

period of six years, are indications of the attempts that are being made to bring together between the two covers of a book the modern knowledge in this field.

Most of the recent books, however, are content to present more detailed accounts of restricted phytochemical groups. Next to the literature on the volatile oils, on the fatty oils, and on the carbohydrates, which have received special consideration, no doubt, because of their industrial significance, the alkaloids have attracted considerable attention.

While the fatty oils and carbohydrates represent decidedly restricted groups of chemical compounds, the volatile oils and alkaloids represent much wider fields, chemically speaking. Biochemically, however, these two groups have been regarded as of much less importance than the carbohydrates and fatty oils. This has, however, not lessened their inherent chemical interest, which has always been appreciated. However, their physiological significance has also grown with our increasing chemical knowledge of the compounds of these two groups. Physiological interest is no longer restricted to skeleton-producing or energy-producing materials. It has broadened and by no means to the disadvantage of the science.

It has been said that modern pharmacology owes its existence to the discovery of the alkaloids. Hence one is not surprised to find the pharmacological aspect of the alkaloids receiving consideration even in a treatise that is predominantly chemical. For the same reason the pharmaceutical aspect of the subject has been given due consideration by the author of the book under consideration. Yet there is a purely phytochemical point of view that deserves more careful study than it has commonly received.

That the author of "The plant alkaloids" should follow conventional lines is possibly to be expected. That he himself does not find satisfaction in so doing becomes only too apparent from various statements that might be quoted from his introduction. From a purely chemical point of view, the alkaloids, like all other carbon compounds, should be classified in accordance with the definition that organic

chemistry is the chemistry of the hydrocarbons and their substitution products. Thus the conflicts and the irrationalities of a classification based on the so-called typical groups would be avoided. From a botanical point of view, the alkaloids of a family should be considered together, totally irrespective of the nuclei they are supposed to contain. Thus and thus only can genetic relationships be brought out satisfactorily. Such a treatment not only proves satisfactory in the consideration of a single phytochemical group, but it tends to destroy the arbitrary boundaries of these groups.

Nevertheless, we welcome the author's new treatise. It may be claimed that it would be better to revise one of the older texts on the subject. That such revision becomes necessary very often in these days of great research activity is apparent to all who have occasion to use these texts. However, if a new text brings the subject matter up to date, it, as a rule, not only fulfills this important requirement, it is also apt to do more. It usually introduces new points of view at least here and there. For this reason we often welcome a new text rather than the up-to-date revision of an older one.

E. K.

THE TEMPERATURE ASSIGNED BY LANGLEY TO THE MOON

In his last publication on the lunar temperature,¹ Langley receded from his previous estimate "according to which the soil of an airless planet at the moon's distance

¹"The Temperature of the Moon. From Researches made at the Allegheny Observatory." *National Academy of Sciences*, Vol. 4, Part 2, Third Memoir, 1889. Two editions of this work were printed. One is said to be "by S. P. Langley," the other "by S. P. Langley and F. W. Very." To prevent misapprehension, I will state that the memoir was the joint work of Mr. Langley and myself. A note written by Mr. Langley, explaining that my name had been omitted from a place on the title page with his own by an oversight noticed too late for correction, was, by some irony of fate, tacked on to the wrong edition, the one which did have my name on the title page.

would have a temperature not greatly above $-225^{\circ}\text{C}.$ "² On page 197 of that work, a summary of our observations in the lunar spectrum yields the result that "the most reliable spectrum comparisons with a blackened screen show an average 'effective lunar temperature' of $+45^{\circ}\text{C}.$ near the time of full moon." Although the temperature which I had derived from the spectrum comparisons is thus cited, and is the only authoritative figure mentioned, a lingering predilection for his earlier value prevented more than a half-hearted acquiescence in my result on the part of Mr. Langley, who, as chief spokesman, said:³

