Samuel Pierpont Langley, 1834-1906

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(With Six Plates)

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August 22, 1934, marks the centenary of the birth of the third Secretary of the Smithsonian Institution. Samuel Pierpont Langley was born at Roxbury, near Boston, Massachusetts, August 22, 1834, and died at Washington, February 27, 1906. After his graduation from the Boston High School in 1851, he studied and practiced civil engineering and architecture until 1864. Then he traveled extensively in Europe, frequently visiting observatories and learned societies there. He and his brother, afterward Prof. John W. Langley, had long been ardent amateur astronomers, and being of mechanical tastes, they had constructed a small reflecting telescope. Returning from his European trip, the future Secretary devoted himself to astronomy. After a short assistantship at Harvard College Observatory and a very brief tenure as assistant professor of mathematics and director of the observatory at the Naval Academy at Annapolis, Md., he was in 1866 appointed director of the Allegheny Observatory, near Pittsburgh, and professor of physics in the Western University of Pennsylvania. He remained there for more than 20 years, during which his remarkable pioneering astronomical work along several different lines gave him a foremost standing in astronomy, along with that triumvirate of distinguished American astronomers of those days, Simon Newcomb, Edward C. Pickering, and Charles E. Young. He raised considerable revenue for the Allegheny Observatory by the then novel device of furnishing astronomical time to the Pennsylvania Railroad. The wealthy Pittsburgh philanthropist, William Thaw, was his helpful friend. By Langley's encouraging advice, John A. Brashear, a steel worker, was transformed from a timid amateur mirror-grinder to the founder of that great optical concern, the John A. Brashear Optical Company, of Allegheny, Pa., and was ever his grateful friend and helper in preparing novel apparatus for his pioneering experiments.

Owing to the failing health of the distinguished naturalist, Spencer F. Baird, second Secretary of the Smithsonian Institution, Langley was appointed Assistant Secretary in 1887. After Baird's death, he was elected by the Board of Regents to be Secretary on November 18, 1887. He retained this position until his death, February 27, 1906. During his tenure, Secretary Langley founded the Astrophysical Observatory, the National Zoological Park, the Regional Bureau for the United States of the International Catalogue of Scientific Literature, and the National Gallery of Art. He broke ground for the beautiful Natural History Building of the National Museum. His strong interest in children led him to set aside and beautify a special room for them in the Smithsonian Building, where the choicest specimens in zoology and geology were assembled to rouse their admiration and wonder. Several bequests came to the endowment of the Institution, notably the Hodgkins Fund for the study of atmospheric air. By annual journeys to Europe, Langley kept the Institution prominently before the eyes of Old World scientists and kept them informed at first hand of his notable researches in astrophysics and aviation.

Langley was a man of varied and discriminating tastes in art and literature. As an author he showed great clarity of expression and delightful rhythm and choice in words. He could never satisfy his fastidious taste in composition, but continually altered and polished his writings up to the very last stage. Only in bound form could they elude his further alterations. Having a generous sense of humor, he found a special pleasure in reading the works of George Borrow. The novelist, William Dean Howells, was a valued friend, from whom he even took lessons in composition, so much did Langley admire the polished style of Howells' writing.

Though unmarried, Langley was a great favorite with children. I have seen him at the resort, Marshall Hall, swinging with two little girls, one on either knee, while he told them fairy stories. He was afflicted by great shyness, and like some others thus handicapped, he carried for the outer world a shell of hauteur, very unrepresentative of the warm heart within. A man of great accomplishment himself, he was often unfairly impatient with assistants, and would betray irascibility by unduly raising his voice when things did not get on to suit him. For these reasons many failed to understand the innate kindliness of the man, so well known to those in closest association with him.

The older men of the Smithsonian Institution still remember many incidents illustrative of Langley's character that would make delightful reading if they could be written without loss of flavor. He often told witty stories, or used bon mots to impress indelibly some point in conversation. He was fond, for instance, of the expressions: "Let sleeping dogs lie"; "The written word remains"; "What has posterity done for us that we should care so much for the opinion of posterity?" One day when he was going to some function he came hurriedly out of his room and said "William, my hat." The colored man ran and got his derby. "I said a HAT!" shouted Langley, as he threw the derby down the hall. He used always to have a messenger boy accompany him when he walked to outlying offices. As befitted his chief's dignity, the boy always walked two paces behind, perhaps carrying an overcoat or a portfolio. In his youthful exuberance, and especially if some crony was looking on, the boy might cut some slightly disrespectful capers. But if so, he reckoned without his chief's knowledge of optics. For observing the boy indistinctly by reflection from the rear of his glasses, Langley would turn around suddenly at a critical moment, to the boy's great discomfiture. These little idiosyncrasies were a spice to us at the time, and endear the memory of our great chief as we look back over more than a quarter century.

In the remainder of this memoir I propose to let Langley tell in his own words of some of his leading pioneer investigations. A list of the exact references to these articles will be found at the end of this paper.

"ON THE MINUTE STRUCTURE OF THE SOLAR PHOTOSPHERE"

"Before we turn with these aids to the study of the photosphere, it will be well to describe briefly appearances presented by the solar surface in telescopes of moderate size.

"Here we see a disk of nearly uniform brightness, which is yet sensibly darker near the circumference than at the center. Usually seen relieved against this gray and near the edges, are elongated and irregular white patches (*faculae*), and at certain epochs trains of spots are scattered across the disk in two principal zones equidistant from the solar equator. On attentive examination it is further seen that the surface of the sun everywhere—even near the center and where commonly neither faculae nor spots are visible—is not absolutely uniform, but is made up of fleecy clouds, whose outlines are all but indistinguishable. The appearance of snow flakes which have fallen sparsely upon a white cloth, partly renders the impression, but no strictly adequate comparison can perhaps be found, as under more painstaking scrutiny, we discern numerous faint dots on the white ground, which seem to aid in producing the impression of a mosslike structure in the clouds, still more delicate, and whose faint intricate outlines tease the eye, which can neither definitely follow them, nor analyze the source of its impression of their existence.

"These appearances have been mentioned, lest they should be confounded in any way with the far minuter structure now to be described.

"Under high powers used in favorable moments, the surface of any one of the fleecy patches is resolved into a congeries of small, intensely bright bodies, irregularly distributed, which seem to be suspended in a comparatively dark medium, and whose definiteness of size and outline, although not absolute, is yet striking by contrast with the vagueness of the cloud-forms seen before, and which we now perceive to be due to their aggregation. The 'dots' seen before are considerable openings caused by the *absence* of the white nodules at certain points, and the consequent exposure of the gray medium which forms the general background. These openings have been called pores; their variety of size makes any measurements nearly valueless, though we may estimate in a very rough way the diameter of the more conspicuous at from 2″ to 4″. The bright nodules are themselves not uniformly bright (some being notably more brilliant than their fellows and even unequally bright in portions, of the same nodule), neither are they uniform in shape. They have just been spoken of as relatively definite in outline, but this outline is commonly found to be irregular on minute study, while it yet affects, as a whole, an elongated or oval contour. Mr. Stone has called them rice-grains, a term only descriptive of their appearance with an aperture of three to four inches, but which I will use provisionally. It depicts their whiteness, their relative individuality, and their approximate form, but not their irregular outline, nor a certain tendency to foliate structure which is characteristic of them, and which has not been sufficiently remarked upon. This irregularity and diversity of outline have been already observed by Mr. Huggins. Estimates of the mean size of these bodies vary very widely. Probably Mr. Huggins has taken a judicious mean in averaging their longer diameter at 1″.5, and their shorter at 1″, while remarking that they are occasionally between 2″ and 3″ and sometimes less than 1″ in length. . . . .

"In moments of rarest definition I have resolved these 'rice-grains' into minuter components, sensibly round, which are seen singly as points of light, and whose aggregation produces the 'rice-grain' structure. These minutest bodies, which I will call *granules*,[1]

[1] As this word is already in use, with another meaning, attention should be given to the restricted and definite significance which is here assigned to it.

it will appear subsequently can hardy equal 0".3 in diameter, and are probably less. (Secchi is the only observer, as far as I know, who appears to have seen and measured them. He observed them in the edges of the pores, and reckons their size at ".$\frac{1}{3}$ to "$\frac{1}{5}$, but does not estimate their number or point out their relations to the 'rice-grains.') They are irregularly distributed, with a tendency to aggregation in little clusters (the clusters being the rice-grains), and their existence accounts for the diversity and irregularity in the outline of the latter. Mr. Huggins has acutely remarked upon, while it of course makes clear the reason of the apparent increase in the number of 'rice-grains' with increasing telescopic power.

. . . .

"We are now prepared to study the minute structure of the photosphere under another aspect, as it appears in the spots. It is impossible to make such a drawing as that here given from any single delineation, owing to the rapidity with which spots change their form. I have accordingly, while taking the general contour and many details from drawings of the great spot of March 5 and 6, 1873, added the results of numerous studies of detail in other spots, made during the past two years. . . . .

"To represent the gradations of light from the intensest splendor to the darkness of the nuclei, we have here only the limited range between a white and a black pigment. This almost compels partial falsity in the degrees of shade, and there is, for instance, in the drawing, a relative exaggeration of the shade which marks the outer boundary of the penumbra, and without which the important details would be hardly visible.

. . . .

"It is practically impossible, in the brief intervals of perfect definition during which such work can be carried on, to so multiply micrometric measurements, that from their concordance any idea of their probable error is obtainable by the usual treatment. Measurements taken at different times, and on different parts of the penumbra, by counting the number of filaments in a given space, give from 0".7 to 1".0 as the average distance from center to center of parallel filaments separated by scarcely measurable intervals; at the same time that the distance in some parts is greater, it is in others much less.

. . . .

"Solar cyclones, which, even without the aid of the spectroscope, we see are incomparably more violent than our own tropical tornados, act on the filaments without destroying their identity. It is probable that both the filaments and the granules I have so minutely described, may hereafter be resolved into smaller components still, but their persistent individuality as a whole under such disturbance, impresses me as a most striking feature, and one for which, under similar circumstances, we have no exact analogy in our own meteorology.

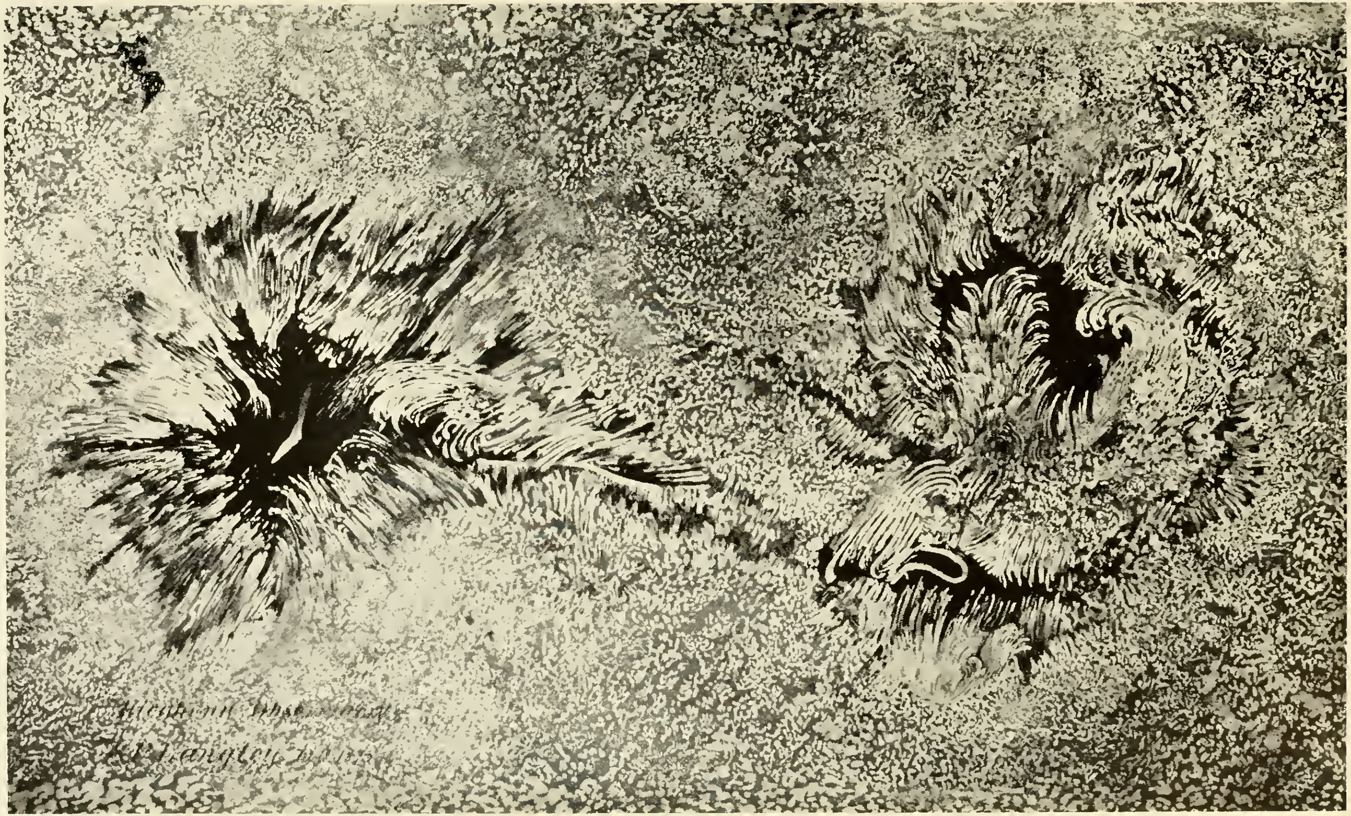
. . . .

"Are these round, nearly central openings, so that looking into one we are looking into the axis of the cyclone to which the spot is due—into the vortex of the great whirl *down* which the chromospheric vapors are being sucked by mechanical action? Are they ragged apertures—the craters as it were of eruptions whence metallic vapors are being forced *up?* The answer to this question, were there but these two alternatives, would be definitive as to our choice between the principal theories of solar circulation."

Dr. George E. Hale has told me that the better he perceives by photography or vision with the great outfits at Pasadena and Mount Wilson the features of sun spots and the photosphere, the more do they approach Langley's drawings and descriptions of them. It is interesting to add that photography has plainly shown that high-lying solar clouds of matter are indeed sucked into the umbrae of sun spots just as Langley suggested.

"THE TOTAL SOLAR ECLIPSE OF JULY 29, 1878: OBSERVATIONS AT PIKE'S PEAK, COLORADO"

"Upon the 22d Prof. John W. Langley arrived, and, as the rain poured freely through the roof upon the boxes which lay in the wet, as the best means of protecting the telescope, we mounted it in the open air on a partly level spot of a few feet square some yards from the hut. Procuring some lard from the kitchen, I covered every part of the steel-work with it, and wrapped the instrument in a piece of canvas. Upon the 23d Prof. Cleveland Abbe, of the Signal Service, arrived, and on the same evening two tents were pitched, which had been sent by the order of General Myer. There was no piece of level ground or rock large enough to lie upon; but we procured some logs which had been brought up for fire-wood, and, laying these between the bowlders, spread on them a sack of hay for each, and blankets which had been brought up in the rain; these were all damp, and our first night under canvas in a cold and high wind was not agreeable, particularly as the difficulty of breathing decidedly increased rather than diminished. In the morning all of the party were ill. The day was passed in fruitless attempts to adjust the equatorial. In the morn-



Drawing of Sun Spot Made by S. P. Langley at Allegheny Observatory, 1873

ing the canvas which covered it was frozen and loaded with hail. A little later the sun shone out suddenly and with surprising warmth, turning the hail to water. I commenced unwrapping the canvas, and was lifting it off, when the sun disappeared as suddenly as it came out, and, before I could put the cover on again, it was hailing once more, and we were involved in dense cloud. This cloud was continuous, except for several brief moments of sunshine, during which I uncovered the instrument several times to no purpose. I may say briefly that this was nearly the history of the weather for the ensuing week, during which we had several clear sunrises and sunsets, but in the course of which neither Professor Abbe nor myself got so much as the requisite observations for adjusting our equatorials, which remained on the day of the eclipse in the position in which they were first set up. During the first days, our illness increased, and, with a great difficulty of breathing and greatly increased action of the heart, we felt constant and severe headache, and nearly every symptom which attends sea-sickness. Exertion was extremely difficult, and that of building the stone piers for the heliostat, photometer, and other instruments for which we had at first no assistance, was carried on only by a very strong effort of will as well as of strength.

