"ON THE BEST VALUE OF THE SOLAR CONSTANT"

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It seems rather discouraging, after having spent ten of the best years of our lives on the determination of the solar constant of radiation, to be told\(^2\) that we have relinquished reformed methods having a rational basis given them by Langley in favor of methods which do not differ essentially from those of Pouillet; that we have abandoned an essential principle recognized by many able investigators; that our value of the solar constant is to all intents and purposes the same as that of Pouillet, whose observations would yield our result if known corrections were applied for defects in the water pyrheliometer; that the admirable agreement of our separate measures and the small apparent probable error of our final result are but characteristics of the original Pouillet model, not to be regarded as evidences of the trustworthiness of our work; and finally that it is known that reliable measures of solar radiation may be made within the atmosphere which exceed the supposed value outside the atmosphere, as will be shown by Very in a subsequent paper.

We await with great interest the last item, namely: "reliable measures of solar radiation made within the atmosphere which exceed" 1.93 calories per square centimeter per minute. Our own pyrheliometers we thoroughly believe to be trustworthy instruments, for we have spent about seven years in establishing the standard scale of radiation. We have read our pyrheliometers in three successive years on Mount Whitney, altitude 4420 meters, the highest point in the United States, with exceptionally pure and dry air above us, and never yet have obtained readings exceeding 1.75 calories per square centimeter per minute. Had we used Ångström's pyrheliometer, adopted by the Solar Union as a standard instrument, our readings would have been 5 per cent lower.

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Ångström himself observed on Teneriffe, at an altitude of 3683 meters, and never obtained values exceeding 1.63 calories.

To grasp the essential features of our determination of the solar constant of radiation the reader must consult: (1) the theory of our method given in Annals of the Astrophysical Observatory, Vol. II, pp. 13 to 17, 18 and 19; (2) the discussion of sources of error, pp. 58 to 82; (3) the procedure we pursue, pp. 50 to 57; (4) the measurements, pp. 83 to 98. Also, in recent years we have made very important improvements and measurements additional to those given in Vol. II of the Annals, as stated in this Journal, 29, 281; 33, 125; 33, 191, and 34, 197. Our work comprises spectro-bolometric and pyrhiometric measurements made at high and low sun according to the method of Langley, as stated by him in his Report of the Mount Whitney Expedition, pp. 135 to 142, and Table 120, values 1 to 5. Our measurements have been made at Washington (sea-level), 1902 to 1907; Mount Wilson (1750 meters), 1905 to 1911; Mount Whitney (4420 meters), 1908, 1909, 1910; and at Bassour, Algeria (1160 meters), 1911. The total number of spectro-bolometric solar-constant determinations now exceeds 600. All agree within narrow limits of variation to fix the value of the solar constant in terms of our standard water-flow pyrhiometric scale at about 1.93 calories per square centimeter per minute.

This value is supported to within the limits of his error of measurement by the determinations of Langley at Lone Pine and at Mount Whitney, where he obtained 2.06 and 2.22 calories respectively. We have proved (Annals, 2, pp. 119 to 121) that the value 3 calories which Langley gave as the result of his Mount Whitney expedition is erroneous because of an error of logic which he committed in his discussion in the Report of the Mount Whitney Expedition, pp. 143 to 145.

This is a summary of the work against which Mr. Very has made the charges above mentioned. We should naturally expect that, having made such charges, he would have proceeded at once to attack one or more of the essentials of our work above enumerated, and have shown distinctly and numerically wherein, by errors of logic or of measurement, the work fails, and how large resulting errors have arisen.

But far from attacking our main work in this straightforward fashion he begins by misleading the reader to suppose that our value of the solar constant depends on raising to the 26th power some coefficients of water-vapor transmission obtained by Rubens and Aschkinass. If this matter formed any part of our definitive determination of the solar constant we should proceed to show how unfairly Mr. Very has used what we said about it. As it forms no part at all, we merely invite the reader to see what we did say from p. 168 clear through to p. 172 of Vol. II of the Annals, and then compare with what he says we said.

According to Mr. Very the reflection of Mars for total radiation is 0.27, that of Venus 0.92, and that of the earth "is more likely than not" to exceed the mean, or about 0.60. We have no confidence in any of these figures, and see no reason why a solar constant of 3 calories or upward must be conceded to allow them to stand. When he chose to blow hot instead of cold, Mr. Very attempted to show that Neptune could be maintained at 50° C. (!) by the solar radiation, although the earth at less than 1/30 of Neptune's distance from the sun is only at 15° C. If planetary atmospheres can make such differences as this, we hardly think our careful direct measurements of the solar constant can be overthrown by computations made from the terrestrial temperature by the aid of guesswork about the reflecting and emitting power of the earth.

Mr. Very goes on to say that the temperature of the moon, as determined by him, requires a higher solar constant than 2 calories. We remember a very good story by Newcomb of a person who believed gravitation extended no farther than the atmosphere, and fell far short of the moon. Newcomb ascertained that his caller had never been to the moon to see, and told him that as he had never been there either, he doubted if they could agree! We feel some skepticism as to the temperature of the moon, and incline to praise the moderation of Langley, who, in summing up his classical investigation of it (in which Mr. Very assisted), says:^2

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^1 *Phil. Mag. (6)*, 16, 478, 1908.

