THE
AMERICAN JOURNAL
OF
SCIENCE AND ARTS.

CONDUCTED BY

PROFESSORS B. SILLIMAN AND JAMES D. DANA,

IN CONNECTION WITH

PROFESSORS ASA GRAY, AND WOLCOTT GIBBS,
OF CAMBRIDGE,

AND

PROFESSORS H. A. NEWTON, S. W. JOHNSON,
GEO. J. BRUSH, AND A. E. VERRILL,
OF NEW HAVEN.

SECOND SERIES.
VOL. L.—[WHOLE NUMBER, C.]
Nos. 148, 149, 150.
JULY, SEPTEMBER, NOVEMBER.

NEW HAVEN: EDITORS.
1870.

PRINTED BY TUTTLE, MOREHOUSE & TAYLOR, 221 STATE ST.

CONTENTS OF VOL. L.

NUMBER CXLVIII.

SCIENTIFIC INTELLIGENCE.

NUMBER CXLIX.

SCIENTIFIC INTELLIGENCE.

NUMBER CL.

SCIENTIFIC INTELLIGENCE.

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

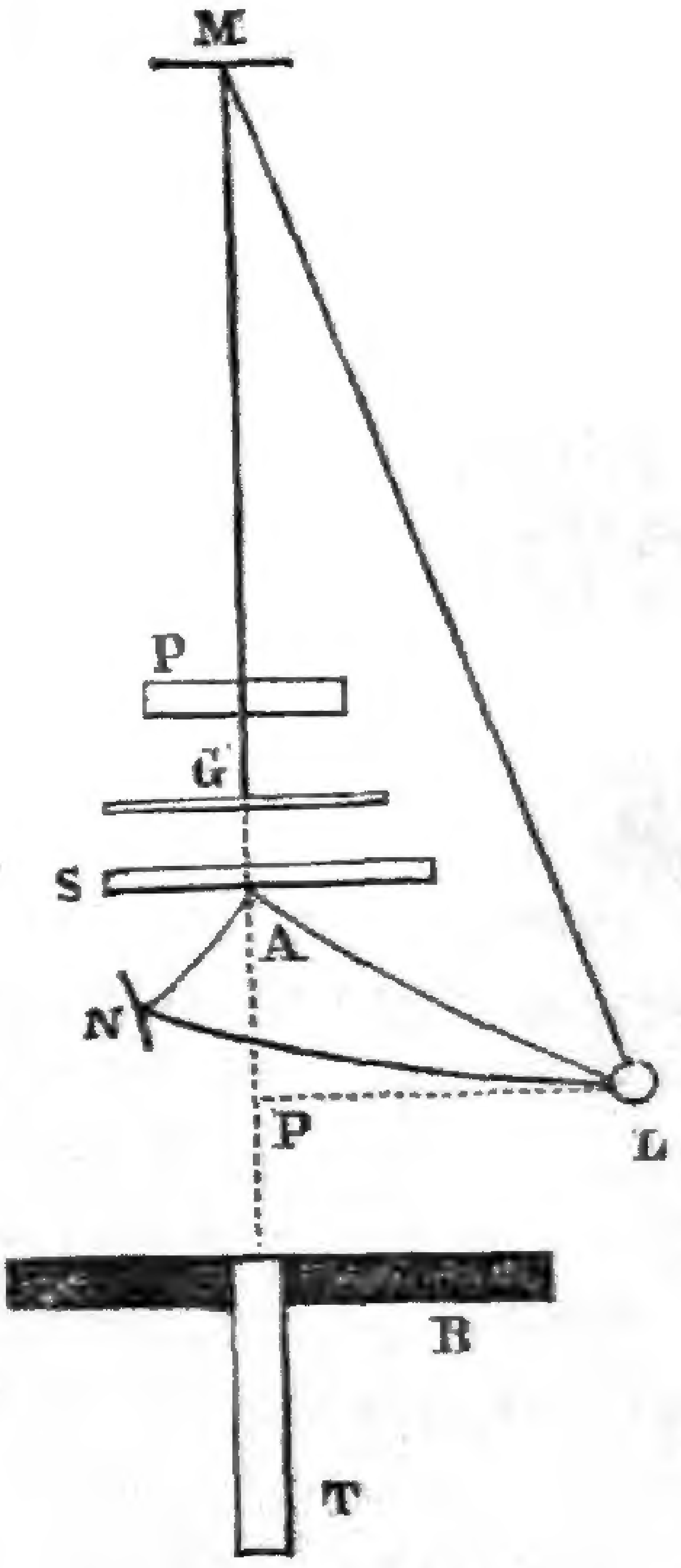
ART. I.—*Photometric Experiments.* PART SECOND.—*On the amount of light transmitted by plates of polished crown glass at a perpendicular incidence;* by OGDEN N. ROOD, Professor of Physics in Columbia College.

IT would seem that in the direct photometric experiments on this matter, the instrumental means employed have been more or less defective, in consequence of which the results gained, although having a certain amount of general correctness, still leave much to be desired. The main sources of error have been, first, the use of a defective mode of making the compensations, not allowing the experimenter to take full advantage of the sensitiveness of the eye, and secondly the employment of *two sources* of light, which always brings with it another set of unavoidable errors. Apart from this, in cases when the amount of reflected or transmitted light has been thus approximately obtained, the indices of refraction of the substances used do not seem to have been determined, so that for *perpendicular incidence*, slender data exist for comparing the results of theory and experiment.

It will be seen in what follows, that, to avoid the variability caused by two sources of light, I have devised a method in which only a single gas-flame is employed, the light being divided in such a way that a certain portion of it always illuminates the posterior side of the screen, while the other portion is reflected from a movable mirror, and pursues its way unobstructed to the screen, or is transmitted to it by the plate under experiment. The compensation is made by moving the mirror, and when once effected of course remains undisturbed by fluctuations of the flame. In addition to this it will also be observed that a more delicate screen, constructed, however, on the general principles indicated in the first part of this paper, has been employed, and these modifications, taken in connection with certain precautions described below, have given the results an accuracy superior to that attained in the experiments on silver mirrors.

Arrangement of the photometer with a single gas flame for experiments on the amount of light transmitted by plates of various substances.

The source of light is a small gas flame at L, about an inch in height, the gas flowing as before from a plain circular opening. A portion of its light, L A, directly illuminates the screen S, while another portion N A is reflected on the screen by a Liebig's mirror, one protected by a coating of copper being selected: this equalizes the light on both sides of the bare "spot" in S. A third portion of the light from L falls on M, a mirror like that just mentioned, and there is reflected perpendicularly through P, the plate under examination, on G the ground glass. It will be observed that the path pursued by the light in this latter case is the distance L M + M G: by the graduation on the instrument M G is actually measured, G P is known; also P L; hence we have the means of calculating the distance M L, which is to be added to M G, giving the distance required. As these calculations involve some labor, I constructed for my instrument a table by which the *measured* distance G M, is readily converted into the *total* distance.



The photometer was the same as that described in the first part of this paper, the following changes having been introduced: the "spot" on the screen was made much smaller, being only about $\frac{1}{30}$ of an inch in horizontal diameter, with a length about three times as great; it was observed by a small telescope T, magnifying six diameters. This reduction in the size of the "spot" rendered it possible to illuminate the ground surrounding it in a manner beyond reproach.

Mode of adjusting the apparatus for experiment.

1st. The center of the flame at L, and the center of the mirror M, must be at the same height above the common base.

2nd. The screen and plates to be examined are to be made perpendicular to the axis of the instrument, by the aid of the small gas flame mentioned in the first part of this paper: the telescope T is also most readily collimated by the use of the same flame, the screen, &c., having been temporarily removed.

3rd. The mirror M evidently must be brought into such a position that it shall send the ray MP perpendicularly to P, or along the axis of the instrument when its distance is such as to effect compensation; for if the reflected ray were sent obliquely through the ground glass plate G, noticeable errors would be introduced. This adjustment, which is important if good results are expected, is best effected by making one or two compensations so as to determine approximately the correct distance of the mirror, which is then rotated on its vertical axis so that the reflected image of the flame is made to fall in the center of the field of the telescope, the screen having been previously removed. By a repetition of this operation finally the mirror is brought into its proper position, when a *single* series of measurements can be made, the minute differences in the individual compensations introducing no appreciable errors. When, however, the compensation point itself has been shifted by the introduction or removal of a plate of glass, the mirror will correspondingly be moved away or toward S, and of course the ray MP will be sent a little to the right or left, and it becomes necessary to devise some simple way in which this difficulty can be obviated, without in each instance removing the screen and altering the focus of the telescope.

The mirror then being actually in adjustment, this, when lost, can be recovered as follows: in the screen B about four inches above the telescope is a circular aperture $\frac{1}{10}$ of an inch in width; the reflection of the flame in the mirror is observed through it with the naked eye, and the image of the flame is seen higher up on the mirror, and is made to coincide with a short black line drawn there previously. The ray MP can always thus be made to coincide with the axis of the instrument, it not even being necessary for the observer to leave his seat or diminish the sensitiveness of the eye by exposure. I am particular in describing this precaution, having in my own case rejected the results of 1300 compensations which were made with a comparative lack of attention to this single point.

Mode of measuring the amount of light transmitted by a plate of glass, &c.

1st. The plate is placed at P and collimated by the use of the small gas flame.

2nd. A compensation is effected by moving the mirror.

3rd. The mirror is adjusted by the use of the aperture in B, and another compensation made, followed by a second adjustment of the mirror, if necessary.

4th. A series of careful compensations are now made, *alternately by the approach and recession of the mirror;* these are registered on the fillet of paper.

5th. The plate is removed without the experimenter changing position or exposing the eye to bright light, and a compensation is made which necessitates of course a new adjustment of the mirror by the aid of the aperture in the screen B. Finally, a series of compensations are effected with the free flame and registered.

At this point it may be well to notice an objection which might be urged to this mode of experimenting, viz: it is evident that the angles at which the light is reflected from the mirror will not be identical in the presence and absence of the plate of glass, and as the amount of light reflected by glass and silver varies with the angle of reflection, this might become a source of error and necessitate a correction. With the plates of colorless glass employed by me, however, the angle in one case was 5° 30′ and in the other 5° 18′, the difference of 12′ being much too small to produce an appreciable effect, as we know from the results of older experiments.

For the purpose of showing what can be effected by this method, I can perhaps do no better than give the results of five sets of experiments; each of them consisted of four sets of eight double, or sixteen single compensations, and during the progress of each trial the conditions remained the same. The experiments were made as follows: eight double compensations were effected with the photometer and registered; then everything being arranged as before, a new set of compensations were made, &c., all of which was repeated four times. The arithmetical mean of the distances of the source of light from the screen was then obtained separately for each of the four experiments, and from these four mean values of the distance, a final mean value was deduced. This last quantity was then squared and compared separately with the squares of each of the first four mean distances, which proceeding tells at once the difference of the four results from the final mean result, in per cents or fractions of a per cent, and is adapted to give an idea of the degree of accuracy to be expected in this kind of observation.

Differences from mean in per cents:

No. 1.	No. 2.	No. 3.	No. 4.	No. 5.
+·012	−·275	−·252	+·573	−·102
+·347	+·126	−·137	−·208	+·057
+·158	−·124	+·385	−·060	+·041
−·512	+·275	+·007	−·308	+·005

It will be observed that in the twenty experiments, only two cases occur in which the difference rises slightly above one half per cent, or above $\frac{1}{200}$ of the whole quantity of light, while in nearly all the other cases the difference falls considerably below this quantity, and the average of their differences is less than 0·2 per cent, or less than $\frac{1}{500}$ of the whole amount.

As the reliability of measurements with a photometer is evidently connected with the power of the eye in distinguishing different degrees of brightness in adjacent surfaces, it is well to review here briefly the results that have been obtained by different observers, as to the sensitiveness of the eye under favorable conditions.

The least difference which Boguer with his method, (two shadows,) was able to distinguish was $\frac{1}{64}$ of the whole; Fechner's friends using the same method were able to reduce this quantity to $\frac{1}{168}$. Arago remarked that where one of the illuminated surfaces was in motion smaller differences could be perceived, and was able under favorable circumstances to distinguish $\frac{1}{131}$, while Masson, who used revolving discs, along with his friends, was able to distinguish between $\frac{1}{50}$ to $\frac{1}{120}$ of the whole quantity of light. Near a window Helmholtz was able to distinguish with certainty a difference of $\frac{1}{133}$, and occasionally as small a quantity as $\frac{1}{167}$, while in the middle of a room he was able to appreciate certainly only $\frac{1}{117}$, seldom and uncertainly $\frac{1}{133}$.*

In all the better of the above mentioned experiments, photometers in any proper sense of the term were not employed, the best of them being made by the use of revolving discs of white paper furnished with narrow black sectors, which are calculated to discover the absolute sensitiveness of the eye under the most favorable conditions. It must not however be imagined that because a good eye can distinguish a difference of $\frac{1}{120}$ on a revolving disc, that the same eye would be able to obtain a similar result with ordinary forms of the photometer, where the conditions are far less favorable, and accordingly it seemed of interest to me to determine by a series of experiments the average sensitiveness of the eye while using the photometric method described in this paper, particularly as the plan of using

---

* Physiologische Optik, von H. Helmholtz, page 311.

a single flame and mirror offered great facilities for making these observations in a reliable manner. The following mode was employed: the light from the mirror was allowed to fall on the unobstructed screen, and the mirror was *drawn up* till the spot had disappeared, when the observation was registered; the next compensation was made by *pushing* the mirror *away*, and so on alternately, *the compensations by pull and push being registered on separate fillets of paper by two pens.* In the first experiment fourteen compensations were made by the advancing, and an equal number by the retreating mirror, and the mean of these twenty eight quantities was taken as the true distance of the mirror from the screen; then on comparing the mean of either set of compensations with this true distance, it was easy to ascertain how great a difference from the truth had been tolerated by the eye under the given circumstances. In the first experiment this quantity was found to be $\frac{1}{114}$, in a second similar trial $\frac{1}{125}$ of the total amount of light. The "screen" in these experiments was quite *new*, and an inferior result was obtained by using a "screen," which had for six weeks been exposed quite unprotected to the action of the air; here the usual darkening of the edges had begun and progressed so far as to be faintly visible during compensation.