Contrary to all previous expectations, [the extreme infra-red] nevertheless reaches us, thus bringing evidence of the partial transparency of our terrestrial atmosphere even to such rays as are emitted by the soil of our planet. It is probable, as remarked elsewhere, that even of the heat of arctic ice some minute portion escapes by direct radiation into space. 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 $0^{\circ}\text{Centigrade}.$

The last part of this extract refers to the belief that the moon's maximum temperature exceeds that of boiling water, a belief which rested on the opinion of Sir John Herschel and on some thermopile measures with a very large probable error which had been made of

²*Op. cit.*, p. 193. Langley's original statement in regard to the moon's temperature was founded on what he says ("Researches on Solar Heat," p. 213, 1884) concerning the earth's temperature in the absence of a selectively absorbing atmosphere, namely: "The temperature of the earth's surface is not due principally to this direct [solar] radiation, but to the quality of selective absorption in our atmosphere, without which the temperature of the soil in the tropics under a vertical sun would probably not rise above $-200^{\circ}\text{C}.$ " This passage contains a mixture of truth and error. The radiation from a tropical surface at 300°Abs. is 285 times that of a similar surface at 73°Abs. The atmosphere has a protective influence, but not one as extraordinary as this. It must be remembered that Stefan's law was not yet fully accepted.

³*Op. cit.*, p. 193.

the total lunar radiation by Lord Rosse, and which, as is well known, had previously been our most reliable source of information.

I may perhaps be permitted to say that the preceding citation from our joint work did not represent the opinion of the junior contributor. Abbot and Fowle, referring to this memoir, speak of my "most recent revision of the evidence";⁴ but this is a misapprehension, inasmuch as I have never revised the material contained in the above memoir. My later publications have been founded on new evidence which is entirely distinct.

Mr. Abbot also says in his work on "The Sun" (p. 311):

Upon the moon there is no atmosphere and by the observations of Lord Rosse, of Langley and of Very, the moon's sunlit surface falls from about the temperature of boiling water nearly to that of liquid air within the short duration of a total lunar eclipse.

But the preceding quotations prove that Langley never accepted the doctrine of the "hot moon" while he was director of Allegheny Observatory, and that even when pushed by the evidence, he hung back.

Professor W. H. Pickering in 1902 said:⁵

We do not certainly know the temperature of the moon's surface when exposed to a vertical sun; but according to Professor Langley, it can not be far from $32^{\circ}\text{F}.$

N. S. Shaler, in a work published by the Smithsonian Institution in 1903 under Secretary Langley's supervision,⁶ said:

The temperature of the moon has been made the matter of numerous experiments. These, for various reasons, have not proved very effective. The most trustworthy, the series undertaken by S. P. Langley, indicate that at no time does the heat attain to that of melting ice.

Consequently, up to 1903, Langley had not accepted the results which I published in 1898.

⁴*Annals of the Astrophysical Observatory of the Smithsonian Institution*, Vol. 2, p. 174.

⁵"Is the Moon a Dead Planet?" *The Century Magazine*, May, 1902, p. 91.

⁶"A Comparison of the Features of the Earth and Moon," *Smithsonian Contributions to Knowledge*, No. 1438. Part of Vol. 34, p. 6.

Although I have not heretofore attempted to revise the memoir on "The Temperature of the Moon," let me say here that the temperature for which I am responsible in that work (namely, $+45^{\circ}$ C. on the average), and which is given, indeed, but in such a guarded way as to lose much of its force, is certainly too low. In proof of this, reference may be made to figures 11 and 12,⁷ which show the position of the lunar image on the very wide slit which was necessary in order to obtain a readable galvanometer deflection in the almost evanescent lunar spectrum. It will be seen that at no time was the slit completely filled by the lunar image. There were always corners occupied by bits of sky, or by the unilluminated part of the moon, while the blackened screen containing boiling water always completely filled the slit aperture. Consequently, the lunar heat was underestimated. Moreover, the lunar radiation was an average pertaining to regions which include a wide range of temperatures, and necessarily fell much below the maximum radiation from the subsolar point.

