would seem to be "impossible"; moreover, such compounds would be chemically neutral, assuming hydrogen to lose all affinity when it unites with carbon. In the model before you, as in that of benzene, the unit of affinity and, therefore, the hydrogen atom is a regular dodecahedron. Four such dodecahedra arranged in a pyramid represent the carbon unit: consequently, the dodecahedral units are distributed in two layers. The hydrogen atoms, in the fatty acids, are all brought down, as they are in the model of benzene, into these two layers: therefore, an upper and a lower face in the model are free carbon surfaces. The models can, in consequence, be superimposed at these faces and also interlocked at the sides. Verily, we have done well to use the name *paraffin*, not *sinaffin*. They can be joined, in the same plane, at the carboxylic ends and at the sides. To join them at the hydrocarbon ends, the models must be stepped, so as to raise one a layer above the other: this mode of packing is commonly met with in benzene derivatives. The attachment of the hydrogen units is peculiar, different from that adopted in our printed formulae, in that, although they occur in pairs, alternate carbon units carrying each a pair, both are on either one or the other margin of the model. These models, therefore, portray a new method of geometrical analysis, more powerful and intimate perhaps than any yet devised. It is further noteworthy, that although carbon surfaces are exposed, no complete carbon unit of affinity comes to the surface: whereas, in the model of benzene, there are three such affinities on each face.

From the measurements made by Langmuir and by Adam, we are led to conclude that the molecules of a fatty acid spread out upon water in single layers, so arranged that they stand upright, the carboxyl tip dipping into the water, each upright molecule being closely fitted against its neighbours. The measured thickness of the thinnest black soap film is such that it may well be supposed to consist of two such layers, held upon a belt of water very few molecules thick, one standing upon the upper, the other depending from the lower face of the belt. Interpreting the film in this way, there is no reason to believe in the existence of a complex unit or micelle.

I can overlook sixty of the hundred years since Chevreul's invention. When I first studied chemistry, we were not entirely persuaded of the existence of atoms and were only beginning to form clear conceptions of molecular structure. We did not venture even to dream that we should ever be able to measure and speak with a close approach to certainty of the actual distances between atomic centres, which we now rate at little more than an Ångström unit, one hundred-millionth of a millimetre. Well might Chevreul say: "On doit tendre avec effort à l'infallibilité sans y prétendre": We seem to be near reaching it: though we can still make no claim to infallibility, we are probably far nearer to certainty than Chevreul ever thought possible. The methods which he did so much to make known and appreciated are the methods to which progress is due. As a result, the *bougie stéarique* is no mere illuminant to-day but something at which we can greatly marvel. The chemist can see massed in it wondrously built, tall staircases of atoms, up which the imagination may climb to infinite heights, seeing

. . . . successive zones
Of several wonder open on some spirit
Flying secure and glad from heaven to heaven.
Paracelsus.

Does the Solar Heat Stream Vary?[1]

FIVE notable contributions to the literature of the "solar constant" have come recently from the United States: three, published by the Smithsonian Institution, give the evidence for variation in solar radiations and for the influence of that variation on terrestrial weather; the other two, appearing in the *Monthly Weather Review*, the organ of the Weather Bureau of the United States, contain critical analyses of the radiation statistics.

The first of these papers is an apologia by Prof. C. G. Abbot. He realises that critics have not been convinced hitherto that the fluctuations in the Smithsonian determinations of the "solar constant" represent real variations in the radiation received by the earth, and he sets out to marshal the evidence for variation in the most convincing way.

As a preliminary he narrows discussion by throwing over the earlier observations as too rough for the purpose in view. The paragraph is of such importance that it must be quoted in full:

> 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 . . . 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 reduction 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.

It will be seen that it is only the comparatively short series of observations, those since January 1919, made mostly at Harqua Hala in Arizona, and of Mt. Montezumá in Chile, that need be taken seriously as evidence for rapid variations in the solar heat stream. Data for the greater part of this period, August 1920 to November 1924, have been published in a convenient form by the Smithsonian Institution (Misc. Collections, vol. 77, No. 3, Feb. 1925).

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[1] Washington, Smithsonian Miscellaneous Collections, vol. 77 (1925), No. 5: "Solar Variation and Forecasting," by C. G. Abbot; No. 6: "Solar Radiation and Weather or Forecasting Weather from Observations of the Sun," by H. H. Clayton; No. 7: "Solar Radiation and the Weekly Weather Forecast of the Argentine Meteorological Service," by Guillermo Hoxmark. Washington. *Monthly Weather Review*, July 1925. "On the Question of Day-to-day Fluctuations in the Derived Values of the Solar Constant," by C. F. Marvin. *Smithsonian Solar-constant Values*, by W. W. Kimball.

The average value of the solar constant is about 1·945 in terms of the unit in general use for this study, the gram calorie per square centimetre per minute. In other words, if the heat were all absorbed by a layer of water one centimetre deep, the temperature of the water would be raised 1·945° C. per minute.