In the experiments just detailed the highest *average* sensitiveness of the eye is only $\frac{1}{125}$, while as before shown in practical use the average difference from the mean is less than $\frac{1}{500}$ of the whole. This higher degree of accuracy results naturally from the mode in which the compensations are made, (alternately by approach and recession,) and the fact that the compensations are thus interwoven, in such a manner as to eliminate errors introduced by the slightly varying sensibility of the eye.

Experiments on the amount of light transmitted by plates of glass at a perpendicular incidence.

A knowledge of the amount of light transmitted and reflected by colorless transparent substances has a certain degree of interest from a technical point of view, enabling opticians to calculate the loss necessarily experienced in various instruments from this source, as well as the intensity where the light is reflected from a single or from two parallel surfaces of glass or other material. Of far more importance, however, is the interest attaching to this subject from a theoretical point of view, and especially in connection with the Undulatory Theory of Light. Formulas for the intensity of the reflected and transmitted beams at all angles have been deduced by eminent supporters of this theory, which in some cases have occasioned considerable discussion. The following very simple formula for the intensity of common light reflected at a perpendicular incidence was first given by Thomas Young; $I = \frac{(n-1)^2}{(n+1)^2}$; the intensity of the incident beam being equal to unity, and *n* being the index of refraction of the reflecting substance. The same formula was afterwards reached by Poisson in a rigid and learned analysis of the subject, and it was again deduced by Fresnel.* As is well known Fresnel's formulas were subsequently modified by the celebrated Cauchy, but in Cauchy's formula for reflection as soon as the incidence of the light differs considerably from the polarizing angle, the small quantities which depend on *e*, the coefficient of ellipticity, become so much reduced that they can be omitted from the numerator and denominator, and Cauchy's formula becomes identical with Fresnel's.† It hence appears that the formula above quoted is as well theoretically established, as any which has been deduced under the guidance of the Undulatory Theory, and one would naturally suppose that it had been often tested carefully by experiment. This does not seem to have been the case, and I do not know that it has ever been rigidly tried by a delicate photometric method. On this account I have made a series of observations on plates of colorless glass which are detailed below.

Mode of experimenting.

When a beam of light falls on a plate of glass or other transparent colorless substance, a certain portion will be reflected, another portion transmitted, a third absorbed. If the plate of glass be colorless and thin, the portion absorbed will be smaller than the necessary errors of observation, so that it can safely be neglected. For example, Bunsen found that in using a plate of crown glass 4·7 millimeters thick, that it absorbed only $\frac{4}{10}$ of one per cent of the *chemical* rays that fell on it at a perpendicular incidence.‡ The thickest plate of glass employed by me was 1·67 millimeters from surface to surface, i. e. about one-third of that used by Bunsen, and as we know that the chemical rays are extinguished by glass in a far larger proportion than those which are luminous, it follows that in the plates mentioned below we can safely neglect the internal extinction. This point being settled, the mode of proceeding becomes quite simple; it is only necessary to measure the amount of light transmitted, and the difference between the incident and transmitted light gives the amount of that reflected, and after making a correction for internal reflection, we shall have the means of comparing the results of theory and experiment.

---

* Pogg. Annalen. Bd. xxii, p. 98.

† Jamin in Pogg. Erg., Bd. iii, p. 256.

‡ Pogg. Annalen, Bd. ci, p. 241.

Mode of determining the indices of refraction.

In my experiments on transparent substances, always where it was possible prisms of the substance were ground and the index of refraction of the sodium line determined as usual with a graduated circle, collimating and observing telescope. In the particular experiments detailed at the termination of this article, the plates of glass used were so thin that it was not practicable to grind from them prisms, and for all such cases I contrived, tested and used two somewhat new modes of procedure, as neither the method of the Duke de Chaulnes (alteration of the focus of a microscope,) nor that of Bernard (displacement of an image viewed obliquely through a plate,) were here found to give reliable results.

1. A minute angular fragment of the glass to be experimented on was placed in a cell, on a glass slide, like those used for mounting microscopic objects, and surrounded with a mixture of "body-sperm-oil" and oil of cassia, the proportions being varied till the refraction of the glass for the sodium line had been exactly compensated by the oil. *Olive oil* became turbid when mixed with the oil of cassia and hence could not be used. The mode of comparing the refractive power of the mixture of the oils and glass was as follows: at the distance of half an inch below the level of a microscope stage was a fine slit, cut in tinfoil which had been pasted on glass; the microscope was focussed on this, a sodium flame being used to illuminate it; the cell with the oil and fragment of glass was then placed on the stage of the microscope, and moved so that the light from the slit passed through the angular fragment, when it would happen that the line of light would be refracted to the right or left hand according as oil or glass predominated in refractive power, which made it instantly evident whether sperm oil or oil of cassia was needed. A number of experiments were made to test this method, which was found to answer well, the index of refraction as determined by prism corresponding with that obtained by the use of the fragment: so in the case of a sample of crown glass a triangular prism gave the index of refraction as 1·526, while by the new method it was found to be 1·529. This method, however, is capable of still more accurate results, as in the above mentioned experiments, the compensation was pushed only far enough to answer my immediate purpose; that is to say, the fragment may be considered to consist of one *large* triangular prism with a moderate angle, and a number of smaller prisms, some of which are sure to have very large angles; these latter become effective when the glass is under oil, total reflection no longer taking place, and they act powerfully on the light coming from the slit, still furnishing faint images having

a considerable deviation even after the main portion of the glass fragment has ceased to perceptibly deflect it.

In no case did I push the compensation far enough to gather in all these outstanding beams.

2. Another method, which in the case of crown glass was found to answer quite well enough for my purpose and to be very convenient, consisted in fusing to a spherical globule a fragment of the glass, placing it in the mixed oils and effecting compensation by observing when the globule ceased to act as a lens, for which purpose a small telescope or the microscope can be employed. Thus, for example, a certain kind of crown glass when ground into a prism gave as index of refraction for the sodium line 1·526, while when tested according to this second method, the results of two experiments were 1·5232 and 1·5235.

I give now the results of careful sets of experiments on the amount of light transmitted by two different samples of crown glass: in each case the results of four independent trials are given, each trial being worked out with the aid of seven double compensations. In the first case the thickness of the glass was ·15 millimeters, the index of refraction 1·5236, and allowing for the effects of internal reflection, it should, according to theory, have transmitted 91·736 per cent of the light falling on it. Experiment gave:

$$\begin{array}{r} 92{\cdot}227 \\ 91{\cdot}371 \\ 91{\cdot}019 \\ 91{\cdot}143 \\ \hline 91{\cdot}440 \end{array}$$

The difference ·296 being hardly larger than the necessary error connected with the method of making the determination.

In the second case the index of refraction was 1·5225, the thickness 1·677 millimeters, and by theory it should have transmitted 91·763 per cent of the light falling on it. Below are the actual results obtained:

$$\begin{array}{r} 90{\cdot}886 \\ 90{\cdot}948 \\ 90{\cdot}892 \\ 91{\cdot}895 \\ \hline 91{\cdot}155 \end{array}$$

The difference here of ·500 per cent, or $\frac{1}{200}$ of the whole amount, is almost equally satisfactory, and these experiments show, I think, that the reflecting power of glass with the above index of refraction, *conforms in the closest manner to the predictions of theory.* Elaborate experiments were also made with flint glass, quartz and calc-spar, but I suppress the results, as it afterwards turned out that they were contaminated with minute errors of the character described in this article under the head "mode of adjusting, &c." 3rd; the adjustment alluded to having indeed been always made, but not with a sufficient degree of care to exclude the last trace of error.

New York, March 29th, 1870.

ART. II.—*The Ethers of Arsenic Acid and of Arsenious Acid*; by J. M. CRAFTS.*

MR. FRIEDEL and I observed, while studying the ethers of silicic acid, that the silicate of ethyl is readily decomposed by heating it with anhydrous boracic acid in a sealed tube; that pure borate of ethyl is formed, and that silicic acid, or an ether containing a very large proportion of silicic acid, is deposited in the tube. It occurred to me that a similar reaction might offer a convenient method of preparing the ethers of feeble acids. The ethers of the acids of arsenic and antimony and of tungstic acid have not yet been obtained, and experiments were made with a view to their preparation, but I have only succeeded in the case of arsenious acid in obtaining a new ether by the action of the acid upon silicic ether. Another method, however, afforded the means of preparing the ethers of arsenic acid, and several different methods of preparing the ethers of arsenious acid have been discovered. The present paper is devoted to the description of the methylic, ethylic and amylic ethers of both acids of arsenic.

ARSENIATE OF ETHYL.

The first attempts were made to prepare this body by the action of arsenic acid on the silicate of ethyl. The arsenic acid was dried by heating it in a current of dry air, and it was then sealed in a glass tube with the silicate of ethyl, and heated in an air-bath. After heating 10 hours at 210° centigrade there appeared to be no reaction; on heating 3 hours longer at 230°, a considerable quantity of the arsenic acid dissolved and at a little higher temperature the tube exploded, probably in consequence of the oxydation of the ether by the arsenic acid.

In another experiment 20 grams of silicate of ethyl were heated with 8 grams of arsenic acid for 6 hours at 220°–230°. A gelatinous silicate of ethyl was deposited, and on opening the tube about $\frac{1}{4}$ litre of a gas having the properties of ethylene was evolved. A considerable quantity of common ether was formed, and the remainder of the liquid contents of the tube was par-

---

* The chemical symbols used have the values which belong to them in the new system.

tially decomposed on distillation. After several distillations a small quantity of a body was isolated, which contained arsenious, but no arsenic acid, and which proved to be an impure arsenious ether. It is evident from these results that, at the temperature required for the action of arsenic acid upon the silicate of ethyl, the greater part of the product is decomposed with formation of common ether and ethylene, while a small quantity of the arsenic acid is reduced and arsenite of ethyl is formed, and that the reaction does not furnish any arseniate of ethyl.

The arseniate of ethyl can be easily prepared by the action of the iodid of ethyl on the arseniate of silver—

$$3C_2H_5I+Ag_3AsO_4=3AgI+(C_2H_5)_3AsO_4.$$

When the two bodies, mixed with pure anhydrous ether are heated together at about 90° in a sealed tube, the reaction is completed in a few hours, and the point at which this takes place is marked by the change of color of the silver salt from red to light yellow. It is important that the arseniate of silver should be in excess and that the temperature should not exceed 100°, as the iodid of ethyl decomposes a portion of the arseniate of ethyl, and the decomposition takes place more readily, the larger the quantity of iodid and the higher the temperature.

The liquid together with the iodid of silver is taken from the tube and washed with pure ether until it is entirely freed from arseniate of ethyl. This operation can be most conveniently performed on a funnel, provided with a glass plate ground to fit it. A hole should be drilled in the plate, and a funnel tube ground into it, in order that the liquid may be poured upon the filter without removing the glass plate.

The solution in ether is distilled and finally heated in a water-bath, while a current of dry air is passed through it to carry off the last trace of ether. The arseniate of ethyl thus obtained can be most easily purified by distillation in a partial vacuum, as it is decomposed in part by distillation in the air. In operations of this kind it is preferable to distil under a diminished pressure rather than to push the exhaustion of the air as far as possible since the boiling point remains more constant, when the pressure is high. This probably is due in great part to the fact, that a slight variation of pressure does not alter the relative pressure so much, when the exhaustion is incomplete, as when it is nearly complete. For instance, the pressure may well vary 2 millimeters during the course of a distillation, and a variation from 5 to 7 millimeters affects the boiling point much more than a variation from 200 to 202 millimeters.

In the case of the arseniate of ethyl it was found that it distilled under a pressure of 60 millimeters at about 85° lower than in the air, and that this difference of temperature was sufficient to prevent decomposition. The distilling apparatus was provided with an air reservoir and with a mercury guage, and the pressure was kept nearly constant at 60 millimeters during the course of the distillation.

Arseniate of ethyl distils at 148°–150° under a pressure of 60 millimeters.

About $\frac{4}{5}$ of the theoretical quantity are obtained by the above method of preparation.

The arsenic ethers are immediately decomposed by smallest traces of water and it is essential that the common ether used in their preparation should be absolutely anhydrous. It is also important for the operation in the sealed tube, that it should contain no alcohol, since the iodid of ethyl acts upon alcohol with formation of ordinary ether and water. Ether can be most easily freed from small quantities of water and alcohol by digesting it several times with shavings of sodium, which can be cut with a knife from a large piece, allowing it to stand each time 24 hours with the sodium, and then distilling. Ether so prepared can be heated with sodium to 90° in a sealed tube without the smallest evolution of hydrogen.

