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THE

AMERICAN

JOURNAL OF SCIENCE.

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THIRD SERIES.

VOL. XXXV.—[WHOLE NUMBER, CXXXV.]

Nos. 205—210.

JANUARY TO JUNE, 1888.

WITH VII PLATES.

NEW HAVEN CONN.: J. D. & E. S. DANA.
1888.

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THE

AMERICAN JOURNAL OF SCIENCE

[THIRD SERIES.]

ART. XXII.—*The Absolute Wave-length of Light;* by LOUIS BELL.

THIS paper contains the final results of the research partially reported in this journal for March 1886. In view of the wide discrepancies in the value of this physical constant as determined by various observers and methods, it has seemed desirable to give in brief the history of the subject, and critically to discuss certain portions of the investigation which have proved stumbling-blocks in the past. I refer particularly to the verification of the standards of length employed, and to those errors of ruling in the gratings which may, and usually do, produce errors in the result obtained.

The first portion of this paper will be devoted to the methods and results of the pioneers in this work and the methods, apparatus, and standards of length employed in the present investigation.

The second portion will contain the details of the experimental work, together with a discussion of the final results and those questions of theoretical and practical interest which raise themselves in connection with the work of recent experimenters. With this preliminary notice is presented the first half of the paper.

Historical.—Fraunhofer's first paper on the lines which bear his name marks a new era in the science of optics. Up to that point any careful study of spectra had been impossible for lack of definite standards of reference, and because the apparatus was as yet very defective. Fraunhofer's research, "Bestimmung des Brechungs- und Farbenzerstreuungs-Vermögens verschiedener Glasarten," was presented to the Munich Academy of Sciences in 1814, and was published in the fifth volume of the "Denkschriften." It then became possible to study in detail the properties of rays of definite position and the work was taken up almost immediately. Almost the first step was to determine the wave-lengths of prominent points in the solar spectrum, and, as is well known, Fraunhofer himself took it, determining the wave-lengths corresponding to his lines B, C, D, E, F, G, H. As there seems to have been—noticeably in Verdet's papers—some confusion concerning his papers on this subject, it may be well here to clear the matter up.

Fraunhofer's first paper dealing with the subject was presented to the Munich Academy in 1821. It is entitled: "Neue Modifikation des Lichtes durch gegenseitige Einwirkung und Beugung der Strahlen, und Gesetze derselben," and was printed in the eighth volume of the "Denkschriften." It is of considerable length and deals with various diffraction phenomena, but its chief interest lies in the wave length measurements made with wire gratings. The experiments made with ten of these are given in detail and are remarkably careful and consistent. The gratings were quite various, the wires being from $0{\cdot}04^{mm}$ to $0{\cdot}6^{mm}$ in thickness and the grating space as ordinarily measured, from $0{\cdot}0528$ to $0{\cdot}6866^{mm}$. From these proportions it is evident enough that the spectra must have been very imperfect, but in spite of this, Fraunhofer obtained results which agreed remarkably well with each other. The wave lengths of D as obtained from the above mentioned ten gratings were as follows: reduced to millimeters.

	mm		mm
(1)	0·0005891	(6)	0·0005888
(2)	0·0005894	(7)	0·0005885
(3)	0·0005891	(8)	0·0005885
(4)	0·0005897	(9)	0·0005882
(5)	0·0005885	(10)	0·0005882

The mean value adopted was $0{\cdot}0005888^{mm}$, which considering the gratings and the fact that most of the angles of deviation were less than 1°, is certainly remarkably accurate. It should be noted too, that the finer gratings (1) to (4) gave even better results.

A brief discussion of this paper appeared in the seventy-third volume of Gilbert's Annalen and a French reprint in Schumacher's Astronomische Abhandlungen (ii, 46).

Fraunhofer's second and more complete paper appeared in 1823 in Gilbert's Annalen (lxxiv, 337). Its title is: "Kurzer Bericht von den Resultaten neuerer Versuche über die Gesetze des Lichtes, und die Theorie derselben." This paper gives a detailed account of his experiments with two glass gratings. Of these, the grating spaces were respectively $0{\cdot}0033$ and $0{\cdot}0160^{mm}$. The former was apparently much the better, and upon it Fraunhofer based his final result, which for D was $0{\cdot}0005886^{mm}$ while the experiments with the coarser grating gave $0{\cdot}0005890^{mm}$. These values apply quite certainly to the mean of the two D lines, and not, as has been sometimes supposed, to one of them alone.

The experimental work with these glass gratings was much better than with the previous wire ones, since the angular deflections were very much larger and the gratings themselves were susceptible of far more exact measurement. But at best they were but indifferent instruments and the terminal lines were so bad that they had to be retraced before the grating space could be determined. So, between poor gratings and indifferent standards of length, Fraunhofer's determination of absolute wave length left very much to be desired. However, nothing much better could be accomplished until the art of making gratings was very much improved, and it was not until Nobert's gratings became tolerably well known, that any serious attempt was made to improve on Fraunhofer's results. From time to time various investigators worked on the problem, both with Nobert's earlier gratings and by utilizing various interference phenomena. When, however, the great investigations of Bunsen and Kirchhoff revolutionized spectroscopic work and emphasized its great importance, the attention of scientific men was called to the need for accurate measurements, and for half a dozen years investigators were active, and Mascart, Ditscheiner and Ångström appeared on the field almost simultaneously. Each published a paper in 1864, and of these that of Mascart is probably the most accurate and painstaking, though now it is quite certain that the values he obtained were considerably too small. He employed four or five of Nobert's gratings and instead of placing the grating perpendicular to either the collimator or the observing telescope, used it in the position of minimum deviation, that is to say, so that the plane of the grating should bisect the angle formed by the incident and diffracted rays. This position has certain advantages, but as the experimentation is rather more difficult than in the ordinary position, the method appears to be of somewhat questionable utility. It avoids, to be sure, the necessity of placing the grating normal to the axis of either telescope, but as there is very little trouble in making this adjustment with a high degree of accuracy, and keeping it through a series of measurements, the gain is by no means considerable. Aside from this question, Mascart's spectrometer read only to five seconds, and while his results with different gratings agree very well individually they are certainly collectively in error by quite a large amount, very possibly owing to bad standards of length.

It is a fact to be noted in discussing all these earlier wave-length determinations, that sufficient attention was not paid to the measurement and study of the gratings—by all odds the most difficult part of the problem. The angular measures of any one of the above investigators were good enough to have given very exact results had they been combined with proper investigations of the grating spaces. As most of Nobert's gratings were small and by no means accurately ruled there was peculiar need of care in measuring them, and when one considers that the defining lines on most standards of length are far from being good, it is clear that the chances for error were numerous. In Ångström's first paper he even relied on the grating space assigned by the maker. Ditscheiner employed a grating which had belonged to Fraunhofer himself, but the number of spaces was uncertain and this led to a large error which he corrected, in part, in a supplementary paper some years later. Ditscheiner's principal paper was published in 1866, and was followed in 1868 by an elaborate discussion of the whole problem by van der Willigen, whose paper is valuable mainly for a particularly elaborate review of sources of error. Like his predecessors he used Nobert's gratings, but as the construction of his spectrometer confined his angular measurements to the deviation on one side of the normal, their accuracy may be open to some question, while his standard of length was anything but reliable, as it was a glass scale only three centimeters long and the only assurance of its accuracy was the certificate of the maker that it was "tres exacte" at 50° Centigrade. For one or both of the above reasons van der Willigen's results were larger than any which have been obtained, before or since his time.

In the same year appeared Ångström's great research which has so long served as the standard in all questions of wave length. It is hard to say too much of the conscientious and painstaking experiments on which his results were based, and any want of accuracy in the final result was due to no lack of skill or care on his part but rather to the imperfect instruments with which he was obliged to work. Like every one before him he used Nobert's gratings and in spite of the fact that like all Nobert's gratings they gave very imperfect definition and showed numerous "ghosts," his results were more than usually

consistent. But in spite of all Ångström's care the event has shown that his wave lengths are in error by as much as one part in seven or eight thousand mainly through an error in the assumed values of his standards of length. Ångström measured his gratings by means of a dividing engine the screw of which was very exactly determined by comparisons resting on the Upsala meter which, in turn, had been compared by M. Tresca with the prototype of the Conservatoire des Arts et Metiers. Had this comparison given the correct value of the Upsala meter Ångström's wave-lengths would have been very nearly exact except for corrections due to errors of ruling in the gratings.

After Ångström's research the question of absolute wave-length was not seriously raised for ten years, when Mr. C. S. Peirce under the auspices of the United States Coast Survey again attacked the problem, armed with Rutherfurd gratings far superior to those used in any previous research. No official report of his very elaborate and exhaustive experiments has ever been published save a very brief preliminary report in the American Journal of Science in 1879. Such of his results as have been made in any way public will be discussed in the experimental part of the present paper.

Meanwhile Thalén, who so efficiently aided Ångström in his work, has taken up the part of it left uncompleted by the latter's death and in his paper "Sur le Spectre du Fer," published at Upsala in 1885, has discussed the corrections which must be applied to Ångström's values by reason of the error in the Upsala meter. It seems that through the experiments of Professor Lindhagen, Ångström became aware as early as 1872 that the assumed value of his standard was considerably too small. His death prevented his verification of M. Lindhagen's results and nothing further was done till Thalén took up the work. Tresca's comparisons had shown that the true length of the Upsala metre at 0° was 999·81^{mm}. But the very exact experiments of M. Lindhagen have shown the above to be somewhat too small and that the correct value is 999·94. This difference makes, of course, a marked error in the wave-lengths based on Tresca's results. Applying the appropriate correction, the wave length of E, the line most carefully determined by Ångström, becomes

5269·80,

instead of the original 5269·12.

This final result of Ångström is certainly entitled to considerable respect and seems to be subject only to those corrections which may be due to irregularities in the gratings. These were, however, so poor compared with the gratings of to-day, that such corrections would necessarily be of uncertain magnitude.

At all events it is quite sure that of the wave-length determinations made up to 1880, those of Peirce, and Ångström corrected by Thalén are by all odds the best. Of the two, Peirce's is probably the better by reason of better gratings, but his work will be discussed in another part of this paper in connection with the very recent works of Müller and Kempf and Kurlbaum, which merit more extended study than would be in place at this point.

