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MEASUREMENTS OF SOLAR RADIATION AND THEIR INTERPRETATION

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[Presidential address delivered before the American Meteorological Society meeting at Philadelphia, December, 1926]

Solar energy in the form of thermal radiation which enters and penetrates the atmosphere of the earth is recognized by all, I believe, as the primary source of all the phenomena of weather, and, indeed, of life itself on our planet.[1] Surely, then, no other subject is more deserving of exhaustive measurement and investigation than solar radiation, especially if we also include the study of sun spots, prominences, and like solar phenomena as other evidences of solar activity.

This address, however, is limited to a very brief consideration of certain sources of error inherent in fundamental measurements of solar radiation and to the interpretation of the observational data.

Systematic study of a scientific question calls first of all for a sound theoretical background and plan of procedure; second, for adequate instrumental apparatus by means of which the third prerequisite of investigation may be secured, namely, a homogeneous body of observational data. The fourth and last stage is a search for the correct interpretation of the observations.

In the case of the solar constant as a question for scientific investigation, it remained for Samuel Pierpont Langley first to transform the imperfect, insufficient older theories of atmospheric transmission and depletion of incoming radiation to the more complete and adequate theoretical background such a problem required. Second, by the invention of the bolograph he finally, for the first time, made it possible to evaluate correctly the atmospheric absorption, and hence the original intensity of the solar radiation as it reaches the outer limits of the earth's atmosphere. This physical magnitude Langley called the solar constant of radiation. Only in later years did he contemplate the possibility that the intensity of solar radiation was a variable.

In the introduction to his report to the Chief Signal Officer on his solar expedition to Mount Whitney in 1880, Langley wrote these significant words:

> If the observation of the amount of heat the sun sends the earth is among the most important and difficult in astronomical physics, it may also be termed the fundamental problem of meteorology, nearly all whose phenomena would become predictable if we knew both the original quantity and kind of this heat; how it affects the constituents of the atmosphere on its passage earthward; how much of it reaches the soil; how, through the aid of the atmosphere, it maintains the surface temperature of this planet; and how, in diminished quantity and altered kind, it is finally returned to outer space.
>
> Meteorologists have till lately occupied themselves more with the secondary effects of this solar radiation than with the considerations just referred to, though this primary study will at least enable us to survey subordinate and familiar phenomena from a more general point of view and will correct some errors.[2]

The Weather Bureau is sometimes reproached for seeming to give investigations of solar radiation as a possible aid to weather forecasting an unsympathetic reception. In this connection it is interesting to note that Langley's solar expedition to Mount Whitney was not only indorsed, but also financed, by the then juvenile Weather Service of the Signal Corps. From 1880 to the present time the meteorologists of the Weather Bureau, although naturally skeptical of unproven claims of solar variability and the direct response of weather to such alleged solar variation, have always retained a keen interest in the ultimate outcome of the remarkable investigations inaugurated by Professor Langley at Mount Whitney and carried forward with such consummate skill and persistence by Dr. Charles Greeley Abbot.

In the beginning of Langley's research he spent more than a score of years upon what we have called the second or instrumentation stage of the extremely difficult investigation necessary before it was possible to reach even the approach to the third stage; that is, the beginning of the collection of systematic observations.

In fact, at the time of his death in February of 1906, Professor Langley had before him only the merest fragment of the observational data now available. Nevertheless, he was impressed with the possibility of appreciable day-to-day variation in the true values of the solar constant. This interpretation of the data then available was plausible enough, because even the best measurements then made at Mount Wilson in the summer of 1905 showed a probable day-to-day variation of nearly 1½ per cent. The variability of far less satisfactory earlier observations made at Washington was still greater. It is now generally believed these large variations were chiefly, if not wholly, caused by the combined sources of variation due to instrumental errors and atmospheric influences.

After another score of years of progress there is available to-day a large body of measurements of the solar constant. Let us examine these carefully in order that the evidence bearing on the question of constancy or variability of the sun may be more clearly understood and the true thermal basis of theoretical meteorology more accurately evaluated.

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1 Fully recognizing the probability that some fluctuations in intensity of solar radiation occurred during geological times, I am reluctant to believe that radiant solar energy alone dominated past climates as it does at the present time, or that solar control will continue to be what it is to-day in the remote future. For the climatologist I must add my conviction that solar heat alone, as we now know it, is not necessarily sufficient to have caused or to explain the climates which geologic history shows the earth has experienced in the past, climates which there are many reasons to believe may recur in like kind in the remote future.

The thesis of *variations in the availability at the earth's surface of the stores of its internal heat* has a claim to be recognized as a possible and at times an important factor in causing the climates of the ages, past and future (see Marsden Manson's "The Evolution of Climates"). Geophysics and isostasy compel us to recognize that what we call the solid crust of the earth is after all of only apple-skin thickness if the earth were reduced to the size of the apple, a crust less than 100 miles thick at the most. The matter within is plastic, or even fluidlike, under slowly acting great forces. Such properties are due to high temperatures as well as great pressures.