In his discussion Abbot states that the average daily difference Harqua Hala minus Montezuma is only 0·011 units. This agreement has been attained, however, by an adjustment of the original readings by a process which has not been explained yet in print. It is clear that this process is calculated to minimise discrepancies between the stations.

Knowing the average difference between the approximately simultaneous observations, and assuming tacitly a Gaussian distribution, Abbot finds that the probable error of the daily measurement at either station is 0·0065 units. He proceeds to discuss the deviations from average in the estimates. These deviations are, it is true, of the same order of magnitude as the probable error. In the case of 398 estimates, each based on nearly simultaneous measurements at the two stations, the probable error is $0{\cdot}0065/2^{\frac{1}{2}}$ or 0·0046, and the number of such deviations exceeding 0·0046 is 214. The natural deduction would seem to be that the results show just as much consistency as would be expected if the solar heat stream really were constant and the deviations were due to errors of observation. There are, however, a few cases in which the deviations of the estimated radiation from the average are comparatively large. There are 6 deviations greater than 0·0245 units. With a Gaussian distribution of errors, the proportion of such large deviations would have been only 1 in 4000. Dr. Abbot seizes on this as strong evidence of real fluctuations in solar radiation. He suggests that the contrast between the extreme estimates and the ordinary run is comparable with that between the Washington Monument and the blades of grass around it. Dr. Kimball's remark that the determinations on which the extreme estimates are based are generally in the lowest grade of the Smithsonian classification, and mainly observations from one station only, seems more to the point. By a pair of telling diagrams Kimball shows that such correlation as exists between Montezuma and Harqua Hala values is due, not to any agreement in the variations during short periods, but to the large drop in the solar constant at one epoch. For October 1920 to March 1922 the average estimate at each station was 1·945, and for April 1922 to November 1924 it was 1·922. This contrast naturally dominates the situation when the observations of the four years are treated as a single group.

Thus the direct evidence for day to day variations in solar radiation is decidedly weak. Dr. Abbot supports his case, however, by evidence for an association of changes in radiation with the appearance of sunspots and faculæ on the sun.

As to sunspots, this evidence is given in graphical form by Clayton. During the years 1918–1924 there were large sunspots, and the average values of the solar constant, reckoned for each of the 6 days before a spot passed the central meridian of the sun to 22 days after, fluctuate between 1·944 and 1·936. If the days are numbered − 6 to + 22, 0 being the day the spot was on the meridian, it so happens that days 0, 1, and 12 are on the lower of these limits; days 9, 16, and 22 on the higher. The probable error of a single determination of the "constant" is supposed to be 0·0065 in the years for 1921 onwards. The earlier observations for 1918–1920 had much larger fluctuations. The probable error would average for the whole period at least 0·01. For a mean of 114 determinations the probable error would be about 0·001, so that variations for the mean up to 0·004 are scarcely significant. Moreover, Clayton's figures show no regular sequence. If they are accepted at their face value they indicate that a sunspot causes as violent fluctuations in radiation when it is on the far side of the sun as on the near side. We are forced to the conclusion that the analysis is worthless as evidence for the phenomenon Mr. Clayton wishes to establish, an association between the Smithsonian "solar constant" and sunspots.

Dr. Abbot and his colleagues have not been content to work at the improvement of their sunshine measurements. With the generous aid of Mr. J. A. Roebling, they have investigated the possibility of forecasting the weather by referring its changes to solar variations. The greater part of Mr. Clayton's paper is devoted to tables, graphs, and maps showing the average of pressure and temperature at various places so many days after high or low values of the solar constant. There is no attempt to show that the results are not attributable to chance, and indeed the general run of the graphs is in accordance with the hypothesis that they are.

Having reached this conclusion, how are we to explain the fact that Mr. Clayton can claim a considerable measure of success in forecasting temperature in New York several days in advance? Judging the forecasts by the departure of the mean temperature from normal, we see that when the forecast was "above normal" the average departure was actually nearly + 1° F., and when the forecast was "below normal" the average departure was − 1·28° F. The system of forecasting was based largely on the observation of sunspots and faculæ: from these observations the solar constant was estimated and the subsequent pressure and temperature changes foretold. But Mr. Clayton did not trust to solar observations alone. "These were supplemented by the temperatures observed at Seattle, Withston, and Chicago, in order to ascertain to what extent the temperatures at American stations were responding to solar changes." Mr. Clayton will forgive us for thinking that his forecasts would have been equally successful if he had trusted to the weather telegrams and ignored the sunspots.

In Mr. Hoxmark's paper we have some account of the method adopted in the preparation of the forecasts which have been published at Buenos Aires for at least two or three years. The forecast published on a Wednesday gives the anticipated temperature at 8 A.M. and 8 P.M. for each day of the following week. As a sample of the results the paper includes a table showing the temperatures forecasted and observed for 12 weeks in the middle of 1924. If the reader takes the trouble to plot these figures he will find little correlation between forecasts and sequel. Calculations give 0·25 as the correlation coefficient in the case of 8 A.M. temperatures. As the standard value of a correlation coefficient computed from the values of correlated variables is about 0·11, no importance can be attached to 0·25. For this particular period the forecasts must be regarded as a failure.