A tolerably complete bibliography of the subject up to date is annexed. Many of the papers are of little except historical value, but they will at least exhibit the various methods employed and the growth of exact experimentation.

1821 Fraunhofer. Denkschr. d. Akad. d. Wiss. zu München. viii, Heft: II, 38. "Neue Modifikation des Lichtes durch gegenseitige Einwirkung und Beugung der Strahlen und Gesetze derselben."

1823 Fraunhofer. Schumacher's astronomische Abhandlungen, ii. 46.

1823 Fraunhofer. Gilbert's Annalen, lxxiv. 337. "Kurzer Bericht von den Resultaten neuerer Versuche über die Gesetze des Lichtes und die Theorie derselben."

1835 Schwerd. "Die Beugungserscheinungen" (Mannheim).

1849 Stokes. Athenaeum No. 1143. Inst. xvii, 368. "On the Determination of the Wave Length corresponding with any Point of the Spectrum."

1851 Nobert. Proc. Roy. Soc., vi, 43; Phil. Mag., IV. i. 570. "Description and Purpose of the Glass Plate which Bears the Inscription: Longitudo et celeritas undularum lucis cum in aere tum in vitro."

1851 Nobert. Pogg. Ann., lxxxv. 83. "Ueber eine Glasplatte mit Theilungen zur Bestimmung der Wellenlänge und relativen Geschwindigkeit des Lichts in der Luft und im Glase."

1852 Drobisch. Pogg. Ann., lxxxviii. 519. "Ueber die Wellenlänge und Oscillationszahlen der farbigen Strahlen im Spectrum."

1853 Esselbach. Berlin Monatsber. 757. "Ueber die Messung der Wellenlänge des ultravioletten Lichts."

1856 Esselbach. Pogg. Ann., xcviii, 513. Ann. de Chim. e. d. Phys., III, l. 121, "Eine Wellenmessung im Spectrum jenseits des Violetts."

1856 Eisenlohr. Pogg. Ann., xcviii, 353, xcix. 159. Ann. de Chim. e. d. Phys., III, xlix, 504. "Die brechbarsten oder unsichtbaren Lichtstrahlen in Beugungspectrum und ihre Wellenlänge."

1863 Müller. Pogg. Ann., cxviii, 641. "Bestimmung der Wellenlänge einiger heller Spectrallinien."

1863 Mascart. C. R., lvi, 138. "Détermination de longueur d'onde de la raie A."

1864 Mascart. C. R., lviii, 1111. Détermination des longueurs d'onde des rayons lumineux et des rayons ultraviolets.

1864 Mascart. Ann. de l'École normale, i, 219. Recherches sur la détermination des longueurs d'onde.

1864 Bernard. Mondes, v, 181. "Theorie des bandes d'interférence * * * Longueur d'onde de la raie A," * * *

1864 Stefan. Ber. d. Wien. Acad., l, Heft 2, 31. Pogg. Ann., cxxii, 631. "Ueber die Dispersion des Lichtes durch Drehung der Polarizationsebene im Quarz."

1864 Bernard. C. R., lviii, 1153; lix. 352. "Memoire sur la détermination des longueurs d'onde des raies du spectre solaire au moyen des bandes d'interférence."

1864 Ditscheiner. Ber. d. Wien. Acad., l, Heft 2, 296. "Bestimmung der Wellen länge der Fraunhofer'schen Linien des Sonnenspectrums."

1864 Ångström. Pogg. Ann., cxxiii, 489. Œfvers. af Förhandl. (1863) 41. "Neue Bestimmung der Länge der Lichtwellen nebst eine Methode auf Optischen Wege die fortschreitende Bewegung des Sonnensystems zu bestimmen."

1866 Ditscheiner. Ber. d. Wien. Acad., lii, Heft 2. 289. "Eine absolute Bestimmung der Wellenlänge der Fraunhofer'schen D Linien."

1866 Mascart. Ann. de l'Ecole normale, iv, 7. "Recherches sur la détermination des longueurs d'onde."

1868 Mascart. Ann. de Chim. e. d. Phys., IV, xiii, 186. "Note sur différents travaux relatifs aux longueurs d'onde."

1868 van der Willigen. Arch. du Museé Teyler. i, 1, 57, 280. "Memoire sur la Détermination des longueurs d'onde du Spectre Solaire."

1868 Ångström. Upsala, 1868. "Recherches sur le Spectre Solaire."

1871 Ditscheiner. Ber. d. Wien. Acad., lxiii, heft 2. 265. "Zur Bestimmung der Wellenlänge der Fraunhofer'schen Linien."

1879 Peirce. Am. Jour. Sci., III, xviii, 51. "Note on the Progress of Experiments for comparing a Wave length with a Metre."

1884 Thalén. Upsala, 1885. "Sur le Spectre du Fer obtenu a l'aide de l'arc électrique." p. 18.

1886 Müller and Kempf. Publicationen des Astrophysikalischen Observatoriums zu Potsdam, v. "Bestimmung der Wellenlänge von 300 Linien im Sonnenspectrum."

1886 de Lépinay. Jour. de Ph., II, v, 411. "Détermination de la valeur absolue de la longueur d'onde de la raie D^2."

1887 Bell. Am. Jour. Sci., III. xxxiii, 167. "On the absolute Wave length of Light."

1887 Kurlbaum. Berlin. 1887. "Bestimmung der Wellenlänge einiger Fraunhofer'schen Linien."

In general the determination of absolute wave-length involves two quite distinct problems—first the precise determination of some quantity which is an exact function of the wave-length and some other linear dimension; and second, the reduction of this dimension to terms of some recognized standard of length. The first process can be made to give relative wave-lengths with a very high degree of accuracy, and is, in nearly every case, more exact than the second, which constitutes the main difficulty of the investigation. It is because the diffraction grating lends itself readily to linear measurement, that its use is preferable to the other interference methods which involve, usually, the exact determination of a single very small linear quantity. The ingenious attempt of M. de Lépinay* to avoid this difficulty is interesting theoretically but practically it involves a quantity even more uncertain than the average standard of length—the relation between the kilogram and the meter—to say nothing of the experimental difficulties of the method. The angular measurements of nearly all the later investigators have been quite good enough to furnish very exact values of wave-length, but in every case it has been the measurement of the grating space that has produced the manifold errors and discrepancies

* Journ. Phys., II, v, 411.

in the results. It has been the aim of the present research to investigate this fruitful source of errors and as far as possible to avoid the difficulties springing from it.

In a previous paper,* I briefly discussed the advantages of transmission and reflection gratings. It only remains to add that further experience has convinced me that not only are the speculum metal gratings far superior in brilliancy and sharpness of definition, but that it is possible, contrary to what one might suppose from their large coefficient of expansion, to rule them with almost perfect uniformity, over a length as great as a decimeter. This large size too, gives a great advantage in determining the grating space, aside from the fact that speculum metal has a coefficient of expansion not widely different from that of any one of the materials usually employed for standards of length, and that its temperature can be obtained with comparative ease.

Methods and Instruments.

The plane grating can be used for wave-length measurement in a variety of ways according to the preference of the investigator or the arrangement of the spectrometer. Five tolerably distinct methods may be enumerated. The general relation between the wave-length and the angles of incidence and diffraction is

$$\lambda = s(\sin i + \sin(\varphi - i))\frac{1}{n}$$

Where λ is the wave-length, s the grating space, i and φ the angles of incidence and diffraction respectively, and n the order of the spectrum observed. Making $i=0°$ this at once becomes the ordinary formula

$$\lambda = \frac{1}{n} s \sin \varphi$$

which applies to the two methods of normal incidence, one in which the grating is kept accurately perpendicular to the collimator; the other in which it is kept perpendicular to the observing telescope.

Next is the method used by Ångström in which i is not reduced exactly to 0°, but measured and retained in the formula, the grating in this case being kept nearly perpendicular to the collimator. In this method a reading on the slit is necessary, and if α and α' are the readings on the circle, and M that on the slit, the working formulæ are:

$$\frac{\alpha + \alpha'}{\alpha} - M = \delta \text{ and } \frac{\alpha - \alpha'}{2} = \varphi$$

* Am. Jour. Sci., III, xxxiii, 167.

then, if i is as before, the angle of incidence,

$$\lambda = \frac{1}{n} s \sin \varphi \cos (i + \delta)$$

$$\sin i = \sin(i + \delta) \cos \varphi$$

$$\tan i = \frac{\cos \varphi}{1 - \cos \varphi} \delta$$

In the fourth method also, i is retained, but given a definite value. Putting the general formula in the form,

$$\lambda = \frac{1}{n} 2 s \sin \frac{\varphi}{2} \cos\left(i - \frac{\varphi}{2}\right)$$

the deviation represented by the angular term will evidently be a minimum when $i = \frac{\varphi}{2}$. If then one observes in the position of minimum deviation,

$$\lambda = \frac{1}{n} 2 s \sin \frac{\varphi}{2}$$

In the fifth method collimator and observing telescope are kept at a fixed angle with each other and the grating is turned. In this case if φ is the angle of deviation and θ the angle between the telescopes

$$\lambda = \frac{1}{n} 2 s \sin \varphi \cos \frac{\theta}{2}$$

These methods are general and the choice between them is simply a question of the convenient application of the apparatus at hand. Probably the first and the second methods are the most generally useful, while the third is the most objectionable. The method of minimum deviation slightly increases the experimental difficulties, but often improves the definition of the gratings and is capable of giving very exact results. The last method is applicable only when the spectrometer is so rigid as to ensure the permanence of the angle between the telescopes. When this condition is fulfilled, however, the method is very valuable, since it reduces the moving mass to a minimum and allows the method of repetition to be readily used.

In the present research for the work with glass gratings the second method was selected as best suited to the arrangement of the spectrometer. This was a very good instrument by Meyerstein. The circle is 32^{cm} in diameter, divided on silver to 6′ and reading by two microscopes directly to 2″ and by estimation easily to within 1″. The collimating and observing telescopes are of 4^{cm} clear aperture and about 35^{cm} focal length, well corrected and firmly supported.

For the second part of the work, with speculum metal gratings, it was desirable to use gratings of the largest size practicable, far larger than could be used on the above described instrument, both by reason of the small aperture of the telescope and the inability of the grating holder to carry the requisite mass steadily. This part of the work was, therefore, carried out on a very large instrument, designed by Prof. Rowland especially for using gratings of the largest sizes as yet ruled. This instrument has virtually fixed telescopes, solidly clamped, with a small lateral range of adjustment, to a T-shaped casting bedded in cement which in turn forms the top of a large brick pier resting on a stone slab.