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The conclusion of the whole matter is, that we have been dealing with a subject almost on the limit of our power of investigation with the present means of science, and have reached no conclusion which we are absolutely sure of. . . . . If beyond this we can be said to be sure of anything it is that the actual temperature of the lunar soil is far lower than it is believed to be; but the evidence does not warrant us in fixing its maximum temperature more nearly than to say it is little above 0° Centigrade.

But Mr. Very admits no such uncertainty. For him the effective temperature of the moon’s equatorial sunlit surface is 454° absolute, and nothing less than 3 calories will do for the solar constant to correspond. He maintains this, notwithstanding that Coblentz has shown that the moon is probably a very bad radiator!  

Mars, according to Mr. Very, has a temperature too high to admit of a solar constant of 2 calories. We suppose he has no direct information as to the radiating power of that planet or its absorption of solar radiation. If we had not already had some experience of the unpleasantness of discussing Mars, we should go on to say what we think about its temperature. We confess, however, that we know very little about Mars. Still, if Neptune can be maintained at 50° C. by solar radiation of about 1/1000 the intensity that reaches the earth, we should think Mars might have some chance without a solar constant of 3 calories.

Mr. Very gives a meteorological argument which according to him shows that we have underestimated the loss of solar radiation in our atmosphere, and thus have derived too small a value of the solar constant. He gives an expression for the insolation of the earth, into which expression the average transmission of the atmosphere for solar radiation enters. He says the distribution of temperatures on the earth June 21 agrees with the requirements of his formula better when one takes the transmission at less than 0.25 than for higher values. He considers the best value 0.18 as the average transmission over the whole sunlit surface of the earth. For this he requires a solar constant exceeding 3 calories.

Mr. Very does not give sufficient details of this comparison to enable us to understand clearly what he has done. But at all

3 F. W. Very, Phil. Mag. (6), 16, 478, 1908.
events we are of the opinion that the earth's surface temperature is a complicated function of many variables besides the insolation. Among these are cloudiness, distribution of land and water, mountains, ocean currents, and winds. Their effect is shown by the well-known differences of temperature at equal latitudes, as for instance between Europe and America. To choose that value of the atmospheric transmission which makes an insolation-curve match best with a temperature-curve, without any regard to these other factors, seems to us as indefensible as it would be to determine the reading of a Pouillet pyrheliometer merely by the rise of temperature on exposure to the sun, neglecting altogether the cooling due to the surroundings. We therefore can attach no weight at all to this method of fixing the solar constant.

In the remainder of this article we propose to take up the criticisms which Mr. Very makes of our work. He claims that we have abandoned Langley's methods and employ methods which do not differ essentially from those of Pouillet. Pouillet observed only with the pyrheliometer. We employ the pyrheliometer, as did Langley, only to determine the scale of energy of our spectro-bolometric determinations. Like Langley we determine the form of the solar energy-curve at different solar altitudes, correct it for instrumental absorption, determine numerous coefficients of atmospheric transmission from high- and low-sun observations on nearly homogeneous rays, determine thereby the form of the energy-curve outside the atmosphere, and assume, like Langley, that there is no water or oxygen absorption in the sun. We reduce the results to calories per square centimeter per minute as did he by the aid of the simultaneous readings of the pyrheliometer. Great improvements have been made in 30 years. We have the advantage of a better pyrheliometer than Langley, an automatic recording bolometer free from drift, and we can determine in 15 minutes all the data that it took Langley several successive days to obtain, and far more besides. We use many more atmospheric transmission coefficients than Langley did, and can determine them more accurately. We have made the measurements at sea-level, 1750 meters, and 4420 meters. In one thing only do we fail to follow Langley's example—we do not build a 2-calory solar constant up to 3 calories. We cite pp. 14 to
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16 and 119 to 121 of Vol. II of the Annals, as against pp. 143 to 148 of Langley's Report of the Mount Whitney Expedition, as our defense for this course.

Mr. Very says Pouillet's result, if corrected for known errors in his pyrheliometry, will not differ essentially from ours. It will differ by about 10 per cent from ours, and for the reason that Pouillet made no spectrum observations.¹

It will be a doctrine new to physicists that close agreement of independent determinations made under circumstances so diverse as those in our measurements at Washington, Mount Wilson, and Mount Whitney is no recommendation of the probable accuracy of the result; yet Mr. Very implies as much. As to his reflections on the effective radiating temperature of the earth and the transmission of water-vapor for long-wave rays, we think there is too much guesswork and too few facts, both on his side and ours, to make an argument about it worth while. After about five years more of experiments, we hope we may be in a condition to talk with him about these complicated questions without having to piece out our data with assumptions. We are surprised, however, by the roughshod manner in which Mr. Very overrides our value of the earth's reflection of total radiation. He merely remarks that he has "no hesitation in saying this is too small," without taking up the data by which we determined it. He suggests a mean between 0.27 and 0.92. We took more pains to obtain our value.