In the description of the instrumental arrangements⁸ we read:

Care was taken that the lunar image formed by the condensing mirror, and having a diameter of 6.4 mm., should fall accurately upon the central portion of the slit, and thus only that portion is illuminated. In reducing the observations to a standard width of slit, the variation in this width from night to night having been considerable, the assumption is made that the amount of heat passing through the slit varies simply as the width, which is the same as to disregard the curvature of the upper and lower limbs of the lunar image, as well as the secondary effect of the variation of this lunar semi-diameter.

The inaccuracy of this disposition of lunar image and slit is obvious. The only excuse for neglecting it was that the condition of steadiness of our galvanometer at that time was not such as to call for any greater nicety in the other adjustments. No attempt was

made to assign a probable error to the numerical results. They were quantitative, but only roughly so. Hence it has seemed to me a waste of time to attempt to revise these measures. It would be better to repeat them with improved facilities.

Whether Secretary Langley ever accepted the results of the later measures which assign a temperature of 454° Abs. Cent.⁹ to the lunar subsolar point is not known; but as he had already stepped up from -225° C. to a temperature "a little above zero," let us hope that he may finally have been willing to go the rest of the way.

To those who find it difficult to accept a lunar surface temperature above that of boiling water, because of the low temperature at great elevations in the earth's atmosphere, where the rarefaction is still much less than on the moon, it may be pointed out that the insolation temperature attained by a planetary surface, after allowing for the variation in the intensity of sunshine, depends mainly on two factors: The duration of continuous insolation, and the absorbent power of the planetary atmosphere for return radiation from the planet's surface. The diminution of solar radiation in proportion to the inverse square of the sun's distance determines the available radiant energy, but the temperature acquired through exposure to sunshine depends to a still greater extent upon the nature of the atmospheric trap by which heat is captured. In this respect the greatest variety prevails among the planets of the solar system. The major planets possess denser and more highly absorbent atmospheres, capable of trapping greater and greater amounts of heat, as their distances from the sun increase. The evidences of heat, namely, strong aqueous absorption-bands in the spectrum, prevalence of cloud, and a vigorous circulation in the atmospheres of planets at so great a distance from the sun, may be explained on these principles, as is shown in my paper on "The Greenhouse Theory and Planetary Tempera-

⁷*Op. cit.*, p. 12.

⁸*Op. cit.*, p. 121.

⁹Frank W. Very, "The Probable Range of Temperature on the Moon," II., *Astrophysical Journal*, Vol. 8, p. 284, December, 1898.

tures,"¹⁰ where, however, the application of the theory to Neptune is purely illustrative.

Mars has a rather rare atmosphere, but a climate of the continental type, giving it warm summers. Quantitative measures of the intensification of the aqueous absorption-bands in the spectrum of Mars¹¹ confirm the evidence of melting polar snows, and assure us that the summer temperature of Mars is considerably above the freezing point.

The earth, since it is nearer to the sun than Mars and has a denser atmosphere, is, on the whole, hotter than Mars. Terrestrial summer climates would be even hotter than they are, if it were not for the tempering effect of its oceans.

The air modifies surface temperatures both by its absorbent and its convective properties. All parts of the earth's insulated surface are cooled by contact with air in motion. This source of thermal depletion is very much smaller on the moon. On the other hand, the earth's temperature is very much increased by the absorbent action of its atmosphere on telluric radiation, an action which is probably very small on the moon, since its visible spectrum shows not the faintest atmospheric absorption. It is well known that the moon's atmosphere is excessively rarefied, yet a minute amount of some especially absorbent vapor might make a considerable difference in the night temperature, if the absorption-bands were of wave-lengths corresponding to low-temperature radiation. Spectrobolometric observations have not favored the supposition, but are hardly delicate enough to reject it absolutely. Without demanding any exact compensation in these two opposite tendencies, it is sufficient to see that they do oppose each other, and that the final controlling factor is duration of insolation. This is great enough on the moon to permit the formation of a steady subsurface thermal gradient, and the attainment of a maximum temperature de-

¹⁰ *Philosophical Magazine* (6), Vol. 16, p. 478, September, 1908.