It is probably impossible at the present time for anyone to set out a satisfactory and acceptable explanation of the mechanism and process by which earth heat has at any time in the past or may again dominantly influence climatic conditions. Nevertheless, the climatologist can not afford to disregard the cumulative effects on climate over long ages of time of probable variations in the availability at the surface of the internal heat of the earth. The stores of heat seem to be within, the crust is relatively very thin, and who can say that slow variations in availability at the surface do not occur?

2 Professional Papers of the Signal Service, No. XV, p. 11.

In this age of marvelous advances in scientific measurements it is perfectly easy to recognize that in all probability the sun is a slightly variable star. To state it thus simply, however, is to mislead the general public and even a great many men of science. Suppose I ask one of these the question: "Tell me, now, just how much do you think the sun varied from day to day during the year? The Smithsonian Institution gave the Weather Bureau the values whenever observed, and they were printed on the Washington weather map. How much variation do you think these observations show?" Outside of the experts familiar with these daily values I have not found a single person who could answer my question in any quantitative way. Every one, however, has the mental impression that the variation is appreciable or considerable. I have no doubt there are a number of persons who, if pressed for an answer, would put the amount of day-to-day solar variation at the order of 1 or 2 per cent or more. What are the facts? No one can form an independent appraisal of the facts without a fair acquaintance with the major sources of secular and accidental errors. Let me lay some of these before you.

A least-square computation, based on the good and bad observations during the past 18 months, indiscriminately, shows that *the total probable variation due to all causes*, that is, errors and atmospheric causes of variation combined with solar changes, was *one-third of 1 per cent.* If we reject 42 values graded by the observer as unsatisfactory because observing conditions were poor, the total day-to-day variation becomes less than *one-quarter of 1 per cent.* Analysis of the observations has shown that only a *part* of this total, probably not even half of it, can be ascribed to the sun itself. Is there after all any real evidence that the whole of this small quantity is not the inevitable errors of observation? I hope to throw some light on this question later in the present paper.

It is well known that the pyrheliometer is the ultimate standard of reference in all measurements of solar radiation.

Doctor Abbot has stated this matter forcefully on page 89, Volume IV, of the Annals of the Astrophysical Observatory:

> The basis of our research lies in the exactness and stability of its pyrheliometry. We are watching for changes in the radiation of the sun from day to day and from year to year. In doing so we determine the values of the intensity of solar radiation outside of the atmosphere in calories per square centimeter per minute. The accuracy of the comparison depends, however, primarily on the exact comparability over long intervals of our observations at the earth's surface. As the bolometer (which we are obliged to use in order to determine the transmission of the earth's atmosphere) is not a standard instrument for radiation and gives merely relative values, it is necessary to standardize it against some other instrument. For this purpose we have chosen the pyrheliometer.

How great is the need, then, for the utmost constancy in our pyrheliometry.

In what follows we consider only certain sources of errors unavoidable even in pyrheliometers of the highest type. These seem to me to be of such magnitude in themselves that they help to explain both day-to-day as well as secular changes in the derived values of the solar constant. This is especially the case when the day-to-day variations amount to only a few tenths of 1 per cent of the mean intensity. Only a few of the several standard pyrheliometers in use will be discussed.

Pyrheliometers are *absolute* instruments when their indications can be transformed directly into units of thermal energy of radiation received, without comparison or reference to any other type of instrument. *Secondary* pyrheliometers are those instruments whose indications are in arbitrary units and must be reduced to standard thermal units by comparison with absolute or other standardized instruments.

In the class of absolute instruments we must mention the water-flow and water-stir pyrheliometer of the Astrophysical Observatory of the Smithsonian Institution, the electrical compensation pyrheliometer of Ångström, and the silver disk electrical pyrheliometer used by the Weather Bureau since 1912.[3] All these instruments are subject to an uncertain error due to the amount of radiation exchanged between the sky and the sensitive surfaces within the pyrheliometer, which radiation is added algebraically to the direct radiation from the sun. The instruments also require a greater or less correction for the incomplete absorption of the radiation which is transmitted to the sensitive surfaces for measurement.

The sources of error which we select for consideration arise from the sky radiation admitted to the sensitive surfaces, also from losses due to imperfect absorption by the blackened silver disk or by the walls of the water-flow pyrheliometer.

CHARACTERISTICS OF PYRHELIOMETER VESTIBULES

It is impossible to expose the sensitive element of any pyrheliometer to solar radiation alone. A wide area of sky around the sun also adds its radiation partly or wholly to that from the sun's disk. While the intensity of sky radiation is feeble compared with that from the sun, the sky area in some instances is several hundred times the area of the solar disk. Therefore the feeble sky radiation and its variation can not be disregarded if one attaches great importance to changes of small fractions of 1 per cent in the derived values of the solar constant.

The word *vestibule* refers to that part of the pyrheliometer which delimits the admission of radiation to the sensitive measuring apparatus within. This vestibule serves the purpose of well-known collimating devices. The amount of radiation admitted is defined by an outermost and innermost diaphragm, separated a greater or less distance. Intermediate diaphragms are also required, whose function is to cut off interval reflections in the vestibule and to reduce air circulation as much as possible. Figure 1 illustrates diagrammatically the critical elements of the vestibules of certain standard pyrheliometers indicated.