The telescopes are of 16·4^{cm} clear aperture, and about 2·5 meters focal length and the objectives are of excellent quality. Each telescope is fastened to an arm of the T, which has a total length of over 2 meters, and bears, at the extremity of the shaft the spectrometer proper. This is an instrument by Schmidt and Haensch, having a circle 32^{cm} in diameter divided to 6′ and, as in the other spectrometer, reading by two microscopes directly to 2″ and by estimation to less than half that amount. The original central platform had been removed and replaced by a grating holder large enough to carry if necessary a 6 inch grating. Such an apparatus limits one, of course, to the fifth method, but so rigid is the whole affair that experience soon showed that the angle between the telescopes did not change by any appreciable amount. The circle, however, was not finely enough graduated, nor were the microscopes of sufficient power to derive the fullest benefit from the size of the telescopes; over and over again has the line in the spectrum appeared slightly displaced from the crosshairs, when no difference whatever could be detected in the micrometer readings. However, there was gained the great advantage of using gratings of a decimeter in length, giving spectra of great brilliancy and superb definition, and which could be measured with vastly greater exactness than is possible with the small gratings generally employed.

Gratings.

Four gratings have been used in my experiments—two of glass and two of speculum metal. The former are probably the best of the very few glass gratings that have been ruled on Prof. Rowland's engine. They are ruled on plane sextant mirrors of rather hard glass.

Grating I, contains 12,100 spaces in a length of very nearly thirty millimeters, the lines being nineteen millimeters long. It was ruled in Jan., 1884, at a temperature of 6°·7 C. gives spectra of excellent definition, quite free from ghosts or false lines, and having almost exactly the same focus on both sides of the normal.

Grating II has 8600 spaces with almost exactly the same length and breadth as I, is free from ghosts and false lines and like I, is very smoothly ruled, though it is somewhat inferior to I in the matter of regularity. The definition is excellent and the spectra alike in focus on both sides of the normal. It was ruled in Nov., 1884 at 11°·6 C.

Gratings III and IV are on speculum metal. The plates are five inches square and five-eighths of an inch thick, and were worked plane with especial care. The ruled surface is of the same size in each, four inches long by two inches length of lines.

Grating III was ruled in April, 1885, at a very nearly constant temperature of 10° C. It contains 29,000 spaces, having very nearly the same grating space as II. It is a phenomenal grating both in its superb definition and extraordinary regularity of ruling, and was selected from a large number because of its very unusual perfection. The focus of the spectra on each side of the normal is the same and the ruling is flawless.

Grating IV was ruled on the new dividing engine just completed by Prof. Rowland, and was one of the first large ones completed. While the new engine has even now not received the finishing touches, it has turned out a few gratings of remarkable excellence. One of these is IV, which was ruled in Dec., 1887, at a constant temperature of 17°·2 C. It contains 40,000 spaces within the same dimensions as III, is equal to it in definition, and but very little inferior in regularity of ruling. It has very nearly the same focus on both sides of the normal, and the ruling is wonderfully even and perfect.

It should be noted that these four gratings are widely diverse, being ruled at different temperatures and under different conditions. I and II were ruled to widely diverse grating spaces on different parts of the screw, III was on speculum metal and with more than six times the ruled surface of I or II, and finally IV was ruled to a new grating space on a new dividing engine. These differences may not favor close agreement in the experimental results, but they certainly serve to eliminate anything like systematic errors due to the gratings.

The above gives a general view of the gratings employed, but some further details will be mentioned in the second part of this paper in connection with the determination of the grating spaces.

On the Standards of Length.

Very many of the discrepancies in the determinations of absolute wave-lengths are the direct result of uncertainty in the standard of length employed. The cases of Ångström and van der Willigen have been already alluded to, and the same source of error is common to all other determinations. It seems, therefore, desirable to give at some length the various comparisons on which the wave-length as given by my experiments is based. Reserving for the present the actual measurement of the gratings, which is a comparatively simple matter, I will therefore discuss the standards directly employed, their relations to the Metre des Archives as found by various comparisons, and finally the changes which have taken place in those relations since they were first determined.

The standards with which the gratings have been directly compared are two double decimeters on speculum metal, designated respectively S^a_1 and S^a_2. They were graduated and compared by Prof. W. A. Rogers in 1885. The bar S_1 is 23^{cm} long and bears near its edge the double decimeter S^a_1, subdivided to centimeters. The defining lines are less than $1\,\mu$ in width and beautifully sharp and distinct. S_2 is 27^{cm} long and is graduated in the same way, with lines of the same width. Both standards are of the same speculum metal, and are of very nearly the same mass, while the surfaces and graduation leave little to be desired. The coefficient of expansion of these bars was very thoroughly investigated by Prof. Rogers and was found to be,

$$17{\cdot}946\,\mu \text{ per meter per degree C.}$$

The absolute lengths of S^a_1 and S^a_2 depend on long series of comparisons with Prof. Rogers's bronze yard and meter R_2 and steel copies thereof. Upon the relation existing between R_2 and the Metre des Archives depends then the absolute value assigned to the wave-length of light, since the close agreement of the various series of comparisons executed by Prof. Rogers between R_2 and the speculum metal standards show that no sensible uncertainty exists in the relations between them.

The yard and meter R_2 is of the alloy known as Bailey's metal, this being the material of the Imperial Yard and many other standards. The graduations are upon platinum iridium plugs, the polished faces of which are in the plane of one surface of the bar when supported at its neutral points. The relation of the meter R_2 to the Metres des Archives rests on a very large number of comparisons made with two entirely independent secondary standards; the copper meter designated T, and the brass yard and meter designated C. S. A full account of these comparisons is contained in vol. xviii of the Proceedings of the American Academy of Arts and Sciences.

The meter T is on platinum plugs in a pure copper bar and was traced and compared by M. Tresca in 1880, from the Conservatoire line meter No. 19, the relation of which to the Metre des Archives was very exactly known.

The yard and meter C. S. has its graduations on silver plugs in a brass bar. The yard was compared directly with the Imperial Yard in 1880, and the standard was then sent to Breteuil where it was compared with the International Meter by Dr. Pernet.

There were thus two completely independent sources from which the relation of R_2 to the Metre des Archives could be obtained. The results derived by very elaborate comparisons with each of these were as follows:

$$\left.\begin{array}{ll}\text{From T} & R_2 - A_0 = +\ 1{\cdot}5\,\mu \\ \text{From C.S.} & R_2 - A_0 = +\ 1{\cdot}1\,\mu\end{array}\right\}\text{ at } 16^\circ{\cdot}67\text{ C.}$$

Where A_0 is the Metre des Archives. In addition to the very close agreement of the above, further evidence was obtained by deriving the relation between the yard and meter from R_2, the yard R_2 having been exactly determined by comparisons with C. S. and with "Bronze 11," one of the primary copies of the Imperial Yard, which had been recompared with that standard in 1878.

From the comparisons of S_1 and S_2 made in 1885 the following value of those standards were deduced:

$$S^a_1 + 0{\cdot}98\,\mu = \tfrac{1}{5}A_0, \text{ and}$$
$$S^a_2 + 0{\cdot}2\,\mu = \tfrac{1}{5}A_0. \quad \text{Hence}$$
$$S^a_2 = S^a_1 + 0{\cdot}78\,\mu, \text{ and for the first decimeters}$$

were found the relations:

$$Dm_1S^a_1 + 0{\cdot}05\,\mu = \tfrac{1}{10}A_0 \qquad \text{Whence,}$$
$$Dm_1S^a_2 - 0{\cdot}01\,\mu = \tfrac{1}{10}A_0$$
$$Dm_1S^a_2 = Dm_1S^a_1 + 0{\cdot}06\,\mu$$

On these equations were based the results embodied in my former paper. In the latter part of May, 1887, these standards were very carefully compared with each other and with a speculum metal bar graduated by Prof. Rowland, as I desired to take one or more of the standards to Berlin during the summer in order to get a comparison with the standard used by Müller and Kempf.

The results of this examination were of a somewhat startling character, as follows:

$$S^a_2 = S^a_1 + 1{\cdot}2\,\mu, \text{ direct}$$
$$S^a_2 = S^a_1 + 1{\cdot}1\,\mu, \text{ through the Rowland bar}$$

designated R_B. Also,

$$Dm_1S^a_2 = Dm_1S^a_1 + 1{\cdot}7\,\mu, \text{ through } R_B$$

In 1885 Rogers had found for the relation between the two decimeters of each bar:

$$Dm_2S^a_1 = Dm_1S^a_1 - 0{\cdot}56\,\mu$$
$$Dm_1S^a_2 = Dm_2S^a_2 + 0{\cdot}46\,\mu$$

I now found for the same quantities:

$$Dm_2S^a_1 = Dm_1S^a_1 + 0{\cdot}64\mu, \text{ direct},$$
$$Dm_2S^a_1 = Dm_1S^a_1 + 0{\cdot}60\mu, \text{ from } R_B$$
$$Dm_1S^a_2 = Dm_2S^a_2 + 1{\cdot}60\mu, \text{ direct}$$
$$Dm_1S^a_2 = Dm_2S^a_2 + 1{\cdot}65\mu, \text{ from } R_B$$

All these relations being for 16°·67 C.

The standard S^a_2 was taken to Berlin during the summer and through the kindness of Dr. Nieberding, Director of the Normal Aichungs Commission, I was enabled to have it compared with R_{78} the standard meter to which the wave length measurements of Müller and Kempf, and Kurlbaum had been referred. From this comparison was derived the relation:

$$S^a_2 - 1{\cdot}68\mu\ (\pm 0{\cdot}15\mu) = \tfrac{1}{5}A_0$$

On returning to Baltimore the first step was to redetermine the length of S^a_2. A series of comparisons was therefore instituted between it and the steel yard and meter A_1, the relation of which to R_2 was accurately known, A_1 having been traced and determined by Prof. Rogers and furnished by him to the Johns Hopkins University. Only half of this standard is subdivided to decimeters but a series of comparisons with the various pairs of decimeters gave the relation,

$$S^a_2 + 1{\cdot}3\mu = \tfrac{1}{5}A_0$$

This result taken together with the relations found between S^a_1 and S^a_2 made it tolerably clear that a change had taken place in the speculum metal standards, and to obtain a further confirmation Prof. Rogers kindly consented to give them a rigid examination and again compare them with all attainable accuracy to R_2. His results for S^a_2 were as follows:

$$Dm_1S^a_2 = Dm_2S^a_2 + 1{\cdot}70\mu$$
$$S^a_2 + 1{\cdot}0\mu = \tfrac{1}{5}A_0$$

There is no escape from the conclusion, therefore, that the speculum metal bars S_1 and S_2 have changed both in absolute length and the relative lengths of their parts. Here are two bars of the same shape, mass, material, and constant of expansion. Each had the relation between its halves determined in the early part of 1885. Two years later these relations are found to have changed by at least 1μ, and an independent determination by the original observer confirms this result in the most unequivocal way. Further, the original observer recompares one of these standards with the standard from which it was originally determined and finds a change of 1μ.