" Not to enlarge on the personal discomforts of a week which we all had reason to wish over, I may add that towards its close Professor Abbe's condition gave us cause for alarm. His symptoms were the same as my brother's and my own, but much aggravated, and while we grew rather better, he grew worse, and upon Sunday morning he was unable to rise. At this time the tents were not the place for an invalid. The snow, which had blown into one during the night and spread thickly over one of the sleepers, I remember finding ten inches deep beside me when I woke. Professor Abbe's own resolution to stay, if possible, was unaltered; but one of our party, a physician, pronouncing his life endangered by another day, he was, on the evening of the 28th, put into a litter and carried down to a lower altitude, where his recovery was rapid.

. . . .

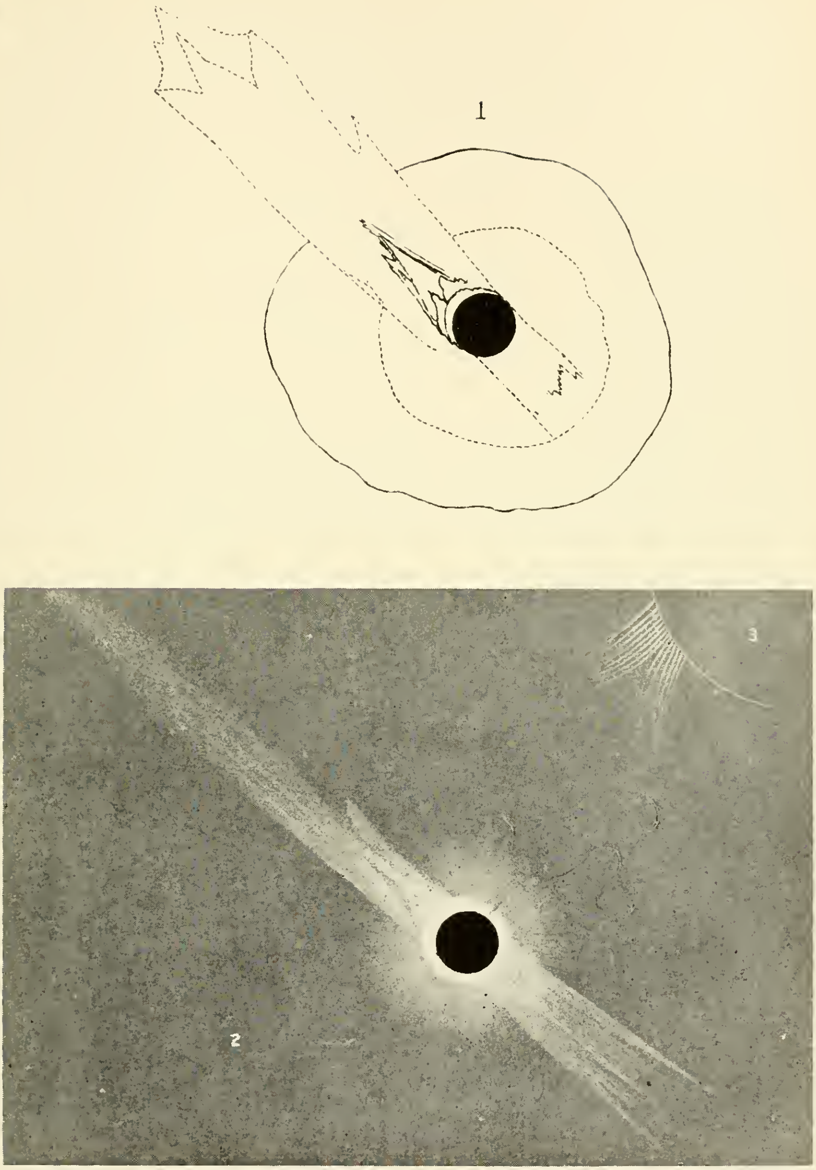
" The morning of the 29th was clear, and the whole of the important day was a complete contrast to its predecessors, the sky being almost cloudless and of a deep and transparent blue never seen near the Atlantic coast. . . . .

" I pass over phenomena preceding totality, observed by myself, as of little value, with a reference to the letters of Messrs. Shields and Manning, given below. At the moment of totality I removed the dark glass.

"As original records of an observation are trustworthy in proportion as they have been presented in their first crude state, I endeavor to give the impressions as they rose in my mind, and will comment on them later. My first impression, then, of the corona was, 'It is not so bright as those I have seen before'; my second, 'but it is far more extended.' I had before me a sheet of drawing-paper with a $\frac{3}{4}$-inch circle on it to represent the sun, and on this I traced an outline (Plate 3, Fig. 1) of what I then saw, before the eye had recovered its sensibility. The sun was surrounded by a narrow ring—hardly more than a line—of vivid light, presenting to the naked eye no trace of structure; which faded with great suddenness into a nebulous luminosity that at first appeared to extend to a distance of about two and one-half solar diameters all around. . . . .

"There were but a few moments left when I turned to the telescope. It happened to be directed toward the northern part of the sun. I adjusted the eye-piece for distinct vision, which appeared excellent, but the view after this lasted, I think, not more than four or five seconds before totality was over. What I saw thus momentarily was not in the least what I expected. If there were any structure in the very inner corona, it had escaped me when I had searched for it in a previous eclipse (Jeres, in 1870). It is true that the sky was hazy on that occasion, and that on this it was exquisitely clear. Now what I saw in this brief view was a surprisingly definite filamentary structure [2] somewhat coarser and decidedly more sharply defined than I have ever seen filaments in the photosphere, not disposed radially, or only so in the rudest sense, sharpest and much the brightest close to the disc, fading rapidly away into invisibility at a distance of five minutes of arc or more (possibly in some cases of ten). The salient point to me was this very remarkable definiteness and precision of these forms, and this *impression,* made on my mind in that too brief moment, is reproduced in this sketch (Plate 3, Fig. 3), taken from one made within ten minutes of the event. It is in no way a 'picture' but a reproduction of the original memorandum of the first impres-

[2] The action which produces these definite forms goes on over the surface of a sphere, in reality, and not a disc, and they are doubtless presented to us under all possible foreshortenings, and, questionless, lie one behind and across another, so that, *a priori* one would expect they would obscure one another, and that such definiteness would not, in fact, exist. But, to me, the actual appearance was very much like that which we might have if the sun were not a globe at all, but a flat disc, fringed with such threads. Doubtless, there was really an intricate cross-hatching of them, and various obscurer forms might have been made out with time for study.



TOTAL SOLAR ECLIPSE, JULY 29TH, 1878, PIKE'S PEAK, COLORADO

Drawings by S. P. Langley

sion of the features of the (telescopic) inner corona, which were, to repeat: (1) Extraordinary sharpness of filamentary structure; (2) arrangement not radial, or only so in the rudest sense; (3) generally curved, not straight lines; (4) curved in different directions; (5) *very* bright close to the edge, and fading *very* rapidly—fading out wholly at from 5 to 10 minutes from it."

"THE BOLOMETER AND RADIANT ENERGY"

"Our knowledge of the distribution of heat in the solar spectrum really begins with this century and the elder Herschel, ...

"No one, so far as I know, has hitherto succeeded in measuring the heat from a diffraction grating except in the gross, ...

"I have tried at intervals for the past four years to do this, and having long familiarity with the many precautions to be used in delicate measures with the thermopile, and a variety of specially sensitive piles, had flattered myself with the hope of succeeding better than my predecessors. I found, however, that though I got results, they were too obscure to be of any great value, and that science possessed no instrument which could deal successfully with quantities of radiant heat so minute.

"I have entered into these preliminary remarks as an explanation of the necessity for such an instrument as that which I have called the Bolometer (βολή, μέτρον), or Actinic Balance, to the cost of whose experimental construction I have meant to devote the sum the Rumford Committee did me the honor of proposing that the Academy should appropriate.

"Impelled by the pressure of this actual necessity, I therefore tried to invent something more sensitive than the thermopile, which should be at the same time equally accurate—which should, I mean, be essentially a '*meter*' and not a mere *indicator* of the presence of feeble radiation. This distinction is a radical one. It is not difficult to make an instrument far more sensitive to radiation than the present, if it is for use as an indicator only; but what the physicist wants, and what I have consumed nearly a year of experiment in trying to supply, is something more than an indicator—a *measurer* of radiant energy.

"The earliest design was to have two strips of thin metal, virtually forming arms of a Wheatstone's Bridge, placed side by side in as nearly as possible identical conditions as to environment, of which one could be exposed at pleasure to the source of radiation. As it was warmed by this radiation and its electric resistance proportionally increased over that of the other, this increased resistance to the flow of the current from a battery would be measured (by the disturbance of the equality of the 'bridge' currents) by means of a galvanometer.

. . . .

"This promptness in the action of the metal strip gives it a great advantage over the thermopile for measures of precision. But, beside this, the deflection produced by the single strip and bridge is greater than that from the thermopile, if the element of time enter into the comparison, and still more if the relative areas exposed to radiation be considered.

"Although (for the reasons just cited) far from as sensitive as we can make it, such a strip then is yet more sensitive than the pile. A number of thermopiles, selected as the most sensitive in the writer's collection, have been exposed to the same source of radiation, placed at the same distance as in the previous experiments. They were . . . . as follows:

"*A.* Large thermopile, by Elliott (Tyndall-lecture pattern), composed of sixty-three couples, ...

"*B.* Very sensitive thermopile of extra small elements (16 couples) . . . .

"*C.* Delicate linear thermopile (7 couples). Working face about 1 mm. by 10 mm. = 10 sq. mm. . . . .

"*S.* The iron strip, which was about 7 mm. by .176 mm. and whose working face was therefore about 1 sq. mm. . . . .

"The time of exposure was about five seconds for the thermopiles and about one-half this for the strip, the latter time corresponding to the rapid swing of the (designedly) insensitive galvanometer.

"In the table, the first column gives the name of instrument; the second, the cross-section of the beam of radiant heat which is received upon it; the third is the actual deflection in galvanometer divisions; and the fourth the deflection for each square millimetre of exposed surface. . . . .

Instrument	Area sq. mm.	Deflection div.	Sensitiveness
A	240	211	.9
B	34	125	3.7
C	10	147	14.7
S	1	204	204.0

"After nearly a year's labor (I began these researches systematically in December 1879), I have procured a trustworthy instrument. It aims, as will have been inferred from the preceding remarks, to use the radiant energy, not to develop force directly as in the case of the pile, but indirectly, by causing the feeble energy of the ray to modulate the distribution of power from a practically unlimited source.

"To do this I roll[3] steel, platinum, or palladium into sheets of from 1/100 to 1/500 of a millimetre thickness; cut from these sheets strips one millimetre wide and one centimetre long, or less; and unite these strips so that the current from a battery of one or more Daniell's cells passes through them. The strips are in two systems, arranged somewhat like a grating; and the current divides, one half passing through each, each being virtually one of the arms of a Wheatstone's Bridge. The needle of a delicate galvanometer remains motionless when the two currents are equal. But when radiant heat (energy) falls on one of the systems of strips, and not on the other, the current passing through the first is diminished by the increased resistance; and, the other current remaining unaltered, the needle is deflected by a force due to the battery directly, and mediately to the feeble radiant heat, which, by warming the strips by so little as 1/10000 of a degree Centigrade, is found to produce a measurable deflection. A change in their temperature of 1/100000 degree can, I believe, be thus noted; and it is evident that from the excessive thinness of the strips (in English measure from 1/2000 to 1/12500 inches thick) they take up and part with the heat almost instantly. The instrument is thus far more prompt than the thermopile; and it is also, I believe, more accurate, as under favorable circumstances the probable error of a *single* measure with it is less than one percent. When the galvanometer is adjusted to extreme instability, the probable error of course is larger; but I have repeated a number of Melloni's measurements with the former result.

"I call the instrument provisionally the 'Bolometer,' or 'Actinic Balance,' because it measures radiations and acts by the method of the 'bridge' or 'balance,' there being always two arms, usually in juxtaposition, and exposed alike to every similar change of temperature arising from surrounding objects, air-currents, etc., so that the needle is (in theory at least) only affected when radiant heat, from which one balance-arm is shielded, falls on the other.

. . . .

"The first measures, on nearly homogeneous rays in the *diffraction* (reflection) spectrum, ever taken by any one that I know of, were

[3] Experiments are now in progress with still thinner films of metal produced by electrical or by chemical deposition. I have had the good fortune in experiments now making in this direction, to secure the aid of Professor A. W. Wright of Yale College, and of Mr. Outerbridge of the United States Mint at Philadelphia.

taken by this instrument on Oct. 7, 1880, used with an extremely delicate reflecting galvanometer by Elliot, of about 20 ohms resistance and a reflecting grating on speculum metal by Mr. Rutherford of 681 lines to the millimetre. Measures have been taken every fair day since, the source of energy being the sun.

. . . . The 'Balance' then, whose acting face is only about 1/30 the length of the visible spectrum, and less than 1/100 the length within which energy is found in a degree sufficient for it to measure, receives nearly homogeneous rays (which have passed through no absorbing medium whatever except the solar and terrestrial atmospheres), and this extremely minute amount of heat is found to give a galvanometer deflection of some hundred divisions, where thermopiles have hitherto failed to register any (on homogeneous rays).

". . . . They are hitherto unpublished, and they at least, though as yet approximate, show that the *heat maximum in a normal spectrum is not in the ultra-red, but is at least as far up the spectrum as the orange near D;* and this result may be relied on, any smaller values below $\lambda = .0007$, as well as all favorable atmospheric circumstances (high sun, blue sky, etc.), rather tending to move it toward the violet."

"ON THE AMOUNT OF THE ATMOSPHERIC ABSORPTION"

"Let us first suppose the radiation of the heavenly body to be really composed before absorption of two portions, A and B. Let A have a special coefficient of transmission (a), and B another, special to itself (b). Then, if we assume (still for considerations of convenience only) that each of these portions, is, separately considered, homogeneous, we may write down the results in the form of two geometrical progressions, thus:

TABLE 1

Original radiation	Ratio	Radiation received after absorption by one stratum	By two strata	By three strata	By four strata, etc.
A	a	Aa	Aa^2	Aa^3	Aa^4
B	b	Bb	Bb^2	Bb^3	Bb^4
A + B		$Aa + Bb = (M)$	$Aa^2 + Bb^2 = (N)$	$Aa^3 + Bb^3 = (O)$	$Aa^4 + Bb^4 = (P)$

"Then will

$$\frac{Aa+Bb}{A+B} < \frac{Aa^2+Bb^2}{Aa+Bb} < \frac{Aa^3+Bb^3}{Aa^2+Bb^2} < \frac{Aa^4+Bb^4}{Aa^3+Bb^3} < \text{etc.}$$

and

$$\frac{Aa^2+Bb^2}{Aa+Bb} < \left(\frac{Aa^3+Bb^3}{Aa+Bb}\right)^{\frac{1}{2}} < \left(\frac{Aa^4+Bb^4}{Aa+Bb}\right)^{\frac{1}{3}} < \text{etc.} \ldots\ldots$$

"The fractions here are the coefficients of transmission, as deduced from observations at different zenith distances. They evidently differ, and (as will be shown) each is larger than the preceding.