The apparent semidiameter of the sun is slightly more than half a degree. On this account the vestibule of the pyrheliometer must be made to flare to the extent of at least a whole degree or more to allow for imperfect pointing of the vestibule at the sun and for errors in following the solar motion. This flare is provided by making the outer diaphragm a small amount larger than the inner one, but while the vestibule thus arranged admits all the radiation from the full disk of the sun, nevertheless sky radiation from a relatively large area is also admitted.

Before passing to the detailed analysis of the geometric relations, it may be mentioned that the vestibule of the standard type of silver disk pyrheliometer used by the Smithsonian Institution at all its stations prior to 1925 admitted a comparatively wide angle of sky radiation. Since the older type has been used for all observations published prior to 1927 and upon which have been based the claims of solar variability, methods of weather forecasting dependent upon the supposed

[3] Annals of the Astrophysical Observatory, Vols. III and IV; also MONTHLY WEATHER REVIEW 47:798, and 52:302.

variability, etc., it is necessary that we discuss the characteristics of the vestibule of this and other instruments in order to appreciate the difficulties which arise when we try to interpret solar constant values as published. It was probably because of certain anomalous results mentioned hereafter, arising from analysis of the Harqua Hala and Montezuma observations, that action was taken in the latter part of 1925 to lengthen the vestibules of the silver disk pyrheliometer from about 7 to 30 inches, thereby reducing the sky area to about 5.5°.

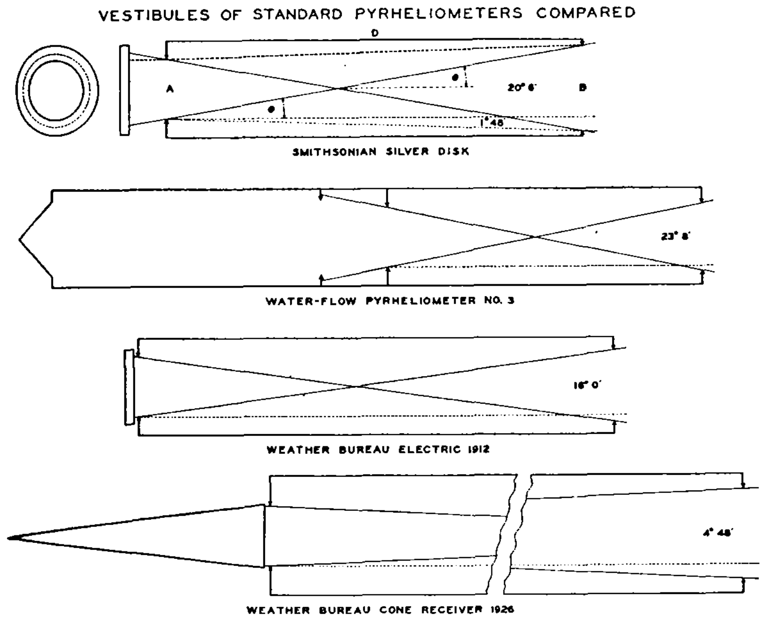

FIG. 1.—Vestibules compared and the geometric angular aperture formulated

Analysis of vestibules.—If we call the diameter of the inner diaphragm of the vestibule A, the diameter of the outer diaphragm B, and the distance between the diaphragms D, it is easy to see from the construction lines of the vestibule of the silver disk pyrheliometer in Figure 1 that θ, the half angle of the aperture of the vestibule, is found from the equation $\tan\ \theta = \frac{A+B}{2\ D}$.

There is some difference of view as to how the angular aperture of any particular vestibule should be calculated. It is true that no single point of the sensitive surface receives radiation from the entire sky area. Nevertheless, all that solar and sky radiation which passes through the vestibule of the pyrheliometer must impinge somewhere upon the sensitive surface and exert its influence upon the measurements. In a recent discussion of this question Doctor Abbot demonstrated that the effective area equivalent to the geometric angle given above is approximately the angle given by the expression $\tan\ \theta = \frac{B}{D}$, which is about half the geometric angle and is the appropriate angle to use in evaluating the effects of sky exposure of different pyrheliometers. If, as in some pyrheliometers, notably the Ångström, the sensitive surfaces do not intercept all the solar and sky radiation which passes through the vestibule, then the incoming radiation which passes by the edges of the receiver is entrapped within the inclosure, and the general effects thus produced upon the measurements are likely to be more harmful than if all the radiation had been intercepted.

Figure 2 is a graphic picture of what the myriad of points on the receiving surface of various pyrheliometers "see" when the instrument is pointed centrally toward the sun.

The detailed analysis of conditions in the case of the well-known silver disk pyrheliometer is shown in the upper left-hand corner of the diagram by the large circle whose outer limit represents the outermost points of visible sky from which radiation can pass through the vestibule of the instrument. The sun's disk, drawn to the same scale, will occupy a tiny area in the center of the outer circle.

The vestibule of this instrument admits a solid cone of radiation having a geometric aperture of 20° 6′ = 1,206 minutes of arc. The solar disk subtends an angle of about 32.6′ of arc. The total area of the sky which radiates to the receiver is, therefore, one thousand three hundred eighty-six times the area of the solar disk.

A single point at the center of the silver disk sees only the sky radiation within the circle *c c c* with the sun exactly in its center, assuming the instrument to be pointed centrally at the sun. This circle subtends an angle of 10° 38′. Points on the disk which are eccentric in position see about the same angular area, but this is composed partly of other portions of the sky than those seen from the central point, and the sun is eccentric therein as indicated by the circle *x*.