The following analyses were made of arseniate of ethyl distilling at 148°–150° under a pressure of 60 millimeters.

I. *Substance* = 0·2980 *grms.*; CO_2 = 0·3470 *grms.*; H_2O = 0·1740 *grms.*

II. *Substance* = 0·9260 *grms.*; *arseniate of magnesium and ammonium* = 0·7705 *grms.*

The determination of arsenic was made by decomposing a weighed quantity of the arseniate of ethyl by water, evaporating with nitric acid, and determining the arsenic acid as the arseniate of magnesium and ammonium on a weighed filter.

	I.	II.	Calculated for $(C_2H_5)_3\ AsO_4$
C	=31·79	....	31·86
H=	6·49	....	6·64
As	=....	32·85	33·18

The density of liquid arseniate of ethyl at 0° compared with that of water at 4°=1·3264. Its density at 8·8°=1·3161.

The density of vapor of arseniate of ethyl can not be taken on account of its decomposition by heat. This decomposition, however, is not so great, but that its boiling point in the air can be determined with sufficient accuracy. When a portion of the arseniate of ethyl, which has been purified by distillation under a diminished pressure, is distilled in the air, the greater part of it passes at 235°–238°, but toward the last part of the distillation decomposition ensues, and a white frothy deposit of arsenic acid is left in the retort.

Arseniate of ethyl attracts moisture from the air and is immediately decomposed with formation of arsenic acid and alcohol. It dissolves immediately in water, undergoing the same decomposition. Particular attention was paid to the question of the existence of ethyl-arseniates, and experiments were made under the most favorable conditions for observing their formation, but thus far with negative results. Felix D'Arcet, (Ann. Chem. und Pharm., xix, p. 202), gives the analysis of the barium salt of the di-ethyl arsenic acid $Ba(C_2H_5)_4(AsO_4)_2$; but Hugo Schiff (Ann. Chem. und Pharm., cxi, p. 370,) failed to obtain this body on dissolving arsenic acid in alcohol, evaporating until the solution acquired the consistency of a syrup, dissolving in water and neutralizing with baric carbonate. This result proves at least that no ethyl arsenic acid can exist in aqueous solution, and my own experiments confirm those of Mr. Schiff. It appeared possible that when the arseniate of ethyl is decomposed by water, the decomposition might pass through two stages, and that ethyl arsenic acids might at least exist for a time in the aqueous solution, even though they were ultimately completely destroyed. In order to test this idea, a weighed quantity of arseniate of ethyl was dissolved in water, litmus solution was added and it was neutralized immediately with a standard solution of ammonia. If the decomposition with water had been a gradual one with formation of the acids mentioned, the neutralized solution should have developed an acid reaction after the lapse of a considerable time. This was not the case. The same amount of ammonia was required for neutralization, either immediately, or after the solution had stood 24 hours, and moreover, the reaction was exactly similar to that of a corresponding quantity of pure arsenic acid dissolved in water. It should be noticed that no sharp change of color can be obtained by neutralizing a solution of arsenic acid containing litmus with ammonia.

Another attempt was made to obtain the ethyl arsenic acid in an alcoholic solution. Pure arseniate of ethyl was mixed with alcohol, and only enough aqueous alcohol was added to furnish water for the decomposition expressed by the reaction:

$$(C_2H_5)_3\,AsO_4+H_2O = H(C_2H_5)_2\,AsO_4+C_2H_6O;$$

and the mixture was distilled in vacuo. It behaved exactly like a solution of arsenic acid in arseniate of ethyl: the latter distilling and leaving the anhydrous arsenic acid as a residue. It was thought probable that in this experiment the first product of the action of water would enter into the following reaction with some of the undecomposed arseniate of ethyl:

$$H(C_2H_5)_2\,AsO_4+(C_2H_5)_3\,AsO_4=(C_2H_5)_4As_2O_7+C_2H_6O;$$

but no condensed ether like the one whose formula is given was obtained, and the only result of the decomposition with aqueous alcohol under these circumstances is the setting free of arsenic acid and alcohol.

Arseniate of ethyl is immediately decomposed by ammonia with the formation of a crystalline body, which it is very difficult to obtain pure on account of the extreme avidity with which it absorbs moisture. The results of several analyses lead to the conclusion, that the body which is first formed has the composition: $(C_2H_5)_2NH_2AsO_3$, and that it absorbs water to form a di-ethyl arseniate of ammonium, $(C_2H_5)_2NH_4AsO_4$, but the study of this product is not yet completed.

ARSENIATE OF METHYL.

The arseniate of methyl can be prepared from the iodid of methyl and arseniate of silver in the same way that the arseniate of ethyl is prepared. The crude product is purified by distillation under a pressure of sixty millimeters. At this pressure it distils without decomposition at 128°–130°.

I. *Substance*=0·4500 *grms.*; CO_2=0·3285 *grms.*; H_2O=0·1980 *grms.*

II. *Substance*=0·8070 *grms.*; As_2O_5=0·4966 *grms.*

The determination of arsenic was made by decomposing the ether with water, evaporating with nitric acid, and heating the arsenic acid thus formed with oxyd of lead.

	I.	II.	Calculated for $(CH_3)_3AsO_4$.
C =	19·90	----	19·57
H =	4·91	----	4·89
As=		40·15	40·76

The density of liquid of arseniate of methyl at 14°·5, compared with water at 4°, =1·5591.

When pure arseniate of methyl is distilled in the air, the greater part passes at 213°–215°, but there is a partial decomposition with separation of arsenic acid.

It resembles the arseniate of ethyl in all its properties. It is a colorless liquid, miscible in all proportions with water, and it is decomposed immediately into arsenic acid and alcohol by the action of water or moisture for the air.

ARSENIATE OF AMYL.

This body is produced by action of iodid of amyl on the arseniate of silver, but it cannot be obtained in a state of purity, because it decomposes, even when distilled in vacuo.

The crude product of the reaction, after having been heated to 170°, was mixed with pure ether, when a considerable quantity of arsenious acid was precipitated; after filtering, the solution was heated in an oil-bath, and finally a current of dry air was passed through it, while the bath was kept at 200°. An analysis of the product so obtained gave C=39·31 per cent and H=7·64 per cent, instead of C=51·14 per cent and H=9·37 per cent.

A somewhat better result was obtained by distilling the crude product in vacuo. The greater part is decomposed at about 200°, but a small quantity of ether distils unchanged. Some of this distillate was heated to 195° in current of dry air, in order to free it as far as possible from products of decomposition. The analysis of this product gave C=44·82 per cent and H=8·41 per cent instead of C=51·14 per cent and H=9·37 per cent.

The products analyzed appear to have been arseniate of amyl mixed with arsenic acid.

ARSENITE OF ETHYL.

The ethers of arsenious acid can be prepared more readily than these of arsenic acid, and several reactions may be employed to obtain them.

Arsenite of ethyl is produced in the following reactions:

By the action of arsenious acid on the silicate of ethyl. By the action of iodid of ethyl on the arsenite of silver. By the action of the chlorid or bromid of arsenic on the alcoholate of sodium. The iodid of arsenic and the alcoholate of sodium give no arsenite of ethyl.

When arsenious acid is heated with alcohol to about 200° in a sealed tube, a considerable quantity of the acid dissolves, and crystallizes out on cooling as beautifully formed octahedrons. A small quantity of arsenic is reduced and a corresponding quantity of aldehyd is formed. No arsenious ether is formed.

Arsenious acid was heated with a mixture of common ether and acetate of ethyl for 20 hours at 200°, but no reaction took place.

It seemed highly probable that arsenite of ethyl might be obtained by heating together, common ether and chlorid of arsenic according to the reaction

$$AsCl_3+3(C_2H_5)_2O=(C_2H_5)_3AsO_3+3C_2H_5Cl,$$

but this proved not to be the case, for after the two substances had been heated together 20 hours at 200°, it was found that no chlorid of ethyl and no arsenite of ethyl had been formed.

It has been already noticed that by a process of reduction arsenite of ethyl is produced, when arsenic acid is heated with silicic ether. When arsenious acid is heated to about 200° in a sealed tube with silicic ether, the only reaction which takes place is the replacement of the silicic acid by the arsenious acid, a gelatinous deposit of a silicic ether, containing a very large quantity of silicic acid, is formed in the tube; no alcohol, ether or gaseous body is produced, and the arsenious ether can easily be obtained pure by distillation.

Iodid of ethyl acts less readily upon the arsenite of silver than upon the arseniate of silver, and the amount of product obtained approaches less nearly to the theoretical quantity; there is, however, no difficulty in obtaining pure arsenious ether by heating together iodid of ethyl and dry arsenite of silver mixed with pure ether in a sealed tube to 150°. The arsenious ether can be separated by washing the silver salt with common ether and finally by distilling the solution in ether.

The action of alcoholate of sodium upon the chlorid, or, better still, upon the bromid of arsenic, furnishes the best method for obtaining the arsenite of ethyl.

It is well known that the chlorid of arsenic combines with alcohol with evolution of heat to form a definite compound,* which distils without decomposition.

When the bromid of arsenic is mixed with an equal volume of alcohol, the heat evolved is very slight (an elevation of temperature of 3° was noticed), and no definite combination appears to be formed. If alcoholate of sodium is added to a mixture of either the chlorid or bromid of arsenic with alcohol, a precipitation of chlorid or bromid of sodium takes place immediately, and arsenite of ethyl is formed, but if the addition of the alcoholate is continued until all the chlorine or bromine is combined with sodium, the arsenite of ethyl is destroyed at the same time.† If the operation is terminated before all the chlorid or bromid of arsenic has been acted upon, a mixture of the undecomposed chlorid or bromid with arsenite of ethyl is obtained.

It was thought at first that the difference between the boiling point of the bromid of arsenic (222°) and that of the arsenite of ethyl (166°) would suffice for their separation, but this proved not to be the case, for a mixture of the two bodies in any proportion distils like a definite compound, and no separation by fractional distillation is possible. This kind of combination between the chlorid or bromid of arsenic and the ethers of arsenious acid will be noticed below.

The arsenite of ethyl, which is produced in the reaction above described, can only be isolated by taking advantage of its singular property of resisting entirely the action of dry ammonia gas, while the chlorid or bromid of arsenic combines with ammonia to form a crystalline solid insoluble in alcohol and ether. The method which was usually employed to prepare the arsenite of ethyl consists in dissolving 5 parts of bromid of arsenic in absolute alcohol and in adding to the solution the liquid obtained by adding 1 part of sodium to a considerable quantity of alcohol. The reaction is not violent, and it is not necessary to cool the vessel in which it takes place.

---

* De Luynes, Ann. Chem. und Pharm., cxvi, p. 368.

† See paper in Comptes Rendus, April, 1867.

The arsenite of ethyl and undecomposed bromid of arsenic are separated from the bromid of sodium precipitate by washing with alcohol. The alcoholic solution is then distilled in a water-bath, pure ether is added to the residue, and dry ammonia gas is passed through the solution in ether in order to precipitate the bromid of arsenic, which is mixed with the arsenite of ethyl. The latter is separated from precipitate by washing with ether, the ether is distilled in a water-bath, and finally a current of dry air or carbonic acid is passed through. The residue consists of arsenite of ethyl containing some arsenious acid and bromid of sodium in solution. It can be purified completely by two or three distillations. Pure arsenite of ethyl boils at 165°–166° (no correction).

Analyses were made of products obtained by the three methods of preparation described above.

I. *Substance*=0·2320 *grms.*; CO_2=0·2925 *grms.*; H_2O=0·1590 *grms.*

II. *Substance*=0·4960 *grms.*; C_2O=0·6245 *grms.*; H_2O=0·3160 *grms.*

III. *Substance*=0·2510 *grms.*; *arseniate of magnesium and ammonium*=0·2311 *grms.*

The determination of arsenic was made by weighing the substance in a bulb, breaking the bulb under a solution of caustic potash, oxydizing the arsenious acid by chlorate of potash and chlorhydric acid at a temperature of 50°, and precipitating by sulphate of magnesium and chlorid of ammonium in the ordinary way.

	I.	II.	III.	Calculated for $(C_2H_5)_3As\,O_3$.
C=	34·38	34·33	----	34·29
H=	7·61	7·08	----	7·14
As=	....	----	36.34	35·71

The density of liquid arsenite of ethyl compared with that of water at 4°=1·224.

Several vapor density determinations were made at different temperatures by Dumas' method.

Difference between the weights of the bulb=0·9610 *grms.*
Temperature of the balance, = 4·°2
Temperature of the oil-bath (air thermometer) =267·°0
Capacity of the bulb, =269·5 *c. c.*
Air remaining, = 0·5 *c. c.*

Vapor density=7·389 Theory=7·2673

The vapor density at 209·°5=7·615
" " 213° =7·608
" " 233° =7·197

The last determination is probably too small through some fault of manipulation. It is to be expected that the numbers should be a little too high, since the arsenite of ethyl is decomposed by the slightest trace of moisture, leaving a deposit of arsenious acid in the bulb. The arsenite of ethyl is a colorless liquid with a peculiar, but not disagreeable, odor, and sufficiently volatile to make its presence plainly perceptible in a room, where it is standing in an open test-tube.