¹¹ Frank W. Very, "Measurements of the Intensification of Aqueous Bands in the Spectrum of Mars," *Lowell Observatory Bulletin*, No. 36; and "New Measures of Martian Absorption Bands on Plate Rm 3076," *Ibid.*, No. 49.

pending only on the absorptive coefficient of the surface and the solar constant of radiation. The fact to be emphasized is that no estimates of planetary temperatures are possible without considering the nature of the planetary atmospheres and the duration of insolation, and applying a knowledge of the principles of thermal conduction and of the "greenhouse" theory. But the moon is near enough to permit measurements of its radiant emission, in which the only hypothetical element remaining concerns the explanation of the observed facts. Since Abbot and Fowle, in connection with their objections to my value of the lunar temperature,¹² have referred favorably to the opinions of Dr. W. W. Coblentz, it may be well to point out a few statements by the latter writer which demand reconsideration. Dr. Coblentz in his paper, "Radiation from Selectively Reflecting Bodies,"¹³ says:

The reflecting power of the moon for visible rays, according to Langley, is only 1/500,000 full sunlight. Assuming that at 9μ the reflecting power is, on the average, ten times that at 0.5 to 4μ (a low [*sic*] estimate), the value becomes 1/50,000.

Here, "reflecting power," or albedo, has been confused with the total amount of light reflected by the moon, expressed as a fraction of sunlight. I found that the average reflecting power of the moon for solar rays of every wave-length, both visible and invisible, was about 13 per cent.¹⁴ Zöllner obtained, for visible rays, a lunar albedo of 17.4 per cent.¹⁵

¹² *Annals Smithsonian Observatory*, Vol. 2, p. 175.

¹³ *Physical Review*, Vol. 24, p. 314, March, 1907.

¹⁴ *Astrophysical Journal*, Vol. 8, p. 275, December, 1898.

¹⁵ This includes a fraction due to specular reflection which causes the phase-curve for moonlight to differ from that for a diffuse reflector, as well as from the phase-curve for proper lunar radiation emitted from the heated surface (for which see Fig. 15 of my "Prize Essay on the Distribution of the Moon's Heat and its Variation with the Phase," and compare with Zöllner's curve in his "Photometrische Untersuchungen"). If the definition of albedo is restricted so as to include only diffuse luminous reflection, we have such values as the following: Wollaston, 0.12; Bond, 0.071;

Ten times these values would be a reflection of more than the whole and "a low estimate"!

To make my meaning entirely clear, let it be noted that the moon occupies on the average about $1/97,300$ part of the hemisphere of the sky, and could send to the earth no more than this fraction of sunlight if it had an albedo of unity, or if it were a perfect diffuse reflector. With an albedo of 17.4 per cent., light from the full moon should be $0.174/97,300 = 1/559,300$. This is the fraction for which Coblentz adopts in round numbers $1/500,000$. A smooth sphere having the property of specular reflection would yield a small star-like image of the sun of great brilliancy, the rest of the surface remaining dark. Nothing of the sort occurs, and the reflection is mainly diffuse; but the notable increase of brightness at, or near, full moon, and the somewhat greater brightness of the limb, as compared with the center, signifies that there is enough crystalline material in the rocky surface of the moon, and especially upon the cliffs which are presented favorably for observation along the lunar limb, to give an appreciable percentage of scattered specular reflections from innumerable crystalline facets. The distribution of such reflection may differ enough from that for a matte surface to account for the peculiarities of lunar reflection. Infra-red rays, on the whole, are less reflected than visible rays by the moon.

Dr. Coblentz finds for his hypothetical quartz moon an emissive power of 0.1 for a limited section of the spectrum near 9μ ; though his Fig. 5,¹⁷ founded on the observations of Rosenthal, makes this fraction nearer 0.25. We may remark in passing that there are no common minerals with a relative emissivity as low as 0.1, even if we confine attention to this limited region of the spectrum, and that the most notable depression in the lunar spectrum at this point is also shown in the solar spectrum and is probably atmospheric. I speak of a "depression" in the Zöllner, 0.1195; W. H. Pickering, 0.0909. Compare Zöllner, "Berechnung der wahren und scheinbaren Albedo des Mondes," *op. cit.*, pp. 161, 162.

