

lands has gone on at the present rate. The phenomenon has been approximately the same for 6,000 years, but during the Bronze Age and just after, it was possibly slower. The more ancient phenomena are difficult to discuss, as a damming up of the Baltic outlet would produce results similar to actual land elevation.—*W. A. Richardson*.

#### THE WAVE-RAISING POWER OF NORTHWEST AND SOUTH WINDS COMPARED.<sup>1</sup>

I recall that sailors on the Great Lakes have claimed that a wind of a given velocity in winter caused a higher

<sup>1</sup> Cf. February, 1920, issue MONTHLY WEATHER REVIEW, pp. 100-101.

#### DISCREPANCIES BETWEEN ÅNGSTRÖM AND SMITHSONIAN INSTRUMENTS.

By C. G. ABBOT, Director, Astrophysical Observatory.

[*Smithsonian Institution, Washington, May 3, 1920.*]

In the issue of the MONTHLY WEATHER REVIEW for November, 1919, Dr. A. K. Ångström has three papers of great interest. In one paper he gives comparisons which must be highly gratifying to all those who are interested in the constancy of the scale of radiation measurements. He shows that in the seven years, 1912 to 1919, there had occurred no appreciable change in the Ångström and Smithsonian scales relatively to each other. During this interval Smithsonian observers have made several unpublished comparisons against the Standard water-flow pyrheliometer No. 3, which also supported the constancy of the Smithsonian scale with very satisfactory accuracy of experimentation. Thus we may be sure, it seems to me, that no change in the scales on which pyrheliometric and spectrobolometric measurements have been made for many years has occurred so large as 1 per cent.

Dr. Ångström finds the Smithsonian scale to be 3.2 per cent above the Ångström scale. Of this discrepancy, he admits that 1.8 per cent is due to the two small sources of error which he discussed in a former publication. The other 1.5 per cent he is inclined to throw upon the Smithsonian scale.

In regard to this latter suggestion, I am only able to say as was said in Volume III of the *Annals*: "The system which we call 'Smithsonian Revised Pyrheliometry of 1913' rests on 72 comparisons on 20 different days of 3 different years with 3 standard pyrheliometers of different dimensions and 2 widely different principles of measurement, all capable of recovering and measuring within 1 per cent test quantities of heat, and all closely approximating to the 'absolutely black body.'" The 72 comparisons, 40 at Washington, 32 at Mount Wilson, were made in 6 groups. 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."

In Volume III of the *Annals* the determination of the constants of the Standard pyrheliometers Nos. 2, 3, and 4, and the comparisons which have been made with them, are given with great detail from pages 55 to 72, so that readers will be able to see for themselves at every step how far the claim just quoted is justified.

It appears to me that before we can be warranted in admitting Dr. Ångström's suggestion that the Smithsonian scale is 1.5 per cent in error because it exhibits

sea than a wind of the same velocity in summer. They attributed this to the fact that in summer the relatively cold water of the Lakes reduced the temperature of the surface air layers, producing a temperature inversion. As a result, a wind movement in the upper air layers, which might be strong at the height of the masthead, would be light at the surface of the water. In winter, on the contrary, the air is generally colder than the water of the Lakes, the air movement is felt down to the surface and causes high seas.

Perhaps a similar explanation may apply to the difference in wave-raising power of northwest and south winds, since in the northern hemisphere the former are apt to be the colder.—*H. H. Kimball*.

that degree of divergence from the corrected Ångström scale, we ought to have equally full details of measurements and comparisons on which the Ångström scale and comparison between it and the Smithsonian scale rest.

Especially I would call attention to these points:

1. Since the electrical resistance of the Ångström strips in the standard instruments is measured by a potentiometer device between points of known distance apart it would be possible, by making the Wheatstone's bridge measurement of the actual resistance between the terminals of the Ångström strips, to determine the actual distance through which the heating of the strip occurred rather than to make an estimation with regard to that distance, as was done by Dr. Ångström in his experiments which led him to the correction of 1.3 per cent.<sup>1</sup> This is very important, for he will agree that the mathematical theory of the subject shows that if the difference in length between the sun-heated and electrically-heated portions of the strip should be above his estimate of it the magnitude of the correction would very rapidly grow.