The sun and sky areas as seen by one or more points near the extreme edge of the illuminated portion of the disk are represented by circles such as *y y y*, etc. In these circles the sun is just within the edge on one side of each circle. If the disk or receiving surfaces are some distance behind the innermost diaphragm, there will be quite an appreciable annular ring of penumbra and diffraction effects just outside of the fully illuminated central portion of the disk.

In receivers of the black body type, like the water-flow pyrheliometer, many sensitive portions of the surface are located quite a distance behind and outside of the innermost diaphragm of the vestibule. Therefore, portions of the sensitive surfaces will receive radiation from sky areas within which the solar disk does not appear at all. One edge of these circles lies near the sun and extends from it to the outer limits of the whole sky zone admitted. In a very slight degree this condition is true with each of the disk instruments, but is least so when the disk is near the innermost diaphragm of the vestibule.

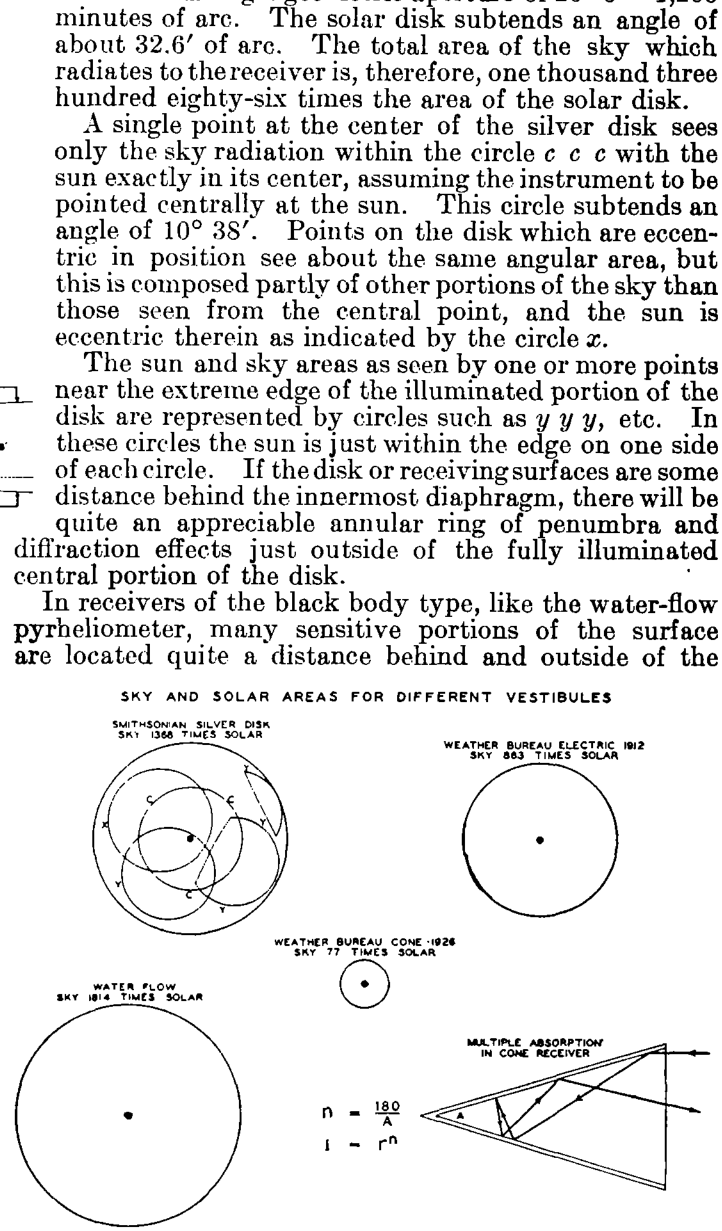

FIG. 2.—Relative geometric areas of sun and sky whose radiations pass through the vestibules of various pyrheliometers

The vestibule characteristics of the pyrheliometers shown in figures 1 and 2 are tabulated below:

Table of approximate characteristics of vestibules of pyrheliometers

Instrument	A	B	D	Angle θ $\tan\theta = \frac{B}{D}$	Ratio of sky solar disk
	Inches	*Inches*	*Inches*	° ′	
Smithsonian Silver Disk	1.020	1.461	7	11 42	492
Smithsonian Vestibule, 1925	1.024	1.850	30	3 50	52
Smithsonian Water Flow, No. 3 [1]	.906	1.142	5	11 10	449
Weather Bureau Electric, 1912	1.475	1.775	11.56	8 44	275
Weather Bureau Cone Receiver, 1926	*.700*	*1.120*	*24.2*	*2 40*	*26*

[1] Corrected for reflecting surfaces inside.

Let us now suppose that two or more of the pyrheliometers whose characteristics we have analyzed are undergoing comparison by being pointed simultaneously at the same sun. In an extreme case one instrument is registering radiation from nearly 20 times as much sky area as the other, and in any case the two instruments are not, strictly speaking, measuring the same physical quantity. If comparisons are made on different days the differences between the quantities measured by the instruments will differ. These amounts may be small, but they can not be disregarded when an ultimate accuracy of a few tenths of 1 per cent is required.