The ethers of arsenic do not appear to have the extraordinarily poisonous properties, which are attributed to arseniuretted hydrogen, at least I have suffered no inconvenience after having worked with them for several years.

Arsenite of ethyl is miscible in any proportion with alcohol and ether, but it is decomposed by aqueous alcohol. The only products of decomposition are arsenious acid and alcohol, and no compound ether with the formula, $(C_2H_5)_4As_2O_5$, is formed. This ether is also not produced according to the reaction:

$$4(C_2H_5)_3\ AsO_3+As_2O_3=3(C_2H_5)_4As_2O_5$$

when arsenite of ethyl is heated to 300° with arsenious acid. Arsenite of ethyl at its boiling point dissolves about $\frac{1}{3}$ its weight of arsenious acid, but a considerable quantity separates out, when the solution is allowed to become cool. Arsenite of ethyl can be distilled unchanged over dry arsenious acid.

The reaction between the arsenite of ethyl and bromhydric acid is the reverse of that which is usual between bromhydric acid and the ethers of acids. Usually the bromid of the alcoholic radical is formed, and the acid is set free. In this case the reaction leads to the formation of alcohol and bromid of arsenic, and it may be expressed by the equation:

$$(C_2H_5)_3AsO_3+3HBr=3C_2H_6O+As\ Br_3.$$

All that is required to study this reaction is to pass dry bromhydric acid through arsenite of ethyl, when the alcohol can be separated by distillation, leaving a crystalline residue of bromid of arsenic.

It was thought possible that bromine might combine directly with the arsenite of ethyl to form the compound: $(C_2H_5)_3AsO_3Br_2$, but it was found that the bromine substitutes itself for hydrogen in the ethyl, and that the substituted ether thus formed is decomposed by the bromhydric acid which is evolved in its formation.

Iodine does not act upon the arsenite of ethyl, and the two bodies can be separated by distillation. Dry ammonia gas is entirely without action upon the arsenite of ethyl as well as upon the other ethers of arsenious acid, and they can even be distilled in an atmosphere of ammonia without alteration.

ARSENITE OF METHYL.

The arsenite of methyl was prepared by heating dry arsenious acid with the silicate of methyl for 12 hours at 160°, and very nearly the theoretical quantity was obtained.

It was also obtained by the action of iodid of methyl on arsenite of silver in a sealed tube. The reaction does not take place at a temperature lower than 150°, and at that temperature a considerable part of the arsenious ether is destroyed, so that the first method of preparation is to be preferred.

The arsenite of methyl boils at 128°–129° (no correction).

I. *Substance*=0·4990 *grms.*; Co_2=0·3966 *grms.*; H_2O=0·2442 *grms.*

II. *Substance*=0·3725 *grms*; As_2O_5=0·2525 *grms.*

The arsenic determination was made by decomposing the ether with water, oxydizing with nitric acid, evaporating, and combining the arsenic acid with oxyd of lead.

I.	II.	Calculated for $(CH_3)_3AsO_3$.
C=21·67	----	21·44
H= 5·44	----	5·36
As= ----	44·21	44·64

The density of liquid of arsenite of methyl at 9°·6 compared with that of water at 4°=1·428. The vapor density was determined from the following data:

Difference between weights of bulb=0·5265 *grms.*
Temperature of the balance, =16°·4
Temperature of the oil-bath (*air-thermometer*)=197°
Capacity of the bulb, =161·5 *c. c.*
Air remaining, = 1·0 *c. c.*
Vapor density=6·006 Theory=5·818

The determination probably came out too high on account of the decomposition of a small quantity of ether by the moisture of the air.

The arsenite of methyl resembles the arsenite of ethyl in its properties. It is decomposed immediately by water and also by the moisture of the air with formation of arsenious acid and methylic alcohol.

In endeavoring to obtain the chlorhydrines of these ethers a singular class of compounds in indefinite proportions, but with constant boiling points, was discovered. The composition of the chlorhydrines of the arsenite of methyl would be represented by the formulas: $(CH_3)_2ClAsO_2$; CH_3Cl_2AsO; and in order to obtain them, arsenious acid was heated with the chlorhydrines of the silicate of methyl.* In this way bodies can be easily obtained, having the required composition and con-

---

* This Journal, II, xliii, p. 155 and 331.

stant boiling points, but bodies with their constituents in every intermediate proportion between these, whose formulas are given above, can also be obtained, and they have equally constant boiling points. In fact, by combining the chlorid of arsenic and an arsenious ether in indefinite proportions, a body with a constant boiling point can be obtained; and what renders this fact most remarkable, is that the boiling point is not a mean between those of the two bodies mixed, but it is higher than either of them. The boiling point of arsenite of methyl is 129° (no correction). The boiling point of chlorid of arsenic is 133° (no correction). A product containing 42 per cent of chlorid of arsenic, and 58 per cent of arsenite of ethyl distilled unchanged at 143°–144°. A product containing 11·5 per cent of chlorid of arsenic and 88·5 per cent of arsenite of ethyl distilled at 131°–132°. I am not aware that an analogous case of combination in indefinite proportions has ever been observed.

The boiling point of the arsenite of ethyl is 166° (no correction). Two parts of arsenite of ethyl and one part of chlorid of arsenic mix with slight disengagement of heat, and the product distils constantly at 159°–164° (no correction).

A mixture of equal parts of chlorid of arsenic and of arsenite of ethyl distils at 150°–152°.

A body was also obtained by the action of alcoholate of sodium upon the bromid of arsenic in excess, which contained 65·75 per cent of bromid of arsenic and 34·25 per cent of arsenite of ethyl, and which distilled constantly at 182°–185° (no correction). The chlorid or bromid of arsenic in all these bodies combines directly with dry ammonia gas, leaving the arsenious ether free.

ARSENITE OF AMYL.

The arsenite of amyl was prepared by the action of the amylate of sodium upon the bromid of arsenic in the same way as was described for the preparation of the arsenite of ethyl.

The product, however, cannot be purified by distillation in the air, since it decomposes in part at its boiling point. It can, however, be purified by distillation under a pressure of 60 millimeters. The following analysis was made of a product boiling at 193°–194° (no correction) under a pressure of 60 millimeters.

Substance=0·4090 *grms.*; CO_2=0·7950 *grms.*; H_2O=0·3578 *grms.*

	Calculated for $(C^5H_{11})_3AsO_3$.
C=53·01	53·57
H= 9·72	9·82

The density of liquid arsenite of amyl at 0° compared with that of water at 4°=1·0525. Arsenite of amyl, which has been purified by distillation under a diminished pressure, distils in the air at 288°–290°, but toward the last part of the distillation arsenious acid separates out.

Arsenite of amyl is not more stable than the other ethers of arsenious acid. It is immediately decomposed by water and also by the moisture of the air.

Arsenite of amyl has analogous properties with the arsenites of ethyl and methyl.

The relations between the boiling points of the bodies described in this paper present some curious anomalies, and do not fall within Kopp's law derived from the study of the more simple ethers.

Boiling points under a pressure of 60 millemeters:

$$\begin{aligned}(C_2H_5)_3AsO_4 &= 149° \\ (CH_3)_3AsO_4 &= 129° \\ \hline \text{Difference} &= 20°\end{aligned}$$

Boiling points under the atmospheric pressure:

$$\begin{aligned}(C_2H_5)_3AsO_3 &= 166° \\ (CH_3)_3AsO_3 &= 129° \\ \hline \text{Difference} &= 37°\end{aligned}$$

In the first case the difference of boiling point for each increment of CH_2 is $6\frac{2}{3}$°.

In the second case the difference is $12\frac{1}{3}$°.

I have already stated in a communication to the French Academy of Sciences, (April 2, 1867), that tungstic and antimonius acids give no ethers when they are heated with the silicate of ethyl.

ART. III.—*On the probable seat of Volcanic Action;* by T. STERRY HUNT, LL.D., F.R.S.*

THE igneous theory of the earth's crust, which supposes it to have been at one time a fused mass, and to still retain in its interior a great degree of heat, is now generally admitted. In order to explain the origin of eruptive rocks, the phenomena of volcanos, and the movements of the earth's crust, all of which are conceived by geologists to depend upon the internal heat of the earth, three principal hypotheses have been put forward. Of these the first supposes that in the cooling of the globe a solid

---

* From the Geological Magazine for June, 1869.

crust of no great thickness was formed, which rests upon the still uncongealed nucleus. The second hypothesis, maintained by Hopkins and by Poulett Scrope, supposes solidification to have commenced at the center of the liquid globe, and to have advanced toward the circumference. Before the last portions became solidified, there was produced, it is conceived, a condition of imperfect liquidity, preventing the sinking of the cooled and heavier particles, and giving rise to a superficial crust, from which solidification would proceed downward. There would thus be enclosed, between the inner and outer solid parts, a portion of uncongealed matter, which, according to Hopkins, may be supposed still to retain its liquid condition, and to be the seat of volcanic action, whether existing in isolated reservoirs or subterranean lakes; or whether, as suggested by Scrope, forming a continuous sheet surrounding the solid nucleus, whose existence is thus conciliated with the evident facts of a flexible crust, and of liquid ignited matters beneath.

Hopkins, in the discussion of this question, insisted upon the fact established by his experiments, that pressure favors the solidification of matters which, like rocks, pass in melting to a less dense condition, and hence concludes that the pressure existing at great depths must have induced solidification of the molten mass at a temperature at which, under a less pressure, it would have remained liquid. Mr. Scrope has followed this up by the ingenious suggestion that the great pressure upon parts of the solid igneous mass may become relaxed from the effect of local movements of the earth's crust, causing portions of the solidified matter to pass immediately into the liquid state, thus giving rise to eruptive rocks in regions where all before was solid.*

Similar views have been put forward in a note by Rev. O. Fisher, and in an essay on the formation of mountain chains, by Mr. N. S. Shaler, in the proceedings of the Boston Society of Natural History, both of which appear in the *Geological Magazine* for November last. As summed up by Mr. Shaler, the second hypothesis supposes that the earth "consists of an immense solid nucleus, a hardened outer crust, and an intermediate region of comparatively slight depth, in an imperfect state of igneous fusion." In this connection it is curious to remark that, as pointed out by Mr. J. Clifton Ward, in the same Magazine for December (page 581), Halley was led, from the study of terrestrial magnetism, to a similar hypothesis. He supposed the existence of two magnetic poles situated in the earth's outer crust, and two others in an interior mass, separated from the solid envelope by a fluid medium, and revolving, by a very

---

* See Scrope on Volcanos, and his communication to the *Geological Magazine* for Dec., 1868.

small degree, slower than the outer crust.* The same conclusion was subsequently adopted by Hansteen.

The formation of a solid layer at the surface of the viscid and nearly congealed mass of the cooling globe, as supposed by the advocates of the second hypothesis, is readily admissible. That this process should commence when the remaining envelope of liquid was yet so deep that the refrigeration from that time to the present has not been sufficient for its entire solidification, is, however, not so probable. Such a crust on the cooling superficial layer would, from the contraction consequent on the further refrigeration of the liquid stratum beneath, become more or less depressed and corrugated, so that there would probably result, as I have elsewhere said, "an irregular diversified surface from the contraction of the congealing mass, which at last formed a liquid bath of no great depth, surrounding the solid nucleus." Geological phenomena do not, however, in my opinion, afford any evidence of the existence of yet unsolidified portions of the originally liquid material, but are more simply explained by the third hypothesis. This, like the last, supposes the existence of a solid nucleus, and of an outer crust, with an interposed layer of partially fluid matter, which is not, however, a still unsolidified portion of the once liquid globe, but consists of the outer part of the congealed primitive mass, disintegrated and modified by chemical and mechanical agencies, impregnated with water, and in a state of igneo-aqueous fusion.

The history of this view forms an interesting chapter in geology. As remarked by Humboldt, a notion that volcanic phenomena have their seat in the sedimentary formations, and are dependent on the combustion of organic substances, belongs to the infancy of geology. To this period belong the theories of Lémery and Breislak (*Cosmos*, v, 443; Otte's translation). Keferstein in his *Naturgeschichte des Erdkörpers*, published in 1834, maintained that all crystalline non-stratified rocks, from granite to lava, are products of the transformation of sedimentary strata, in part very recent, and that there is no well-defined line to be drawn between Neptunian and volcanic rocks, since they pass into each other. Volcanic phenomena, according to him, have their origin not in an igneous fluid center, nor in an oxydizing metallic nucleus (Davy, Daubeny), but in known sedimentary formations, where they are the result of a peculiar kind of fermentation, which crystallizes and arranges in new

---

* The elevated temperature of the interior of the globe would probably offer no obstacle to the development of magnetism. In a recent experiment of M. Trève, communicated by M. Faye to the French Academy of Sciences, it was found that molten cast iron, when poured into a mould, surrounded by a helix which was traversed by an electric current, became a strong magnet when liquid at a temperature of 1300° C., and retained its magnetism while cooling (Comptes Rendus de l'Acad. des Sciences, Feb., 1869.

forms the elements of the sedimentary strata, with an evolution of heat as a result of the chemical process (*Naturgeschichte*, vol. i, p. 109; also *Bull. Soc. Geol. de France* [1], vol. vii, p. 197). In commenting upon these views (*Am. Jour. Science*, July, 1860), I have remarked that, by ignoring the incandescent nucleus as a source of heat, Keferstein has excluded the true exciting cause of the chemical changes which take place in the buried sediments. The notion of a subterranean combustion or fermentation, as a source of heat, is to be rejected as irrational.