2. Since the width of the strip is only 2 mm., accuracy to 0.5 per cent demands that the width should be known to within 0.01 mm. In view of the presence of the particles of platinum black and of soot required for blackening the strips, is it possible to define the edges of the strips to within this degree of accuracy? Dr. Knut Ångström,<sup>2</sup> the distinguished inventor of the instrument, states with regard to this point: "Since the coating with lampblack leaves the edges a trifle rough, an error of 0.01 mm. in measures of the width evidently can not be avoided, which in the width of the strips here used may make an error of 0.5 per cent in the final value."

3. Although the measurements of Kurlbaum indicate that the effect of introducing the heat at the front of the strip when heated by the sun, as against introducing it through the body of the strip when heated by the current, produces but a small amount of error, is it quite certain that the blackening Dr. Kurlbaum experimented with is so nearly similar to the blackening of the Ångström strips that this correction is as small for the Ångström pyrheliometer as for the Kurlbaum metal foil? Dr. Ångström's computations lead him to admit 0.5 per cent for this effect. But the magnitude of it must depend on the intimacy of contact between each individual strip and its blackening. Is this known to be uniform and that negligible opposition to the flow of heat occurs

<sup>1</sup> *Astroph. Jour.*, vol. 40, p. 279. It is by no means certain that the ends of the strips electrically were at the edges of the pole pieces visually.

<sup>2</sup> *Astroph. Jour.*, vol. 9, p. 336.

at the boundary? In short, is it not necessary to determine the error for the strips of the actual instruments which serve to fix the Ångström scale, when accuracy to a few tenths of 1 per cent is claimed for that scale?

4. Although K. Ångström, Coblenz, and Royds have determined the reflecting power of certain surfaces when blackened in certain ways, is it sure that the blackening of the strips of the actual instrument on which the Ångström scale depends is so exactly similar to the blackening of their surfaces that the correction for reflection is identically the same? Dr. Knut Ångström<sup>3</sup> gives results of his determinations of this correction and then states: "If we take the coefficient of absorption of these surfaces as constant for different wave lengths and equal to 98.5 per cent then we make at most an error of 0.5 per cent in the determination of the intensity of radiation."

It appears to me that until all of these points are settled to the same degree of certainty and published with the same degree of fullness with which the Smithsonian scale has been set forth, it is not justifiable to make such a suggestion as Dr. Ångström has offered.

At the same time, I am not disposed to claim perfection for the Smithsonian scale, and if equal weight can be thrown upon any other determination which indicates a different scale, I am quite willing to divide the discrepancy equally between the two, but I must require that the weights of the two determinations be equal before such equal division of the discrepancy can be made. Dr. Ångström, however, after correcting his scale to his best knowledge, gives it infinite weight rather than equal weight to the Smithsonian scale in dividing the discrepancy between them.

In another paper, Dr. Ångström gives results of comparisons between his instrument for measuring sky radiation and a copy of the pyranometer purchased by the Weather Bureau from the Smithsonian Institution. He states that the average result of the comparisons showed agreement within 2 per cent, but that some of the comparisons differed by as much as 6 per cent, and he is inclined to throw this larger divergence upon a certain source of error in the pyranometer. This source of error is the one which was pointed out at length in a paper entitled "On the Use of the Pyranometer,"<sup>4</sup> by C. G. Abbot and L. B. Aldrich.

The nature of the error is this: When exposed to daylight sky, the nickel-plated cover absorbs about 30 per cent of the solar rays which meet it, and thereby is warmed and warms the glass hemisphere below it by air convection. When the nickel cover is removed the glass can cool rapidly by long wave-length radiation to the colder atmosphere and to space. As the glass is almost completely transparent to solar rays, it is hardly warmed at all by them. Hence the glass, after exposure, grows cooler than before, and as it subtends a full hemisphere, it tends strongly to reduce the temperature of the blackened strips below, thus causing the gradual decrease of the galvanometer deflection due to opening the instrument to the skylight.

This source of error varies in its influence from place to place, depending upon how strong is that which we call "nocturnal radiation," and this, in turn, depends upon the quantity of water vapor prevailing and the temperature of the air immediately above the instrument. At Washington, and in general in moist climates, and

especially in summer, the outgoing radiation is small,<sup>5</sup> so that the error tends to be small also. At a high-level station, like Mount Wilson or Mount Whitney, the magnitude of the disturbing cause is considerably larger.