"In the above $Aa+Bb$ is the sum of the two kinds of radiation as observed after absorption by one unit stratum ($\sec.\zeta=1$) by the photometer, or actinometer; Aa^2+Bb^2 is the sum of the radiations observed after absorption by two strata ($\sec.\zeta=2$) etc.; but we are here supposed to independently know the really dual constitution of the radiation, which the photometer or actinometer does not discern. According to the usual hypothesis, the coefficient of transmission, which is the quotient obtained by dividing the value after n absorptions by that after n − 1 absorptions, or more generally that from the expression

$$\left(\frac{\text{Value after } n \text{ absorptions}}{\text{Value after } m \text{ absorptions}}\right)^{\frac{1}{n-m}}$$

is a constant. It is in fact not a constant, as we shall prove later; but we shall first show that, if we proceed upon the ordinary assumption, the value obtained for the original light of the star before absorption will in this case be too small. For, if we observe by a method which discriminates between the two radiations, we shall have, if we separately deduce the original lights from our observation of what remains after one and again after two absorptions, the true sum

$$A+B=\frac{(Aa)^2}{Aa^2}+\frac{(Bb)^2}{Bb^2}$$

while if we observe by the ordinary method, which makes no discrimination, we shall have the erroneous equation

$$A+B=\frac{(Aa+Bb)^2}{Aa^2+Bb^2}$$

which is algebraically less than the first, or correct value, for the expression

$$\frac{(Aa)^2}{Aa^2}+\frac{(Bb)^2}{Bb^2} > \frac{(Aa+Bb)^2}{Aa^2+Bb^2}$$

readily reduces to the known form

$$a^2+b^2>2ab.$$

Moreover since $a^2+b^2-2ab=(a-b)^2$, *the error increases with the difference between the coefficients.*

"Now, in the general case, if we suppose the original radiation L to be composed before absorption, of any number of parts A_1, A_2, A_3, $+$ . . . . having respectively the coefficients of absorption a_1, a_2, a_3, $+$ . . . . the true value of L is given by a series of fractions which may be written in the form

$$L = \frac{\Sigma(Aa)^2}{\Sigma Aa^2} = \Sigma A$$

whereas the value of the original energy by the customary formula would be

$$L_1 = \frac{\Sigma(Aa)^2}{\Sigma Aa^2}$$

so that, all the quantities being positive, by a known theorem, $L > L_1$, and for the same values of A_1, A_2, A_3, ... this inequality is greater, the greater the difference in the values of the coefficients a_1, a_2, a_3, ...

"But this is stating in other words that the true values, found by observing separate coefficients of transmission, are always greater than those found when we do not distinguish between the radiations of which the light (or heat) of the star or sun is composed, and also that the amount by which the true values are greater, increases with the difference between the coefficients.

"We have stated above that the usual hypothesis makes the coefficient of transmission a constant. It will be seen from the above table, however, that it varies from one stratum to the next; that it is least when obtained by observations near the zenith; and that it *increases progressively as we approach the horizon.*"

"RESEARCHES ON SOLAR HEAT AND ITS ABSORPTION BY THE EARTH'S ATMOSPHERE. A REPORT OF THE MOUNT WHITNEY EXPEDITION."

"If the observation of the amount of heat the sun sends the earth is among the most important and difficult in astronomical physics, it may also be termed the fundamental problem of meteorology, nearly all whose phenomena would become predictable, if we knew both the original quantity and kind of this heat; how it affects the constituents of the atmosphere on its passage earthward; how much of it reaches the soil; how, through the aid of the atmosphere, it maintains the surface temperature of this planet; and how, in diminished quantity and altered kind, it is finally returned to outer space.

. . . .

". . . . We are trying to estimate the amount of solar heat before absorption (the *solar constant*).

" Could we ascend above the atmosphere, this heat might be directly measured. Evidently, since this is impossible, and since we can only observe the portion which filters down to us after absorption, we must add to this observed remnant a quantity equal to that which the atmosphere has taken out, in order to reproduce the original amount.

" To find what it has taken out, we must study the action in detail, and, from the knowledge thus gained frame a rule or formula which shall enable us to infer the loss since we cannot directly determine it.

" It is because the exact determination of the solar constant thus presupposes a minute knowledge of the way in which the sun's heat is affected by the earth's atmosphere; and because every change in our atmosphere comes from this same heat, that the solution of the problem interests meteorology as well as astronomical physics.

". . . . Let us consider what the problem appears to be at a first glance, and what the first suggestion is for solving it. If a beam of sunlight enters through a crevice in a dark room, the light is partly interrupted by the dust particles in the air, the apartment is visibly illuminated by the light reflected from them, and the direct beam having lost something by this process, is not so bright after it has crossed the room as before it entered it. If a quarter of the light was thus scattered, and the beam after it crossed the room would be but three-fourths as bright as when it entered it, and if we were to trace the now diminished beam through a second apartment altogether like the other, it seems at first reasonable to suppose that the same proportion, or three-fourths of the remainder, would be transmitted, and so on, and that the light would be the same kind of light as before, and only diminished in amount. The assumption originally made by Bouguer [4] and followed by Herschel and Pouillet was that it was in this manner that the solar heat was interrupted by our atmosphere, and that by using such a simple progression the original heat could be calculated.

" Now, it is no doubt true that a very sensible portion of light and heat are scattered by an analogous process in our atmosphere; but we have in our present knowledge to consider that heat is not a simple emanation, but a compound of an infinite number of radiations, and that these are affected in an infinite diversity of ways by the different

[4] Bouguer, Traité de la lumière. Paris, 1760.

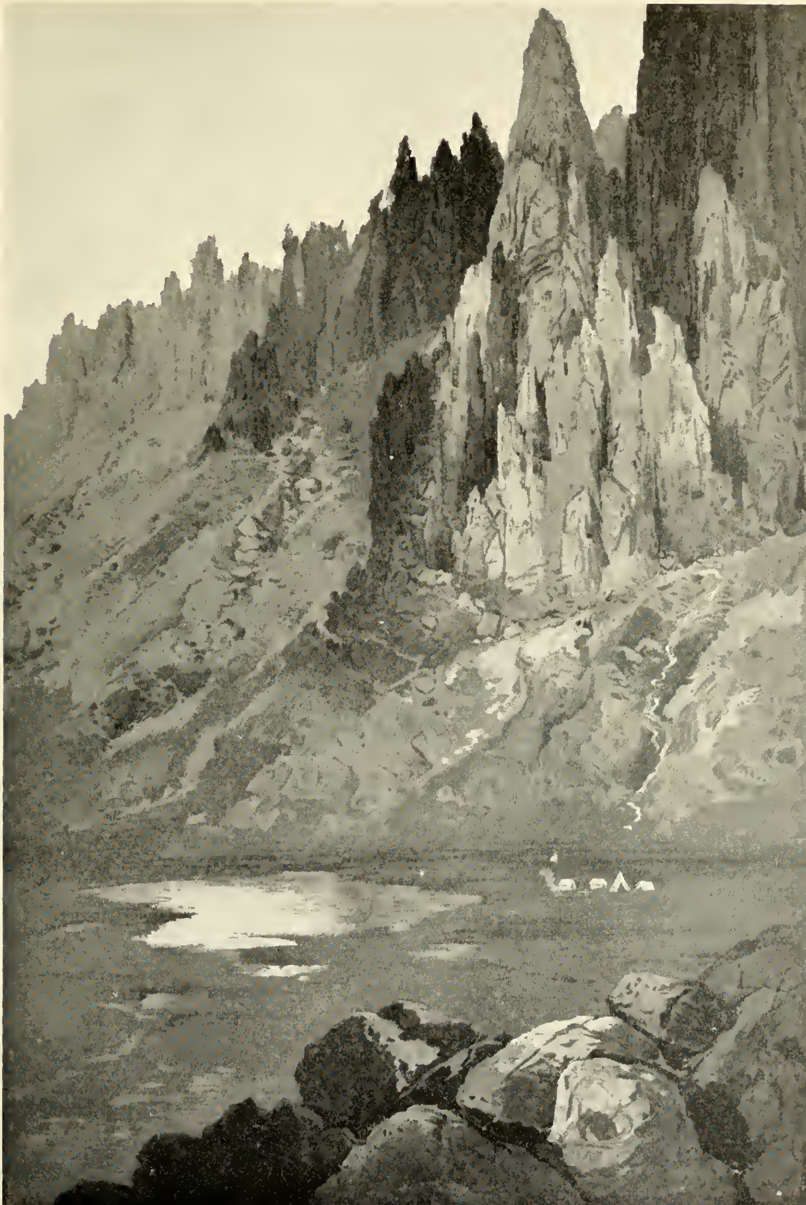
atmospheric agents, the grosser dust particles affecting them nearly all alike, or with a general absorption; the minuter ones beginning to act selectively, or, on the whole, more at one end of the spectrum than another; smaller particles, whether of dust or mist, and smaller still, forming a probably continuous sequence of more and more selective action down almost to the actual molecule, whose action is felt in the purely selective absorption of some single ray.

"The effect of the action of the grosser particles then is to produce a general and comparatively indifferent absorption of all rays, so that the spectrum after such an absorption would simply seem less bright or less hot. The effect of the smaller ones is, as has just been said, to act more at one end of the spectrum than another, with a progressive absorption, so that the quality of the radiation is sensibly affected as well as its quantity. The effect of the molecular absorption is to fill the spectrum with evidences of the selective action in the form of the dark telluric lines, taking out some kinds of light and heat and not others, so that after absorption what remains is not only less in amount but *quite altered in kind*. . . . .

"The writer has demonstrated that in neglecting to observe approximately homogeneous rays we not only commit an error, but an error which always has the same sign, and that the absorption thus found is always too small. He accordingly devoted much time to the construction of an instrument (the bolometer, which will be described in its place) for the special study of such heat rays, and, with this, observations were carried on in the years 1880 and 1881 at Allegheny, with the conclusions which have just been stated. With this instrument the heat in some approximately homogeneous ray (that is in some separate pencil of rays of nearly the same wave-length) is measured in the pure and normal spectrum at successive hours of the day, and the calculation of the absorption on Bouguer's principle (justly applicable to strictly homogeneous waves) gives the heat outside the atmosphere in this approximately homogeneous portion with a degree of approximation, depending on the actual minuteness of the part examined. The process is then repeated on another limited set of rays, and another, until the separate percentage and the separate original heat is found for each heat pencil directly or by interpolation, and then finally the whole heat, by the summing of its parts, the result being that the solar constant is much greater than it was believed to be, and the absorption of the atmosphere much greater.

. . . .

"Toward the close of 1880 it had already become clear that the gain in our knowledge by repeating the observations then in prog-



MOUNTAIN CAMP, MOUNT WHITNEY
From a sketch by T. Moran

ress at the Allegheny Observatory, at the base and at the summit of a lofty mountain, would justify the labor and expense of such an undertaking. There would have been little probability, however, of such a plan being carried out by the Observatory, were it not for the generosity of a citizen of Pittsburgh [William Thaw], who placed at its disposal the considerable means demanded for the outfit of an expedition for this purpose.

. . . .

"Upon the objects of the expedition and their bearings upon meteorology becoming known to the Chief Signal Officer of the United States Army, he consented to give it the advantage of his official direction and the aid of Signal Service Observers, and upon the reasons which made the choice of its objective point in a remote part of the United States territory being approved by him, he contributed further material aid in transportation. . . . . Finally, upon the advice of Mr. Clarence King, and with the concurrently favorable opinion of officers of the Coast Survey and others familiar with that region, Mount Whitney, in the Sierra Nevada Range of Southern California—approximate longitude, 118°30′ (7h.54m.); latitude, 36°35′—was found to be, on the whole, most desirable. Its height was known to be between 14,000 and 15,000 feet. Its eastern slopes are so precipitous that two stations can be found within 12 miles, visible from each other, and whose difference of elevation is 11,000 feet, and it rises from and overlooks one of the most desert regions of the continent, while its summit is almost perpetually clear during June, July, August, and September."

On account of limitations of space, it is impossible to give by quotations a fair idea of this extraordinary expedition. Space even forbids that we should quote from the inspiring description Langley gives of the expedition, its guard of soldiers, the desert journey, the insufferable heat under which observations were nevertheless made at Lone Pine, the ascent of the mountain, its grandeur, the dark blue of its cloudless sky, the long delays waiting for the mule train and instruments, and the observations at Mountain Camp.

Many kinds of observations were carried through. Measurements of total radiation of the solar beam by the globe and the Violle actinometers; measurements of homogeneous solar rays by the linear spectrobolometer; measurements of the brightness of the sky by day and by night; measurements of the temperature and humidity of the air at frequent intervals; barometric measurements for determining the then only approximately known elevation of Mount Whitney;

measurements of the percentage of carbonic acid in the atmosphere. Besides all these, even other types of measurements were made in profusion at Lone Pine, at Mountain Camp, and to some extent on the peak of Mount Whitney. The reduction of this immense mass of evidence was a task which occupied Langley's small force for two years, though it included the immortal Keeler and the assiduous Very. The great object was to determine the transparency of the atmosphere with such certainty, by these operations in one of the purest atmospheres of the world, as to fix the value of the solar constant of radiation. Langley thought to check the determination by computing from the results at Lone Pine what ought to be found on Mount Whitney. No less than a fifth of the atmosphere lay between these observing stations. Unfortunately Langley was misled by this apparently reasonable idea. For at Lone Pine he measured the average transparency for all atmospheric layers to the limit of the atmosphere, a transparency obviously greater than that of the more humid and dusty layers between him and Mountain Camp. He could not fairly use his average results at Lone Pine to compute, as he did, what ought to be observed at Mountain Camp. By this error of logic, aggravated by a moderate plus error in the absolute readings of his actinometers, Langley persuaded himself that the Mount Whitney Expedition indicated 3.07 calories per square centimeter per minute as the solar constant of radiation, a value more than 50 percent too high. His justly great authority maintained this erroneous value for more than 20 years.

But it is not this unfortunate aspect of the reduction of Mount Whitney observations, but the tremendous driving power and fertility of invention of this astonishing pioneer that should fix our attention. He practiced for the first time what the problem demanded, namely: occupation of a high-level desert station, observations of both total radiation and homogeneous rays, and their combination after a definite method. These essentials are still the basis of solar-constant work. He traced and accurately outlined the energy spectrum of the sun far beyond all previous observers. He obtained for the first time accurate transmission coefficients for homogeneous rays. In short, Langley by the Mount Whitney Expedition set up the ideal toward which all later observers strive to approximate.

"THE TEMPERATURE OF THE MOON"

"That the moon gives light, but no sensible heat, has been a matter of observation even by the unaided senses of the primitive man, and the idea that we should expect heat to be associated with the light seems to be essentially a modern one. This modern view, until very recently, has been that the light of the moon penetrated to us, while the rays which give only heat were kept back by our own atmosphere. Melloni, the most conspicuous early asserter of our present doctrine that radiant heat and light are but different manifestations of the same energy, was led to pursue his lunar heat work on Mount Vesuvius by these a priori considerations, and his perseverance was justified by obtaining finally most minute yet real indications of heat. Save the observations of Piassi Smyth on the Peak of Teneriffe, and of M. Marie-Davy in France, we shall find, however, that with the exception of Lord Rosse, of the persons who have sought to observe the heated moon, nearly all have left only records of failure or of purely imaginary and therefore misleading, successes.