The quantitative amount of sky radiation passing through the vestibules of different instruments can be estimated in some cases from studies like those carried out by Doctor Kimball. These amounts may indeed be quite negligible at stations in very arid regions and at high altitudes overlain with nearly dust-free air. Nevertheless, the general practice has been to compare the secondary pyrheliometers used in the daily work with ultimate standards at some base station located under very different and very far from ideal atmospheric conditions.

The Ångström pyrheliometer has not been included in the foregoing analysis, because the characteristics of the vestibule are very complicated and unsatisfactory.

The physical and electrical principles of the Ångström instrument are excellent, but the design and mechanical construction of the models put out by European manufacturers admit of very great improvement. The circular form of aperture and receiver are far superior to any rectangular form, and should be adopted for the Ångström instrument if possible. The vestibule can then be easily designed to include as small an angle of sky radiation as possible.

However small may be the area of sky radiation permitted to pass through the vestibule, we must not overlook the fact that whatever portion *is* admitted is always the brightest and the most changeable, and therefore the most objectionable portion.

COEFFICIENTS OF ABSORPTION OF PYRHELIOMETERS

Pyrheliometer measurements fail to command our entire confidence, because we know that in general the sensitive surfaces do not absorb more than 96 to 98 per cent of the total radiation which passes through the vestibule. Here again secular and accidental changes in small losses due to this cause must be duly considered when we are striving for an accuracy and constancy of measurement of a few tenths of 1 per cent.

In order that we may clearly follow a short analysis of the "lack of blackness" of the receiving surfaces of pyrheliometers, we must have in mind the exact meaning of certain words we use.

A surface is perfectly black, as this word is here used, only when it absorbs all the radiation which impinges upon it at any angle. Unfortunately no such surface is available. The surfaces we are compelled to use are, therefore, to be thought of as partial, that is, imperfect or poor, reflectors. We must now think of two kinds of reflection, the diffuse and the specular.

A sheet of pure white uncalendered paper is a good example of a nearly perfect diffuse reflector, because radiation, at least visible radiation, falling upon it from any single direction is nearly all reflected away again, but equally in *all* directions. Such paper is said to have a matte surface, as distinguished from a glossy or specular surface. A mirror, on the other hand, is a nearly perfect specular reflector, because a single ray of light falling upon its surface is nearly totally reflected at the one angle which is equal to the angle of incidence. The diffuse reflection in this case is practically nil.

In pyrheliometry we are limited to the use of surfaces which satisfy only imperfectly the definitions we have given. No surface we employ is perfectly "*black*." Some reflection always occurs, amounting to at least 2 to 4 per cent. With a matte surface, as for example one evenly coated with soot, nearly all of this 2 to 4 per cent will be reflected diffusely, some of it out through the opening of the vestibule. With another kind of surface, for example a glossy enamel black one, nearly all the reflection will be specular.

Now, there is only one way by which this reflected radiation can be entrapped with a loss of less than one-thousandth part of the radiation transmitted through the vestibule. This requires that the absorbing surfaces be not diffuse reflectors, as in all ordinary practice, but specular reflectors, for the reasons explained in what follows.

(1) *The "black body."*—A hollow chamber with a blackened interior and a vestibule upon one side for admission of radiation is widely known as a "black body." The use of this in pyrheliometry is exemplified by Doctor Abbot's waterflow pyrheliometer, whose vestibule characteristics we have already analyzed.

The widespread general assumption is that any "black body box" absorbs practically 100 per cent of incoming radiation. This assumption, however, is partly fallacious, and calls for the correction brought out in the following analysis.

The mistake arises from not taking proper account of the difference between specular and diffuse reflection, and also because the universal practice seems to be simply to blacken the inner walls of the chamber dead black with lampblack mixed in alcohol, with a little shellac added to cause the lampblack to stick. These are the standard instructions for producing matte black surfaces.

The prevalent impression is that the solar and sky radiations which have passed through the vestibule of the black body, if not absorbed at first incidence, will be reflected to and fro within the chamber and will be at length almost completely absorbed somewhere upon the walls.

Disregarding the sky radiation as a negligible factor in the present connection, let us say that of the whole solar beam, wherever it falls within the blackened chamber, 96 per cent is at once absorbed on the first incidence. As the walls are matte surfaces, 4 per cent of the solar beam will be diffusely reflected from every point, and a part of this will at once pass right out through the vestibule; there is no chance for it to be reflected to and fro. Of course, not all of the reflected 4 per cent can pass out through the vestibule, but the loss is appreciable and can not be disregarded. The result is quite different if the inner walls are blackened with a thin, glossy, black enamel which *reflects specularly*. The effects are best exemplified in the cone receivers, which will now be discussed.

(2) *Narrow cone receivers.*—If the whole solar beam is received in a cone-shaped sensitive chamber the multiple absorptions which result from to-and-fro reflections are realized in the highest degree and the losses by diffuse and final reflection can be made vanishingly small. In fact, the black body inclosure, in pyrheliometry at least, is largely unnecessary, for in the new type of receiver we are successful in absorbing even more than 999 parts in 1,000.