We attempted to do away as much as possible with this source of error by introducing a reading of the galvanometer by the method of first swing. The galvanometer which we were accustomed to use had a time of single swing of 4 seconds. In this short interval we believed that the inner surface of the glass hemisphere could hardly change temperature sufficiently to produce any influence on the strips. The question now at issue is whether we were misled in this belief to the extent that errors of 5 or 6 per cent might have arisen in Dr. Ångström's comparisons, made in August in Washington.

In order to test the matter, Mr. Aldrich and I have performed the following experiment. We supported a large-bottomed teakettle, partly filled with ice water, at a short distance centrally above the pyranometer. Between it and the pyranometer we introduced an asbestos screen. Immediately beneath it was attached a nitrogen-filled tungsten lamp. When this lamp was lighted the intensity of radiation which it furnished to the pyranometer was found to be 0.20 calories per square centimeter per minute. When the screen and lamp were swung aside, the lamp extinguished, and the glass removed from the pyranometer, we found that the outgoing radiation from the pyranometer to the cold bottom of the teakettle was 0.097 calories per square centimeter per minute.

Having placed the screen and lamp in position above the pyranometer and lighted the lamp, we waited until the galvanometer had reached a perfectly constant zero. We then suddenly withdrew the screen and lamp, extinguishing the lamp, so that the room was in darkness, and as quickly as possible, within 2 or 3 seconds, opened the shutter, so that the glass of the pyranometer was exposed to the cold bottom of the teakettle. In this way we reproduced very approximately the conditions which we have just described when measurements are made upon the sky, but with the difference that after the exposure of the glass no short wave-length radiation came through it to disturb the galvanometer. But the cooling of the glass would go on quite as fast under these circumstances as if it had been actually exposed to a negative skyward radiation stream of intensity of 0.10 calories per square centimeter per minute, after having been for a long time under the influence of incoming sky radiation of the intensity of 0.20 calories per square centimeter per minute.

As a result we found that in the first 5 seconds after exposure of the glass to the cold teakettle, a deflection averaging in three experiments 0.9 millimeter was produced, which on being compared with the deflections due to the introduction of current into the pyranometer strip proved to correspond to 0.0014 calories per square centimeter per minute. In other words, for a galvanometer whose time of first swing is 5 seconds, the error to which Dr. Ångström calls attention would be of the magnitude of 0.0014 calories per square centimeter per minute, which would be of about the order of 1 per cent compared to the sky radiation which would be observed. The error could be very materially reduced—probably to a third or a fifth of these dimensions—by using a galvanometer of only 3 seconds single swing, because the plotted curves of the observations show that the change of temperature due to the cooling of the glass did not set in in the pyranometer

<sup>3</sup> *Astroph. Jour.*, vol. 9, p. 339.

<sup>4</sup> *Smith. Misc. Coll.*, vol. 66, No. 11.

<sup>5</sup> Smaller, of course, by day than by night.

strips till one or two seconds after the exposure had been made. If, however, the time of swing of the galvanometer was larger than 5 seconds—10 seconds, for instance—the curves show that the error would increase nearly proportionally to the time of swing of the galvanometer.

I am not aware what was the time of swing of the galvanometer which Dr. Ångström employed in his comparisons, but I suppose it to have been of the order of 3 or 4

seconds, which is that which we customarily employ in pyranometer observations. If this is the case, I am of the opinion that it is quite impossible that error of the order of 5 or 6 per cent, such as he calls attention to, could have been due to this source. The tendency of the error is, of course, to make our instrument read too low. Dr. Ångström does not say in what direction the discrepancy between the two instrument lies.

#### FORECASTING THE WEATHER ON SHORT-PERIOD SOLAR VARIATIONS.

By CHARLES F. MARVIN, Chief U. S. Weather Bureau.

[Washington, D. C., April 4, 1920.]

In the remarkable paper<sup>1</sup> cited below, Mr. Clayton claims he has established important relations between high and low values of the day-to-day intensities of solar radiation,  $E_0'$ , as measured by the Smithsonian Institution, chiefly at Mount Wilson, Calif., and Calama, Chile, and the values of the mean temperature at Buenos Aires. By means of these relations he claims material improvements in forecasting the weather are made possible. These investigations are an extension of earlier studies by which this author<sup>2</sup> endeavored to show that the whole earth responds in a complex but definite manner to the small changes of a few per cent in the reduced values of solar radiation as measured at Mount Wilson, Calif.