It should be borne in mind that with the comparator used by me in this work, 1μ is completely outside of any possible errors of observation. The microscope used was especially made for micrometric work and has a power of two hundred and fifty diameters, while one division of the micrometer equals $0{\cdot}28\mu$. The average error of a single comparison between two decimeters is rarely greater than $0{\cdot}1\mu$, while the temperature of the observing vault can be kept for several days constant within 0°·5 C. and during a day's observations usually remained constant within half that amount. The bars under comparison were side by side, symmetrically placed with reference to the illumination, and were at temperatures very near to 16°·67, at which they were standard.

The facts then concerning the speculum metal bars are these: In about two and a half years S^a_2 has shortened by very nearly $1{\cdot}0\mu$ and S^a_1 by a little over that amount. In S^a_2 this change has taken place exclusively in the last decimeter and in S^a_1 it has been confined to the first decimeter.

The apparent slight increase in $Dm_1S^a_2$ and $Dm_2S^a_1$ I do not regard as beyond the effect of the experimental errors. The changes in the lengths of the subdivisions of these standards are very curious and some explanation may be offered by the fact that the bars were cast in a nearly vertical position and annealed in sawdust, a method hardly sufficient for a material so strongly crystalline as speculum metal. I think, however, one is justified in drawing the conclusion that speculum metal, so tempting on account of its beautiful surface and the exquisite sharpness of the graduations drawn upon it, is a material thoroughly unsuitable for standards of length by reason of its tendency to change with time. I have thus entered into somewhat minute details in the case of these bars, because the whole question of changes in standards of length is in a somewhat unsettled state, and it seems desirable to put on record this case, which has been investigated with more than ordinary care by both Prof. Rogers and myself, and in which the changes found have taken place within a comparatively short time.

It is quite well-known that in 1855 this question was raised by Mr. Sheepshanks, then engaged in constructing the new British standards. Discrepancies amounting sometimes to 2 or 3μ appeared in his measurements, but after a considerable amount of study, these differences appeared to be too irregular to be fairly ascribable to actual changes. Slight variations of temperature, especially when the standards compared were of different materials, the lagging of the real temperatures of the bars behind the thermometer indications, and particularly the effect of coarse and sometimes unsymmetrical defining lines, are perhaps enough to account for the observation.

The work, however, done on the U. S. bar "Bronze 11," as reported in the report of the Coast Survey for 1877, seems to show genuine change in that standard.

A long series of comparisons with the Imperial Yard and its copies in 1878, showed systematically a shortening relative to the Imperial Yard of over 4μ. Although further measurements have tended to somewhat lessen this discrepancy it seems to be sufficient, considering the fact that "No. 11" and the Imperial Yard are of the same shape, material and mass, and were compared on the same apparatus as during the original comparisons in 1857, and at nearly the standard temperature, to establish the fact of a real change. While 3 or 4μ is absolutely a small quantity, its systematic appearance under conditions almost identical with those of the original measurement can hardly be ascribed to experimental errors. The other cases cited in the above mentioned paper tend to confirm this conclusion.

The gradual and sometimes very irregular changes that are known to take place in both the bulbs and stems of thermometers, would lead one to expect that glass standards of length would be liable to similar changes, though probably in far less amount. It was, therefore, with special interest that I examined glass Decimeters III and IV belonging to the Coast Survey, and used by Peirce in his wave-length measurements. These scales are on plate glass, of the same dimensions, and having coefficients of expansion not widely different. A series of comparisons made at a nearly constant temperature of 16°·5 C. gave the direct relation

$$\text{III} = \text{IV} + 2{\cdot}1\mu$$

While the same relation deduced from Peirce's direct comparison by applying the coefficients of expansion assigned by him, is

$$\text{III} = \text{IV} + 1{\cdot}3\mu$$

The defining lines on both standards are fine and sharp, and unless Peirce's coefficients are grossly in error, the evidence of change between 1879 and 1887 is very strong indeed.

Having now the exact present relation of S^a_2 to the original standard R_2, it remained only to investigate the difference between this result and the length of S^a_2 as deduced from the Berlin comparisons. I have been unable to obtain the details concerning R_{78}, the standard used in these comparisons, but it was determined by comparison with the standard meter of the International Bureau. The comparisons of S^a_2 with R_{78} were carefully made by two observers, and it is probable that the result represents the relation between these standards with considerable exactness. It should, however, be borne in mind that the microscopes had power of only 50 diameters, and that the bars in question are of very different material and mass, thus giving a chance for small errors due to varying temperature.

It is possible, however, to check this result by referring S^a_2 to the Berlin platinum standard through the medium of the Coast Survey meter "No. 49." This latter standard was compared in 1876 with meter 1605 and directly with the platinum meter. The details are given in Prof. Foerster's report contained in the report of the Coast Survey for 1876. The result of the direct comparison was

$$\text{Pl} - \text{“49”} = +24{\cdot}4\mu.$$

But now Prof. Rogers has compared R_2 with "49," obtaining in terms of the assumed length of R_2

$$\text{“49”} = A - 19{\cdot}3\mu,$$ the assumed value of R_2 was,

$$R_2 = A + 1{\cdot}3\mu.$$ Hence we have,

$$R_2 - \text{“49”} = 20{\cdot}6\mu,$$ from which follows,

$$\text{Pl} - R_2 = 3{\cdot}8\mu.$$ If now the equation between Pl and the Metre des Archives established by direct comparison in 1860 be correct:

$$A_0 - \text{Pl} = -3{\cdot}01\mu.$$ And therefore,

$$R_2 - A_0 = -0{\cdot}8\mu,$$ a result which is in close accordance with those derived from the Conservatoire meter and Type I of the International Bureau by means of the Standards T. and C. S.

In my final determination of wave-length, I have used the mean value of S^a_2 as derived by the foregoing methods. Collecting equations,

$$S^a_2 + 0{\cdot}96\mu = \tfrac{1}{5}A_0 \quad \text{From T.}$$
$$S^a_2 + 1{\cdot}04\mu = \tfrac{1}{5}A_0 \quad \text{“ C. S.}$$
$$S^a_2 + 1{\cdot}40\mu = \tfrac{1}{5}A_0 \quad \text{“ “49.”}$$
$$S^a_2 - 1{\cdot}68 = \tfrac{1}{5}A_0 \quad \text{“ } R_{78}.$$

Giving to the equations derived from C. S. and R_{78} twice the weight of the others, we have finally,

$$S^a_2 + 0{\cdot}27\mu = \tfrac{1}{5}A_0.$$

I have given the relations derived from C. S. and R_{78} double weight because these standards have been compared directly with the standard of the International Bureau, which now, probably, should be regarded as the ultimate standard of reference. Especially is this true, since it is rumored, apparently not without foundation, that the Metre des Archives is, at present, for some unassigned reason, undesirable as a direct standard of reference.

It is unfortunate that there is not more general uniformity in the material, shape and mass of standards of length. Difference in these particulars are fruitful sources of error in comparisons, and when one adds to this the trouble arising from bad defining lines and imperfect focus, the wonder is that the results are as good as they usually are. It is hard to say what material is least liable to changes, but it is quite certain that substances of crystalline structure, and alloys of which the physical properties are largely dependent on a nearly definite composition, should be avoided. Probably pure platinum, silver, and copper, annealed with the utmost care, and kept for some years before final graduation, are less likely to change than any other material which we know. For short standards, possibly bars of native copper, prepared with as few strains as possible, would give the closest possible approximation to a material which has arrived at a permanent state.

Physical Laboratory, Johns Hopkins University, Feb. 22, 1886.

[To be continued.]

ART. XXIII.—*History of the changes in the Mt. Loa Craters;* by JAMES D. DANA. Part I. KILAUEA. Continuation of the Summary and Conclusions. (With Plates IV and V).

[Continued from xxxiii, 433; xxxiv, 81, 349; xxxv, 15, 181, 1888.]

B. *The Ascensive Action in the Conduit lavas.*

1. *Evidence.*—The evidence in favor of an uplifting action by the ascensive force has been presented in volumes xxxiv, pp. 83, 89, and xxxv, p. 25. It is briefly as follows:

(1) The observations in 1846 by Mr. Chester Lyman demonstrate that in six years the lower pit of 1840, averaging 10,000 feet by 2,500 in its diameters and nearly 400 feet in depth, had gradually become obliterated, and chiefly through an uplift of the floor; for the floor bore on its surface the talus of lava blocks that had fallen from the walls. Overflows had done part of the work, but "subterranean force," as Mr. Lyman concluded, the larger part. Mr. Coan, who was with Mr. Lyman at the time, appreciated the evidence, and later described the lifting as "not uniform in all parts; as sometimes taking place here and there abruptly; but as producing nearly uniform results, except a greater rise toward Halema'uma'u."

(2) In 1868, Mr. Brigham gave further evidence as to the Lyman ridge by the representation of what remained of it in 1865 (xxxiv, 89, and xxxv, 24), on his valuable map, though not, as his memoir shows, understanding its origin. Besides this, the painting of the crater of about the same date (1864 or earlier) by Mr. Perry afforded confirmatory proof as to its position and extent at that time (xxxv, 25).

(3) In 1848, Mr. Coan observed that a cone of broken lava that had formed within the Halema'uma'u basin, was lifted by "subterranean action," as he argued, because only slight addi-

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THE

AMERICAN JOURNAL OF SCIENCE

[THIRD SERIES.]

ART. XXX.—*The Absolute Wave-length of Light;* by LOUIS BELL. Part II.