The high effective absorption that can be realized by the use of cones with specularly reflecting walls, as also the difference between specular and diffuse reflection in such cases, was discussed long ago by Dr. Charles Mendenhall.[4] I am not aware, however, that proper advantage has been taken of these principles in the construction of pyrheliometers.

The number of reflections of a beam of parallel rays entering a cone of angle A will be $n=\frac{180°}{A}$ and the intensity of the beam after the last reflection will be $I=r^n$, in which r is the coefficient of reflection. As we are now interested in absorption we can write the equation for effective absorption

$$A=1-r^n.$$

This equation gives the effective absorption on the basis that the coefficient r is pure specular reflection and that the matte reflection is zero. Some matte reflection is inevitable. Any diffuse reflection will tend to nullify the advantages of the multiple reflections. The ultimate result is to set a limit upon the narrowness of the cone we need to choose in order to realize the maximum attainable effective absorption.

The physical data upon which this question can be settled for the kinds of surfaces we must use are not now available. but the Weather Bureau is planning, with the cooperation of the Bureau of Standards, to secure the necessary observations.

The ultimate standard of all solar constant values up to the present time rests upon a considerable number of intercomparisons of various silver disk pyrheliometers with duplicate standard water-flow pyrheliometers of the kind we have mentioned. Intercomparisons were made in Washington and at Mount Wilson.

In addition to the known unsatisfactory sky conditions at Washington, the 40 comparisons made there were in a beam of reflected radiation and the losses due to reflection had to be evaluated by a separate set of pyrheliometer readings, thus increasing the sources of error and variability.

The 32 comparisons at Mount Wilson were made under better sky conditions and in a direct solar beam followed by means of an equatorial mounting. The 72 observations were discussed in six groups. Of the results Doctor Abbot writes:

> The maximum divergence of the mean results of these groups is 1 per cent. Hence it is believed that the mean result of all the comparisons made under such diverse circumstances must be within 0.5 per cent of the truth. The probable error is 0.1 per cent. It is believed that this standard scale is reproducible by the secondary pyrheliometers with the adopted constants given to within 0.5 per cent. The divergence of this scale from that of Ångström appears to be 3.9 per cent.

Ångström in October, 1919, fixed his value at 3.23 per cent lower than Abbot's.[5]

The unexplained divergence of 3 to 4 per cent between the normal pyrheliometers by Abbot and Ångström indicates that further research is necessary before a final definitive standard instrument is realized.

These citations refer to the outstanding errors of the pyrheliometer as an absolute instrument, and depend upon the mean results of a considerable number of comparative readings.

Our chief concern in this analysis, however, is not with the mean error of large groups but with the magnitude of the probable changes of scale of any given instrument in its daily use, due to variations of sky brightness, secular changes incident to age, changes of absorption, occasional changes of instruments, changes of observers, etc. These fluctuations are necessarily greater than the fluctuations of group means.

It must be remembered also that before we get a derived value of the solar constant for any particular day there must be added to the foregoing causes of variation all those entailed by the use of the bolograph, which include empirical corrections for losses of radiation at the reflecting surfaces of mirrors, prism absorptions both partial and complete, the empirical factors for interpretation of pyranometer readings, and other entirely terrestrial causes of fluctuations in the final values of the solar constant.

Changes in the final derived values of the solar constant, due to all the foregoing causes, must certainly exist and obviously should not be ascribed to solar origin, especially when very careful statistical analysis of past observations over a number of years shows that the total variation due to possible solar changes and terrestrial fluctuations combined is less than one-half of 1 per cent in the case of the best observations. The smaller this quantity becomes the greater is the probability that all of it is caused by terrestrial and instrumental effects, because these never can be reduced to zero.

Attention has been called in the foregoing to certain physical sources of error and variability in measurements of solar radiation, which must of course be considered in any effort to interpret critically the significance of such observations. It still remains to consider a final highly important matter, namely:

INDEPENDENT MULTIPLE STATIONS

Observations at separate stations must be kept absolutely independent of each other.

Seemingly, without at first fully realizing the importance of this requirement, methods were employed to harmonize and reduce to the same scale observations made at approximately the same time at separate stations. However, the result proved so unsatisfactory that a new computation and reduction of the entire body of original observations was undertaken. The difficulty is easily understood, for this is a question of statistical relationship, hence adjustments of individual observations from separate stations by intercomparisons necessarily tends to introduce artificial correlation between them.

[4] Mendenhall, C. E., On the Emissive Power of Wedge-shaped Cavities and Their Use in Temperature Measurements. Astrophys. Jour., vol. 33, No. 2, March, 1911.

[5] Annals Astrophysical Observatory, vol. 3, p. 72, MONTHLY WEATHER REVIEW November, 1919, 47: 799.

All methods thus far devised for deriving a value of the solar constant from fundamental observations inherently introduce errors peculiar to the particular station, its instrumental equipment, the daily atmospheric condition, and the seasonal march of the weather elements. That these influences operate is shown by the well-known correlation between solar constant values and the atmospheric transmission, which is, for well-known reasons, nearly always considerable and negative, whereas it should be zero. A climatic effect of a 12-month period, also found where none should appear, is further proof of the presence of the errors mentioned.