"Lord Rosse's work excels greatly in importance that of his predecessors, as he not only obtained unquestionable evidence of lunar heat, but was able to make the important generalization that since a considerable part of this is intercepted by glass, a great deal of the moon's heat is probably radiated from her soil. As regards the temperature of the sunlit surface of the moon, Lord Rosse determined in his first paper that it ranges through 500° of the Fahrenheit scale; but in a subsequent memoir in the Philosophical Transactions of the Royal Society for 1873, this range is stated to be more nearly 200° Fahrenheit, a large error having crept into the previous work. The assiduous labor of observation and the instrumental means employed in these researches have acquired great and deserved repute; but few perhaps have noticed minutely that in the computation of the ratio of solar to lunar radiation, the error of assumption is made that all, or nearly all, of the invisible heat is stopped by glass, with other postulates equally inadmissible in the light of our present knowledge. We must, then, while rendering a tribute of respect and even admiration to the conscientious labors of observation and reduction point out that some of the values derived from them by their author must be revised, as resting on assumptions which the progress of science has contradicted.

. . . .

"In a previous memoir[5] we have given the results of various experiments in regard to the distribution of light in the lunar spectrum

[5] On the Temperature of the Surface of the Moon, Mem. Nat. Acad. Sci., vol. III.

together with bolometric measurements of the total lunar radiation and its transmission, which we here briefly summarize.

"Experiment showed that the moon sends us a little more than 1/100000 part of the heat which we receive from the sun. Of this lunar radiation we found at the beginning of December only 14 percent transmitted by a specimen of glass which allowed over 75 percent of the solar rays to pass. An ebonite disk, which was almost completely opaque to light, transmitted 32 percent of the solar and only 7 percent of the lunar radiation. Very little difference was found in the apparent transmission of the solar and of the lunar beam by the earth's atmosphere as inferred from comparisons at high and low altitudes above the horizon.

"Photometric spectral comparisons showed that sunlight is much richer in the violet rays than moonlight, indicating a selective reflection by the lunar surface, which, however, becomes less marked as the red end of the spectrum is approached.

"Comparisons, made in the month of December, 1884, between the total radiation of the moon and that from a blackened vessel of hot water, subtending the same angle, showed that the heating effect of the moon (as received through our absorbing atmosphere) could be replaced by the (unabsorbed) heat of a lamp-blacked surface at about +80°C., or 353°C. above absolute zero. A part of the lunar radiation is reflected from the sun and a part never reaches us, being absorbed by the atmosphere. Due allowance for the former would diminish, and for the latter would increase, the indicated lunar temperature; but owing to the selective character of the reflection to which we have already alluded, to our ignorance of the moon's emissive power, and to the fact that the radiations of our atmosphere itself are of a wave-length similar to a considerable part of those we now study, no precise deduction can be made. . . . .

"We have in the last three years pursued these researches with constantly improving instrumental means, and the following pages are a description of them and of the results. It will be seen that the great labor bestowed on them has been given, not to determine a point of abstract or merely theoretical interest, but that it is justified by the fact that the whole subject of terrestrial radiation, the temperature of the surface of our planet, and the conditions of organic life upon it are intimately related to that of our present research. The entire radiation of the soil of our earth towards space goes on in a spectral region of which we have hitherto known nothing. These observations, in connection with those recently published on invisible spectra and the wave-lengths of extreme infra-red rays[6] give us our first knowledge of this *terra incognita*. I say 'knowledge,' with the admission that this knowledge is as yet alloyed with those imperfections which are inherent in the most painstaking work in an utterly new field. All here is so new that the difficulties themselves are of a quite unfamiliar kind; for it is well to bear in mind that though all our observations, from first to last, are made on an amount of heat which may be well called infinitesimal, it is still the kind of radiations which produce this heat rather than the amount which forms the greatest difficulty. This, as we shall see, is because this heat seems to be largely that absorbed and reradiated from the substance of the lunar soil, and whose temperature is consequently so low as to be in constant danger of being confused with the heat from the terrestrial media it has passed and from the different parts of the apparatus itself—a difficulty which, when the thing in question is to ordinary sense both invisible and inappreciable, constitutes an obstacle almost insurmountable, when we design to go beyond those features which Lord Rosse succeeded in noting. We notice, in particular, that however successfully we may protect our apparatus from the radiations of surrounding objects, we must always, in the nature of the case, either actually or virtually, interpose a screen at intervals to interrupt the heat we are measuring. In ordinary spectrothermal work, as in that on the sun, the radiations of this screen are perfectly negligible, and would be so if the sun's heat, while the same in kind as now, were no greater in amount than the moon's. Here, on the contrary, because they are of the same *kind* as those radiated from the moon's cold surface, they become of the first importance, so that a special study of the radiation of the screen becomes a necessity.

. . . .

"There are three principal methods of investigation: First, the measurement of the total heat of the moon with a concave mirror of short focus, concentrating it as much as possible and admitting the interposition of a sheet of glass to rudely indicate the quality of lunar rays as compared with those of the sun. This method, which was that employed by Lord Rosse, has been very thoroughly practiced here with results which have been partly given in the previous memoir. The second method has been to form, usually with this same mirror, an image of the moon, but this now falls upon the slit of a special spectroscope

[6] See Am. Journ. of Sci., XXXII, August, 1886, 'On Hitherto Unrecognized Wave-Lengths'; also an article in Annales de Chim. et de Phys., 6 ser., T. IX, December, 1886, 'Sur les spectres invisibles.'

provided with a train of rock-salt lenses and a salt prism of exceptional size and purity; and after expanding this excessively minute heat in this way it has been found possible, with late improvements in the apparatus, to measure by the bolometer the different degrees of heat in the different parts of this lunar spectrum; and the doing of this, with its results, forms the principal subject of the present memoir. . . . . Third. Since such a mirror as that just mentioned, owing to its short focus, forms an extremely small lunar image, in certain observations, carried on, however, only during a limited time, we have taken advantage of the sensitiveness of our apparatus to explore a large lunar image with the bolometer in spite of the diminished heat in such a one. For this purpose a special mirror 303 mm in diameter and 3,137 mm focus, giving a lunar image of about 30 mm diameter, has been employed.[7] On the special occasion of a lunar eclipse the last-named apparatus has also been used.

. . . .

"Let it be remembered that every observation on radiant heat, however conducted, whether by the thermometer, the bolometer or thermopile, on the sun or moon, or on a neighboring candle—every observation in radiant heat, we repeat, involves the use of a screen at some stage in the process; since its use is inherent from the very nature of the observation. Again, let it be remembered that, in this peculiar case, the screen itself not only intercepts other rays, but contributes radiations of its own of like quality and amount to those which we would study, and the importance of the investigation to be shortly given on its theory becomes manifest. It will be seen later that the screen is used as little as possible, and that to this end every observation on the moon is preceded by one on the adjacent sky to the east and followed by one on the adjacent sky to the west; and that the lunar radiation is compared in every case immediately with the mean of the last two and only mediately with that of the screen, whose use we might here appear to be able to dispense with, but which is in fact imposed upon us, we repeat, at some time in the course of the observations by conditions inherent in the nature of the observations themselves.

. . . .

"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

[7] This special mirror has been kindly loaned to us by Mr. J. A. Brashear, of Allegheny.

the present means of science, and have reached no conclusions which we are absolutely sure of. As regards the main point, concerning the radiant heat of the moon, we know that it is divided into two salient kinds, reflected and emitted heat, and that the latter overlaps the former and extends probably between the deviation 40° of a rock-salt 60° prism (corresponding to $\lambda = 1^{\mu}.03$) and a deviation of over 33° in the extreme infra-red (λ=perhaps 50^{μ}). Contrary to all previous expectations, it 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° centigrade; but, it will be seen, the writer is sensible that this conclusion militates against one drawn by him from the Mount Whitney observations, according to which the soil of an airless planet at the moon's distance would have a temperature not greatly above −225°C. Great experimental labor on this expedition was expended in ascertaining the excess of temperature which a thermometer-bulb would attain in space at the earth's distance from the sun, which was found to be approximately 48° centigrade. From this observation, which appears to be quite trustworthy, the writer drew the inference that the sunward surface of an airless planet would be very greatly below the zero of the centigrade thermometer, and materially colder than the moon's surface appears by these observations to be. As between my observations and my inferences, I hold to the former; and since later and long-continued observations, of the character detailed in this volume, show that the temperature of the sunward surface of the moon (which is certainly nearly airless) is almost as certainly not greatly below zero, I have been led to believe myself mistaken in one of the inferences drawn from former experiments, in themselves exact, where this inference is not supported by these later observations.

. . . .

"Several methods have been tried for obtaining the ratio of the total radiation of the full moon to that of the sun, with results ranging from 1/70000 to 1/110000. The liability to error in the comparison of such diverse quantities is obvious; but a portion of the discrepancy is undoubtedly due to variation in the transmissibility of our atmosphere to the peculiar rays emitted by the moon.

. . . .

" From measures in different parts of the lunar image, we find that the rays absorbed by glass are present in greater proportion in the radiation coming from the dark areas, or so-called " seas ". . . . . The smaller radiation of the dark regions is presumably due to the presence of a larger proportion of those longer waves to which our atmosphere is partially opaque.

" Measurements in the lunar image during an eclipse of the moon showed a very rapid diminution of the heat as the eclipse progressed, a small amount (not over 2 percent, however) remaining in the umbra, of a quality to which glass was entirely opaque. The increment of the lunar radiation on the passing of the eclipse was apparently almost as rapid as its previous decrease.

" Less rapid than the change during an eclipse, but still strongly marked, is the transposition which occurs in the degree of heat observable at the east and west limbs, respectively, a few hours before and after the full. Thirty-six hours before the full the radiation of the west limb in terms of that from the central region of the moon was 0.958, that of the east limb being 0.574; while thirteen hours after the full the order was reversed, the west limb giving 0.611 and the east 0.727.

. . . .

" We next give the observations reduced to the full and to a mean distance, but uncorrected for atmospheric absorption, arranged according to the season in two groups, the object of this arrangement being to compare any systematic variation of the atmospheric absorption with the change of season. [Only the mean values given here.]

Lunar Spectrum—Winter Observations (November to April), Reduced to Full Moon and Mean Distance

No. ..	40°00′	39°45′	39°30′	39°15′	39°00′	38°45′	38°30′	38°15′	38°00′	37°45′	37°30
Mean..	16.9	15.6	17.4	16.1	15.3	14.1	11.4	12.4	24.1	39.2	48.8
No.		37°15′	37°00′	36°45′	36°30′	36°15′	36°00′	35°45′	35°30′	35°15′	35°00
Mean		43.0	36.0	38.4	32.2	25.9	25.6	21.8	18.3	17.4	11.6

Lunar Spectrum—Summer Observations (May to October), Reduced to Full Moon and Mean Distance

Mean..	15.7	22.5	19.9	17.8	15.0	6.5	4.8	4.2	10.1	30.5	35.7
Mean		41.0	37.1	33.9	20.4	27.4	15.5	17.2	13.0	10.7	9.7

" It will be seen from the above table and from the curves in Plate 11 that there is on the whole a slight increase in the atmospheric absorption in the summer. This increase would be still more marked if only the coldest and driest days of winter had been compared with the most humid of summer. . . . .

". . . . The most reliable spectrum comparisons with a blackened screen show an average 'effective lunar temperature' of +45.°C. near the time of full moon.

". . . . A measurement . . . . gives for the ratio of

$$\frac{\text{reflected radiation}}{\text{emitted radiation}} = 1 : 4.$$

This, it is to be remembered, is the ratio after absorption by the earth's atmosphere; but the extreme infra-red rays may have suffered unduly in passing this barrier. . . . ."

These researches on the temperature and spectrum of the moon entailed observations at Allegheny on more than 50 nights spread over the coldest of winter and the hottest of summer, as well as in months less trying to the observers, from October 1884 to February 1887. The spectrum observations alone, absolutely pioneering in character, of which only mean values are quoted here, occupied 22 nights, besides the preparation for them on uncounted days.

In order to avoid errors from the scattering of the more abundant rays of other wave lengths into the weaker regions observed in the lunar spectrum, Langley was obliged to use two spectroscopes in tandem, each employing a rock-salt prism because glass is opaque to such rays as are emitted by cool bodies like the moon. The common experience of the salt shaker at the dinner table has taught us how readily rock salt absorbs water. The slightest cloud of mist upon a rock-salt prism is prejudicial to its optical performance. It is easy to imagine, therefore, how often in summer the spectral observations of the moon were interrupted, and Dr. Langley's good friend Mr. Brashear came to the rescue by resurfacing the prisms.

"ON HITHERTO UNRECOGNIZED WAVE-LENGTHS"

" We are led to take this labor, not primarily to settle the theoretical questions involved in determining the relation between dispersion and wave-length (though these are most interesting), but with the object of providing a way which will hereafter enable any observer to determine the visible or invisible wave-lengths of any heat, whether from a celestial or terrestrial source, observed in any prism; and thus to gain that knowledge of the intimate constitution of radiant bodies which an acquaintance with the vibratory period of their molecules can usually alone afford us. It is this considerable end—the opening up to research of the whole unexplored region of infra-red energy, not only from celestial but from terrestrial sources—which will, we trust, justify the labor devoted to the following determinations."

He describes the arrangement of his apparatus, which includes a diffraction grating and a prism in tandem. A beam of radiation from the sun or the electric arc first traverses the diffraction grating spectroscope, whereby a group of rays of even multiples of the wave length of a certain selected visible ray are all concentrated upon the slit of the prismatic spectroscope. In the latter, the prismatic deviations are measured, and from them are readily computed the indices of refraction of each of these rays of selected wave lengths.

"There are in fact, passing through the same slit and lying superposed on one another by an unavoidable property of the grating, an infinite number of spectra in theory, of which in this case nearly twenty are actually recognizable, by photography, by the eye, or by the bolometer, and of which, to consider only those where the wave length is equal to or greater than that of the sodium line D_2[8] we have six spectra as follows:

					Wave length
a. (visible)	6th	spectrum		D_2..................	$\lambda = 0^{\mu}.5890$
b. "	5th	"	6/5	D_2..................	0 .7068
c. (invisible)	4th	"	6/4	D_2..................	0 .8835
d. "	3d	"	6/3	D_2..................	1 .1780
e. "	2d	"	6/2	D_2..................	1 .7670
f. "	1st	"	6	D_2..................	3 .5341

"It is in this invisible underlying first spectrum, buried, so to speak, beneath five others, of which three are themselves invisible also, that lies the wave-length we are seeking; consequently, there are (to consider no others) at least six qualities of heat, of six distinct refrangibilities, whose wave-lengths are equal to or greater than that of D_2, which pass simultaneously through the slit S_2. They pass through the prism, and on looking through a telescope occupying the position of the bolometer tube, we shall by suitably directing the arm of the spectroscope see the light from the sixth one at *a*. Its wave-length will be $0^{\mu}.5890$, corresponding to a measured deviation (in the case of the rock-salt prism, of an angle of 60°00′00″ and a temperature of 20°C.) of 41°05′40″. Now on replacing the telescope by the

[8] We have heretofore adopted Ångström's notation in calling the more refrangible sodium line 'D_1'. We shall hereafter, however, in conformity with the now more general usage, call this line, whose wave-length in Ångström is 5889, 'D_2'. The corrections to Ångström are due to the researches of Messrs. Peirce and Rowland.

bolometer, the bolometer wire will feel this same ray which the eye has just recognized by its light, and, if the galvanometer be in a sensitive condition, the image will be thrown by the heat off the scale, while a little on either side of this position no indication will be given. The beam and the slit S_2 remaining in the same position, let us next suppose that the bolometer arm is carried toward *b*, in the direction of B. There will be no sensible deflection until it reaches the position *b* in the red, corresponding to a wave-length of $0^{\mu}.7068$, and in the prism to an angle of $40°33'$ nearly, for there is no sensible heat except in the successive images of slit S_2 formed by the prism P in the line PB. Passing farther toward B we come into the heat in *c*, and next to the heat in *d* which is less than 1/100 that in the direct prismatic image, when no grating is employed.