[Continued from page 282.]

THIS continuation of my previous paper contains the angular measurements and the details of the measurement and calibration of the gratings, together with the final results. In addition I have endeavored to point out the probable sources of error in some recent determinations of absolute wave-length.

Angular Measurements.

In my former paper (this Journal, March, 1887) the work with glass gratings was described in detail, so that it will only be necessary to summarize it here.

Grating I was used during October and November, 1886, and forty-eight series of observations were obtained as follows, each series consisting of three to seven observations.

Date.	Number of series.	Angle.
Oct. 19,	1	45° 1′ 47″·2
20,	1	45 1 48 ·4
22,	2	45 1 48 ·2
23,	1	45 1 49 ·8
26,	4	45 1 49 ·3
27,	3	45 1 48 ·2
31,	1	45 1 50 ·1

Date.	Number of series.	Angle.
Nov. 3,	1	45° 1′ 48″·6
4,	3	45 1 47 ·4
5,	2	45 1 47 ·9
10,	4	45 1 47 ·8
11,	6	45 1 49 ·7
16,	8	45 1 48 ·2
17,	5	45 1 47 ·5
20,	6	45 1 47 ·5

Grating I was used at an average temperature of very nearly 20°, to which all observations were reduced. The average barometric height was 761^{mm}, so that no correction was required for this cause. Weighting and combining the above observations the final value is

$$\varphi = 45^\circ\ 1'\ 48''{\cdot}24 \pm 0''{\cdot}11,$$

corresponding to the spectrum of the third order.

The resulting probable error in wave-length is about one part in a million.

Grating II was used in March, 1887, at an average temperature of very nearly 20° and an average pressure of 760^{mm}. Thirty-six series of observations were obtained in the fourth order, as follows:

Date.	Number of series.	Angle.
Mar. 6,	2	42° 5′ 1″·2
10,	1	42 4 58 ·6
11,	7	42 5 1 ·4
15,	1	42 5 4 ·0
16,	6	42 4 57 ·8
17,	6	42 4 58 ·5
18,	7	42 4 59 ·1
23,	6	42 4 58 ·3

Combining and weighting, the mean value is:

$$\varphi = 42^\circ\ 4'\ 59''{\cdot}28 \pm 0''{\cdot}2.$$

The probable error is equivalent to about one part in six hundred thousand in the wave-length.

Both the glass gratings were used exclusively for the line D_1, which was on the whole most convenient for measurement, D_2 being rejected by reason of the troublesome atmospheric lines. The relative wave-lengths of a very large number of lines have been so exactly determined by Prof. Rowland that any one of them would have given results equally valuable, and in the subsequent work with gratings III and IV, two of these standard lines were employed.

In this second part of the investigation, the gratings as before mentioned were used on the large spectrometer in which the telescopes were kept at a fixed angle and the grating was turned. This method is, of course, applicable only to very solid instruments in which the angle can readily be kept constant, and it should be further noted that it also requires the use of very perfect gratings, since the grating is used asymmetrically. As a result of this the spectra on the two sides differ in dispersion, and if the ruling is irregular either in spacing or in contour of the individual lines, may differ quite widely in focal length, definition and illumination. After critical examination gratings III and IV appeared to be so nearly perfect in ruling, as to be quite secure from the dangers of the method. The method has moreover the distinct advantage of enabling the angle of deviation to be varied within certain narrow limits. Hence it becomes possible so to arrange the apparatus as to give to some convenient line a double reflection that shall be an exact submultiple of 360°. This once accomplished it becomes an easy matter completely to eliminate the errors of the divided circle and obtain a value of $n\varphi$, dependent only on the micrometer constants, which in turn may be themselves almost eliminated. To be sure, this method practically confined observations to the spectra of a given order and limits the choice of lines for measurement, but the first objection does not apply to gratings of which the ruling is very nearly perfect; and since the relative wave-lengths of a large number of lines are known with very great exactness, measurements of the absolute wave-length are quite comparable even if made on different lines.

As regards the constancy of the angle between the collimator and observing telescope there was every reason to expect entire permanence throughout the experiments, and observation soon justified this expectation. The telescopes were firmly secured at both ends to one and the same casting, which in turn was firmly bedded in a brick pier. In addition the size of the apparatus was such that a variation of even 1″ in the angle was quite improbable. The angle measured in the ordinary way with a collimating eye-piece could be determined to 1″ of arc, exclusive of errors of graduation in the circle. At first there appeared to be distinct variations in the angle as determined at the beginning of each series of observations, reaching sometimes more than 10″. It soon appeared however that when the same part of the circle was used the angle between the telescope was sensibly the same and the apparent variations were then traced to a periodic error in the divided circle, which by the method of repetition was completely eliminated from the measurements of angles of deviation and only appeared in the determinations of θ. This error was finally eliminated by measuring θ in various portions of the circle.

The method of determining φ was as follows: The instrument being adjusted by the ordinary methods, a suitable line was selected for measurement and then the angle θ was slightly increased or diminished until by measurement of a double deflection $n\varphi$ was found to be very close indeed to 360°. Then a double deflection was carefully measured and if time permitted several times repeated, an observer always being at the eye-piece to see that the line should not move from the cross hairs while the micrometers were being read. Then, clamping the main circle, the grating holder was turned through 2φ until the line was very closely upon the cross hairs, any slight readjustments made necessary by this disturbance of the instrument were made, and the process was repeated. In this way the initial line of the circle was finally reached and a value of $n\varphi$ obtained which depended only on the algebraical sum of the micrometer readings, always a small quantity.

The determination of the temperature, a very difficult and uncertain matter in the case of glass gratings, is here comparatively simple. A sensitive thermometer (Baudin 6156) was kept in contact with the grating, its bulb being carefully shielded by cotton. The construction of the spectrometer made it impracticable effectively to shield the grating from radiation from the observer's body; but the thermometer apparently proved effective in giving the real temperature since no discrepancies in the results could be traced to thermal causes. The thermometer readings were made to 0°·05, and the temperature of observation rarely varied more than two or three degrees from 20° C.

The temperature being thus obtained, the necessary correction was introduced directly into the angle of deviation. Writing the formula for wave-length in the form

$$\lambda = C\, s \sin \varphi,$$

where C is a factor depending on the method in which the grating is used, and differentiating we obtain

$$\frac{\delta s}{s} = -\cot \varphi \delta \varphi,$$

where if we take 1° for the temperature variation $\frac{\delta s}{s}$ is the coefficient of expansion. Whence

$$\delta\varphi = -\frac{\frac{\delta s}{s}}{\cot \varphi} =$$

correction for 1° variation in temperature. For grating III for instance $\delta\varphi = 2''\cdot688$ and by this means all the deviations were

reduced to 20°. Writing again the equation for wave-length in the form for the method here used,

$$c = \sin \varphi \cos \theta.$$

Now to obtain the variation in φ due to a change in the angle between the telescopes,

$$\delta\varphi = \tan \varphi \tan \theta \delta\theta.$$

Taking now $\delta\theta = 1''$ and φ as found in these experiments

$$\delta\varphi = 0''\cdot089.$$

By this means the necessary correction could be introduced in the angle of deviation, but the angle between the telescopes was so nearly constant as to render this correction needless.

The line selected for measurement with III was a sharp one in the green at 5133·95 of Rowland's map. The angle θ between the telescopes was adjusted so that in the eighth order the double deflection was 72°. Eighteen complete series of observations were then obtained, each giving a value of 10φ from which the errors of the circle were completely eliminated. The results in detail were as follows, corrected to 20° on thermometer used,

Date.			φ.			
1887.	Nov.	2,	36°	0′	27″	·19
	"	3,	36	0	25	·87
	"	4,	36	0	24	·40
	"	5,	36	0	24	·95
	"	5,	36	0	26	·83
	"	9,	36	0	26	·14
	"	16,	36	0	27	·40
	"	16,	36	0	27	·37
	"	17,	36	0	27	·57
	"	22,	36	0	25	·16
	"	29,	36	0	25	·69
	"	29,	36	0	25	·99
	"	29,	36	0	25	·91
	"	30,	36	0	26	·10
	"	30,	36	0	25	·86
	"	30,	36	0	25	·81
	Dec.	1,	36	0	25	·68
	"	1,	36	0	25	·80

The last decimal place is retained simply for convenience in averaging. The mean value of φ is 36° 0′ 26″·07 which reduced for the error of thermometer at 20° gives finally,

$$\varphi = 36°\ 0'\ 25''\cdot17.$$

The probable error of this value is 0″·14. The effect of a small error in φ on the resulting wave-length is given at once by

$$\delta\lambda = \cos \varphi \delta\varphi.$$

In this case the error introduced by an error of 1″ in φ is a little less than 1 part in 250000.

The mean value of θ during these measurements was

$$\theta = 6^\circ\ 59'\ 58''\cdot 6.$$

In case of grating IV the line selected for observation was one of Rowland's standards at w.l. 5914·319 of his preliminary list. It is a very close double, the components being distant from each other something like $\frac{1}{15000}$ of their wave-length. The double deflection was as before 72° but in the fifth order. As with grating III eighteen series of observations were obtained, with the following resulting values of φ

Date.			ϕ.
1887.	Dec.	16,	36° 0′ +1″·16
	"	16,	36 0 +0 ·66
	"	16,	36 0 +0 ·67
	"	19,	36 0 +0 ·64
	"	19,	36 0 +1 ·56
	"	19,	36 0 +0 ·85
1888.	Jan.	12,	36 0 −1 ·19
	"	12,	36 0 −1 ·61
	"	12,	36 0 −1 ·79
	"	14,	36 0 −1 ·09
	"	14,	36 0 −0 ·95
	"	14,	36 0 −0 ·89
	"	19,	36 0 −0 ·48
	"	19,	36 0 −0 ·59
	"	19,	36 0 −0 ·49
	"	20,	36 0 +0 ·51
	"	20,	36 0 −0 ·11
	"	20,	36 0 +0 ·55

The mean value, corrected as before for error of thermometer, is:

$$\varphi = 35^\circ\ 59'\ 59''\cdot 06 + 0''\cdot 15$$

The effect of this probable error is obviously the same as in case of grating III. The mean value of the semiangle between the telescopes was

$$\theta = 6^\circ\ 58'\ 31''.$$

During the observations with grating III the barometric height reduced to the place of observation was very nearly 762^{mm}, but during the work with grating IV it was phenomenally high, reaching an average value of 766^{mm}, an amount so far from normal pressure as to render a small correction necessary.