If it is ever to be possible to evaluate these instrumental and terrestrial causes of fluctuation and to disentangle from them possible solar changes, multiple station reports must be separately reduced and published as independent values. Inherent errors, certain to be present, can be satisfactorily evaluated and eliminated, if at all, only by analyzing a long series of such independent observations.

A rigorous scientific procedure would seem to require that the observations at wholly independent stations shall be reduced by methods and by the application of corrections, coefficients, factors, etc., *based upon purely physical observations at the one station*, just as if no other station in the world existed.

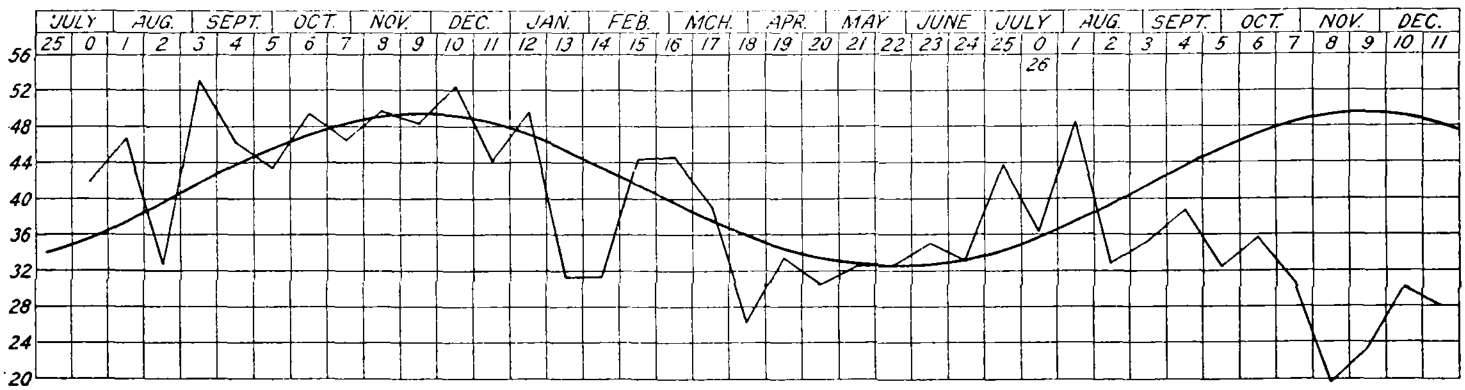

FIG. 3.—Fourteen-day mean values of provisional solar constants as published from July, 1925, to December, 1926. Smooth curve is the seasonal or 12-month period (a terrestrial effect) deduced from five years of earlier observations. Nearly all the 14-day mean values could have been forecast from this seasonal curve with few failures and more than a year in advance. The values for October, November, and December are conspicuously discordant.

Inconsistencies between independent stations must be expected in the present stage of the art, and may well be regarded as a mark of the independent origin of the data. We need to know the magnitude of the discrepancies which would be disclosed by bringing together the derived values of the solar constants from several stations whose observations are truly independent of one another.

The investigator wants the fundamental original observations. The pyrheliometer readings and the bolographic solar constant secured by the pure Langley method, and wholly independent of measurements made at any other station, are the real original observations.

INTERPRETATION OF OBSERVATIONS

So much has been published in this Review [6] on the analysis of solar constant values that only the major conclusions need be summarized here.

Day to day or short period variability.—Critical examination of all observations published up to November of 1924 indicates:

(1) The total variability of solar constant values from a single high-grade station is well under one-half of 1 per cent. If not all of this is of terrestrial origin, the solar part can be evaluated only by a least square analysis of comparable observations from two or more stations.

(2) All observations consistently show a well-defined climatic effect in the form of a 12-month period.

(3) The annual periodicity in the latest Montezuma values is verified by the fit of these values to the 5-year smooth normal curve as shown in Figure 3. The zigzag line connects 14-day mean values of solar constants from July 16, 1925, to December, 1926. These are the latest values published. With the exception of the values for October, November, and December, 1926, the conformity to the seasonal period deduced from the 5-year means is striking. For some unknown reason the low October, November, and December values are inconsistent, not alone with corresponding values for the preceding six years, but with the view that we should expect high values of the solar constant at the present time of sun-spot maximum.

Long period or secular changes.—Data for the analysis of slow, progressive changes over years, sun-spot periods, etc., as yet are very meager.

The observations at Mount Wilson from 1905 to 1920 were made only during summer months. Two sets of values were derived from the same original parent observations, which latter represent the results derived by the rigorous Langley method and are hereinafter designated E_0. The second set is obtained from the first by the application of a water vapor correction, and are hereinafter designated E_0'.

Assuming now that all derived values fluctuate from day to day, due to combined solar and terrestrial causes, it is plain that any correction for terrestrial errors will be valid only when it reduces the total variability of the parent data.

Since the corrected data show the same variability as the parent data, they can not claim to be superior. More conclusive proof of the inferiority of the corrected values is shown by the amplitude of the 12-month period in the corrected data, which is nearly double that in the parent data. The amplitude would probably be nearly zero if the values were true solar constants.

Of the parent data published for Mount Wilson (E_0) and the corrected values (E_0'), the latter are the only values used in discussions by both Doctor Abbot and Mr. Clayton, notwithstanding that the internal evidence cited above indicates that the errors are probably least for the pure bolographic values.