"This was the utmost limit of our power of measurement in 1883, beyond this point radiations from the grating being then absolutely insensible, and the radiation at the point *d* itself being excessively minute, even in the solar spectrum, where the heat, so far as any is found, is as a rule far greater than that in the spectrum of the arc. Accordingly I have elsewhere observed that these measures could be carried on as well by a large electric arc as by the sun; but in fact, owing to the difficulties attendant on bringing the arc, which must be of immense heat, close to slit S_1, and to other causes, the sunlight would be preferable wherever it could be used.

"Our observation of June 7, 1882, gave the value of the index of refraction corresponding to $\lambda = 2^{\mu}.356$, which was the lowest possibly attainable by our then apparatus. Incessant practice and study, resulting in improvements already referred to, have enabled us finally to measure down to a wave-length of $9 \times \lambda D_2$ corresponding to a position much below *f*. We may add that in doing so, it is sometimes convenient to employ a bolometer wide enough to overlap the images in the other adjacent spectra of the higher orders, which we may usually do without confusing them, owing to their feebleness compared with that of the first spectrum in which we are searching.

"We usually, however, employ a bolometer of not more than 1 mm aperture, and this demands excessive delicacy in the heat-measuring apparatus, since the heat here is, approximately speaking, about 1/10000 of that in the region between the sodium lines in the direct spectrum of a rock-salt prism. This is near the limit of our present measuring powers with the grating, even when every possible device is used to increase the extremely feeble heat in this part of the spectrum.

"We commenced by using an electric arc with carbons 12 mm in diameter in the position indicated. These were supplied by an engine of three horse-power; but even in this case the pit of the crater did not nearly cover the very short slit (its length is 8 mm). For these last and most difficult measurements, we have been obliged to procure the use of an engine of twelve horse-power and carbons 25 mm (one inch) in diameter. With this enormous current the hottest part is not easily maintained in place. To keep it directly in front of the slit we have tried various plans, such as boring out the carbons lengthwise, so as to form hollow cylinders of them, and filling the core with a very pure carbon tempered to the requisite solidity. Ordinarily it will be sufficient however to first form the central crater by a drill. This gives us a persistent crater, whose light, in the position shown in the engraving, filled a slit whose vertical height is 8 mm. It is probably the intensest artificial heat ever subjected to analysis.

. . . .

"In the following brief table we have summarized the results of all this labor. Our working method gave the index in terms of the wave-length, but since ordinarily the former is the known, and the latter the unknown quantity, we here give the mean probable error as finally corrected as a function of the latter.

Given indices of refraction in rock-salt prism	Wave-lengths from direct observation (*a*) by the eye (*b*) by the bolometer
1.5442	$\lambda D_2 = 0^{\mu}.5890 \pm 0.000$ (*a*)
1.5301	$2 \times \lambda D_2 = 1\ .1780 \pm 0.002$ (*b*)
1.5272	$3 \times \lambda D_2 = 1\ .7670 \pm 0.005$ (*b*)
1.5254	$4 \times \lambda D_2 = 2\ .3560 \pm 0.009$ (*b*)
1.5243	$5 \times \lambda D_2 = 2\ .9451 \pm 0.013$ (*b*)
1.5227	$6 \times \lambda D_2 = 3\ .5341 \pm 0.019$ (*b*)
1.5215	$7 \times \lambda D_2 = 4\ .1231 \pm 0.029$ (*b*)
1.5201	$8 \times \lambda D_2 = 4\ .7121 \pm 0.043$ (*b*)
1.5186	$9 \times \lambda D_2 = 5\ .3011 \pm 0.065$ (*b*)"

Compared to our later determinations and those of Paschen, these observed indices of refraction of rock salt are found to differ but one or two units in the fourth place of decimals from the true values. To estimate the wave lengths of his lunar spectrum, Langley extrapolated, using the best formula then available. As this formula was erroneous for these great wave lengths, its results gave him exaggerated impressions of the greatness of the wave lengths he actually observed. For instance, in Appendix No. 1 of his paper "The Solar and Lunar Spectrum," he gives a wave length as 21.5 microns which, corrected by modern data, should read 10.7 microns. Similarly the latter values of the table which concludes "On Hitherto Unrecognized Wave-Lengths" are considerably too great.

ANNALS OF THE ASTROPHYSICAL OBSERVATORY OF THE SMITHSONIAN INSTITUTION, VOLUME 1

As indicated in the remarkable passage already quoted from the Mount Whitney Report, Langley's prophetic instinct told him that in the study of the sun's radiation rested the main hope of long-range weather forecasting. He moved toward the establishment of solar research at the Smithsonian Institution soon after becoming Secretary. He writes:

"This book is the result of a research originally due to a discovery, made in the year 1881 with the then newly invented bolometer, in the clear air of an altitude of over 12,000 feet, of solar heat in a then unknown spectral region now called the 'lower infra-red spectrum.' The bolometer has since been used to explore and to map the region in question, through the long succeeding intervals, in the latter part of which it has reached an accuracy and a sensitiveness greater than I could once have hoped for.

"This map is now (June 18, 1900), after years of constant work, finally published in the present form; not because this edition is final, but because the long labor must come to some term, and because I desire to see its results published while I may hope to see them made useful.

. . . .

"While we are far from looking forward to foretelling by such means the remoter changes of weather which affect the harvests, or to results of such importance as the power of such a prevision would indicate, still it is hardly too much to say that we appear to begin to move in that direction, and it seems to me that my own early hopes of making the study of the solar energy not simply an interesting scientific pursuit, but one of material usefulness, may one day be justified.

. . . .

"In the reports of the Secretary of the Smithsonian Institution for the years ending June 30, 1888 and 1889, mention is made of the hope then cherished of erecting and equipping an observatory for astrophysical research; and in the year following, 1890, he is at last 'able to say that this object has assumed definite shape in the construction of a temporary shed, begun on November 20, 1889 and . . . . completed about the 1st of March, 1890. This building is of the most inexpensive character, and is simply intended to protect the instruments temporarily, though it is also arranged so that certain preliminary work can be done here. Its position, however, immediately south of the main Smithsonian building, is not well suited to refined physical investigations, on account of its proximity to city streets and its lack of seclusion.'

. . . .

"The distinct object of astrophysics is, in the case of the sun, for example, not to mark its exact place in the sky, but to find out how it affects the earth and the wants of man on it; how its heat is distributed, and how it in fact affects not only the seasons and the farmer's crops, but the whole system of living things on the earth, for it has lately been proven that in a physical sense it, and almost it alone, literally first creates and then modifies them in almost every possible way.

"From the beginning of regular operations at the observatory in June, 1891, till the 1st of March, 1892, efforts were chiefly directed to getting the apparatus in satisfactory condition for observations. Much time was spent on the improvement of galvanometers, in testing bolometers and prisms, and in the determination of their constants.

"At length, on March 2, 1892, a 'rehearsal' occurred, in which the procedure followed in the bolometric investigations of the infra-red solar spectrum at Allegheny, already referred to on a previous page, was gone through with for the first time at the observatory. A second rehearsal occurred on the following day, and on reviewing it an entry was made by the writer March 4, 1892, in the record book in use by Dr. William Hallock, from which the following quotation is taken:

"I think your yesterday's spectral maps were quite successful for a first attempt—indeed, notably so, and give evidence of the goodness both of the system and of the instrumental means. The salient defect of the latter is in the 'drift' of the galvanometer, which, though reduced to limits which are insignificant compared to those which it had when I first began the study, is still a barrier to the best work.

"My idea (if drift could be eliminated) would be to have a vertical strip of sensitive paper rolled perpetually upward by a clockwork in the focus of the galvanometer mirror. The sides of this paper are marked in degrees and minutes, corresponding to divisions of the spectrometer circle, whose arm is moved by the same clockwork (through electric or other intermediary), so that when the circle is turned through n minutes of arc, the paper is moved upward linearly by a quantity corresponding to the same angular measure. A light is reflected from the mirror onto the paper, on which are traced the movements of the mirror due to the varying heat of the spectrum and to passing inequalities of the sky transmission. (The mirror movement has to be dampened so that there is no sensible swing.) The whole spectrum could be thus traversed in five minutes or less, as many as twelve curves could be taken in an hour, and a composite photograph would eliminate the accidental disturbances.

"All this implies that 'drift,' if not eliminated, is to be greatly reduced. Please consider this 'drift,' as well as the little movements of the needle due to changes in the apparatus itself, under these three heads:

(*a*) Changes due to alterations in the galvanometer.

(*b*) Changes due to alterations in the bolometer.

(*c*) Changes due to alterations in the battery, and all other sources.

"It seems quite certain that these are due largely to temperature.

. . . .

"*Our object hereafter is to map the lines.*"

Under his assistants, Hallock, Wadsworth, and R. C. Child, this program was so far fulfilled that in the year 1894 Langley exhibited at the Oxford meeting of the British Association for the Advancement of Science a map of the infrared solar spectrum as far as a wave length of 4.2 microns. This was based on automatic energy curves produced by continuous photographic records of the warming and cooling of the strip of the fine linear bolometer, as expressed in the swings of the sensitive galvanometer.

The present writer and his colleague, Mr. Fowle, continued this mapping of the infrared solar spectrum. Volume 1 of the Annals of the Astrophysical Observatory contained a discussion of the apparatus, a map of the infrared solar spectrum containing 579 lines and bands between wave lengths 0.76 and 5.3 microns, a highly accurate measurement of the dispersion of rock salt to 5.3 microns, and various subsidiary reports. The finest details of the infrared spectral map depended on a decision by the observers as to whether small nicks in the energy curves denoted solar or atmospheric absorption, or merely accidental error from shaking or electrical disturbance. This led Langley to what seemed to me the smoothest piece of dictation I ever heard. Unfortunately the stenographer was inexperienced, and it lost something before printing, even though Langley spent considerable time over it in manuscript and proofs. It is as follows:

"When we approach the limits of vision or audition, or of perception by any other of the human senses, no matter how these may be fortified by instrumental aid, we finally perceive, and always must perceive, a condition still beyond, where certitude becomes incertitude, although we may not be able to designate precisely where one ceases and the other begins.

"This is always the case, it would seem, on the boundaries of our knowledge in every department, and it is so here.

"It is impossible, for instance, to look at the great and notable deflection of a line such as *A*, or to the deflections corresponding to yet larger bands below it, and to see these in exactly the same place on scores of plates taken for years together without feeling an absolute certitude of their real existence as regions of special absorption in the solar or terrestrial atmosphere. After longer study it is found that as absolute a certainty exists as to many hundred smaller lines seen in the same conditions, and yet as we improve our apparatus and recognize still minuter solar deflections, we finally come to a condition where these are reduced to the same order of magnitude as those which may be due to earth tremors and to similar accidental disturbances, which are here represented by the irregular line which is called the ' battery record.'

" But, it may be asked, are we not entitled to demand that these last should somehow be eliminated altogether and the ' battery record ' become a perfectly smooth line? The answer is, that this can never be.

"As seismography improves, it becomes more clear that there is no part of the earth's surface free from constant tremor; as the refinements of electrical science advance we constantly discover earth currents where they were not perceived before; as we multiply the sensitiveness of our measuring apparatus, till it comes to what seems almost indefinite delicacy, we find that the most massive apparatus and the most refined precautions which we may take, do not prevent the existence of all but infinitesimally small accidental disturbances, nor of the notation of their sensible effects if the record itself be only minute enough, for this record is a testimony, in fact, to the sensitiveness of the apparatus itself, and minute disturbances are always to be found if the observation itself which deals with them provides in itself the means of detecting them.

" It fell to the writer once to establish a permanent meridian instrument whose supports he desired to build up with every condition of stability which experience and caution could suggest. He personally looked to the obtaining of the required blocks of granite at the quarry and to laying them in the same way in the foundation of the observatory on its bed rock as they lay in the original bed, and he superintended the placing of those, one upon the other, until the foundation was laid for the piers which finally supported the instrument, and which were chosen with the same care. He believed that this instrument was as solidly mounted as anything on the earth could be. He used it for many years in his observations with a confidence justified by the results; but these observations required a powerful telescope, and there was no time at which a tap of the fingers on the side of the monolothic piers which carried the telescope would not be accompanied by an apparent leap in the heavens of the star on which it was directed—a statement which will not surprise any professional astronomer. It is made here to emphasize the like statement that there is, then, no limit to our power of perception of tremors. These are, it will be remembered, instances which may be paralleled in illustrations drawn from the use of other senses, and not peculiar to the present observation.

"Clearly, we may never distinguish the entire number of solar lines which exist here more than we could in visible spectra by the use of the eye or by photography. In every case there must finally come a time when we must stop our investigations because we have reached a degree of minuteness in the solar lines corresponding to the intervening disturbances due to terrestrial causes, which we can never eliminate."

"ON A POSSIBLE VARIATION OF THE SOLAR RADIATION AND ITS PROBABLE EFFECT ON TERRESTRIAL TEMPERATURES"

This was Langley's last important paper. It was based on observations by Mr. Fowle and the present writer made at Washington. After long experience in far better observing locations we cannot suppose that the solar variations indicated in 1903 were real. Nevertheless they embarked us on a long endeavor to determine accurately the limits of the solar variation and its effects on weather. This investigation now [1934] seems certain to be of quite as great importance as Langley ever dreamed, for it gives promise of long-range weather forecasting, not only for seasons but for years in advance. But let us quote from the paper.

"The purpose of the present communication is primarily to discuss the validity of a surmise we may entertain, founded on observation here, as to certain possible changes in the solar constant. There is especially discussed a possible falling off of solar radiation about the close of March 1903, as indicated by certain recent values of solar radiation computed from observations here, and compared with actually observed temperatures for eighty-nine stations of the North Temperate Zone.

. . . .

"The homogeneous rays are observed here by the bolometer, and the bolographic curves from which the atmospheric extinction of radiation is inferred, traced by the movement of the spot of light upon the galvanometer scale, are now very much more satisfactory than formerly. They represent an immense gain over the conditions operating when I began the work at Allegheny. The light-spot should move only by an impulse from the Sun, but, owing to extraneous causes, it was at first frequently impossible to keep it upon the scale of the galvanometer during so short a time as a single minute. The apparatus now, however, operates so well that such drift and tremor is relatively unknown, and the zero of the galvanometer is found almost unchanged for weeks together. . . . ."

After discussing the methods of observing, the solar constant of radiation, and giving a table of 25 values of it observed at Washington in 1902, 1903, and 1904, Langley continues:

"Looking at the general results, these seem then to indicate a possibility that a rapid fall of solar radiation occurred about the close of March,[9] and that subsequently the radiation continued nearly or quite 10 percent less than before. This, if certain, would be important, and we may inquire what causes on the Sun could produce such a change, and what effects might be expected to be produced on the Earth if it occurred.

"The writer showed nearly thirty years ago[10] that the envelope of the Sun profoundly influences by its absorption the radiation received by the Earth. While the absorption in the solar envelope is not exactly known, still so much is known that we may infer that if it were absent for a moment the Earth would receive nearly double its present amount of heat. If a variation of 10 percent in the transparency of this envelope occurred, nearly 10 percent of change in the solar radiation outside the Earth's atmosphere would follow.