A view identical with that of Keferstein, as to the seat of volcanic phenomena, was soon after put forth by Sir John Herschel, in a letter to Sir Charles Lyell, in 1836 (*Proc. Geol. Soc. London*, ii, 548). Starting from the suggestion of Scrope and Babbage, that the isothermal horizons in the earth's crust must rise as a consequence of the accumulation of sediments, he insisted that deeply buried strata will thus become crystallized by heat, and may eventually, with their included water, be raised to the melting point, by which process gases would be generated, and earthquakes and volcanic eruptions follow. At the same time the mechanical disturbance of the equilibrium of pressure, consequent upon a transfer of sediments, while the yielding surface reposes on matters partly liquified, will explain the movements of elevation and subsidence of the earth's crust. Herschel was probably ignorant of the extent to which his views had been anticipated by Keferstein; and the suggestions of the one and the other seemed to have passed unnoticed by geologists until, in March 1858, I reproduced them in a paper read before the Canadian Institute (Toronto,) being at that time acquainted with Herschel's letter, but not having met with the writings of Keferstein. I there considered the reactions which would take place under the influence of a high temperature in sediments permeated with water, and containing, besides siliceous and aluminous matters, carbonates, chlorids, and carbonaceous substances. From these, it was shown, might be produced all the gaseous emanations of volcanic districts, while from aqueo-igneous fusion of the various admixtures might result the great variety of eruptive rocks. To quote the words of my paper just referred to: "We conceive that the earth's solid crust of anhydrous and primitive igneous rock is everywhere deeply concealed beneath its own ruins, which form a great mass of sedimentary strata, permeated by water. As heat from beneath invades these sediments, it produces in them that change which constitutes normal metamorphism. These rocks, at a sufficient depth, are necessarily in a state of igneo-aqueous fusion; and in the event of fracture in the overlying strata, may rise among them, taking the form of eruptive rocks. When the nature of the sediments is such as generate great amounts of elastic fluids by their fusion, earthquakes and volcanic eruptions may result, and these—other things being equal—will be most likely to occur under the more recent formations." (*Canadian Journal*, May, 1858, vol. iii, p. 207).

The same views are insisted upon in a paper "On some Points in Chemical Geology" (Quart. Jour. Geol. Soc. London, Nov. 1859, vol. xv, page 594), and have since been repeatedly put forward by me, with farther explanations as to what I have designated above, *the ruins of the crust of anhydrous and primitive igneous rock.* This, it is conceived, must, by contraction in cooling, have become porous and permeable, for a considerable depth, to the waters afterwards precipitated upon its surface. In this way it was prepared alike for mechanical disintegration, and for the chemical action of the acids, which, as shown in the two papers just referred to, must have been present in the air and the waters of the time. It is, moreover, not improbable that a yet unsolidified sheet of molten matter may then have existed beneath the earth's crust, and may have intervened in the volcanic phenomena of that early period, contributing, by its extravasation, to swell the vast amount of mineral matter then brought within aqueous and atmospheric influences. The earth, air, and water thus made to react upon each other, constitute the first matter from which, by mechanical and chemical transformations, the whole mineral world known to us has been produced.

It is the lower portions of this great disintegrated and water-impregnated mass which form, according to the present hypothesis, the semi-liquid layer supposed to intervene between the outer solid crust and the inner solid and anhydrous nucleus. In order to obtain a correct notion of the condition of this mass, both in earlier and later times, two points must be especially considered, the relation of temperature to depth, and that of solubility to pressure. It being conceded that the increase of temperature in descending in the earth's crust is due to the transmission and escape of heat from the interior, Mr. Hopkins showed mathematically that there exists a constant proportion between the effect of internal heat at the surface and the rate at which the temperature increases in descending. Thus, at the present time, while the mean temperature at the earth's surface is augmented only about one-twentieth of a degree Fahrenheit, by the escape of heat from below, the increase is to be found to be equal to about one degree for each sixty feet in depth. If, however, we go back to a period in the history of our globe when the heat passing upward through its crust was sufficient to raise the superficial temperature twenty times as much as at present, that is to say, one degree of Fahrenheit, the augmentation of heat in descending would be twenty times as great as now, or one degree for each three feet in depth (Geol. Journal, viii, 59.) The conclusion is inevitable that a condition of things must have existed during long periods in the history of the cooling globe, when the accumulation of comparatively thin layers of sediment would have been sufficient to give rise to all the phenomena of metamorphism, vulcanicity, and movements of the crust, whose origin Herschel has so well explained.

Coming, in the next place, to consider the influence of pressure upon the buried materials derived from the mechanical and chemical disintegration of the primitive crust, we find that by the presence of heated water throughout them, they are placed under conditions very unlike those of the original cooling mass. While pressure raises the fusing point of such bodies as expand in passing into the liquid state, it depresses that point for those which, like ice, contract in becoming liquid. The same principle extends to that liquefaction which constitutes solution; where, as is with few exceptions the case, the process is attended with condensation or diminution of volume, pressure will, as shown by the experiments of Sorby, augment the solvent power of the liquid.* Under the influence of the elevated temperature and the great pressure which prevail at considerable depths, sediments should, therefore, by the effect of the water which they contain, acquire a certain degree of liquidity, rendering not improbable the suggestion of Scheerer, that the presence of five or ten per cent of water may suffice, at temperatures approaching redness, to give to a granitic mass a liquidity partaking at once of the character of an igneous and an aqueous fusion. The studies by Mr. Sorby of the cavities in crystals have led him to conclude that the constituents of granitic and trachytic rocks have crystallized in the presence of liquid water, under great pressure, at temperatures not above redness, and consequently very far below that required for simple igneous fusion. The intervention of water in giving liquidity to lavas, has, in fact, long been taught by Scrope, and notwithstanding the opposition of Plutonists, like Durocher, Fournet, and Rivière, is now very generally admitted. In this connection, the reader is referred to the Geological Magazine for February, 1868, page 57, where the history of this question is discussed.

It may here be remarked that if we regard the liquefaction of heated rocks under great pressure, and in presence of water, as a process of solution rather than of fusion, it would follow that diminution of pressure, as supposed by Mr. Scrope, would cause, not liquefaction, but the reverse. The mechanical pressure of great accumulations of sediment is to be regarded as cooperating with heat to augment the solvent action of the water,

---

* **Sorby, Bakerian Lecture, Royal Society, 1863.**

and as being thus one of the efficient causes of the liquefaction of deeply buried sedimentary rocks.

[The following extracts from a note by the author to the Geological Magazine for February 1870, may be cited in further elucidation of this point:—"pressure, which in the first case, that of simple fusion of anhydrous materials, prevents liquefaction by preventing expansion, in the second case (that of igneo-aqueous fusion or liquefaction at high temperatures, by the aid of a small portion of water, as maintained by Scrope, Scheerer and Elie de Beaumont) on the contrary, favors liquefaction by promoting the solution of the water-impregnated mass. As Sorby has shown, a conversion of mechanical into chemical force appears in the increase of solubility under pressure. In other words, pressure prevents fusion when, as in most instances, it is a process of expansion, but favors solution, which is, with few exceptions, a process of contraction. Now since I place the seat of volcanic action in a region where solution, rather than simple fusion, is the cause of liquidity, I am led to consider pressure as one of the efficient causes of the liquefaction of rocks, and to regard its diminution as leading to solidification."]*

That the water intervenes not only in the phenomena of volcanic eruptions, but in the crystallization of the minerals of eruptive rocks, which have been formed at temperatures far below that of igneous fusion, is a fact not easily reconciled with either the first or the second hypothesis of volcanic action, but is in perfect accordance with the one here maintained, which is also strongly supported by the study of the chemical composition of igneous rocks. These are generally referred to two great divisions, corresponding to what have been designated the trachytic and pyroxenic types, and to account for their origin, a separation of a liquid igneous mass beneath the earth's crust into two layers of acid and basic silicates, was imagined by Philips, Durocher, and Bunsen. The last mentioned, as is well known, has calculated the normal composition of these supposed trachytic and pyroxenic magmas, and conceives that from them, either separately, or by admixture, the various eruptive rocks are derived; so that the amounts of alumina, lime, magnesia and alkalies, sustain a constant relation to the silica in the rock. If, however, we examine the analyses of the eruptive rocks in Hungary and Armenia, made by Streng, and put forward in support of this view, there will be found such discrepancies between the actual and the calculated results as to throw grave doubts on Bunsen's hypothesis.

---

* See in this connection, Mr. Scrope on "The Character of Lavas," in the Geological Magazine for March 1870.

Two things become apparent from a study of the chemical nature of eruptive rocks; first, that their composition presents such variations as are irreconcilable with the simple origin generally assigned to them, and second, that it is similar to that of sedimentary rocks, whose history and origin it is, in most cases, not difficult to trace. I have elsewhere pointed out how the natural operation of mechanical and chemical agencies tends to produce among sediments, a separation into two classes, corresponding to the two great divisions above noticed. From the mode of their accumulation, however, great variations must exist in the composition of the sediments, corresponding to many of the varieties presented by eruptive rocks. The careful study of stratified rocks of aqueous origin discloses, in addition to these, the existence of deposits of basic silicates of peculiar types. Some of these are in great part magnesian, others consist of compounds like anorthite and labradorite, highly aluminous basic silicates, in which lime and soda enter, to the almost complete exclusion of magnesia and other bases; while in the masses of pinite or agalmatolite rock we have a similar aluminous silicate, in which lime and magnesia are wanting, and potash is the predominant alkali. In such sediments as these just enumerated we find the representatives of eruptive rocks like peridotite, phonolite, leucitophyre, and similar rocks, which are so many exceptions in the basic group of Bunsen. As, however, they are represented in the sediments of the earth's crust, their appearance as exotic rocks, consequent upon a softening and extravasation of the more easily liquefiable strata of deeply buried formations, is readily and simply explained.*

The object of the present communication has been to call the attention of geologists to the neglected views of Keferstein and Herschel, which I have endeavored to extend and to adapt to the present state of our knowledge. It is proposed in another paper to consider the question of the agencies which have regulated the geographical distribution of volcanic phenomena both in ancient and in modern times.

Montreal, Canada, March, 1869.

---

* See in this connection the Canadian Journal for 1858, p. 203; Quart. Jour. Geol. Society for 1859, p. 494; this Jour., II, xxxvii. 255, xxxviii, 182; also Geology of Canada, 1863, pp. 643, 669, and Rep. Geol. Canada, 1866, p. 230.

ART. IV.—*Notes on some features of the Flora of Eastern Kansas;*

by ELIHU HALL.

[The following consists of two articles (somewhat curtailed) published in the *Prairie Farmer*, which give so clear an exposition of the general features of the vegetation of the region under consideration, that we have sought and obtained permission to reproduce them in this Journal.—EDS.]

TREE GROWTH.—In a trip in September last in a wagon through a portion of Eastern Kansas, from Forts Scott and Humboldt north to Leavenworth and Atchison, I had excellent opportunities for observation of its general flora, and more especially so that of the autumnal months. The general character of its arborescent flora is decidedly adverse to a favorable impression of the adaptation of the country to tree growth. The question arises why all these dwarfed, distorted, abnormally developed specimens everywhere, as compared with the true type of trees, such as we are accustomed to in our own noble denizens of the Mississippi Valley forests. True, on the larger streams, the black alluvial deposit grow some very fine specimens of black walnut, cottonwood and elms, but this area is quite limited.

We should naturally infer from this character of the native arborescent growth of the country that causes had been long existing and were yet in operation to produce so general a result; but since the settlement or since the planting of trees has been commenced there by the inhabitants, everywhere living evidences are springing up directly opposing all such inferences. In some parts of Jefferson county, artificial groves only ten years old, are already 50 feet high, and appearing, through the misty haze of autumn, like natural groves on the prairies of central Illinois. There are evidences, too, that the native growth, that is, the young trees, in all the bushy regions, are making good speed in becoming saplings of proper proportions; this is particularly so of the hickory, (*Carya alba* and *C. porcina.*)

These many facts satisfy us that the causes of the abnormal tree growth here are not ascribable to an uncongenial climate, nor to aridity or sterility of soil, nor to exposure, for the flourishing artificial groves in Jefferson county are upon as high land as there is probably in the state, so near the Missouri river. The growth of fruit trees, apple, peach, pear, and the grape vine, during the past season, has been prodigious.

SPECIES AND DISTRIBUTION OF NATIVE TREES.—The Oak Family is represented by *Quercus obtusiloba* everywhere in the bushy regions, principally as dwarfed low trees and bushes.

Q. macrocarpa is less abundant, but occurs throughout, some specimens of fair proportions. *Q. Prinus*, (var. *humilis*,) every where in the bushy regions from one foot to forty feet high, fruiting abundantly; the very smallest bushes. The White Oak (*Q. alba*) was not met with. I doubt if there is a specimen in the State. authors of books on Kansas and travelers to the contrary notwithstanding. It is known to be one of the first oaks that fails westward, and probably does not reach the State at any point.

Quercus coccinea was frequently met with, but is not abundant; all dwarfed specimens.

Quercus rubra is more plentiful, principally young trees.