The mean temperature during the observations with III was about 21° C., but in case of IV it averaged almost exactly 20° C. varying at most only two or three degrees from that figure.

Measurement of the Gratings.

The comparator on which this, the most important portion of the research, was accomplished was the same one described in my previous paper. It had however been improved in several particulars. The platform carrying the standards had been fitted with smooth rack and screw adjustments, and the microscopes and micrometers were new. The illumination of a grating under the power used,—two hundred and fifty diameters—is by no means an easy matter, and at the same time a powerful and symmetrical illumination is absolutely necessary for the most accurate work, particularly in case of rather small grating spaces. I had been thoroughly dissatisfied with the illumination previously used—a lamp at a suitable distance—and now made a radical change. A three candle-power electric lamp was attached directly to the microscope just below the eyepiece and about a foot above the objects measured. A small mirror carried by an arm screwed to the objective reflected the beam into the Tolles illuminator. A glass bulb filled with water surrounded the light and served the double purpose of stopping radiation and partially condensing the beam upon the mirror above mentioned.

I am aware that such an arrangement is somewhat revolutionary, and it was only after a careful trial that I convinced myself that the heat from so near a source was not injurious.

In the first place it should be noted that the lamp is only used for a few moments at a time and at intervals long compared with the time of observation. Thus the very minute heat wave that reaches the bar through the bulb of water cannot possibly produce a perceptible rise of temperature during the time of an observation, while during the intervals it is completely dissipated.

As an experimental fact, no heating effect whatever is sensible even after a whole day's observations. To show at once this fact, and the general character of an average series of comparisons I subjoin ten comparisons of $Dm_1S^a_2$ with a certain decimeter on glass, made at intervals of about three-quarters of an hour on two successive days. The figures are taken directly from my note book.

Date.		T=
June 1, 1887	$Dm_1S^a_2$ = G + $21^d\cdot 3$	17°·4
"	" " + 21·6	17 ·4
"	" " + 22·1	17 ·5
"	" " + 22·1	17 ·5
"	" " + 20·8	17 ·5
"	" " + 20·1	17 ·5
June 2,	" " + 21·4	17 ·0
"	" " + 21·0	17 ·0
"	" " + 21·0	17 ·0
"	" " + 21·0	17 ·1

The temperature was given by a thermometer in contact with S^a_2 and 1δ of the micrometer equalled $0^{\mu}\cdot28$. In a comparison of two standards with such unequal coefficients of expansion as glass and speculum metal, the evil effects of radiation should be at their maximum, but the preceding series, including as it does all the experimental errors and showing an extreme variation of but $0^{\mu}\cdot5$, leaves, I think, little to be desired.

The comparator was placed in a vault some six feet below the level of the street, which was provided with thick double walls with an air space between. This observing room enabled the temperature to be kept down to a daily variation of less than half a degree, the extreme range for several days being frequently less than that amount. Before this vault in the new Physical Laboratory was completed the comparator had been placed in an upper room of one of the old buildings, where it was well nigh impossible to keep anything like a constant temperature, particularly since the heat was unavoidably partially shut off during the night. Owing to this state of affairs the measurement of the gratings on which my preliminary wave-length was based, was made under difficulties and in most of the series necessarily under a rising temperature. Now when a glass standard is measured against a metal one, glass being a notoriously bad conductor, and having a very small coefficient of expansion, if any rise of temperature takes place the length found for the glass will be too small, for responding less readily to a change it will be actually measured at a lower temperature.

It therefore became necessary to re-measure the glass gratings Nos. I and II, to eliminate this source of error, which was done before the results for III and IV were obtained. These gratings are very nearly 3^{cm} long and they were therefore compared with successive triple centimeters of S^a_2 until the fifteen centimeter mark was reached. Grating I was first taken in hand and six complete series of observations were obtained, each micrometer reading being the mean of several, and the extreme limits of temperature variation during the two days occupied by the comparisons being $0^{\circ}\cdot3$ C. The following gives a summary of the results.

$$\left.\begin{array}{l} 5G = 15^{cm}S^a_2 + 19^{\delta}\cdot0 \\ 5G = 15^{cm}S^a_2 + 21\cdot5 \\ 5G = 15^{cm}S^a_2 + 18\cdot1 \\ 5G = 15^{cm}S^a_2 + 23\cdot9 \\ 5G = 15^{cm}S^a_2 + 22\cdot6 \\ 5G = 15^{cm}S^a_2 + 18\cdot3 \end{array}\right\} \text{At } 19^{\circ}\cdot9 \text{ C.}$$

Hence combining these and reducing them to the standard temperature of 20° we have:

$$60000 \text{ spaces} = 5G = 15^{cm}S^a_2 + 5^{\mu}\cdot2 \text{ at } 20^{\circ}$$

The micrometer constant here used was that of the new micrometer where $1\delta = 0^{\mu}\cdot257$.

In precisely the same way Grating II was remeasured, the six series giving the following relations

$$\left.\begin{array}{l} 5G = 15^{cm}S^a_2 + 157^{\delta}\cdot4 \\ 5G = 15^{cm}S^a_2 + 154\cdot9 \\ 5G = 15^{cm}S^a_2 + 154\cdot5 \\ 5G = 15^{cm}S^a_2 + 152\cdot4 \\ 5G = 15^{cm}S^a_2 + 154\cdot9 \\ 5G = 15^{cm}S^a_2 + 162\cdot4 \end{array}\right\} \text{At } 19^{\circ}\cdot8 \text{ C.}$$

Combining and reducing these results as before we have the equation

$$42640 \text{ spaces} = 5G = 15^{cm}S^a_2 + 39^{\mu}\cdot9 \text{ at } 20^{\circ}$$

The temperature variation in the two days of observation was only $0^{\circ}\cdot2$.

Gratings III and IV were then measured. In this case a large number of comparisons were obtained at both high and low temperatures with the object of detecting any differences which might exist between the coefficients of expansion of the gratings and those of the speculum metal standards. III and IV being a little over a decimeter in length were very easy to measure, particularly since the lines were very sharp and of approximately the same width as those on the standards.

III proved to have sensibly the same coefficient as the standards. I subjoin the comparisons made at or very near 20°.

$$\begin{array}{l} G = Dm_1S^a_2 + 32^{\delta}\cdot9 \\ G = \quad \text{"} \quad + 33\cdot0 \\ G = \quad \text{"} \quad + 32\cdot7 \\ G = \quad \text{"} \quad + 33\cdot2 \\ G = \quad \text{"} \quad + 32\cdot3 \\ G = \quad \text{"} \quad + 32\cdot6 \\ G = \quad \text{"} \quad + 34\cdot5 \\ G = \quad \text{"} \quad + 33\cdot4 \\ G = \quad \text{"} \quad + 34\cdot2 \\ G = \quad \text{"} \quad + 32\cdot6 \end{array}$$

Combining these and other series of observations gives finally

$$28418 \text{ spaces} = G = Dm_1S^a_2 + 8^{\mu}\cdot5 \text{ at } 20^{\circ}$$

It should be noted that the extreme variation in the above series is $2^{d}\cdot 2$, very nearly $0^{\mu}\cdot 5$, or one part in two hundred thousand.

In the case of IV the coefficient appeared to be somewhat smaller than that of S^{a}_{2}. The range of temperature secured was not large but as nearly as could be ascertained the coefficient is about $16^{\mu}\cdot 1$ per meter per degree, while that of the standards is $17^{\mu}\cdot 9$ per meter per degree. However, since the measurements of φ made with IV were distributed with a tolerable degree of symmetry on both sides of 20°, any error due to an inexact value of the coefficient of expansion would appear mainly in the probable error in φ. The variation found would, as a matter of fact have changed the final value of φ by less than $0''\cdot 2$.

The comparisons of IV made near 20° were as follows:

$$G = Dm_1S^a_2 + 35^{d}\cdot 8$$
$$G = \quad " \quad + 35\cdot 5$$
$$G = \quad " \quad + 35\cdot 6$$
$$G = \quad " \quad + 36\cdot 0$$
$$G = \quad " \quad + 34\cdot 0$$
$$G = \quad " \quad + 35\cdot 8$$
$$G = \quad " \quad + 34\cdot 6$$
$$G = \quad " \quad + 36\cdot 3$$
$$G = \quad " \quad + 33\cdot 3$$
$$G = \quad " \quad + 36\cdot 7$$

Combining these and the other observations,

$$39465 \text{ spaces} = G = Dm_1S^a_2 + 9^{\mu}\cdot 1 \text{ at } 20°.$$

The probable error of the relations found for III and IV can hardly exceed one part in a million so far as the distance between the terminal lines selected is concerned. These terminal lines were varied at each comparison so that while each of the above relations represents 39,465 spaces, the lines measured between, though in the same vicinity, are seldom or never identical.

In gratings I, II, III the number of spaces was very easily counted as the dividing engine automatically rules every hundredth line longer, and every fiftieth line shorter, than the others. In grating IV the number of spaces was found readily enough by ruling at a known temperature the terminal lines of a test plate almost exactly a decimeter long, and containing a known number of lines. A comparison of this with the grating gave the quantity required.

Calibration of the Gratings.

In my previous paper the need and method of determining the errors of ruling in a grating were briefly noticed. It is fitting here to enter somewhat more into detail.

The grating space is never perfectly uniform throughout the whole extent of the ruled surface. The variations may be in general classed as regular and irregular. In the first class we put variations in the grating space which are purely periodic or purely linear. These produce respectively "ghosts," and difference in focus of the spectra on opposite sides of the normal. Either fault might be large enough to unfit the grating for wave length determination, and would be always undesirable, but nevertheless would introduce no gross errors into the result. Variations of the second class include the displacement, omission or exaggeration of a line or lines, and what is of great importance, a more or less sudden change in the grating space producing a section of the grating having a grating space peculiar to itself. The former types of accidental error, unless extensive are harmless, and are present in most gratings usually showing as faint streaks in the ruling. It is with the last mentioned error that we mainly have to do.