Whether we include or exclude the observations known to be impaired by Katmai dust in 1912 and 1913, the true mean solar constant from all observations at Mount

[6] MONTHLY WEATHER REVIEW, 1925, 53: July, 285-306; August, 343-348; December, 519-528.

Wilson is $E_0=1.912$, and the $E_0'=1.933$, which latter is artificially in excess of the former by 1.1 per cent. These mean values are both derived by the harmonic analysis. which is the best, if not the only, way to compute a true mean of a periodic function when values for part of the cycle are missing, and are therefore probably the strongest and most definitive values which can be drawn from the Mount Wilson work.

It was not known at the time observations began at Calama on July 27, 1918, that the past as well as the future values of the solar constant would exhibit the seasonal feature of a 12-month period. Moreover, little consideration seems to have been given to the important prerequisite that values at separate stations must be kept wholly independent.

What is found in the published results is that the pure bolographic values at Calama under winter conditions began in close agreement with the high summer values, E_0', at Mount Wilson. If the values at both stations were influenced by no other error than the annual periodicity, then it must follow that the simultaneous summer and winter values in the two hemispheres should differ by the sum of the amplitudes of the two periodicities, because later computations proved these periods to be in opposite phase relations with a maximum difference of about 0.016 calories, or 0.8 per cent. The agreement between the summer values at the two stations seems therefore to indicate that the Southern Hemisphere observations started upon a scale in accord with the E_0' values at Mount Wilson, which were then regarded as the most dependable.

This condition had the very harmful effect of starting the observations at the Southern Hemisphere station upon a scale of values which was too high, first because the summer and fall values at Mount Wilson were running near the maximum phase, due to the annual period with a large amplitude; second, because the E'_0 values are themselves probably a full 1 per cent too high, due to the application of a water vapor correction of doubtful validity.

The writer is compelled to emphasize the doubtful accuracy of the high values of the pure bolographic observations as published for Calama and Montezuma up to January, 1922. It is very doubtful, indeed, in the writer's mind, at least, whether absolutely independent pure bolographic observations would ever have given these high values had the station at Mount Wilson never existed.

There is no physical basis by which an artificial scale can be preserved. When the station at Mount Wilson was discontinued in favor of a new station at Harqua Hala, and the station at Calama was moved to Montezuma in 1920, the latter became and still remains the primary observatory. In the course of a year or two thereafter direct physical verification of the mirror, prism, and other corrections at the station would have to be made in order to maintain the accuracy of the instrumental constants and corrections required by the pure bolographic method. Thus the values at Montezuma would tend to revert to the magnitude of values at a wholly independent station—that is, to a scale similar to the original pure bolographic values, E_0, at Mount Wilson when that station was a wholly independent one.

The writer, unfortunately, has not had access to original data and observations by which to establish the correctness of the deductions stated above, but we do know that the published solar constants fell off in 1922 to unprecedented low values, which have averaged closely the same up to the present time. These values were only apparently low. They could not be explained in relation to the former high values, E'_0, except to misinterpret them as evidence of a marked decline in solar radiation. The fact is, these low values agree very well with the former pure bolographic values, E_0, obtained at Mount Wilson, whose average was 1.912.

It is plain from the adjustments described above that artificial changes run through the solar constant values since 1918. Moreover, the final values are not yet available, since many of the observations are now undergoing recomputation. This will doubtless produce some homogeneity, but it is not obvious how it will ever be possible to eliminate from past observations the correlations due to intercomparisons between nonindependent stations.

CONCLUSIONS

Langley's pure bolographic process for evaluating the transmission losses of radiation in the earth's atmosphere still stands alone and supreme, provided the observations at any one station are reckoned in a manner that makes them completely independent of those at any other station.

Nevertheless, while frequent daily observations by the full Langley or long method furnishes very valuable data, these are now no longer required for the pursuit of practical investigations in solar and terrestrial relationships. For reasons already given by me,[7] pyrheliometer readings alone at high-grade stations serve nearly all purposes, especially if supplemented by simple meterological observations, all of which are very easily made.

What meteorologists and other investigators need is simply the prompt release of the original basic observations. These are not subject to any change except for occasional *errata* or possible material deterioration in instrumental constants. We are glad to point out that a step in the desired direction was taken in 1926 when the Smithsonian Institution arranged for the advance publication by the Weather Bureau of the Montezuma Pyrheliometry, 1920–1926.[8]

There are still needed, however, similar and additional prompt reports from all existing stations. The writer has proposed that arrangements be made for the prompt publication of some such original observations from each station, as follows:

(*a*) Pyrheliometer observations whenever possible at three or more different air masses.

(*b*) Psychrometer readings or equivalent measures of local humidity, including air pressure and state of sky with respect to haze, clouds, etc.

(*c*) Some evaluation from one or more bolograms of the area, or other quantitative measure in comparable units, of the major water vapor absorption bands in the solar spectrum.

It is earnestly hoped some such program of prompt publication of actual original observations may soon be realized.

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[7] MONTHLY WEATHER REVIEW, July, 1925, p. 290, Caption II.
[8] Supplement No. 27, MONTHLY WEATHER REVIEW, 1926.