Mr. Hoxmark's own method of judging the points of his results is to find a correlation coefficient for each week. Of 131 weeks, 87 had positive coefficients and 44 negative. This disproportion must be counted as a point in favour of the forecasts. On the other hand, the fact that more than sixty per cent. of the week's forecasted yield correlation coefficients exceeding 0·3 is in no wise remarkable. Let it be noted that a correlation coefficient formed from seven pairs of samples is likely to be large even when samples are taken quite at random. The standard value of the coefficient derived from such samples is $1/\sqrt{6}$ or 0·41. In spite of the enthusiasm with which the results are recorded, we are left with the impression that these forecasts are not of practical value.

Thus the contention of Dr. Abbot and his collaborators that day-to-day estimates of solar radiation can already be used efficiently in weather forecasting would seem to fail. We have already seen that the reality of the fluctuations in solar radiation is itself highly problematical.

There is, it appears, a much stronger case for large swings in the value of the solar constant, such as was found in 1922. The measurements show a notable reduction of so much as ·1 per cent. in the radiating power of the sun as between 1920, 1921, and 1923, 1924. The possibility that such a change may be due to a failure to make complete allowance for some change in the earth's atmosphere is not to be overlooked. Prof. Marvin has brought out how difficult it is to be sure that the atmospheric effects have been eliminated. It is interesting to learn from Dr. Abbot that the solar changes are localised in short wave-lengths. The energy in the green, yellow, red, and infra-red was not affected by the 1922 drop; the effect was confined to the blue, violet, and ultra-violet. It is an obvious comment that this adds to the difficulty of accurate determination, atmospheric scattering being more serious with the short wave-lengths. To Dr. Abbot, a change, such as took place in 1922, indicates a reduction in the effective solar temperature, attending lessened solar activity, and the effects should be larger for shorter wave-lengths.

Let us accept this doctrine as a working hypothesis and look to its verification in the course of the sunspot cycle. At present most of the evidence available dates from the "medieval" era and therefore requires confirmation.

F. J. W. W.

Obituary.

PROF. WILFRID KILIAN.

CHARLES CONSTANT WILFRID KILIAN, whose death is announced at sixty-three years of age, was one of the most eminent of French geologists. For thirty-three years professor at the University of Grenoble, he was a man of enormous industry, his published papers and memoirs numbering nearly a thousand. The range of his work was very wide, but he is best known by his classic researches into the stratigraphy and tectonic structure of the French Alps, and by his palæontological work, dealing chiefly with the Lower Cretaceous Cephalopoda; on this group he was acknowledged to be the leading authority.

Born at Schiltingheim, in Alsace, in 1862, Kilian was educated at Strasbourg and at the Alsatian School in Paris, proceeding finally to the Sorbonne, where he was a contemporary of his lifelong friend and collaborator, Émile Haug. Here he was especially influenced by the teaching of Marcel Bertrand, and in 1885 served his apprenticeship in the field as assistant to this great Alpine geologist, on the expedition sent by the Paris Academy of Sciences to investigate the geology of Andalusia, following the great earthquake there. Three years later he presented as his thesis for a doctorate the "Monographie de la Montagne de Lure," a stratigraphical and palæontological study which stamped him as a worker of great promise. The next year found him in charge of the school of geology at Grenoble, and from this time onwards his field-work was devoted entirely to the French Alps, where he toiled with untiring enthusiasm. Spending each summer vacation in the mountains, he can almost be said to have explored every inch from Provence to Mont Blanc, discovering many fossil localities, and bringing precise evidence to bear on the age and stratigraphical relations of the rocks of this intensely complicated region. So enormous was the total of facts recorded by him that the titles of his papers occupy 25 pages of small text in the "Bibliographie géologique du sud-est de la France" compiled by him and a collaborator in 1922.

But never did the call of his field-work in the mountains detract from the care and accuracy of his palæontological studies in his laboratory at Grenoble. Kilian's work on the phylogeny and ontogeny, and on the faunal succession, of Lower Cretaceous ammonites will always be considered classical, and his unique collection at Grenoble is one which many palæontologists of the future will go there to see. In his rôle as ammonite specialist he was frequently called upon to report upon collections from other countries also, and so acquired an unrivalled knowledge of Lower Cretaceous stratigraphy. In the volume contributed by him to "Lethaea Geognostica" he attempted to bring this knowledge together into a comprehensive study. Three parts were published in 1907, 1910, and 1913, but the War intervened, and the work has remained unfinished.

Notwithstanding his immense scientific labours, Kilian never allowed himself to become too preoccupied to fulfil thoroughly his duties as a university teacher. Under his influence the school of geology at Grenoble has become one of outstanding importance, and already some of his pupils rank among France's leading geologists. Many were the awards made to him in recognition of his services to science; finally, in 1921, he received the Gaudry Medal, the highest honour that can be awarded by the Société Géologique de France; and so, after a short and sudden illness, has passed away one whose name will always occupy an honoured place in the history of French geology.

L. R. C.