¹⁷ *Op. cit.*, p. 317.

emission-curve, but the depression is only a minor feature in what is otherwise a maximum.

Describing his Fig. 7,¹⁸ Coblentz says:

In Fig. 7, curves *b*, *c*, *d* show several of Langley's observed lunar radiation curves, and as a whole there is a close parallelism between the theoretical and the observed curve, especially at 10.7μ , where we have to consider only atmospheric absorption.¹⁸

There is a mistake here. Curve *d*, Fig. 7, is transferred from curve *c*, Fig. 6, which is derived in turn from the hypothetical emission curve with superposed atmospheric absorption. The supposed "close parallelism" vanishes when this mistake is corrected. The principal feature of the lunar curves is that they show a region of maximum radiation between 8 and 10μ (highest point at about 8.3μ), where the hypothetical emission curve has a minimum. The mistake is indeed corrected a little farther on where we read (p. 318):

The computed emission curve is the most intense at 10.2μ , while the observed curve is the most intense at 8.3μ .

But here another error is introduced, for we are informed that

this is to be expected if the observed energy curve is the composite of the selectively emitted energy of the moon and the selectively reflected energy of the sun. The selectively reflected energy of the sun would, to a certain extent, fill up the minima in the lunar emission curve, and as far as our present knowledge goes would explain the observed curves *b*, *c*, *d* [a?], (Fig. 7), which lack a minimum at 8.5μ . As a whole, from whatever standpoint we view this matter, we come to the same conclusion, viz.: that in the region from 8 to 10μ the energy emitted from the moon consists of its own proper radiation and of reflected energy from the sun.

The explanation, unfortunately for this writer, does not explain, since, as I shall show, the reflected radiation can not possibly exceed about $1/3,000$ of the emitted.

At this point in his argument, Dr. Coblentz

¹⁷ *Op. cit.*, p. 319.

¹⁸ *Op. cit.*, p. 318.

introduces the assumption that the absolute temperatures of sun and moon are $5,900^{\circ}$ and 350° , and calculates by Planck's formula for the distribution of energy in the spectrum that the ratio of the radiations emitted at these temperatures by complete radiators is 1:0.00316.

This ratio of the emissive power [meaning by this the radiation from equal areas] of the moon to that of the sun [considering the latter to be a complete radiator but the former to have a relative emissivity of 0.1] will then be 0.000316, which is 16 times ($0.000316 \div 0.00002$) the reflected energy of the sun from the moon.¹⁹

But, as we have just seen, the derivation of the fraction 0.00002 is erroneous, the value assumed for the lunar emissivity is improbable even for a narrow region of the spectrum, and still more so for the entire spectrum, and the argument founded on the supposed lunar "reflected energy" is equally inadmissible, as we shall now see.

When it is remembered that the maximum deflection in the lunar spectrum (furnished by a rock-salt prism) at about wave-length 1μ in the Allegheny measures was usually not over 10 divisions of a millimeter scale, and that the solar radiation at 9μ is certainly *not more than a hundredth part of that at 1μ* , or 0.1 mm., the supposition by Coblentz that any appreciable part of the lunar spectrum at 9μ , coming indiscriminately from all parts of the lunar surface, can be composed of *reflected* solar radiation, is seen to be preposterous.

The question of specular reflection does not enter here. Isothermal charts of the moon²⁰ show an entirely different distribution of total radiant energy on the moon's apparent disk from that of moonlight, but this distribution of total radiation is not much altered by the small amount of reflected light which it includes, and is altogether appropriate to that of the emission from a heated body having its highest temperature at the subsolar point.

¹⁹ *Op. cit.*, p. 315.