Consider a grating the space of which is sensibly uniform except throughout a certain portion. Let that portion have a grating space distinctly larger or smaller than that of the remainder of the grating. If the abnormal portion is a considerable fractional part of the whole grating it will, in general, produce false lines and injure or ruin the definition of the grating. Such a grating we should nowadays throw aside as useless, although many of the older gratings are thus affected. Suppose however that the abnormal portion is confined to a few hundred lines. Such a series of lines will have little brilliancy and less defining power and consequently will simply diffuse a certain amount of light without either producing false lines or, in general, injuring the definition. In short, when the full aperture of the grating is used, the spectra produced will be due only to the normal grating space, the abnormal portion having little or no visible effect. If however we attempt to evaluate the grating space by measuring the total length of the ruled surface and dividing it by the number of spaces therein contained, we shall obtain an incorrect result, since this average grating space, including, as it does, the abnormal portion, will be necessarily different from the normal grating space which produces the spectra observed.

In general if n be the total number of spaces and s the normal grating space the length of the ruled surface will be $ns+A$, where A is a quantity depending on the magnitude and nature of the abnormal portion. It will have for its maximum value $\Sigma(s-s')$, where s' is the varying grating space, in the case when the change in the space is so local and sudden as to produce no effect at all on the spectrum; and will be variously modified by the considerations now to be mentioned. If we could always assume that the abnormal portion of the grating produced no effect on the spectrum the elimination of errors of ruling would thus become comparatively simple. But in practice it is not very uncommon to find gratings in which there are several portions where the spacing is abnormal, in one case perhaps producing no effect, in a second producing false lines and in a third causing a faint shading off of the lines. For an abnormal portion will produce no effect, a slight shading or reduplicated lines, according to its extent and the amount of its variation from the normal.

The following experiment will readily show the laws which govern these errors of ruling. Place a rather bad grating—unfortunately only too easily obtained—on the spectrometer, and setting the cross-hairs carefully on a prominent line, gradually cover the grating with a bit of paper, slowly moving it along from one end. In very few cases will the line stay upon the cross-hairs. A typical succession of changes in the spectrum is as follows: Perhaps no change is observed until two-thirds of the grating has been covered. Then a faint shading appears on one side of the line, grows stronger as more and more of the grating is covered, and finally is terminated by a faint line. Then this line grows stronger till the original line appears double and finally disappears leaving the displaced line due to the abnormal grating space. This description, I regret to say, is from the examination of a grating which had been used for the determination of absolute wave-length.* This case is exceptionally complete, but even with a very good grating minute displacements can usually be noticed.

When the abnormal portion is sufficiently extensive to produce a faint shading along one side of the lines when the full aperture of the grating is used, the effect of the error on the resulting wave-length may be in part eliminated by the fact that the shading would displace the apparent center of the line and hence slightly change the observed angle of deviation. For this reason a grating so affected would be likely to give results varying with the order of spectrum used, since the appearance of the line would vary somewhat with the illumination. It is at once apparent, however, that no combination of the results from different orders of spectra can possibly eliminate the class of errors we are discussing, since the algebraic sign of the error will be the same for all orders and it will be in every case a nearly constant fraction of the wave length.

* Not by the author it is almost needless to add.

The problem before the experimenter is then the following: To detect the existence and position of any abnormal portion of the grating in use, to separate as far as possible such portions as produce a visible effect from those which do not, and thus finally to determine the proper value to be assigned to the quantity A.

The investigation is somewhat simplified by the fact that, for the most part, abnormal spacing occurs at an end of the ruled surface, generally at the end where the ruling was begun, since, when the engine is started it is likely to run for some little time before it settles down to a uniform state. Then, too, one is able to disregard the slight and gradual variations in the grating space which appear in every grating, since their effects will in general be integrated in the spectrum produced.

It only remains therefore to study those larger and more sudden changes which can produce a sensible error in the result. It is evident that the process of examination indicated above will serve to detect the more extensive faults, together with any errors of figure in the surface, but an abnormal portion consisting of only a few hundred lines will not have resolving power enough to produce a marked effect. Making then a slit in a card just wide enough to expose a sufficient number of lines to give tolerable definition, one can examine the grating section by section, and still further discriminate between the normal and abnormal spacing, errors of figure being included as before. But as the number of abnormal spaces decreases a point will be reached when this method breaks down completely, and since the error in the resulting wave-length may be as large in this case as when the fault is more extended, another method must be sought. So far as I know the only method which will detect and evaluate all these errors is that which I have called calibration, measuring the relative lengths of n grating spaces taken successively along the ruled surface. The process employed was as follows. The stops of the comparator were set as close together as practicable, limiting the run of the carriage to a distance which varied in different cases from 4 to 10^{mm}. Then the grating to be examined was brought under the microscope and micrometer readings were taken on the lines just within the run of the carriage; the grating was then moved along about the length of the run and the process repeated till the whole grating had been gone over. The variations in the micrometer readings then gave the variations in the length of n spaces in different parts of the grating. The only assumption involved was that the variation in the different sections did not amount to an entire space, an hypothesis quite secure in gratings with spaces as large as those employed. It was thus possible to determine quite accurately the variations in the grating space throughout the whole grating.

It should be noted that since these variations may be of almost any kind and magnitude the errors produced by them will not in general be eliminated by combining the results obtained from several gratings. It may happen that the gratings used by one experimenter will have errors that will counterbalance each other, while those used by another will all have errors of the same sign. For instance, by the merest accident the gratings used by the writer gave nearly identical results corrected and uncorrected, while those used by Peirce uniformly required a reduction in the resulting wave-length. The number of gratings used by a given investigator is however so small that the errors will very seldom be eliminated, while no combination of the results obtained from different orders of the same grating can produce any useful effect whatever.

Each of the gratings used in this research was examined minutely by the above methods and in each was found an abnormal portion of one sort or another. Of eight gratings which I have calibrated all have shown a similar error and of more than twenty which I have examined in the spectrometer only one (grating III) failed to show an abnormal section at one end. Since this is the commonest form of the error in question, it is but natural to inquire why it cannot be avoided by covering the defective end. The reason is simple enough. By stopping out the defective portion the grating is reduced to an incommensurable length which enormously increases the difficulty of measuring it. A grating which is in length some convenient submultiple of a meter is easy to measure with a comparatively high degree of exactness, but one which is, say, twenty seven millimeters long, is exceedingly difficult to measure accurately since it involves a long micrometer run or the errors of subdivision down to single millimeters. It is therefore better to use the full aperture of the grating and find A by calibration.

In calibrating the gratings used, I divided I and II, which were thirty millimeters long, into six sections of 5^{mm}, and the large gratings III and IV into centimeters. Each grating was carefully gone over five times and the mean result taken. The following corrections were found.

The actual variations found in each grating are given below, the figures given being the difference of n lines from the distance between the stops, the lines being taken in the consecutive sections of the gratings.

Grating I.

Sections	1	2	3	4	5	6
Residuals,	$0^{\mu}\cdot78$	0·98	0·81	1·03	0·86	1·24

Grating II.

Sections	1	2	3	4	5	6
Residuals,	$2^{\mu}\cdot07$	1·93	1·52	1·68	1·31	0·45

Grating III.

Sections	1	2	3	4	5	6	7	8	9	10
Residuals,	$2^{\mu}\cdot80$	2·85	1·77	2·77	2·70	2·77	2·67	2·64	2·73	2·77

Grating IV.

Sections	1	2	3	4	5	6	7	8	9	10
Residuals,	$0^{\mu}\cdot31$	0·28	0·35	0·43	0·40	0·43	0·31	0·35	0·28	0·82

The calibration of III is worth describing in detail. Centimeter 3 was evidently too long. I therefore measured the centimeters from 15 to 25^{mm} and from 25 to 35^{mm}. The former was quite normal but the latter showed an increase almost identical with that of the whole third centimeter. I then examined the grating in a strong light and detected at 27^{mm} from the end, a faint line, such as usually indicates a few wavering lines caused perhaps by dust under the diamond point. Placing, however, this line under the microscope a band of perhaps twenty lines appeared with spacing noticeably wider than usual. Here was a very serious flaw in a grating to all appearance absolutely perfect. A most critical examination in the spectrometer of course failed to detect it, but it was both detected and located with unerring certainty by the process of calibration. Micrometrical measurements on this group showed an excess of about $2^{\mu}\cdot5$ over an equal number of spaces elsewhere on the grating. This quantity of course had to be taken account of in connection with the previous calibration.

The deduction of the necessary corrections from the data given by calibration requires no little care and judgment, and can be properly done only in connection with a detailed study of the spectra given by various portions of the gratings concerned. For the four gratings used by the author, these corrections, applied directly to the lengths of the gratings in the form of the quantity A before mentioned, are very nearly as follows:

Grating.	A
I	$-0^{\mu}\cdot10$
II	+ 0·40
III	− 2·00
IV	+ 0·45

It should be distinctly understood that the corrections deduced from the calibration are necessarily only approximate. A very minute examination of a grating on the spectrometer is impossible, since a small section of the ruled surface has not sufficient resolving power to give measurable spectra. On the other hand, while calibration gives the variations of the grating space with a high degree of exactness, it obviously cannot definitely decide how far these variations are integrated in the spectrum measured. Consequently while calibration will in every case give a valuable approximation, it must necessarily leave residual errors.

In these experiments the gratings were always measured parallel to the terminations of the lines. Consequently the length of each grating as found directly must be multiplied by cos $(90^\circ - \alpha)$, where α is the angle made by an individual line with the line formed by the locus of the terminations. In case of gratings I, II, III this angle was found by measuring a test plate as described in my previous paper and was found to be within a very few seconds of 89° 56′.

Grating IV ruled on the new engine was tested by measuring the sides and diagonals of the ruled surface and gave an almost exactly identical value of α. No correction therefore need be introduced for this cause, since $\cos(90^\circ - \alpha)$ does not differ sensibly from unity.

Final result for Absolute Wave-length.