Quercus palustris, in the southern portion of the State is abundant, the principal tree-growth mostly bushes and young trees, and some large old specimens of fair proportions on the bottoms of the larger streams.

The Hickory Family is represented by *Juglans nigra*, on the streams, but not plentiful; a few large specimens.

Carya alba, *C. sulcata*, *C. porcina*, *C. amara* and *C. olivæformis*, (the Pecan ;) the latter abounds abundantly in the southern part of the State, but so far as seen, in bushy specimens or young trees of little promise. *Carya alba* and *C. porcina* are the two species springing up thickly in the bushy regions, and are destined soon to afford an abundance of the best of fuel; are already of size for hoop-poles in many places northward. *Ulmus fulva* and *Ulmus Americana* abound on the streams. These are the two commonest elms everywhere westward; the latter principally prevailing.

Populus monilifera, the common cotton-wood, was frequently met with, but is probably not plenty after leaving the Missouri river. Neither of the Aspens, so common in the regions further north, was seen. The maples are rare trees. *Acer sacharinum* was not seen. A few well grown trees of *Negundo aceroides*, (the box elder). The *Diospyros Virginiana*, (Persimmon,) is an abundant shrub southward, and the Pawpaw (*Asimina triloba*) is scarcely less common in that region.

THE HERBACEOUS FLORA, as would be expected, partakes much of the character of the high plain flora west of the Missouri river; the country being chiefly prairie: the number of species is small, but individuals aggregate immensely. Of the grasses, Andropogon furcatus, A. scoparius and Sorghum nutans, compose probably 80 per cent. These are the chief hay grasses, and probably the most used by grazing stock in the summer; but the winter forage plant, as I learned from the inhabitants, the one upon which their cattle graze and fatten during the winter months, and from which Kansas has gained her reputation as a country where stock needed feeding a few weeks in the year, is *Sporobolus heterolepis.* This grass only abounds plentifully in certain localities; it affects the moister and flatter portions of the prairie, and is most common southward. It is a wide-spread species, however, being plentiful in Nebraska, Iowa and Illinois, and, doubtless, in other localities; more or less abundant all over the west and northwest, extending east also to Connecticut. The species may be easily known by its small contracted panicle and round seeds, and especially by its strong odor when bruised. It is inclined to grow in bunches or stools, and where most abundant it nearly occupies the ground.

The relative proportion of species as they occur in the localities mentioned below, will pretty fairly represent the *Graminea* in those regions.

In one hundred square feet on a high prairie in Jefferson county:—Andropogon furcatus, 80 per cent; Andropogon scoparius, 8 per cent; Sorghum nutans, 6 per cent; Kœleria cristata, 4 per cent; Bouteloua curtipendula, 2 per cent.

In the same area in another locality in Jefferson county: Sorghum nutans, formed 20 per cent; Andropogan scoparius, 60 per cent; Sporobolus heterolepis, 8 per cent; Panicum virgatum, 8 per cent; Panicum pauciflorum, 4 per cent.

Same area in Franklin county, high prairie:—Andropogon furcatus, 6 per cent; Andropogon scoparius, 30 per cent; Sorghum nutans, 45 per cent; Bouteloua curtipendula, 10 per cent; Panicum virgatum, 8 per cent; Panicum dichotomum, 1 per cent.

Same area in Anderson county; a thin soil, high prairie:—Andropogon scoparius, 50 per cent; Andropogon furcatus, 2 per cent; Sorghum nutans, 40 per cent; Koeleria cristata, 7 per cent; Panicum pauciflorum, 1 per cent.

These stations are all on the prairie, where no local conditions determine the character of the vegetation, the soils not affected by alkali or a superabundance of lime, but are good agricultural soils. Other stations are occupied with special species. The strongest alkaline places are almost exclusively occupied with *Vilfa cuspidata* and *V. depauperata*, two low-growing species, with very fine leaves thickly set on the ground; probably nearly worthless for grazing purposes. Another peculiar feature of the country consists of the rocky places, so-called. These are generally found in the vicinity of streams on the ground grading from the high prairies. Their peculiarity consists in being everywhere distinctly lined out and occupied abundantly by a very notable *Helianthus*, (sunflower,) *H. orgyalis*, a species peculiar to that region of the country and southward. These localities do not reach the northern portion of the State. These rocky lands furnish the botanist with his richest treasures. Another very notable plant of the southern and middle portions of Kansas, and peculiar to those regions, is *Amphiachyris dracunculoides.* This is an annual, resembling *Solidago linifolia*, but bearing an abundance of showy, yellow flowers in September. It has spread from its native habitat,—the rocky soils,—

to the road sides and into the fields of the farmers, where it has already become a formidable weed. Imagine a field of flax in full bloom, and you have a picture of many fields of the Kansas farmers, only differing in appearance by the flowers being yellow instead of blue. It has different local names, such as Butterweed, Tumble-weed, etc. A shopkeeper in Humboldt, to whom I applied for information, rather contemptuously replied, "Curious any body didn't know tumble weed." Let not our readers imagine that the plant is *Amaranthus albus*, or *Cycloloma platyphylla*, that so often tumble over the fences and hedges in Illinois in the autumn months. But the Kansas plant does tumble some, too. When not growing too thickly, it forms a rather bushy, round head.

But probably the most notable plant on the prairies, at this season of the year, the most universally distributed, and the most showy, is *Salvia Pitcheri*, a species of sage, three to five feet high, bearing spikes of copious, rather large, blue flowers. These give coloring to the prairie landscape more or less abundantly (except in the northern counties, where it becomes much rarer) as far as the vision extends on either side, towering above the grasses and most other plants, crowning the whole verdant plain below with a canopy of blue. Another species of Sage (*Salvia trichostemoides*) is common also. This is more of a weed in appearance, and even threatens to become troublesome to the agriculturist.

The general herbaceous flora is largely represented with the *Helianthi*. *Helianthus lenticularis*, a showy species, much resembling the common annual sun-flower, is too abundant, being more or less troublesome as a weed to the farmer. *Helianthus petiolaris*, another annual species, is less common. *H. grosse-serratus*, *H. Maximiliani* on most rich soils. *H. rigidus* everywhere abounds, and *H. mollis* occurs in all the southern portions in immense quantity, but entirely disappears northward. Only *Silphium laciniatum* was seen. The other notable prairie species of "rosin weed," *Silphium terebinthinaceum* does not probably occur. These plants I find are looked after by land hunters as indications of a good soil. I find the one species here of all heights, from a foot to six feet; the general average is about three feet this wet season, which is much below the ordinary stature of the plant as seen on the prairies of Illinois. Some other Compositæ are largely represented: *Aster multiflorus*, *Solidago rigidus*, *S. linifolius*, and *S. Missouriensis* being exceedingly abundant in specimens. The *Vernonia fascicularis* that gives so much character and notability to the bottom lands and wet prairies of Illinois in the autumn months, is here represented by an allied species, *Vernonia Arkansana*, but in much fewer specimens.

Lespedeza capitata, of the Pea family, is everywhere abundant on the prairies, and cut with the hay often forms too large a portion of tonnage for the interest of the buyer; which also must be said of *Liatris scariosa* and *L. squarrosa*; the former sometimes equal to a fourth or fifth of the gross weight of a stack. Two other species, but less abundant, are represented in the State, *L. punctata* and *L. pycnostachya.* These noble plants empurple the landscape on thousands of acres in August and September. *Gerardia asperifolia* is abundant almost everywhere on the prairie, and contributes a good share to the weedy character of the hay. *Eryngium Leavenworthii* in the extreme south is a showy, handsome plant, and, though a biennial, well deserves a place in ornamental grounds. *Mamillaria vivipara* and a form of *Opuntia Missouriensis* represent the *Cactaceæ*, so far as seen.

WEEDS.—The Kansas farmer has already introduced his quota of foreign weeds; but he also has some formidable natives to combat,—plants that thrive exceedingly with cultivation. *Amphiachyris dracunculoides*, before mentioned; *Croton capitatum*, and, perhaps, worst of all, *Solanum rostratum*, a species that probably combines the bad characters of *Datura Stramonium*, the clot bur and the sand bur. The plant is an annual, and is already easily established, particularly in the neighborhood of Leavenworth City. The common native *Polygonums* in many localities have grown so abundantly and so large in the fields as to make them almost inaccessible to a nervous pedestrian.

Setaria glauca, (the fox tail) was plentifully introduced on almost every farm in some localities, and flourishes exceedingly. Some of the farmers believe it to be native, so unaccountably has it appeared on their farms. But, like purslane, it insidiously marches westward, and often, doubtless, the seeds are abiding their time for the conditions of soil necessary to their development, for years in advance of the agriculturist. But of the introduced plants we sought in vain through all the Middle and Southern counties for *Poa pratensis*, the Kentucky blue grass. Why a plant that is so profitable to agriculture, so tenacious of life, and so encroaching on the neighboring species that few things are able to survive its neighborship (even the hazel with the aid of grazing stock is gradually dying out before it in Illinois, and in most parts, especially of central Illinois, the native grasses have long since yielded to its persistent encroachments),—why this species should be entirely absent was left to us without explanation. The residence grounds in the city of Lawrence, many of them tastefully, elegantly, and expensively improved with buildings, were waving with *Panicum capillare*, varied with species of weeds.

Can it be that all this comes from simple neglect of introduction? In the bushy regions in the neighborhood of Leavenworth and Atchison, some blue grass is met on the roadsides; a few pastures near Leavenworth (looking as if doing well) and the city residence lots are frequently occupied with it; but in no other parts of the State was the plant seen.

PRAIRIE VEGETATION.—The following catalogues represent the entire native flora on the areas and at the localities mentioned. Soon most of these things will have passed away before the restless plowshare and the all-devouring kine. I give them as a natural history record.

One hundred square feet on high prairie in Jefferson county:

Sorghum nutans, 20 per cent.
Andropogon scoparius 60 per cent.
Sporopolus heterolepis 5 " "
Helianthus rigidus 10 per cent.
Euphorbia corollata 3 plants.
Asclepias tuberosa 1 plant.
Petalostemon candidum 3 plants.
Coreopsis palmata 1 plant.
Panicum virgatum 6 plants.
Salvia Pitcheri 1 plant.
Solidago Canadensis 1 plant.
Solidago rigida 1 plant.
Solidago linifolia 1 plant.
Aster multiflorus 10 plants.
Panicum dichotomum 1 plant.
Aster azureus 5 plants.
Liatris scariosa 1 plant.
Liatris squarrosa 1 plant.
Eragrostis capillaris 1 plant.
Lespedeza capitata 1 plant.
Eryngium yuccæfolium 1 plant.
Silphium laciniatum 1 plant.
Phlox pilosa 1 plant.

Same area in Franklin county, high prairie.

Sorghum nutans, 60 per cent.
Andropogon scoparius 20 per cent.
Andropogon furcatus 2 per cent.
Lespedeza capitata 8 plants.
Lactuca elongata 1 plant.
Aster laevis 1 plant.
Aster oblongifolius 3 plants.
Salvia Pitcheri 5 plants.
Liatris scariosa 2 plants.
Bouteloua curtipendula 8 plants.
Panicum virgatum 6 plants.
Erigeron canadense 1 plant.
Kuhnia eupatorioides 1 plant.
Petalostemon violaceum 1 plant.
Rosa blanda 3 plants.
Petalostemon candidum 1 plant.
Silphium laciniatum 2 plants.
Panicum dichotomum 1 plant.
Helianthus rigidus 3 plants.
Euphorbia corollata 1 plant.
Solidago rigida 1 plant.

Same area in Anderson county, high prairie, thin soil.

Andropogon scoparius, 40 per cent.
Andropogon furcatus 1 plant.
Sorghum nutans 30 per cent.
Gerardia asperifolia, 50 plants.
Kœleria cristata, 10 per cent.
Helianthus rigidus 25 plants.
Aster multiflorus 1 plant.
Solidago Missouriensis 2 plants.
Amorpha canescens 1 plant.
Panicum panciflorum 3 plants.
Linum Boottii 10 plants.
Solidago linifolia 4 plants.
Polygala verticillata 3 plants.
Baptisia leucantha 1 plant.
Antenaria dioica 3 plants.
Ruellia ciliosa 1 plant.
Coreopsis tinctoria 1 plant.
Psoralea floribunda 1 plant.
Eryngium yuccæfolium 1 plant.

Same area in Linn county; high prairie.

Andropogon furcatus 80 per cent.
Andropogon scoparius 5 per cent.
Sorghum nutans 8 per cent.
Lespedeza capitata 5 plants.
Solidago Missouriensis 6 plants.
Ceanothus ovalis 1 plant.
Aster azureus 3 plants.
Aster oblongifolius 5 plants.
Solidago linifolia 1 plant.
Salvia Pitcheri 1 plant.
Amorpha canescens 5 plants.
Helianthus rigidus 3 plants.
Liatris squarrosa 8 plants.
Euphorbia corollata 1 plant.
Panicum dichotomum 3 plants.
Kœleria cristata 1 plant.
Oxalis violacea 1 plant.
Echinacea angustifolia 1 plant.
Linum Boottii 1 plant.
Polytænia Nuttallii 1 plant.
Bouteloua curtipendula 1 plant.
Ambrosia pycnostachya 1 plant.
Petalostemon violaceum 1 plant.
Polygala incarnata 1 plant.

