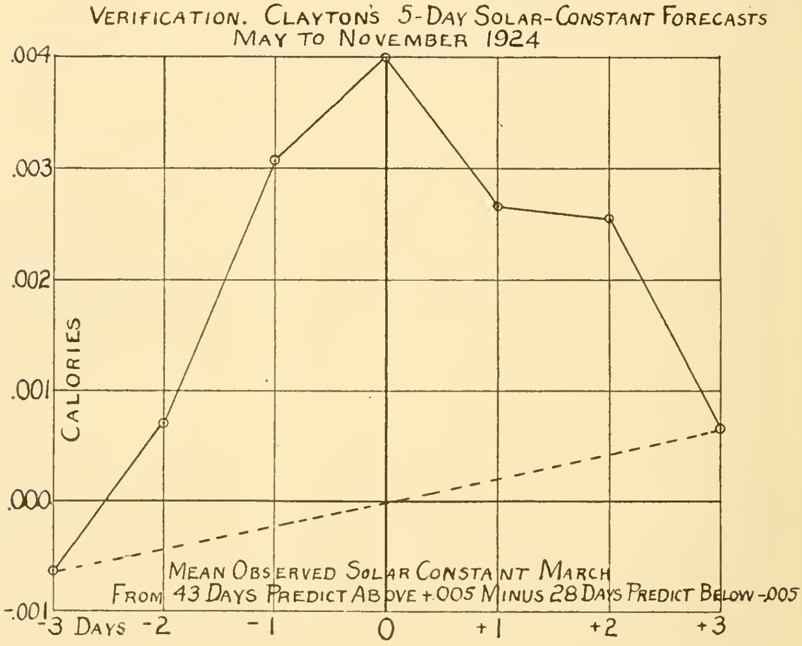


FIG. 17.—Verification of Clayton's solar-constant forecasts. The mean march of solar variation from three days before to three days after Clayton's forecasts is compared for those dates on which he predicted .005 calorie above with those on which he predicted .005 calorie below normal.

I found a strong correlation which reached its maximum exactly on the day he predicted for, as shown in figure 17. Mr. Clayton has discovered other relations between solar changes and faculae observed on the solar disk.

Hence, we may claim that the visible appearances of sun spots, faculae, and flocculi on the sun are clearly associated with the short-period variations of the solar constant.

Thesis (*d*).—Solar changes are localized to short wave lengths.

The question arises whether increase of the solar constant implies increase of intensity in equal proportion over the whole spectrum. To test this, we have used a number of the best determinations made by the fundamental method of Langley. We separated these into groups of high, medium, and low solar constants, and took mean values of the spectrum distribution outside the atmosphere. We divided the numbers representing the distribution curve for low solar constants into the corresponding numbers representing higher ones, first having reduced the curves to such a scale of ordinates as

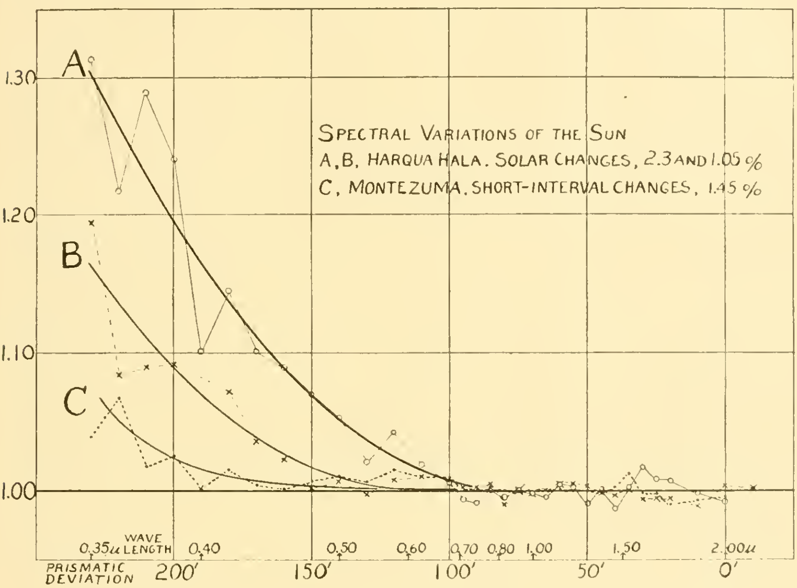


FIG. 18.—Solar variation localized in the violet and ultra-violet.