The forecasting value and possibilities of knowledge such as Mr. Clayton claims to have disclosed is obviously very great and important *provided his claims are true*. The writer, however, quickly became firmly convinced, purely from basic principles, that Mr. Clayton, who seems to regard the day-to-day changes in the observed values of solar intensity are mostly of solar origin, is quite in error. Indeed, great harm is being done to the cause of weather forecasting and the real progress of science by the wide dissemination of unrefuted representations of this character.

The whole matter seems to the writer to be a case of the seemingly complete disregard in the discussion of data of the material errors of observations and of the laws and operations of chance. Such a course has necessarily resulted in a grave misinterpretation of an excellent mass of observational data. Urged by these convictions, the writer has endeavored to evaluate, if possible, the unavoidable random and partially known dominant errors of measurements of solar radiation. This study was approached with a full belief in some solar variability. The results, however, unequivocally show that the observed changes in day-to-day values of radiation are very largely due to the aggregate of all the unavoidable sources of error of determination, all wholly terrestrial. The possible frequent and irregular variations of solar intensity from day to day or over an interval of a few days must be quantitatively such a small fraction of 1 per cent that it can not be satisfactorily evaluated from the existing data even including those now being secured by the new pyranometric method of observation. Such variations, if any actually occur, must be so small as to be quite inconsequential as a controlling factor of the weather and temperature of to-morrow or the next few days at any particular locality.

The only question the writer discusses in this paper is the *changes* of intensity from *day-to-day* or from some daily value of intensity to the next daily value observed a few days later. These are the variations in observed

values which Mr. Clayton has used as the basis of correlation between solar intensities and temperature changes at Buenos Aires.

The writer particularly desires to avoid making any statement either for or against slow long-period solar changes, that is, changes over a few weeks, months, seasons or years, for example. He distinctly desires to leave open the question of regular or irregular changes of this character. The manner in which terrestrial weather responds to such changes can not be intelligently discussed until such changes have been conclusively shown to occur and been at least fairly evaluated in amount. An investigation with this object in view is also in progress.

The real question now at issue is simply the *variability* of daily or quite frequent *observed* values of solar intensity outside the earth's atmosphere, and how much, if any, of this variation is caused by true solar changes and how much caused by errors of measurement and varying depletions of large masses of the atmosphere which transmit all incoming radiation before measurement of its intensity.

Seemingly, one of the most direct, if not the best methods of solving such a problem consists of a critical evaluation, by means of well-known statistical methods, of the *variation* of the observational data of which an excellent body of over 1,500 frequent values of intensity is now available.

Within the past few weeks the writer has made a somewhat hasty preliminary review of these data, and it seems proper to briefly mention in this preliminary note certain important facts which seem to stand out unequivocally.

(1) The frequency distribution of the data is nearly Gaussian, that is, it nearly conforms to the normal error curve of statistics. Therefore, the data may be discussed by the methods of least squares.

(2) The distribution is not entirely elemental, but in this feature it reflects and justifies the composite make-up which the theory of the variations as expressed in equation (1) below calls for.

(3) There is only slight skewness in the distributions, which varies a little in amount and kind (positive or negative) according to the particular group of data analyzed. The evidence from skewness justifies the assertion that for observations at Mt. Wilson, Calif. by the bolographic method and on the average, *changes* in transmission of the atmosphere during observations tend to give a preponderance of slightly too low values of intensity and correspondingly too high values of the coefficient of atmospheric transmission.

(4) *Changes* of transmission during observations also cause greater scattering and dispersion of values than would otherwise occur, thus imposing upon the data many false variations due entirely to atmospheric, not solar, causes.

<sup>1</sup> Variation in Solar Radiation and the Weather, by H. Helm Clayton. Published simultaneously in Spanish in the Boletín Mensual Oficina Meteorológica Argentina, and in English in Smithsonian Miscellaneous Collections, vol. 71, No. 3.

<sup>2</sup> Smithsonian Miscellaneous Collections, vol. 68, No. 3.