²⁰ Frank W. Very, "Prize Essay on the Distribution of the Moon's Heat and its Variation with the Phase," Utrecht Society of Arts and Sciences, The Hague, 1891. Cf. Figs. 7 to 14.

Silicates have an emissive power not very different from 0.9 (nine times as great as this author assumes), or a reflecting power seldom much over 0.1. Throughout a considerable part of the region of proper lunar radiation, the reflected solar spectrum must have been smaller than 0.1, perhaps not more than 0.01 scale division, and it would have been absolutely unrecognizable. The actual deflections which reached upwards of 20 or 30 scale divisions in this part of the lunar spectrum were entirely due to emitted radiation; but the part of the lunar spectrum of wave-length shorter than 4μ corresponded, both in the form of its energy-curve and in the fraction of its included energy, with the reflected solar radiation.

The supposed similarity between the reflection-curves obtained by Coblentz for some common silicates, and the lunar spectral energy-curve, a resemblance which is by no means conspicuous, is purely fortuitous. The lunar curve owes its shape to alteration by absorption in passing through the earth's atmosphere, and not to local abnormal reflection. The resemblance would have been even less approximate if Dr. Coblentz had drawn his theoretical radiation-curve for the temperature which I have indicated for the moon, which is *not* " 300° abs.," notwithstanding that the reader of another work by the same writer, "Infra-red Reflection Spectra,"²¹ might infer from a footnote that this temperature rests upon my measurements. The curves published in Fig. 90 of the same work and labeled "Reflection from Moon (Langley)," can not possibly have the assigned origin, as is evident from the preceding argument. In repeating this figure in the *Physical Review*,²² the designation has been changed from "reflection" to a noncommittal "radiation," but the quotations cited show that the idea of reflection persists.

Dr. Coblentz also infers from observations

²¹ William W. Coblentz, "Investigations of Infra-red Spectra," Part 4, Appendix 1, p. 114. Carnegie Institution of Washington, 1906.

²² Vol. 24, Fig. 3, p. 312.

measured in the image of the eclipsed moon that the radiation is reflected and not radiated; but the curves which he has drawn²³ for the eclipse of September 23, 1885, as measured at Allegheny by Mr. J. E. Keeler and myself, are incorrect, since the heat at no time vanished, although it diminished continually until the end of totality; and in other eclipses which I have observed, the heat measured in the image of the eclipsed moon has never been less than 1 per cent. of its value before eclipse, while the diminution of the light is sometimes a millionfold greater; that is to say, there is simply no comparison between the reflected radiation and that emitted by the heated lunar surface during totality. The radiation enormously exceeds the reflection at that time.²⁴

I am at a loss to know the source of the statement by Dr. Coblentz that "at the last quarter the heat of the moon is certainly not less than at the full."²⁵ This statement is totally at variance with all published observations.

One other misapprehension needs to be corrected. It is found in the work on "The Moon" by Professor W. H. Pickering.²⁶ Professor Pickering says (p. 20):

The most satisfactory test hitherto made seems to be that of Professor Very (*Astrophysical Journal*, 1898, VIII, p. 266), who compared the amount of heat received from the moon by a bolometer with that received from an equal angular area of sunlit melting snow. The heat was next in each case allowed to pass through a piece of clear glass before reaching the bolometer. The glass allows nearly all the reflected heat to pass, but absorbs that radiated by the body itself. The total radiation in the two cases was about the same, but while the reflected heat was much greater from the snow than from the moon, it was found that the radiated heat was much greater from the moon than from the snow. This means that while the snow is the better reflector, as,

²³ *Physical Review*, Vol. 24, Fig. 1, p. 310.

²⁴ Compare Frank W. Very, "The Temperature of the Moon," *Astrophysical Journal*, Vol. 24, p. 354, December, 1906.

²⁵ Carnegie Institution publication, p. 112.

²⁶ Published by John Murray, London, 1904.

indeed, we can see by inspection, the moon is the hotter body. The observation is so direct and simple that it seems impossible to deny the accuracy of the conclusion, but of course it gives us no clue as to what the actual temperature is.