ART. V.—*Notice of some Minerals from New Jersey;* by Prof. W. T. RŒPPER, of Bethlehem, Pa.

1. *Iron, Manganese, Zinc, Chrysolite.*

THE Stirling Hill, Sussex county, N. J., which with its neighbor, the Minehill, seems to be an inexhaustible storehouse of interesting minerals, both scientifically and commercially, has furnished an antitype to Prof. Brush's Hortonolite from the adjoining Orange county.

Some years ago I examined a black crystalline massive mineral from this locality, and found it to be a unisilicate of the protoxyds of iron, manganese, zinc and magnesium, and as it showed many of the characteristic physical and chemical properties of chrysolitic minerals, especially that peculiar mottled coloring, which is so marked in olivines, I supposed it a variety of tephroite, that peculiar subspecies of the group having shortly before been rediscovered by Prof. Brush. During a visit to the locality in the course of last year, I succeeded in finding distinct crystals, which at once ranged the mineral unmistakably among the chrysolites.

Crystallization.—The crystals occur in great numbers, grouped together, and of all sizes, from an eighth of an inch to two inches in length and nearly one inch in breadth. They are mostly rounded, owing to an incipient alteration of the surface through meteoric waters, black and dull on the outside, but with lustrous and brilliant cleavages on being broken. Some of them, however, are sharp, and allow of a measurement of angles at least by the hand-goniometer. The dominant forms are: $i\bar{2}$ (angle over $i\bar{i}$, 130°), $i\breve{i}$ ($i\bar{2}\wedge i\breve{i}$, 115°), $1\bar{i}$ (angle at top, 77°). Generally subordinate I have observed the following forms: $i\bar{i}$, $1\text{-}\breve{i}$, $1\text{-}\bar{2}$, O, and a face replacing combination edge $i\text{-}\bar{2}\wedge 1\text{-}\bar{i}$ with parallel intersection-edges, in which therefore $n' = \frac{2m'}{m'-m}$, to which among simple coefficients the forms $2\text{-}\bar{4}$ or $3\text{-}\bar{3}$ would answer. The face is too small and dull to allow of a measurement of angle; $2\text{-}\bar{4}$ appears however the more likely form, as chrysolites seem to have a preference for the ratio 1:2. *Cleavages*, three rectangular; O and $i\text{-}\breve{i}$ very and almost equally eminent, with vitreo-pearly luster approaching the sub-adamantine; $i\text{-}\bar{i}$ splintery. *Hardness*=5·5–6. *Sp. G.*=3·95–4·08. The average of nine determinations with Jolly's spring balance gave 4·023. *Color*, dark green to black but eminently mottled, so that thin splinters or laminæ transmit a pale yellow light. *Streak*, light-yellowish-reddish-gray. The powder is slightly attracted by the magnet. BB. rather refractory, fusing at thin edges to a dull black slag. On charcoal gives a zinc coating, more distinctly on addition of soda. With the fluxes the usual reactions for silica, iron and manganese. The borax and microcosmic beads give in the O. F. the characteristic brownish purple color, indicating mixtures of iron and manganese, which becomes green in the reducing flame. With acids gelatinizes readily and completely. Some specimens leave a bright green undissolved residue, which I judge to be spinel both from its hardness, its not being attacked by fusion with soda, and complete decomposition by bisulphate of soda.

In the following analyses the silica was separated in the usual manner, the filtrate from the silica neutralized by carbonate of soda, then acidified with acetic acid and a current of sulphuretted hydrogen passed through the solution, which separated the zinc as sulphid. This was filtered off, redissolved in HCl, and then precipitated from the uninterruptedly boiling solution by slowly adding $\dot{Na}\ddot{C}$, and the $\dot{Zn}\ddot{C}$ finally converted into $\dot{Zn}$ by ignition. The filtrate after the separation of the zinc was then boiled with the addition of $1KO,ClO_5$ to sesquioxydize the iron, the iron precipitated in the usual manner as subacetate, redissolved and reprecipitated by ammonia, the manganese separated by bromine and determined as pyrophosphate with the precautions pointed out by Dr. Gibbs, (this Journal, No. XXXI, p. 216), and lastly the magnesia determined as pyrophosphate. I will here remark, that I did not succeed in separating the oxyd of zinc from the iron by the usual acetate of soda process, but that a great and often the greater part of the $\dot{Zn}$ went down with the iron, which I attribute to the necessary boiling of a dilute solution (vid. Johnson's Qual. Fresenius). Hence my former analyses were not correct, gave too little zinc and resulted in uni-oxygen ratio only on account of the near proximity of the equivalents of iron, manganese and zinc-oxyds. I may, however, not have handled the method correctly.

The samples No. 1 and 2 were fresh pieces of cleavage crystals carefully examined by the lens so as to avoid all visible admixtures. No. 1 was lighter in color than No. 2, *a* and *b*, which latter are analyses of the same powder. No. 3 are analyses of two different powders of the massive variety.

	1.	2.		3.		Oxygen				
		a	*b*	*a*	*b*	1.	2, *a*.	2, *b*.	3, *a*.	3, *b*.
$\ddot{\mathrm{Si}}$	30·76	29·90	30·56	30·67	30·42	16·41	15·95	16·30	16·36	16·22
$\dot{\mathrm{Fe}}$	33·78	35·60	35·44	35·37	34·20	7·51	7·91	7·88	7·86	7·60
$\dot{\mathrm{Mn}}$	16·25	16·90	16·93	17·81	17·67	3·66	3·81	3·82	4·01	3·98
$\dot{\mathrm{Zn}}$	10·96	10·66	10·70	9·87	9·09	2·16	2·11	2·11	1·95	1·80
$\dot{\mathrm{Mg}}$	7·60	5·81	5·44	5·69	6·49	3·04	2·32	2·18	2·28	2·60
Insol.		1·03	1·04	1·39	2·65					
	99·35	99·90	100·11	100·80	100·52	16·37	16·15	15·99	16·10	15·98

The foregoing oxygen ratios make the mineral a unisilicate. The crystallization being orthorhombic with the parametric ratios of the chrysolite group, which is confirmed by the other physical and chemical characters; it is hence an iron-manganese-*zinc* chrysolite, the first, to the best of my knowledge, of the group, into the composition of which zinc enters as a constituent.

It occurs, as before said, on Stirling hill, accompanied by Willemite, Franklinite, Jeffersonite and spinel.

2. *Manganesian Dolomite.*

In the vast vein of Willemite, which is being worked on Minehill by the New Jersey Zinc Company, there occur small masses of a beautiful delicate pink mineral with a rhombohedral cleavage, which by their contrast with the purely apple-green Willemite make exceedingly pretty specimens. An analysis gave the following composition:

$\dot{\mathrm{Ca}}\ddot{\mathrm{C}}$	$\dot{\mathrm{Mn}}\ddot{\mathrm{C}}$	$\dot{\mathrm{Fe}}\ddot{\mathrm{C}}$	$\dot{\mathrm{Mg}}\ddot{\mathrm{C}}$	Insol.	
50·40	43·54	·76	5·69	0·08	= 100·47

Specific gravity =3·052. Hardness =4.

The mineral differs from the known dialogites by its greater proportion of carbonate of lime, and may be considered either as a dialogite in which a little more than one-half of the $\dot{\mathrm{Mn}}$ is replaced by lime, or as a dolomite in which about five-sixths of the magnesia is replaced by $\dot{\mathrm{Mn}}$.

3. *A pseudomorph of opal after a micaceous mineral probably some chlorite.*

On Scotch mountain, Warren county, N. J., not far from New Village, among the Laurentian syenitic gneiss formation of that region, there occur, scattered over the ground, numerous masses of a white quartzose mineral apparently of agglutinated rounded granules of about $\frac{1}{8}$ inch diameter. Upon close examination, many of these granules show distinct cleavages, which exhibit a hexagonal outline. Searching the ground carefully I found wormlike contorted crystals, in shape like the similar forms of some chloritic minerals. The substance is distinguished from quartz by its low specific gravity =1·961, and inferior hardness (near 6). It is mostly soluble in caustic potash, leaving only 8 per cent insoluble, which seemed to consist, in part at least, of the original mineral. On ignition it loses 7·27 per cent water. It is therefore manifestly amorphous quartz or opal. Indeed small masses of unquestionable opal of various colors are found in the neighborhood.

It hence appears, that micaceous structure is not, as is frequently assumed, the absolute closing scene of the metamorphism of minerals, but that the replacing power of silica is able to overcome the antimetamorphic energies of minerals even, which have arrived at the micaceous stage.

Bethlehem, April 22, 1870.

ART. VI.—*On the Size of Atoms;* by Prof. Sir W. THOMSON, F.R.S.*

THE idea of an atom has been so constantly associated with incredible assumptions of infinite strength, absolute rigidity, mystical actions at a distance, and indivisibility, that chemists and many other reasonable naturalists of modern times, losing all patience with it, have dismissed it to the realms of metaphysics, and made it smaller than "anything we can conceive." But if atoms are inconceivably small, why are not all chemical actions infinitely swift? Chemistry is powerless to deal with this question, and many others of paramount importance, if barred by the hardness of its fundamental assumptions, from contemplating the atom as a real portion of matter occupying a finite space, and forming a not immeasurably small constituent of any palpable body.

More than thirty years ago naturalists were scared by a wild proposition of Cauchy's, that the familiar prismatic colors proved the "sphere of sensible molecular action" in transparent liquids and solids to be comparable with the wave-length of light. The thirty years which have intervened have only confirmed that proposition. They have produced a large number of capable judges; and it is only incapacity to judge in dynamical questions that can admit a doubt of the substantial correctness of Cauchy's conclusion. But the "sphere of molecular action" conveys no very clear idea to the non-mathe-

---

* From Nature, No. 22, March 31.

matical mind. The idea which it conveys to the mathematical mind is, in my opinion, irredeemably false. For I have no faith whatever in attractions and repulsions acting at a distance between centers of force according to various laws. What Cauchy's mathematics really proves is this: that in palpably homogeneous bodies, such as glass or water, contiguous portions are not similar when their dimensions are moderately small fractions of the wave-length. Thus in water, contiguous cubes, each of one one-thousandth of a centimeter breadth, are sensibly similar. But contiguous cubes of one ten-millionth of a centimeter must be very sensibly different. So in a solid mass of brickwork, two adjacent lengths of 20,000 centimeters each, may contain, one of them nine hundred and ninety-nine bricks and two half bricks, and the other one thousand bricks: thus two contiguous cubes of 20,000 centimeters breadth may be considered as sensibly similar. But two adjacent lengths of forty centimeters each might contain, one of them one brick and two half bricks, and the other two whole bricks; and contiguous cubes of forty centimeters would be very sensibly dissimilar. In short, optical dynamics leaves no alternative but to admit that the diameter of a molecule, or the distance from the center of a molecule to the center of a contiguous molecule in glass, water, or any other of our transparent liquids and solids, exceeds a ten-thousandth of the wave-length, or a two-hundred-millionth of a centimeter.

By experiments on the contact electricity of metals made eight or ten years ago, and described in a letter to Dr. Joule, which was published in the Proceedings of the Literary and Philosophical Society of Manchester, I found that plates of zinc and copper connected with one another by a fine wire attract one another, as would similar pieces of one metal connected with the two plates of a galvanic element, having about three-quarters of the electro-motive force of a Daniel's element.

Measurements published in the Proceedings of the Royal Society for 1860 showed that the attraction between parallel plates of one metal held at a distance apart small in comparison with their diameters, and kept connected with such a galvanic element, would experience an attraction amounting to two ten-thousand-millionths of a gram weight per area of the opposed surfaces equal to the square of the distance between them. Let a plate of zinc and a plate of copper, each a centimeter square and a hundred-thousandth of a centimeter thick, be placed with a corner of each touching a metal globe of a hundred-thousandth of a centimeter diameter. Let the plates, kept thus in metallic communication with one another be at first wide apart, except at the corners touching the little globe, and let them then be gradually turned round till they are parallel and at a distance of a hundred-thousandth of a centimeter asunder. In this position they will attract one another with a force equal in all to two grams weight. By abstract dynamics and the theory of energy, it is readily proved that the work done by the changing force of attraction during the motion by which we have supposed this position to be reached, is equal to that of a constant force of two grams weight acting through a space of a hundred-thousandth of a centimeter; that is to say, to two hundred-thousandths of a centimeter-gram. Now let a second plate of zinc be brought by a similar process to the other side of the plate of copper; a second plate of copper to the remote side of this second plate of zinc, and so on till a pile is formed consisting of 50,001 plates of zinc and 50,000 plates of copper, separated by 100,000 spaces, each plate and each space one hundred-thousandth of a centimeter thick. The whole work done by electric attraction in the formation of this pile is two centimeter-grams.