"If a fall of solar radiation did occur, there ought to have been a similar change of terrestrial temperatures afterward, and we may inquire how great this fall of temperature should be.

"The Earth may be regarded as a body at a mean temperature of 290° absolute (17°C.), maintained at approximately constant temperature by a balance between solar radiation received and terrestrial radiation emitted. It is here assumed that all sources of heat other than the solar radiation are negligible, but if any or all of them are not so, the effect of their presence will be to reduce the effect on temperature of a fall in solar radiation.

"Recent studies of German physicists have experimentally verified, for the perfect radiator, Stefan's law that the emission of a heated

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[9] It is of interest to note that a marked increase of Sun spots occurred on March 21. See Report of the Council, Monthly Notices of the Royal Astronomical Society, 64, 357.

[10] Comptes Rendus, 81, 436, Sept. 6, 1875.

body is proportional to the fourth power of the temperature.[11] Other bodies not perfect radiators depart from this law in the sense that, while radiating less absolutely than the perfect radiator, their emission is more nearly proportional to a power of the temperature higher than the fourth.[12] Suppose T_1 to be the mean temperature of the Earth corresponding to a rate of solar radiation S_1, and T_2, that corresponding to S_2. Assume further that the reflecting power of the Earth remains unchanged, and that no appreciable heat is received from other sources than the Sun. Then

$$\left(\frac{T_2}{T_1}\right)^x = \frac{S_2}{S_1}, \text{ where } x>4.$$

Accordingly if, as supposed, S_2 is $9/10\ S_1$,

$$T_2 > 0.974 T_1.$$

"If $T_1 = 290°$, then $T_2 > 282.°5$, and $T_1 - T_2 < 7°.5$C.

"It may then be stated that if the solar radiation remained for a long period of time at a value which would maintain the Earth's surface at a mean temperature of 17°C., and then fell 10 percent, and so remained indefinitely, the fall of temperature of the Earth's surface would be less than 7°.5C.

"But if the solar radiation fluctuated between limits separated by 10 percent, the fluctuation of terrestrial temperature would be less, according to the frequency of the fluctuations of solar radiation. Again, parts of the Earth's surface most closely associated with the oceans by the influences of winds, ocean currents, and rainfall would be least affected by such solar fluctuations, and would respond most slowly to a permanent alteration of solar radiation.

"From the foregoing considerations we may then infer that the effect of a fall of 10 percent in the solar radiation should diminish the mean temperature of the Earth not more than 7°.5C., and indefinitely less according to the shortness of the time elapsing before the radiation regained its former value. Stations near the sea, or subject to ocean currents and winds, or to heavy rainfall, would lag far behind stations in the interior of great continents in their temperature fluctuations.

"When we come to the study of actual temperatures over the Earth's surface, we find that all collections of temperature data for single stations in the interior of great continents, covering long periods

[11] O. Lummer, Rapports Présentés au Congrès International de Physique, 2, 78-81, 1900.

[12] H. Kayser, Handbuch der Spectroscopie, 2, 77-82.

of time, exhibit nearly every year such considerable irregular variations from the normal temperatures that we are at no loss to find variations comparable in dimensions with those we are supposing to be caused by a fluctuating solar radiation. But it is only within the last year that we have the series of radiation measures with which to compare temperatures, and we now turn to recent temperatures as published in the Internationaler Dekadenberichte of the Deutsche Seewarte for nearly one hundred stations, for each ten-day period of 1903, and accompanied by normal temperatures representing the mean for the same ten-day periods of many former years.[13] . . . .

" On comparing the observed temperatures of 89 stations, distributed over the North Temperate Zone, with the mean temperatures of the same stations for many previous years, it is found that an average decrease of temperature of over 2°C. actually did follow the possible fall of the solar radiation, while the temperature continued low during the remainder of the year. Stations remote from the retarding influence of the oceans show a much greater variation than that of the general mean.

" While it is difficult to conceive what influence, not solar, could have produced this rapid and simultaneous reduction of temperatures over the whole North Temperate Zone, and continued operative for so long a period, the evidence of solar variation cannot be said to be conclusive. Nevertheless, such a conclusion seems not an unreasonable inference from the data now at hand, and a continuation of these bolographic studies of solar radiation is of increasing interest, in view of their possible aid in forecasting terrestrial climatic changes, conceivably due to solar ones."

"EXPERIMENTS IN AERODYNAMICS"

We now turn from astronomy, Langley's primary field, to aviation, a subject which intrigued him from boyhood's days, and in which in his later years he made advances so great that he barely missed the goal of achieving human flight in heavier-than-air machines. While still at the Allegheny Observatory, he began experiments on the lift and resistance of rapidly moving surfaces in air, employing a whirling arm to carry them, and ingenious automatic instruments of his own design to record the results. This work he continued at Washington, resulting in a publication " Experiments in Aerodynamics."

[13] The writer is indebted to Professor Cleveland Abbe and to Dr. W. F. R. Phillips, librarian of the U. S. Weather Bureau, for their aid in making accessible the publications of temperature data in possession of the Weather Bureau.

"Schemes for mechanical flight have been so generally associated in the past with other methods than those of science, that it is commonly supposed the long record of failures has left such practical demonstration of the futility of all such hopes for the future that no one of scientific training will be found to give them countenance. While recognizing that this view is a natural one, I have, however, during some years, devoted nearly all the time at my command for research, if not directly to this purpose, yet to one cognate to it, with a result which I feel ought now to be made public.

. . . .

"Further than this, these new experiments, (and theory also when reviewed in their light,) show that if in such aerial motion, there be given a plane of fixed size and weight, inclined at such an angle, and moved forward at such a speed, that it shall be sustained in horizontal flight, then the more rapid the motion is, the less will be the power required to support and advance it. This statement may, I am aware, present an appearance so paradoxical that the reader may ask himself if he has rightly understood it. To make the meaning quite indubitable, let me repeat it in another form, and say that these experiments show that a definite amount of power so expended at any constant rate, will attain more economical results at high speeds than at low ones—*e. g.*, one horse-power thus employed, will transport a larger weight at 20 miles an hour than at 10, a still larger at 40 miles than at 20, and so on, with an increasing economy of power with each higher speed, up to some remote limit not yet attained in experiment, but probably represented by higher speeds than have as yet been reached in any other mode of transport—a statement which demands and will receive the amplest confirmation later in these pages.

. . . .

"The reader, especially if he be himself skilled in observation, may perhaps be willing to agree that since there is here so little yet established, so great a variety of tentative experiments must be made, that it is impossible to give each of them at the outset all the degree of accuracy which is ultimately desirable, and that he may yet find all trustworthy within the limits of their present application.

"I do not, then, offer here a treatise on aerodynamics, but an experimental demonstration that we already possess in the steam-engine as now constructed, or in other heat engines, more than the requisite power to urge a system of rigid planes through the air at a great velocity, making them not only self-sustaining, but capable of carrying other than their own weight. This is not asserting that they can be steadily and securely guided through the air, or safely brought to the ground without shock, or even that the plane itself is the best form of surface for support; all these are practical considerations of quite another order, belonging to the yet inchoate art of constructing suitable mechanisms for guiding heavy bodies through the air on the principles indicated, and which art (to refer to it by some title distinct from any associated with ballooning) I will provisionally call aerodromics.[14] With respect to this inchoate art, I desire to be understood as not here offering any direct evidence, or expressing any opinion other than may be implied in the very description of these experiments themselves.

. . . .

"The experiments in question, for obtaining first approximations to the power and velocities needed to sustain in the air such heavy inclined planes or other models in rapid movement, have been principally made with a very large whirling table, located on the grounds of the Allegheny Observatory, Allegheny, Pa. (lat. 40°27′41.6″; long. $5^h20^m2.93^s$; height above the sea-level, 1,145 feet).

"The site is a hill on the north of the valley of the Ohio and rising about 400 feet above it. At the time of these observations the hill-top was bare of trees and of buildings, except those of the observatory itself. . . . .

"The whirling table consists essentially of two symmetrical wooden arms, each 30 feet (9.15 meters) long, revolving in a plane eight feet above the ground. . . . . The whirling table was driven first by a gas-engine of about $1\frac{1}{2}$ horse-power, but it was found inadequate to do the work required, and, after October 20, 1888, a steam-engine giving 10 horse-power was used in its stead. . . . .

"This system gives for 120 revolutions of the steam-engine per minute, driving—

18 in. pulley, 48 revolutions of turn-table per minute = 100 + miles per hour at end of arm.

$25\frac{1}{2}$ in. pulley, 24 revolutions of turn-table per minute = 50 + miles per hours at end of arm.

36 in. pulley, 12 revolutions of turn-table per minute = 25 + miles per hour at end of arm.

"By regulating the speed of the engine any intermediate velocities can be obtained, and thus the equipment should be susceptible of furnishing speeds from 10 to 100 miles per hour (4.5 to 45 meters per second); but owing to the slipping of belts the number of turn-

[14] From ἀεροδρομέω, to traverse the air; ἀεροδρομος, an air-runner.

table revolutions was less than this for the higher velocities, so that the highest attained in the experiments did not reach this upper limit, but was a little over 100 feet (30 meters) per second, or about seventy miles per hour. The precise velocity actually attained by the turn-table is determined, quite independently of the speed of the engine, by an electrical registration on the standard chronograph in the observatory."

Langley devised ingenious recording instruments called the "suspended plane," the "resultant pressure recorder," the "plane dropper," the "component pressure recorder," the "dynamometer chronograph," the "counterpoised eccentric plane," and the "rolling carriage," all illustrated in the paper under discussion, and with these made many experiments.

"The most important general inference from these experiments, as a whole, is that, so far as the mere power to sustain heavy bodies in the air by mechanical flight goes, *such mechanical flight is possible with engines we now possess,* since effective steam-engines have lately been built weighing less than 10 pounds to one horse-power, and the experiments show that if we multiply the small planes which have been actually used, or assume a larger plane to have approximately the properties of similar small ones, one horse-power rightly applied, can sustain over 200 pounds in the air at a horizontal velocity of over 20 meters per second (about 45 miles an hour), and still more at still higher velocities. These numerical values are contained in the following table, repeated from p. 66. It is scarcely necessary to observe that the planes have been designedly loaded, till they weighed 500 grammes each, and that such a system, if used for actual flight, need weigh but a small fraction of this amount, leaving the rest of the sustainable weight indicated, disposable for engines and other purposes. I have found in experiment that surfaces approximately plane and of 1/10 this weight are sufficiently strong for all necessary purposes of support.

"Data for soaring of 30 × 4.8 inch planes; weight, 500 grammes

Angle with horizon α	Soaring speed V		Work expended per minute		Weight with planes of like form that 1 horse-power will drive through the air at velocity V	
	Meters per second	Feet per second	Kilogram-meters	Foot-pounds	Kilo-grammes	Pounds
45	11.2	26.7	336	2,434	6.8	15
30	10.6	34.8	175	1,268	13.0	29
15	11.2	36.7	86	623	26.5	58
10	12.4	40.7	65	474	34.8	77
5	15.2	49.8	41	297	55.5	122
2	20.0	65.6	24	174	95.0	209

" I am not prepared to say that the relations of power, area, weight, and speed, here experimentally established for planes of small area, will hold for indefinitely large ones ; but from all the circumstances of experiment, I can entertain no doubt that they do so hold far enough to afford assurance that we can transport (with fuel for a considerable journey and at speeds high enough to make us independent of ordinary winds), weights many times greater than that of a man.

" In this mode of supporting a body in the air, its specific gravity, instead of being as heretofore a matter of primary importance, is a matter of indifference, the support being derived essentially from the inertia and elasticity of the air on which the body is made to rapidly run. The most important and it is believed novel truth, already announced, immediately follows from what has been shown, that whereas in land or marine transport increased speed is maintained only by a disproportionate expenditure of power, within the limits of experiment in *such aerial horizontal transport, the higher speeds are more economical of power than the lower ones.*

" While calling attention to these important and as yet little known truths, I desire to add as a final caution, that I have not asserted that planes such as are here employed in experiment, or even that planes of any kind, are the best forms to use in mechanical flight, and that I have also not asserted, without qualification, that mechanical flight is practically possible, since this involves questions as to the method of constructing the mechanism, of securing its safe ascent and descent, and also of securing the indispensable condition for the economic use of the power I have shown to be at our disposal—the condition, I mean, of our ability to guide it in the desired horizontal direction during transport—questions which, in my opinion, are only to be answered by further experiment and which belong to the inchoate art or science of *aerodromics* on which I do not enter.

" I wish, however, to put on record my belief that the time has come for these questions to engage the serious attention, not only of engineers, but of all interested in the possibly near practical solution of a problem, one of the most important in its consequences, of any which has ever presented itself in mechanics; for this solution, it is here shown, cannot longer be considered beyond our capacity to reach."

The data secured by these experiments have long since been superseded by more accurate observations in modern wind tunnels. Even the conclusions would not now all be considered sound. For instance, " Langley's law," that the more rapid the horizontal flight the less is the power required for support and advance, does not hold for speeds much higher than those he tried. His assumption that skin friction is negligible is also invalid at higher speeds. But a great impetus to aviation was given by the fact that so great a scientist as Langley had devoted himself to a subject which was generally regarded then as the refuge of cranks, nearly in the same class with perpetual motion.

Langley's meditations on soaring flight of birds led in 1893 to his brilliant paper:

"THE INTERNAL WORK OF THE WIND"

"It has long been observed that certain species of birds maintain themselves indefinitely in the air by 'soaring,' without any flapping of the wing, or any motion other than a slight rocking of the body; and this, although the body in question is many hundred times denser than the air in which it seems to float with an undulating movement, as on the waves of an invisible stream.

"No satisfactory mechanical explanation of this anomaly has been given, and none would be offered in this connection by the writer, were he not satisfied that it involves much more than an ornithological problem, and that it points to novel conclusions of mechanical and utilitarian importance. They are paradoxical at first sight, since they imply that, under certain specified conditions, very heavy bodies entirely detached from the earth, immersed in, and free to move in, the air, can be sustained there indefinitely, without any expenditure of energy from within.

"These bodies may be entirely of mechanical construction, as will be seen later, but for the present we will continue to consider the character of the invisible support of the soaring bird, and to study its motions, though only as a pregnant instance offered by Nature to show that a rational solution of the mechanical problem is possible.

"Recurring, then, to the illustration just referred to, we may observe that the flow of an ordinary river would afford no explanation of the fact that nearly inert creatures, while free to move, although greatly denser than the fluid, yet float upon it; which is what we actually behold in the aerial stream, since the writer, like others, has satisfied himself, by repeated observation, that the soaring vultures and other birds appear as if sustained by some invisible support, in the stream of air, sometimes for at least a considerable fraction of an hour. It is frequently suggested by those who know these facts only from books, that there must be some quivering of the wings, so rapid as to escape observation. Those who do know them from observation, are aware that it is absolutely certain that nothing of the kind takes place, and that the birds sustain themselves on pinions which are quite rigid and motionless, except for a rocking or balancing movement involving little energy.

"The writer desires to acknowledge his indebtedness to that most conscientious observer, M. Mouillard,[15] who has described these actions of the soaring birds with incomparable vividness and minuteness, and who asserts that they, under certain circumstances, without flapping their wings, rise and actually advance against the wind.

"To the writer, who has himself been attracted from his earliest years to the mystery which has surrounded this action of the soaring bird, it has been a subject of continual surprise that it has attracted so little attention from physicists. That nearly inert bodies, weighing from 5 to 10, or even more, pounds, and many hundred times denser than the air, should be visibly suspended in it above our heads, sometimes for hours at a time, and without falling—this, it might seem, is, without misuse of language to be called a physical miracle; and yet, the fact that those whose province it is to investigate nature, have hitherto seldom thought it deserving attention, is perhaps the greater wonder.