The recognition of the value of this particular observation is all that could be asked, but the really convincing and conclusive experiments with radiating heated minerals, which were performed under identical experimental conditions, and which do give us a "clue as to what the actual temperature is," are not even mentioned in this work, and have been strangely underrated elsewhere.

Professor Pickering goes on to say:

It would be interesting to repeat Professor Very's observation, comparing the radiation from the surface of the moon with that from the surface of rock illuminated by the sun at temperatures ranging from the melting point of snow to the highest attained by rocks on the earth's surface when exposed to a nearly vertical sun.

Experiments somewhat resembling those suggested, but more instructive, had already been performed.

Another instance of the same misapprehension follows. Abbot and Fowle say:

Coblentz has lately shown that some of the materials likely to be prevalent on the moon's surface are very poor radiators at such temperatures as these, and this would tend to explain why Very has found a temperature so much higher than that of a "black body" under similar conditions.

They also remark:

We do not know what its surface is composed of, and therefore have no means of discovering the relations which connect the lunar temperature and radiation.²⁷

The opinion that "the moon is probably a very bad radiator" is also reiterated by these authors in a recent article,²⁸ where are some personalities to which I need make no further allusion, as they only obscure the real question.

²⁷ *Annals Smithsonian Observatory*, Vol. 2, p. 174.

²⁸ *Astrophysical Journal*, Vol. 25, p. 95, March, 1912.

The language of these quotations implies that I have not considered the radiating power of the actual lunar substance, but have assumed an ideal moon; whereas the truth is that extensive observations were made on the radiations from heated silicates and other substances of which the moon's surface is liable to be composed, and comparisons were instituted with these, and not with an ideal radiator, before reaching a final conclusion. The result of this comparison of the radiant behavior of various materials is important, since I find²⁹ that, given a sufficient duration of insolation to enable a steady state to be reached, there is not much difference in the emission from various materials. Thus a very poor radiator, such as rock salt, radiates from a great depth of its interior substance, and the summation of radiation from many interior layers compensates for the small emission from any one layer. Hence it is not correct in such cases to state emissivity as a surface function. The complete statement of emissivity must be a volumetric one and must include the subsurface thermal gradient.

Ferrel showed on theoretical grounds that the law connecting temperature and radiation for the moon may be expressed as an equation of condition, where, if the coefficients of radiation and absorption of the same substance are always equal, it makes no difference what the substances are; all will eventually reach the same temperature. Some exception to the theory must be made for such substances as ice which are kept cool by melting and evaporation in sunshine. In his actual illustration, Ferrel used the law of Dulong and Petit, but any other formula may be substituted as far as the principle in question is concerned. The important point is that

the same results would be obtained sensibly with any ordinary conductivity for heat if the same side of the moon were permanently exposed to the sun, for the temperature gradient by which the heat would be conducted inward would soon be-

²⁹ "The Probable Range of Temperature on the Moon," I., *Astrophysical Journal*, Vol. 8, pp. 199-217, November, 1898.

come so small, in this case, that the rate by which heat would be conducted inward would be insensible, as in the case in which heat is conducted outward from the interior of the earth.³⁰

In a different category from the books and articles already cited come such works as that by Fauth.³¹ A footnote on page 26 of this book refers to my writings on the moon, but the author does not appear to have read them carefully. On page 139 he says:

Lord Rosse was enabled by his measurements to appreciate the differences in temperature on the moon's surface during full radiation and by night, and found them to be over 300° C. But the temperature can not be determined with any accuracy. Lord Rosse's results have often been questioned, but they are supported by the recent investigations of Very. Very believes that at the moon's equator, when the sun is at its highest, the ground increases its temperature by more than 100° C. (which would be -173° C.).