Mr. Very differs with us for our saying "we know that in general the lower layers of air have smaller transmission coefficients than the upper ones, owing to the generally low level of the larger quantities of dust and humidity." He says this "is true only for unsifted radiation and does not apply to the actual residual radiation which has already experienced its greatest absorption by aqueous vapor in the upper air." We reiterate our statement, both for Mr. Very's excepted kind of radiation and for homogeneous rays. If he still doubts it, let him look about in London, or look down on the dust layers above Pasadena from Mount Wilson. We should have thought his long residence near Pittsburgh would have convinced him that we are right. Certainly we still believe that light is more

¹ See Abbot's The Sun, Appleton & Co., 1911, pp. 293 to 296.
hindered near sea-level than in the higher atmospheric layers. If further verification is needed, see Table 118 of Langley's Mount Whitney Report.

Mr. Very objects to our process of evaluating the energy which would be found in the solar spectrum outside the atmosphere, beyond the wave-length where our observations stop in the infra-red. Perhaps he can suggest a better. At all events we hardly think he will claim that he can build up the solar constant to 3 calories out of energy beyond the wave-length 2.5 μ.

Mr. Very says we ought not to lay much weight on our observations made when the sun is near the zenith, and that we ought to observe at lower sun. He says we abandon low-sun observations "because low-lying mists render such measures uncertain" and prefer "midday observations on the plea that the sky is then clearer." He is quite wrong in both respects. We observe in the morning until about 10 a.m. as a rule, and then stop because no decrease in air-mass worth waiting for occurs afterward, at the latitude of our stations in our observing season, May to November. We do not begin observing before the sun's altitude is 15° because, as we have shown (Annals, 2, 63 to 64), the air-masses are uncertain at lower altitudes. We seldom observe when the sun is less than 40° or more than 75° from the zenith. We see no occasion to change our practice in these respects.

Mr. Very also regrets that we do not observe in winter. So do we, because we strongly suspect that the sun is a variable star. But if we did observe in winter we should require a cloudless region in or near the Southern Hemisphere, where the sun is high during our winter months. We see no advantage in extrapolating from air-mass 2 to air-mass zero when the extrapolation may begin at air-mass 1.2 just as well. However, there is no evidence from our winter observations at Washington that any systematic difference in the solar-constant values would arise on account of winter conditions.

Mr. Very intimates that even our narrow linear bolometer does not discern all the lines of atmospheric absorption, so that conditions may be conceived to exist under which our methods might give results appreciably too low. We have discussed this possi-
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bility at considerable length elsewhere\(^1\) and will not take space here to repeat that discussion in full. Such a condition would exist if what is sometimes called "the general absorption" of the atmosphere was in fact made up of nearly complete absorption and nearly complete transmission following one another in the spectrum in innumerable bands too narrow to be observed separately. We freely admit that the selective absorption of water-vapor in the great infra-red bands, and that of oxygen in its great bands, is of this character. Following the practice of Langley, we employ a special procedure for these bands, making the assumption, as he did, that none of them would exist in a spectrum taken at the outer limit of the atmosphere. We therefore draw a smooth curve for the energy spectrum outside the atmosphere where the terrestrial bands appear. This gives the highest possible solar-constant value. But as regards other regions of the spectrum, Rayleigh has shown satisfactorily that the so-called "general absorption" is really probably a scattering effect of small particles and molecules in the air, plus a diffuse reflection by larger particles. Schuster has shown that our atmospheric-transmission coefficients determined on Mount Wilson are almost exactly what could be predicted by Rayleigh’s theory of the scattering of light by the molecules of air. Diffuse reflection and scattering is, according to Rayleigh, a continuous function of the wave-lengths. Hence our linear bolometer amply suffices to estimate the atmospheric transmission in regions where selective atmospheric absorption does not exist. But Mr. Very would have us admit an atmospheric band at \(0.40 \mu\) to \(0.46 \mu\). Our own work gives no certain intimation of this.\(^2\)

We cannot, however, see how an inspection of the extra-atmospheric energy-curve we have determined\(^3\) could allow anybody to believe that any appreciable increase of the solar constant could come from smoothing the curve from \(0.40 \mu\) to \(0.46 \mu\), as if there were an atmospheric band there, in the manner we employ for the water-vapor bands of the infra-red.

\(^1\) See Annals of the Astrophysical Observatory of the Smithsonian Institution, 2, 64–65.

\(^2\) Ibid., Plate XVII.

\(^3\) Astrophysical Journal, 34, 206, 1911.
CONCLUSION

Mr. Very evolves a value of the solar constant of radiation from such unknown or fragmentary data as the reflection and emission of the earth, moon, and Mars, the temperatures of the two latter, and the dependence of terrestrial temperature on insolation. To clear the way for this he accuses us of doing what we have not, misrepresents what we have done, and suggests certain sources of error in our methods which we had already quantitatively discussed. We base our value on ten years of painstaking work, both theoretical and experimental, in laboratory and afield, at sea-level and high altitudes, comprising over 600 independent determinations by the most approved method.

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