to represent the change in solar constant by the change of area included under them. Figure 18 shows the result. Curves A and B are for Harqua Hala values of high and medium solar constant as compared to low. These represent what happened in the big swing in solar-radiation level from 1921 to 1923. The 1921 values were high because the blue, violet, and ultra-violet were high. The green, yellow, red and infra-red were almost unchanged.

Curve C is from recent Montezuma values of 1924. It represents, therefore, nothing but short-period solar fluctuations. No care was used in computing Curve C to reduce the areas under the curves to proportionality with the solar constant. However, it will be seen that here, too, the change was mainly in the blue, violet, and ultraviolet spectrum.

There are evidently two kinds of solar change. The long-period swings are related to the total solar activity. Great visible activity in the sun, such as numerous sun spots, faculae, or prominences, like stirring a fire, brings hotter radiating surfaces to the front, and produces higher solar constants.

But on the other hand, whenever a sun spot crosses the sun's center, it carries along with it a cloudlike effect, not a cloud, of course, but a diminished transparency. When this diminished transparency points towards the earth, we have for a few days lower solar constants.

Both kinds of change affect the short-wave rays of the spectrum more than the long-wave rays. This is, of course, what one would expect. Increased effective solar temperature, attending increased activity, would produce its larger effects at shorter wave lengths, in accord with the Wien-Planck Law of temperature radiation. Increased opacity of the solar envelope, just like increasing opacity of the earth's atmosphere, would also produce its larger effects at shorter wave lengths, quite in accord with our own observations of atmospheric transmission coefficients. It is yet too early to decide by a comparison of Curve B with Curve C that there is a real difference in the quality of these spectrum changes, depending on the character of the solar change involved. Yet so far as this evidence goes, it indicates a less pronounced contrast between short- and long-wave rays in spectrum change for short-period solar variations than for long-period ones.

CONCLUSION

To sum up:

1. It is not necessary to wait for perfectly impeccable solar-constant determinations to determine changes of the sun's radiation well enough for useful comparison with meteorological phenomena. Better values, however, will soon be available.

2. Such comparisons as have been made indicate that a higher accuracy than the present in solar-constant determinations will be needed to yield high correlations in forecasts for individual days, but that where mean values can be used, as in forecasts for weeks or months, present values are fairly satisfactory.

3. There is no reason to think that the independence of the two solar radiation stations, Montezuma and Harqua Hala, has been lost on account of means used to eliminate systematic errors.

4. The probable error of a fairly satisfactory mean daily value for one of these stations alone is 0.0065 calorie, and for a fairly satisfactory daily mean value derived from results of both stations it is 0.0046 calorie.

5. From a study of numbers of observations, and their departures from the monthly means, in connection with these values of the probable error, it is found that many more departures of magnitudes of from 1 to 2 per cent are found than should arise from accidental error. This investigation ignores the still larger departures of longer periods which attend changes in solar activity.

6. The theory of probability allows us to entertain a belief in short-period solar variations as well as in long-period ones.

7. Both short- and long-period solar variations are associated with observable changes in the appearance of the sun.

8. Two stations 4,000 miles apart agree in disclosing both short- and long-period solar variations of several per cent amplitude.

9. Both short- and long-period solar variations are attended by alterations in the form of the solar energy-spectrum distribution. These alterations are far greater for short-wave rays than for long-wave rays.

10. There is a twofold cause for solar variation. Long-period fluctuations are due to changes in solar activity. Short-period fluctuations are due to obscurations in the solar atmosphere, which, rotating with the sun, produce depressions whenever they point towards the earth. For this cause, solar variation is not closely correlated with sun spots, because, though numerous sun spots betoken great solar activity and high solar constants, yet each individual sun spot, as it passes through the sun's center, carries its obscuring tendency, and produces a temporary depression of solar radiation as viewed from the earth.