Only one equation needs to be added to those already given for S^a_2. This is the one for the third 5^{cm} space, necessary to determine the absolute length of the first 15^{cm}. 5^{cm}, (3) and (4) were compared and the following relation was found between them: $(4) = (3) + 0^{\mu}\cdot4$. The relation found in 1885 was $(4) = (3) + 1^{\mu}\cdot1$. Consequently (3) has not sensibly shortened and nearly the whole change found in S^a_2 has taken place in the last five centimeters. Writing now the absolute lengths of

$$\mathrm{Dm}_1\ S^a_2 \text{ and } 15\mathrm{cm}.\ S^a_2,$$

$$\mathrm{Dm}_1\ S^a_2 = 100\cdot00666^{mm} \text{ at } 20^\circ.$$

$$15\mathrm{cm}\ S^a_2 = 150\cdot90897 \text{ at } 20^\circ.$$

Applying now the relations found for grating I in the foregoing section,

$$s = 0\cdot002500226^{mm}$$

And since

$$\varphi = 45^\circ\ 1'\ 48''\cdot[illegible]$$

$$\lambda = 5896\cdot18.$$

Similarly for grating II,

$$s = 0\cdot003519041^{mm}$$

$$\varphi = 42^\circ\ 4'\ 59''\cdot28$$

$$\lambda = 5896\cdot23$$

Computing the similar quantities for the speculum metal gratings III and IV, for grating III,

$$s = 0\cdot003519358^{mm}$$

$$\varphi = 36^\circ\ 0'\ 25''\cdot17$$

$$\theta = 6^\circ\ 59'\ 58''\cdot56$$

$$\lambda = 5133\cdot89$$

and for grating IV,

$$s = 0\cdot002534306^{mm}$$

$$\varphi = 35^\circ\ 59'\ 59''\cdot06$$

$$\theta = 6^\circ\ 58'\ 31''$$

$$\lambda = 5914\cdot37$$

Reducing now these latter wave lengths to the corresponding values of D_1, introducing the barometric corrections and combining, the final results for that line are

Grating	W. L.
I	5896·18
II	5896·23
III	5896·15
IV	5896·17

Finally, then, the mean value of the absolute wave-length of D_1 in terms of the mean value assigned to S^a_2 is

$$5896\cdot18$$

in air at 760^{mm} pressure and 20° C. temperature, or *in vacuo*,

$$5897\cdot90$$

It is no easy matter to form an estimate of the probable error of this final result. So far as errors of observation go, the result should be correct to within one part in half a million, but there are so many complex sources of constant errors in this problem that such a statement means little. My present result exceeds the estimated probable error of my former result considerably, though it falls within the limit set by Prof. Rowland and myself for the possible error and noted in his paper on "Relative Wave-length" of the same date as my own. The cause of this discrepancy is partly due to the varying temperature under which the glass gratings were first measured, and partly to the change in the value assigned to the standard of length.*

Then too, the corrections applied to gratings II and III may be slightly in error. Taking into account all these sources of uncertainty it is my opinion that the above final result is not likely to be in error by an amount as great as one part in two hundred thousand.

* In terms of the length I originally assigned to S^a_2 the wave-length of D_1 would be 5896·14, while if the value deduced from the Berlin comparison were taken it would be 5896·22. The wave-length quite certainly lies between these values, but the proper weight to be given to the Berlin comparison relatively to the others is rather uncertain.

Taking the above value of the absolute wave-length and applying the appropriate corrections to some of the fundamental lines given in Prof. Rowland's paper (this Journal, March, 1886) the wave-lengths of the principal Fraunhofer lines in air at 20° and 760^{mm} are,

A (line between "head" and "tail" of group)	7621·31
B "	6884·11
C	6563·07
D_1	5896·18
D_2	5890·22
E_1	5270·52
E_2	5269·84
b_1	5183·82
F	4861·51

Comparisons between these wave-lengths and the older ones become somewhat uncertain toward the ends of the spectrum since the appearance of lines like A, B, G and H vary so much with the dispersion employed. The relative wave-lengths above given are certainly exact to within one part in half a million.

It may not be out of place here to discuss the most recent work on this problem. Just before the publication of my first paper the very elaborate paper of Müller and Kempf appeared. Their work is a monument of laborious research and it is unfortunate that so much time should have been spent in experiments conducted with glass gratings of small size and inferior quality. Since the invention of the concave grating, it is a waste of energy to make micrometric measurements with plane ones, and this statement could hardly be corroborated more strongly than by the relative wave-lengths given by Müller and Kempf. The probable error of their wave-lengths is in general not less than one part in two hundred thousand. That the value assigned by them to the absolute wave-length is as near the truth as it probably is, is due to no lack of faults in the gratings. Their results for the line D_1 were as follows:

Grating.	W. L.
"2151"	5896·46
"5001"	5896·14
"8001"	5895·97
"8001L"	5896·33

A discussion of these errors as exemplified in the paper under consideration would take up too much space to be inserted here, but one or two points are worthy of notice. When a grating gives different results in the different orders, it is evident that there are in it serious errors of ruling, and the maximum amount of the variation will give a rough estimate of their size as compared with those of other gratings. Applying this test, the four gratings rank as follows: "5001," "8001 L," "2151," "8001," where the first which gave for the w. l. 5896·14, had no sensible variation in the different orders and the last, which gave 5895·97, varied in the most erratic fashion. It by no means follows, however, that because a grating gives identical results in the various orders, it is therefore free from errors of ruling. Witness Grating III of this paper in which the error was of a kind which could not be detected at all in the spectrometer. Yet it was large enough to give, if neglected, 5896·28 for the wave-length of D_1.* Speaking of errors in gratings a case in point is the work of Peirce. On account of the reasons heretofore noted Peirce's standards of length are somewhat uncertain in value so that no definite correction can be as yet applied to his wave-length from this cause. Three of his gratings, however, I have calibrated, and each of them showed an error tending to diminish the wave-length. If the mean result obtained from these had been assumed to be correct it would have been equivalent to the introduction of a constant error. Peirce's preliminary result is for this reason too large by more than one part in a hundred thousand: how much more, it is impossible to say without knowing the results obtained from each grating and so being able to apply the corrections found. Peirce's method was such as should have secured very excellent results and such will undoubtedly follow a further investigation of the standards and gratings. Still another recent determination is that by Kurlbaum, who used two good sized speculum metal gratings and measured them with particular care. Like the previous experimenters he neglected, although he did not ignore, the errors of ruling and consequently the results he obtained are somewhat in doubt. A serious objection, moreover, to his work is the very small spectrometer he used. To undertake a determination of absolute wave-length with a spectrometer reading by verniers to 10″ only, and furnished with telescopes of only one inch aperture is simply courting constant errors. More especially is this true since it would be hard to devise a method more effective in introducing the errors of ruling, than to use a grating with telescopes too small to utilize its full aperture, and then determine the grating space by measuring the total length of the ruled surface. Kurlbaum's gratings, too, were of an unfortunate size, 42 and 43^{mm} broad respectively, and consequently by no means easy to measure. On the whole his result, 5895·90 is not surprising.

* The results given by the gratings used by the author, neglecting the correction A would be as follows:

I. 5896·20; II, 5896·14; III, 5896·28; IV, 5896·12

Curiously enough the mean would be practically unchanged.

The agreement of relative wave-lengths as determined by different experimenters unfortunately gives no measure as to the accuracy of the work. The relative wave-lengths as determined by Müller and Kempf and by Kurlbaum agree in general to within 1 part in 100,000: the absolute wave-lengths assigned by these experimenters vary by more than 1 part in 30,000.

A very ingenious flank movement on the problem of absolute wave-length has been made by Macé de Lépinay. His plan was to use interference fringes in getting the dimensions of a block of quartz in terms of the wave-length, and then to avoid the difficulties of the linear measurement by obtaining the volume through a specific gravity determination. His results do not indicate, however, experimental accuracy as great as can be obtained by the usual method, and the final reduction unfortunately involves a quantity even more uncertain than the average standard of length, i. e., the ratio between the meter (?) and the liter.

It may be interesting here to collect the various values which have been given for the absolute wave-length within recent years. Results are for the line D_1.

Mascart	5894·3
Van der Willigen	5898·6
Ångström	5895·13
Ditscheiner	5897·4
Peirce	5896·27
Ångström corrected by Thalen	5895·89
Müller and Kempf	5896·25
Macé de Lépinay	5896·04
Kurlbaum	5895·90
Bell	5896·18

These figures are discordant enough. When beginning the present work, I had hoped that it would prove possible to make a determination of absolute wave-length commensurate in accuracy with the relative wave-lengths as measured by Prof. Rowland. This hope has proved in a measure illusory, by reason of the small residual errors of the gratings and the greater uncertainty involving the standards of length. I feel convinced, however, that the result reached is quite near the limit of accuracy of the method. It should be remembered that any and every method involves the uncertainty of the standards of length, an uncertainty not to be removed until a normal standard is finally adopted and exact copies of it distributed. And as far as experimental difficulties are concerned, the next order of approximation will involve a large number of small but troublesome corrections, such as the effect

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of aqueous vapor on atmospheric refraction, varying barometric height, the minute variations in the grating space, failure of thermometer to give temperature of grating exactly, and countless others which will suggest themselves only too readily.

Aside from the use of gratings, decidedly the most hopeful method as yet suggested is that due to Michelson and Morley.* Theoretically the plan is particularly simple and beautiful, consisting merely in counting off a definite number of interference fringes by moving one of the interfering mirrors and measuring, or laying off upon a bar, the resulting distance. The mechanical difficulties in the way, are however formidable, and whether or no they can be surmounted only persistent trial can show. The possible sources of error are of much the same type and magnitude as those involved in the comparison of standards of length, and if these errors are avoided, the uncertainty concerning the standards still remains. Whether or no the practical errors of the method are greater or less than with gratings only experience can prove. Certainly if the method is capable of giving exact results it is in the hands of one able to obtain them from it.

In closing this paper I can only express my sincerest gratitude to the various friends who have done all in their power to facilitate my work, and especially to Professor W. A. Rogers who has been tireless in his endeavors to determine the true value of the standards of length; to Mr. J. S. Ames, Fellow in this University, who has given me invaluable aid in the work with metal gratings; and to Professor Rowland who has furnished all possible facilities and under whose guidance the entire work has been carried out.

Physical Laboratory, Johns Hopkins University, March, 1888.

Art. XXXI.—*Three Formations of the Middle Atlantic Slope*; by W. J. McGee. (With Plates VI and VII.)

(Continued from page 330.)

The Columbia Formation.

General Characters.—The Columbia formation exhibits two phases which, although distinct where typically developed, intergraduate. The thicker and more conspicuous phase occurs commonly along the great rivers at and for some miles below the fall line, and may be designated the *fluvial* phase; while the thinner generally forms the surface over the remainder of the Coastal plain, and may be designated the *interfluvial* phase.

* This Journal, III, xxxiv, 427.