". . . . The common 'Turkey Buzzard' (*Cathartes aura*) is so plenty around the environs of Washington that there is rarely a time when some of them may not be seen in the sky, gliding in curves over some attractive point, or, more rarely moving in nearly straight lines on rigid wings, if there be a moderate wind. On the only occasion when the motion of one near at hand could be studied in a very high wind, the author was crossing the long 'Aqueduct Bridge' over the Potomac, in an unusually violent November gale, the velocity of the wind being probably over 35 miles an hour. About one-third of the distance from the right bank of the river, and immediately over the right parapet of the bridge, at a height of not over 20 yards, was one of these buzzards, which, for some object which was not evident, chose to keep over this spot, where the gale, undisturbed by any surface irregularities swept directly up the river with unchecked violence. In this aerial torrent, and apparently indifferent to it, the bird hung, gliding, in the usual manner of its species, round and round in a small oval curve whose major axis (which seemed toward the wind) was not longer than twice its height from the water. The bird was therefore at all times in close view. It swung around repeatedly, rising and falling slightly in its course, while keeping, as a whole, on one level, and over the same place, moving with a slight swaying both in front and

[15] L. P. Mouillard, L'Empire de l'Air, Paris: G. Masson.

lateral direction but in such an effortless way as suggested a lazy yielding of itself to the rocking of some invisible wave.

"It may be asserted that there was not only no flap of the wing, but not the quiver of a wing feather visible to the closest scrutiny, during the considerable time the bird was under observation, and during which the gale continued. A record of this time was not kept, but it at any rate lasted until the writer, chilled by the cold blast, gave up watching and moved away, leaving the bird still floating, about at the same height in the torrent of air, in nearly the same circle, and with the same aspect of indolent repose.

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"Light came to him through one of those accidents which are commonly found to occur when the mind is intent on a particular subject, and looking everywhere for a clue to its solution.

"In 1887, while engaged with the 'whirling-table' in the open air at the Allegheny Observatory, he had chosen a quiet afternoon for certain experiments, but in the absence of the entire calm which is almost never realized, had placed one of the very small and light anemometers made for hospital use, in the open air, with the object of determining and allowing for the velocity of what feeble breeze existed. His attention was called to the extreme irregularity of this register, and he assumed at first that the day was more unfavorable than he had supposed. Subsequent observations, however, showed that when the anemometer was sufficiently light and devoid of inertia, the register always showed great irregularity, especially when its movements were noted, not from minute to minute, but from second to second.

"His attention once aroused to these anomalies, he was led to reflect upon their extraordinary importance in a possible mechanical application. He then designed certain special apparatus hereafter described, and made observations with it which showed that 'wind' in general was not what it is commonly assumed to be, that is, air put in motion with an approximately uniform velocity in the same strata; but that, considered in the narrowest practicable sections, wind was always not only not approximately uniform, but variable and irregular in its movements beyond anything which had been anticipated, so that it seemed probable that the very smallest part observable could not be treated as approximately homogeneous, but that even here, there was an internal motion to be considered, distinct both from that of the whole body, and from its immediate surroundings. It seemed to the writer to follow as a necessary consequence, that there might be a potentiality of what may be called 'internal work' [16] in the wind.

"On further study it seemed to him that this internal work might conceivably be so utilized as to furnish a power which should not only keep an inert body from falling, but cause it to rise, and that while this power was the probable cause of the action of the soaring bird, it might be possible through its means to cause any suitably disposed body, animate or inanimate, wholly immersed in the wind, and wholly free to move, to advance against the direction of the wind itself. By this it is not meant that the writer then devised means for doing this but that he then attained the conviction both that such an action involved no contradiction of the laws of motion, and that it was mechanically possible (however difficult it might be to realize the exact mechanism by which this might be accomplished)."

He then goes on with experiments made with extremely light and sensitive anemometers to show that the apparently continuous flow of a wind is in reality made up of an extreme contrariety of gusts, capable, if they could be taken advantage of, not only of supporting a body in air, but even of causing it to rise and advance against the general direction of the wind.

"From this, then, we may now at least see that it is plainly within the capacity of an intelligence like that suggested by Maxwell, and which Lord Kelvin has called the 'Sorting Demon,' to pick out from the internal motions those whose direction is opposed to the main current, and to omit those which are not so, and thus without the expenditure of energy to construct a force which will act against the main current itself.

"But we may go materially further, and not only admit that it is not necessary to invoke here, as Maxwell has done in the case of thermodynamics, a being having a power and rapidity of action far above ours, but that, in actual fact, a being of a lower order than ourselves, guided only by instinct may so utilize these internal motions.

"We might not indeed have conceived this possible, were it not that nature has already, to a large extent, exhibited it before our eyes in the soaring bird, which sustains itself endlessly in the air with nearly

[16] Since the term "internal work" is often used in thermo-dynamics to signify molecular action, it may be well to observe that it here refers not to molecular movements, but to pulsations of sensible magnitude, always existing in the wind, as will be shown later, and whose extent and extraordinary possible mechanical importance it is the object of this research to illustrate. The term is so significant of the author's meaning that he permits himself the use of it here, in spite of the possible ambiguity.

motionless wings, for without this evidence of the possibility of action which now ceases to approach the inconceivable, we are not likely, even if admitted its theoretical possibility, to have thought the mechanism solution of this problem possible. But although to show how this physical miracle of nature is to be imitated, completely and in detail, may be found to transcend any power of analysis, I hope to show, that this may be possible without invoking the asserted power of 'Aspiration' relative to curved surfaces, or the trend of upward currents, and even to indicate the probability that the mechanical solution of this problem may not be beyond human skill.

. . . .

"Let me resume the leading points of the present memoir in the statement that it has been shown:

"(1) That the wind is not even an approximately uniform moving mass of air, but consists of a succession of very brief pulsations of varying amplitude, and that, relatively to the mean movement of the wind, these are of varying direction.

"(2) That it is pointed out that hence there is a potentiality of 'internal work' in the wind, and probably of a very great amount.

"(3) That it involves no contradiction of known principles to declare that an inclined plane or suitably curved surface, heavier than the air, freely immersed in, and moving with the velocity of the mean wind, can, if the wind pulsations here described are of sufficient amplitude and frequency, be sustained or even raised indefinitely without expenditure of internal energy, other than that which is involved in changing the aspect of its inclination at each pulsation.

"(4) That since (A) such a surface, having also power to change its inclination, *must* gain energy through falling during the slower, and expend energy by rising during the higher, velocities; and that (B) since it has been shown that there is no contradiction of known mechanical laws in assuming that the surface *may* be sustained or may continue to rise indefinitely, the mechanical possibility of some advance against the direction of the wind follows immediately from this capacity of rising. It is further seen that it is at least possible that this advance against the wind may not only be attained relatively to the position of a body moving with the speed of the mean wind, but absolutely, and with reference to a fixed point in space.

"(5) The statement is made that this is not only mechanically possible, but that, in the writer's opinion, it is realizable in practice.

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"The final application of these principles to the art of aerodromics seems then to be that, while it is not likely that the perfected aerodrome will ever be able to dispense altogether with the ability to rely at intervals on some internal source of power, it will not be indispensable that this aerodrome of the future shall, in order to go any distance—even to circumnavigate the globe without alighting,—need to carry a weight of fuel which would enable it to perform this journey under conditions analogous to those of a steamship, but that the fuel and weight need only be such as to enable it to take care of itself in exceptional moments of calm."

How plainly here does Langley foreshadow the achievements of gliding a third of a century later.

After completing the two papers just referred to, Langley proceeded to use the data gained in a serious attempt to obtain mechanical flight with large heavier-than-air machines. After several years of experimentation in which not only the difficulties of light construction and automatic balance but also of the invention of a very light steam engine were overcome, Langley on May 6, 1896, in the presence of Alexander Graham Bell and others, successfully catapulted from a houseboat on the Potomac a 13-foot steam-powered model which flew over one-half mile and landed softly unharmed upon the water. In November of the same year, another large model made an even longer flight of three-quarters of a mile. Of these experiments Langley said[17]:

" I have thus far had only a purely scientific interest in the results of these labors. Perhaps if it could have been foreseen at the outset how much labor there was to be, how much of life would be given to it, and how much care, I might have hesitated to enter upon it at all. And now reward must be looked for, if reward there be, in the knowledge that I have done the best I could in a difficult task, with results which it may be hoped will be useful to others. I have brought to a close the portion of the work which seemed to be specially mine—the demonstration of the practicability of mechanical flight—and for the next stage, which is the commercial and practical development of the idea, it is probable that the world may look to others. The world, indeed, will be supine if it do not realize that a new possibility has come to it, and that the great universal highway overhead is now soon to be opened."

"EXPERIMENTS WITH THE LANGLEY AERODROME"

" The experiments undertaken by the Smithsonian Institution upon an aerodrome, or flying machine, capable of carrying a man have been

[17] The Langley Aerodrome, Ann. Rep. Smithsonian Inst., 1900, p. 197, 1901.

Langley Model No. 5 in Flight, May 6, 1896

suspended from lack of funds to repair defects in the launching apparatus without the machine ever having been in the air at all. As these experiments have been popularly, and of late repeatedly, represented as having failed on the contrary, because the aerodrome could not sustain itself in the air I have decided to give this brief though late account, which may be accepted as the first authoritative statement of them.

"It will be remembered that in 1896 wholly successful flights of between one-half and one mile by large steam-driven models, unsupported except by the mechanical effects of steam engines, had been made by me. In all these the machine was first launched into the air from 'ways,' somewhat as a ship is launched into the water, the machine resting on a car that ran forward on these ways, which fell down at the extremity of the car's motion, releasing the aerodrome for its free flight. I mention these details because they are essential to an understanding of what follows, and partly because their success led me to undertake the experiments on a much larger scale I now describe.

"In the early part of 1898 a board, composed of officers of the Army and Navy, was appointed to investigate these past experiments with a view to determining just what had been accomplished and what the possibilities were of developing a large-size man-carrying machine for war purposes. The report of this board being favorable, the Board of Ordnance and Fortification of the War Department decided to take up the matter, and I having agreed to give without compensation what time I could spare from official duties, the Board allotted $50,000 for the development, construction, and test of a large aerodrome, half of which sum was to be available immediately and the remainder when required. The whole matter had previously been laid before the Board of Regents of the Smithsonian Institution who had authorized me to take up the work and to use in connection with it such facilities of the Institution as were available.

"Before consenting to undertake the construction of this large machine, I had fully appreciated that owing to theoretical considerations, into which I do not enter, it would need to be relatively lighter than the smaller one; and later it was so constructed, each foot of sustaining surface in the large machine carrying nearly the same weight as each foot in the model. The difficulties subsequently experienced with the larger machine were, then, due not to this cause, but to practical obstacles connected with the launching, and the like.

"I had also fully appreciated the fact that one of the chief difficulties in its construction would lie in the procuring of a suitable engine of sufficient power and, at the same time, one which was light enough. (The models had been driven by steam engines whose water supply weighed too much for very long flights.) The construction of the steam engine is well understood, but now it would become necessary to replace this by gas engines, which for this purpose involve novel difficulties. I resolved not to attempt the task of constructing the engine myself, and had accordingly entered into negotiations with the best engine builders in this country, and after long delay had finally secured a contract with a builder who, of all persons engaged in such work, seemed most likely to achieve success. It was only after this contract for the engine had been signed that I felt willing to formally undertake the work of building the aerodrome.

"The contract with the engine builder called for an engine developing 12 brake horsepower, and weighing not more than 100 pounds, including cooling water and all other accessories, and with the proviso that a second engine, exactly like this first one, would be furnished on the same terms. The first engine was to be delivered before the close of February, 1899, and the frame of the aerodrome with sustaining surfaces, propellers, shafting, rudders, etc., was immediately planned, and now that the engine was believed to be secured, their actual construction was pushed with the utmost speed. The previous experiments with steam-driven models which had been so successful, had been conducted over the water, using a small houseboat having a cabin for storing the machine, appliances and tools, on top of which was mounted a track and car for use in launching. As full success in launching these working models had been achieved after several years spent in devising, testing and improving this plan, I decided to follow the same method with the large machine, and accordingly designed and had built a house boat, in which the machine could not only be stored, but which would also furnish space for workshops, and on the top of which was mounted a turntable and track for use in launching from whatever direction the wind might come.

"Everything connected with the work was expedited as much as possible with the expectation of being able to have the first trial flight before the close of 1899, and time and money had been spent on the aerodrome, which was ready, except for its engine, when the time for the delivery of this arrived. But now the builder proved unable to complete his contract, and, after months of delay, it was necessary to decrease the force at work on the machine proper and its launching appliances until some assurance could be had of the final success of the engine. . . . .

"It was recognized from the very beginning that it would be desirable in a large machine to use 'superposed' sustaining surfaces (that is, with one wing above another) on account of their superiority so far as the relation of strength to weight is concerned, and from their independence of guy wiring; and two sets of superposed sustaining surfaces of different patterns were built and experimented with in the early tests. These surfaces proved, on the whole, inferior in lifting power, though among compensating advantages are the strength of a bridge construction which dispenses with guy wires coming up from below, which, in fact, later were the cause of disaster in the launching.

"It was finally decided to follow what experiment had shown to be successful, and to construct the sustaining surfaces for the large machine after the 'single-tier' plan. This proved to be no easy task, since in the construction of the surfaces for the small machines the main and cross ribs of the framework had been made solid, and, after steaming, bent and dried to the proper curvature, while it was obvious that this plan could not be followed in the large surfaces on account of the necessity, already alluded to, of making them relatively lighter than the small ones, which were already very light. After the most painstaking construction, and tests of various sizes and thicknesses of hollow square, hollow round, I-beam, channel, and many other types of ribs, I finally devised a type which consisted of a hollow box form, having its sides of tapering thickness, with the thickest part at the point midway between contiguous sides and with small partitions placed inside every few inches in somewhat the same way that nature places them in the bamboo. These various parts of the rib (corresponding to the quill in a wing) were then glued and clamped together, and after drying were reduced to the proper dimensions and the ribs covered with several coats of a special marine varnish, which it had been found protected the glued joints from softening, even when they were immersed in water for twenty-four hours.

"Comparative measurements were made between these large cross ribs, 11 feet long, and a large quill from the wing of a harpy eagle, which is probably one of the greatest wonders that nature has produced in the way of strength for weight. These measurements showed that the large, 11-foot ribs ('quills') for the sustaining surfaces of the large machine were equally as strong, weight for weight, as the quill of the eagle; but much time was consumed in various constructions and tests before such a result was finally obtained.

" During this time a model of the large machine, one-fourth of its linear dimensions, was constructed, and a second contract was made for an engine for it. The delay with the large engine was repeated with the small one, and in the spring of 1900 it was found that both contract engines were failures for the purpose for which they were intended, as neither one developed half of the power required for the allotted weight.

" I accordingly again searched all over this country, and, finally, accompanied by an engineer (Mr. Manly), whose services I had engaged, went to Europe, and there personally visited large builders of engines for automobiles, and attempted to get them to undertake the construction of such an engine as was required. This search, however, was fruitless, as all of the foreign builders, as well as those of this country, believed it impossible to construct an engine of the necessary power and as light as I required (less than 10 pounds to the horsepower without fuel or water). I was therefore forced to return to this country and to consent most reluctantly, even at this late date, to have the work of constructing suitable engines undertaken in the shops of the Smithsonian Institution, since, as I have explained, the aerodrome frame and wings were already constructed. This work upon the engines began here in August, 1900, in the immediate care of Mr. Manly. These engines were to be of nearly double the power first estimated and of little more weight, but this increased power and the strain caused by it demanded a renewal of the frame as first built, in a stronger and consequently in a heavier form, and the following sixteen months were spent in such a reconstruction simultaneously with the work on the engines.