This is pretty nearly a hopeless case. I am sure that neither Lord Rosse nor I could recognize our own work in the conclusions attributed to us, where absolute temperatures and temperatures on the centigrade scale are mixed up indiscriminately, in spite of carefully guarded language in the original sources, and where the opinion is hazarded that the *maria* are frozen oceans—a supposition which is completely overthrown by the thermal measures.

The selection of these quotations for special mention does not imply that there are not others equally objectionable in the literature of the subject.

My chief reason for wishing to call attention to the imperfect conceptions of one whose splendid contributions to science condone all minor imperfections is because Langley's early and gradually changing opinions on the subject of lunar temperature still act as a barrier against the acceptance of conclusions which are founded on reliable observations. This will be quite evident from the passages

³⁰ William Ferrel, *SCIENCE*, Vol. 6, p. 542.

³¹ "The Moon in Modern Astronomy," by Ph. Fauth, with an introduction by J. E. Gore, F.R.A.S.

cited from the treatises of Shaler and Pickering.

In conclusion, let me remark that even though we can not go to the moon to see for ourselves what its temperature may be, or whether gravitation acts there as here, or what may be the power of the sun's unabsorbed radiation, we are gifted with reason and can form for ourselves just conclusions from observed facts. Otherwise all astrophysics would be impossible.

FRANK W. VERY

WESTWOOD ASTROPHYSICAL OBSERVATORY,
May, 1912

THE ORE DEPOSITS OF WESTERN UNITED STATES

AMONGST the valuable publications issued by the United States Geological Survey is Mr. James M. Hill's Bulletin 507 with the misleading title "The Mining Districts of the Western United States," as it deals solely with the metalliferous mining districts. These districts are grouped and numbered in each one of the 13 states considered, and their distribution is shown upon 14 maps. The text gives for each district its chief rocks and metalliferous products, the publications of the Survey relating to each one, and the distance and direction of the nearest railroad station, etc. A full index of all the districts mentioned concludes the work, which should be in the hands of every one interested in the ore deposits of the west.

To the general student of metalliferous deposits probably the most instructive portion of the bulletin will be the "Geologic Introduction," by Professor Waldemar Lindgren, late chief geologist of the Survey, present head of the Department of Geology of the Massachusetts Institute of Technology, and one of our foremost mining geologists.

The evidences of the mineral wealth of the Cordillera are found extending territorially from the Pacific shore of United States eastward to western Texas and Oklahoma, and geologically from the Pre-Cambrian to the Recent.

Since the deposition of ores is due to geo-

logic agencies, it is pointed out that in the Cordilleran region, where the rocks are horizontal and undisturbed, the ore deposits are missing or rare and poor. Again, while the metallites occur mostly in the mountain ranges, yet many, even of the highest, are barren; showing that without other conditions, uplift, faulting, and crushing of the rocks, and the circulation of water through them does not always produce ore deposits.

Characteristic important deposits are where Paleozoic sediments have been traversed by moderate-sized eruptive masses of Cretaceous or Tertiary age, the ores apparently being deposited shortly after the intrusion. A less common but often rich deposit is found in Tertiary andesitic and rhyolitic flows.

All these deposits are believed to have been formed by water solutions—largely in fissure veins, chambers, and impregnations. The ores, except gold and its tellurides, were apparently originally deposited as sulphides of lead, iron, zinc, etc., or oxides of iron; but down to or below the permanent water level, which varies from a few hundred to 2,200 feet, these sulphides have been oxidized to cerussite, hematite, calamine, etc. Just below the oxidized zone occur secondary sulphides, like chalcocite and silver-bearing minerals, concentrated by the percolating waters, often into bodies of great richness.

Professor Lindgren gives under each state a more detailed summary, but space does not allow us to continue further, and any one interested can procure a copy by writing to the director of the Geological Survey at Washington.

Without intending to be captious it is suggested that in future editions the term "ores" should be used for "metallic ores" (see pp. 7-9), because there are no ores that are not metalliferous. In the same way "mineral deposits" ought not to be used for metallites or metalliferous deposits when the author intends to exclude the memetallites or non-metalliferous deposits (see pp. 5-9).

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