" The flying weight of the machine complete, with that of the aeronaut, was 830 pounds; its sustaining surface, 1,040 square feet. It therefore was provided with slightly greater sustaining surface and materially greater relative horsepower than the model subsequently described which flew successfully. The brake horsepower of the engine was 52; the engine itself, without cooling water, or fuel, weighed approximately 1 kilogram to the horsepower. The entire power plant, including cooling water, carburetor, battery, etc., weighed materially less than 5 pounds to the horsepower. Engines for both the large machine and the quarter-size model were completed before the close of 1901, and they were immediately put in their respective frames and tests of them and their power-transmission appliances were begun.

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"The engines themselves were successfully completed before the close of 1901, and were of much more power than those originally designed; but nearly a year and a half had been spent not only in their completion, but in properly coordinating the various parts of the frame carrying them, repairing the various breakages, assembling, dismounting, and reassembling the various parts of the appliances, and in general rebuilding the frame and appurtenances to correspond in strength to the new engines.

"There are innumerable other details, for the whole question is one of details. . . . .

"It is impossible for anyone who has not had experience with such matters to appreciate the great amount of delay which experience has shown is to be expected in such experiments. Only in the spring of 1903, and after two unforeseen years of assiduous labor, were these new engines and their appurtenances, weighing altogether less than 5 pounds to the horsepower and far lighter than any known to be then existing, so coordinated and adjusted that successive shop tests could be made without causing injury to the frame, its bearings, shafts, or propellers.

"And now everything seemed to be as nearly ready for an experiment as could be, until the aerodrome was at the location at which the experiments were to take place. The large machine and its quarter-size counterpart were accordingly placed on board the large house boat, which had been completed some time before and had been kept in Washington as an auxiliary shop for use in the construction work, and the whole outfit was towed to a point in the Potomac River, here 3 miles wide, directly opposite Widewater, Va., and about 40 miles below Washington and midway between the Maryland and Virginia shores, where the boat was made fast to moorings which had previously been placed in readiness for it.

"Although extreme delays had already occurred, yet they were not so trying as the ones which began immediately after the work was thus transferred to the lower Potomac.

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"In order to test the quarter-size model it was necessary to remove its launching track from the top of the small house boat and place it upon the deck of the large boat, in order to have all the work go on at one place, as it was impossible, on account of its unseaworthiness, to moor the small house boat in the middle of the river.

". . . . These difficulties might have partly been anticipated, but there were others concerning which the cause of the deterioration and disarrangement of certain parts and adjustments was not immediately detected, and consequently when short preliminary shop tests of the small machine were attempted just prior to launching it, it was found that the apparatus did not work properly, necessitating repairs and new constructions and consequent delay. Although the large house boat with the entire outfit had been moved down the river on July 14, 1903, it was not until the 8th of August that the test of the quarter-size model was made, and all of this delay was directly due to changed atmospheric conditions incident to the change in locality. This test of the model in actual flight was made on the 8th of August, 1903, when it worked most satisfactorily, the launching apparatus, as always heretofore, performing perfectly, while the model, being launched directly into the face of the wind, flew directly ahead on an even keel. The balancing proved to be perfect, and the power, supporting surface, guiding, and equilibrium-preserving effects of the rudder also. The weight of the model was 58 pounds, its sustaining surface 66 square feet, and the horsepower from $2\frac{1}{2}$ to 3.

" This was the first time in history, so far as I know, that a successful flight of a mechanically sustained flying machine was made in public.

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" I have spoken of the serious delays in the test of the small machine caused by changed atmospheric conditions, but they proved to be almost negligible compared with what was later experienced with the large one. . . . .

". . . . . Something of the same troubles which had been met with in the disarrangement of the adjustments of the small engine was experienced in the large one, although they occurred in such a different way that they were not detected until they had caused damage in the tests, and these disarrangements were responsible for broken propellers, twisted shafts, crushed bearings, distorted framework, etc., which were not finally overcome until the 1st of October. After again getting everything in apparent readiness there then ensued a period of waiting on the weather until the 7th of October (1903), when it became sufficiently quiet for a test which I was now beginning to fear could not be made before the following season. In this, the first test, the engineer took his seat, the engine started with ease and was working without vibration at its full power of over 50 horse, and the word being given to launch the machine, the car was released and the aerodrome sped along the track. Just as the machine left the track, those who were watching it, among whom were two representatives of the Board of Ordnance,[18] noticed that the machine was jerked violently down at the front (being caught, as it subsequently appeared, by the falling ways),[19] and under the full power of its engine was pulled into the water, carrying with it its engineer. When the aerodrome rose to the surface it was found, that while the front sustaining surfaces had been broken by their impact with the water, yet the rear ones were comparatively uninjured. As soon as a full examination of the launching mechanism had been made, it was found that the front portion of the machine had caught on the launching car, and that the guy post, to which were fastened the guy wires which are the main strength of the front surfaces, had been bent to a fatal extent.

"The machine, then, had never been free in the air, but had been pulled down as stated.

"The disaster just briefly described had indefinitely postponed the test, but this was not all. As has been said before, the weather had become very cold and the so-called equinoctial storms being near it was decided to remove the house boat at the earliest time possible, but before it could be done, a storm came up and swept away all the launches, boats, rafts, etc., and in doing so completely demolished the greater part of them, so that when the house boat was finally removed to Washington, on the 15th of October, these appurtenances had to be replaced. It is necessary to remember that these long series of delays worked other than mere scientific difficulties, for a more important and more vital one was the exhaustion of the financial means for the work.

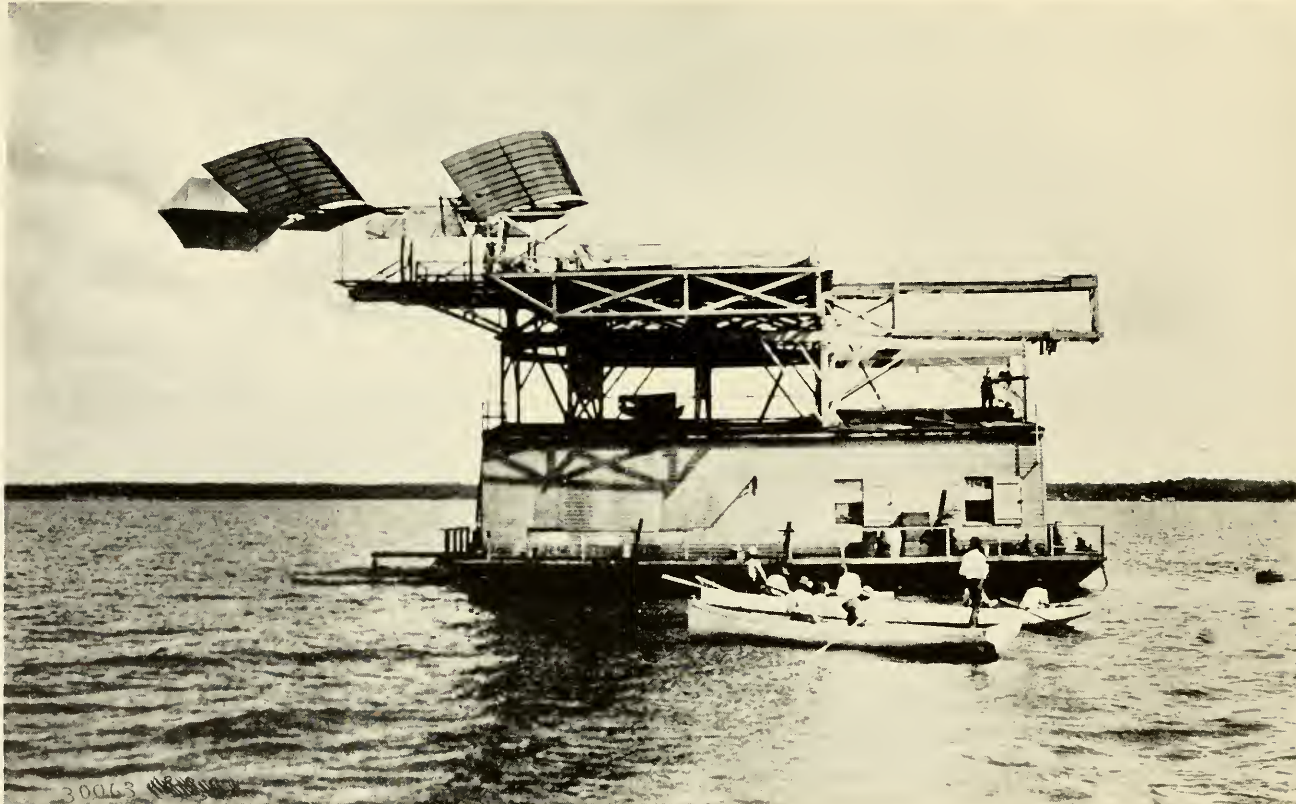
"Immediately upon getting the boat to Washington the labor of constructing new sustaining surfaces was begun, and they were completed about the close of November. It was proposed to make a

[18] Major Macomb, of the Board of Ordnance, states in his report to the Board, that "the trial was unsuccessful because the front guy post caught in its support on the launching car and was not released in time to give free flight, as was intended, but, on the contrary, caused the front of the machine to be dragged downward, bending the guy post and making the machine plunge into the water about 50 yards in front of the house boat."

[19] This instantaneous photograph, taken from the boat itself and hitherto unpublished, shows the aerodrome in motion before it had actually cleared the house boat. On the left is seen a portion of a beam, being a part of the falling ways in which the front wing was caught, while the front wing itself is seen twisted, showing that the accident was in progress before the aerodrome was free to fly.

second attempt near the city, though in the meantime the ice had formed in the river. However, on the 8th of December, 1903, the atmosphere became very quiet shortly before noon and an immediate attempt was made at Arsenal Point, quite near Washington, though the site was unfavorable. Shortly after arriving at the selected point everything was in readiness for the test. In the meantime the wind had arisen and darkness was fast approaching, but as the funds for continuing the work were exhausted, rendering it impossible to wait until spring for more suitable weather for making a test, it was decided to go on with it if possible. This time there were on hand to witness the test the writer, members of the Board of Ordnance, and a few other guests, to say nothing of the hundreds of spectators who were waiting on the various wharves and shores. It was found impossible to moor the boat without a delay which would mean that no test could be made on account of darkness, so that it was held as well as possible by a tug, and kept with the aerodrome pointing directly into the wind, though the tide, which was running very strong, and the wind, which was blowing 10 miles an hour, were together causing much difficulty. The engine being started and working most satisfactorily, the order was given by the engineer to release the machine, but just as it was leaving the track another disaster, again due to the launching ways, occurred.[20] This time the rear of the machine, in some way still unexplained, was caught by a portion of the launching car, which caused the rear sustaining surfaces to break, leaving the rear entirely without support, and it came down almost vertically into the water. Darkness had come before the engineer, who had been in extreme danger, could aid in the recovery of the aerodrome, the boat and machine had drifted apart, and one of the tugs, in its zeal to render assistance, had fastened a rope to the frame of the machine in the reverse position from what it should have been attached and had broken the frame entirely in two. While the injury which had thus been caused seemed almost irreparable to one not acquainted with the work, yet it was found upon close examination that only a small amount of labor would be necessary in order to repair the frame, the engine

[20] Major Macomb again states in his official report to the Board: "The launching car was released at 4.45 p. m. . . . . The car was set in motion and the propellers revolved rapidly, the engine working perfectly, but there was something wrong with the launching. The rear guy post seemed to drag, bringing the rudder down on the launching ways, and a crashing, rending sound, followed by the collapse of the rear wings, showed that the machine had been wrecked in the launching; just how it was impossible to see."



SIDE VIEW OF THE FULL-SIZE MAN-CARRYING AERODROME ON THE HOUSEBOAT JUST BEFORE THE TRIAL OF OCTOBER 7, 1903

This shows the track on which the machine was propelled by springs and shot off into the air

itself being entirely uninjured. Had this accident occurred at an earlier period, when there were funds available for continuing the experiments, it would not have been so serious, for many accidents in shop tests had occurred which, while unknown to the general public, had yet caused greater damage and required more time for repair than in the present case. But the funds for continuing the work were exhausted, and it being found impossible to immediately secure others for continuing it, it was found necessary to discontinue the experiments for the present, though I decided to use, from a private fund, the small amount of money necessary to repair the frame so that it itself, together with its engine, which was entirely uninjured, might be available for further use if it should later prove possible, and that they themselves might be in proper condition to attest to what they really represent as an engineering achievement.

" Entirely erroneous impressions have been given by the account of these experiments in the public press, from which they have been judged, even by experts; the impression being that the machine could not sustain itself in flight. It seems proper, then, to emphasize and to reiterate, with a view to what has just been said, that the machine has never had a chance to fly at all, but that the failure occurred on its launching ways; and the question of its ability to fly is consequently, as yet, an untried one.

" There have, then, been no failures as far as the actual test of the flying capacity of the machine is concerned, for it has never been free in the air at all. The failure of the financial means for continuing these expensive experiments has left the question of their result where it stood before they were undertaken, except that it has been demonstrated that engines can be built, as they have been, of little over one-half the weight that was assigned as the possible minimum by the best builders of France and Germany; that the frame can be made strong enough to carry these engines, and that, so far as any possible prevision can extend, another flight would be successful if the launching were successful; for in this, and in this alone, as far as is known, all the trouble has come.

" The experiments have also given necessary information about this launching. They have shown that the method which succeeded perfectly on a smaller scale is insufficient on a larger one, and they have indicated that it is desirable that the launching should take place nearer the surface of the water, either from a track upon the shore or from a house boat large enough to enable the apparatus to be launched at any time with the wings extended and perhaps with wings independent of support from guys. But the construction of this new launching apparatus would involve further considerable expenditures that there are no present means to meet; and this, and this alone, is the cause of their apparent failure.

"Failure in the aerodrome itself or its engines there has been none; and it is believed that it is at the moment of success, and when the engineering problems have been solved, that a lack of means has prevented a continuance of the work."

A regrettable controversy has arisen regarding the capacity of this machine for flight. As our purpose here is only to recall the work and attainments of Langley, and as far as possible by his own words, we may well leave that question as he himself stated it.

Our summary of Langley's work is far from complete. Such important papers as "The Solar and Lunar Spectrum," "The Cheapest Form of Light," "Energy and Vision," "Observation of Sudden Phenomena," "Good Seeing," "The History of a Doctrine," and others have been entirely omitted. But space forbids further following of the steps of this great man except to quote in closing that inimitable parable from the final pages of his charming book "The New Astronomy":

"I have read somewhere a story about a race of ephemeral insects who live but an hour. To those who are born in the early morning the sunrise is the time of youth. They die of old age while its beams are yet gathering force, and only their descendants live on to midday; while it is another race which sees the sun decline, from that which saw it rise. Imagine the sun about to set, and the whole nation of mites gathered under the shadow of some mushroom (to them ancient as the sun itself) to hear what their wisest philosopher has to say of the gloomy prospect. If I remember aright, he first told them that, incredible as it might seem, there was not only a time in the world's youth when the mushroom itself was young, but that the sun in those early ages was in the eastern, not in the western, sky. Since then, he explained, the eyes of scientific ephemera had followed it, and established by induction from vast experience the great 'Law of Nature,' that it moved only westward; and he showed that since it was now nearing the western horizon, science herself pointed to the conclusion that it was about to disappear forever, together with the great race of ephemera for whom it was created.

"What his hearers thought of this discourse I do not remember, but I have heard that the sun rose again the next morning."

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