The whole mass of metal is eight grams. Hence the amount of work is a quarter of a centimeter-gram per gram of metal. Now 4,030 centimeter-grams of work, accord-ing to Joule's dynamical equivalent of heat, is the amount required to warm a gram of zinc or copper by one degree centigrade. Hence the work done by the electric attraction could warm the substance by only $\frac{1}{16120}$ of a degree. But now let the thickness of each piece of metal and of each intervening space be a hundred-millionth of a centimeter instead of a hundred-thousandth. The work would be increased a million-fold unless a hundred-millionth of a centimeter approaches the smallness of a molecule. The heat equivalent would therefore be enough to raise the temperature of material by 62°. This is barely, if at all, admissible, according to our present knowledge, or, rather, want of knowledge, regarding the heat of combination of zinc and copper. But suppose the metal plates and intervening spaces to be made yet four times thinner, that is to say, the thickness of each to be four-hundred-millionth of a centimeter. The work and its heat equivalent will be increased sixteen-fold. It would therefore be 990 times as much as that required to warm the mass by 10 cent, which is very much more than can possibly be produced by zinc and copper in entering into molecular combination. Were there in reality anything like so much heat of combination as this, a mixture of zinc and copper powders would, if melted in any one spot, run together, generating more than heat enough to melt each throughout; just as a large quantity of gunpowder if ignited in any one spot burns throughout without fresh application of heat. Hence plates of zinc and copper of a three-hundred-millionth of a centimeter thick, placed close together alternately, form a near approximation to a chemical combination, if indeed such thin plates could be made without splitting atoms.

The theory of capillary attraction shows that when a bubble —a soap-bubble for instance—is blown larger and larger, work is done by the stretching of a film which resists extension as if it were an elastic membrane with a constant contractile force. This contractile force is to be reckoned as a certain number of units of force per unit of breadth. Observations of the ascent of water in capillary tubes shows that the contractile force of a thin film of water is about sixteen milligrams weight per millimeter of breadth. Hence the work done in stretching a water film to any degree of thinness, reckoned in millimeter-milligrams, is equal to sixteen times the number of square millimeters by which the area is augmented, provided the film is not made so thin that there is any sensible diminution of its contractile force. In an article "On the Thermal effect of drawing out a Film of Liquid," published in the Proceedings of the Royal Society for April, 1858, I have proved from the second law of thermodynamics that about half as much more energy, in the shape of heat, must be given to the film to prevent it from sinking in temperature while it is being drawn out. Hence the intrinsic energy of a mass of water in the shape of a film kept at constant temperature increases by twenty-four milligram-millimeters for every square millimeter added to its area.

Suppose then a film to be given with a thickness of a millimeter, and suppose its area to be augmented ten thousand and one fold: the work done per square millimeter of the original film, that is to say per milligram of the mass, would be 240,000 millimeter-milligrams. The heat equivalent of this is more than half a degree centigrade of elevation of temperature of the substance. The thickness to which the film is reduced on this supposition is very approximately a ten-thousandth of a millimeter. The commonest observation on the soap-bubble (which in contractile force differs no doubt very little from pure water) shows that there is no sensible diminution of contractile force by reduction of the thickness to the ten-thousandth of a millimeter; inasmuch as the thickness which gives the first maximum brightness round the black spot seen where the bubble is thinnest, is only about an eight-thousandth of a millimeter.

The very moderate amount of work shown in the preceding estimates is quite consistent with this deduction. But suppose now the film to be further stretched, until its thickness is reduced to a twenty-millionth of a millimeter. The work spent in doing this is two thousand times more than that which we have just calculated. The heat equivalent is 1,130 times the quantity required to raise the temperature of the liquid by one degree centigrade. This is far more than we can admit as a possible amount of work done in the extension of a liquid film. A smaller amount of work spent on the liquid would convert it into vapor at ordinary atmospheric pressure. The conclusion is unavoidable, that a water-film falls off greatly in its contractile force before it is reduced to a thickness of a twenty-millionth of a millimeter. It is scarcely possible, upon any conceivable molecular theory, that there can be any considerable falling off in the contractile force as long as there are several molecules in the thickness. It is therefore probable that there are not several molecules in a thickness of a twenty-millionth of a millimeter of water.

The kinetic theory of gases suggested a hunded years ago by Daniel Bernouilli has, during the last quarter of a century, been worked out by Herapath, Joule, Clausius, and Maxwell, to so great perfection that we now find in it satisfactory explanations of all non-chemical properties of gases. However difficult it may be even to imagine what kind of thing the molecule is, we may regard it as an established truth of science that a gas consists of moving molecules disturbed from rectilineal paths and constant velocities by collisions or mutual influences, so rare that the mean length of proximately rectilineal portions of the path of each molecule is many times greater than the average distance from the center of each molecule to the center of the molecule nearest it at any time. If, for a moment, we suppose the molecules to be hard elastic globes all of one size, influencing one another only through actual contact, we have for each molecule simply a zigzag path composed of rectilineal portions, with abrupt changes of direction. On this supposition Clausius proves, by a simple application of the calculus of probabilities, that the average length of the free path of a particle from collision to collision bears to the diameter of each globe, the ratio of the whole space in which the globes move, to eight times the sum of the volumes of the globes. It follows that the number of the globes in unit volume is equal to the square of this ratio divided by the volume of a sphere whose radius is equal to that average length of free path. But we cannot believe that the individual molecules of gases in general, or even of any one gas, are hard elastic globes. Any two of the moving particles or molecules must act upon one another somehow, so that when they pass very near one another they shall produce considerable deflexion of the path and change in the velocity of each. This mutual action (called force) is different at different distances, and must vary, according to variations of the distance, so as to fulfil some definite law. If the particles were hard elastic globes acting upon one another only by contact, the law of force would be —zero force when the distance from center to center exceeds the sum of the radii, and infinite repulsion for any distance less than the sum of the radii. This hypothesis, with its "hard and fast" demarcation between no force and infinite force, seems to require mitigation. Without entering on the theory of vortex atoms at present, I may at least say that soft elastic solids, not necessarily globular, are more promising than infinitely hard elastic globes. And, happily, we are not left merely to our fancy as to what we are to accept for probable in respect to the law of force. If the particles were hard elastic globes, the average time from collision to collision would be inversely as the average velocity of the particles. But Maxwell's experiments on the variation of the viscosities of gases with change of temperature prove that the mean time from collision to collision is independent of the velocity, if we give the name collision to those mutual actions only which produce something more than a certain specified degree of deflection of the line of motion. This law could be fulfilled by soft elastic particles (globular or not globular); but, as we have seen, not by hard elastic globes. Such details, however, are beyond the scope of our present argument. What we want now is rough approximations to absolute values, whether of time or space or mass—not delicate differential results. By Joule, Maxwell, and Clausius we know that the average velocity of the molecules of oxygen or nitrogen or common air, at ordinary atmospheric temperature and pressure, is about 50,000 centimeters per second, and the average time from collision to collision a five-thousand-millionth of a second. Hence the average length of path of each molecule between collisions is about $\frac{1}{100000}$ of a centimeter. Now, having left the idea of hard globes, according to which the dimensions of a molecule and the distinction between collision and no collision are perfectly sharp, something of apparent circumlocution must take the place of these simple terms.

First, it is to be remarked that two molecules in collision will exercise a mutual repulsion in virtue of which the distance between their centers, after being diminished to a minimum, will begin to increase as the molecules leave one another. This minimum distance would be equal to the sum of the radii, if the molecules were infinitely hard elastic spheres; but in reality we must suppose it to be very different in different collisions. Considering only the case of equal molecules, we might, then, define the radius of a molecule as half the average shortest distance reached in a vast number of collisions. The definition I adopt for the present is not precisely this, but is chosen so as to make as simple as possible the statement I have to make of a combination of the results of Clausius and Maxwell. Having defined the radius of a gaseous molecule, I call the double of the radius the diameter; and the volume of a globe of the same radius or diameter I call the volume of the molecule.

The experiments of Cagniard de la Tour, Faraday, Regnault, and Andrews, on the condensation of gases do not allow us to believe that any of the ordinary gases could be made forty thousand times denser than at ordinary atmospheric pressure and temperature, without reducing the whole volume to something less than the sum of the volume of the gaseous molecules, as now defined. Hence, according to the grand theorem of Clausius quoted above, the average length of path from collision to collision cannot be more than five thousand times the diameter of the gaseous molecule; and the number of molecules in unit of volume cannot exceed 25,000,000 divided by the volume of a globe whose radius is that average length of path. Taking now the preceding estimate, $\frac{1}{100000}$ of a centimeter, for the average length of path from collison to collision, we conclude that the diameter of the gaseous molecule cannot be less than $\frac{1}{5000000000}$ of a centimeter; nor the number of molecules in a cubic centimeter of the gas (at ordinary density) greater than 6×10^{21} (or six thousand million million million).

The densities of known liquids and solids are from five hundred to sixteen thousand times that of atmospheric air at ordinary pressure and temperature; and, therefore, the number of molecules in a cubic centimeter may be from 3×10^{24} to 10^{26} (that is, from three million million million million to a hundred million million million million). From this (if we assume for a moment a cubic arrangement of molecules), the distance from center to nearest center in solids and liquids may be estimated at from $\frac{1}{1400000000}$ to $\frac{1}{4600000000}$ of a centimeter.

The four lines of argument which I have now indicated, lead all to substantially the same estimate of the dimensions of molecular structure. Jointly they establish with what we cannot but regard as a very high degree of probability the conclusion that, in any ordinary liquid, transparent solid, or seemingly opaque solid, the mean distance between the centers of contiguous molecules is less than the hundred-millionth, and greater than the two thousand-millionth of a centimeter.

To form some conception of the degree of coarse-grainedness indicated by this conclusion, imagine a rain drop, or a globe of glass as large as a pea, to be magnified up to the size of the earth, each constituent molecule being magnified in the same proportion. The magnified structure would be coarser grained than a heap of small shot, but probably less coarse grained than a heap of cricket-balls.

ART. VII.—*Miscellaneous Optical Notices*; by WOLCOTT GIBBS, M.D., Rumford Professor in Harvard University.

§ 1.

*On the measurement of wave lengths by means of indices of refraction.**

IN a brief notice† communicated to the British Association for the Advancement of Science at its meeting in 1849, Prof. Stokes has given a method for measuring wave lengths, which depends upon the fact that, in substances of medium refractive power, the increment of the index of refraction in passing from one point of the spectrum to another is nearly proportional to the increment of the square of the reciprocal of the wave length. The author showed that even when the intervals were taken much longer than necessary, the error in the wave length was usually only in the eighth place of decimals. At the date of the publication of this notice the subject of wave lengths possessed but little interest. The recent development of the spectral analysis of light has given a new impulse to this branch of optics, and has rendered necessary the construction of a normal map of the entire solar spectrum. This has been most successfully accomplished by Angström,‡ but an attentive study of his work, as well as of the elaborate researches of Van der Willigen§ and Ditscheiner,‖ will show that new measurements will be far from superfluous. The imperfections even of the best ruled glasses are so great that it may be reasonably doubted whether the wave lengths of very fine lines can be satisfactorily measured directly. Methods of determining such wave lengths, depending upon the comparison of the refraction and diffraction spectra, have been given by myself¶ and by Thalen.** As it seems at least desirable to multiply such methods, I will here give first a discussion of the method of Stokes in its original form, and afterward a simplification of that method which will also have its uses.

If Cauchy's formula for dispersion, $n = a + \frac{b}{\lambda^2} + \frac{c}{\lambda^4}$,

---

* Read before the National Academy of Sciences, April 12th, 1870.

† Report of the British Association for the Advancement of Science for 1849. Notices and abstracts, p. 10.

‡ Recherches sur le Spectre Solaire. Berlin, 1869.

§ Archives du Musée Teyler, vol. i, p. 1.

‖ Sitzungsberichte der k. k. Akad. der Wissenschaften Bd. l, 1864.

¶ This Journal, xlvii, March, 1869.

** Mémoire sur la determination des longueurs d' onde des raies métalliques, 1868.

be reduced to its two first terms, and if we then eliminate the constants a and b from three equations of the form

$$n=a+\frac{b}{\lambda^2}$$

we shall obtain the three following equations, involving only wave lengths and indices of refraction:

$$\lambda_1^2=\frac{(n_3-n_2)}{(n_3-n_1)\frac{1}{\lambda_2^2}+(n_2-n_1)\frac{1}{\lambda_3^2}} \qquad (1)$$

$$\lambda_2^2=\frac{(n_3-n_1)}{(n_2-n_1)\frac{1}{\lambda_3^2}+(n_3-n_2)\frac{1}{\lambda_1^2}} \qquad (2)$$

$$\lambda_3^2=\frac{(n_2-n_1)}{(n_3-n_1)\frac{1}{\lambda_2^2}+(n_3-n_2)\frac{1}{\lambda_1^2}} \qquad (3)$$

Of these equations (1) and (3) serve for extrapolation and (2) for interpolation. To test the degree of accuracy attainable in determining wave lengths by these formulas, I have selected the measurements made by Van der Willigen.* The indices of refraction determined by the Dutch physicist are in fact the only indices which are at once sufficiently exact and sufficiently numerous. In addition they have the great advantage of having been made with reference to lines in the solar spectrum the wave lengths of which had been measured by the same observer. There can therefore be no question of identity. As a first example of the method, I give a determination of the wave length of C, taking B as one of the lines exterior to C, and taking in succession 7 other exterior lines more refrangible than C, to combine with B. Formula (2) was therefore employed, and with the following data and results:—

B	1·61079	687·48		
C	1·61252	656·56		
D	1·61436	628·11	656·70	+0·14
11	1·61537	613·96	656·71	+0·15
13	1·61560	610·52	656·56	0·00
14	1·61728	589·56	656·71	+0·15
16	1·61978	561·80	656·76	+0·20
17	1·62064	553·19	656·79	+0·23
19	1·62143	545·83	656·87	+0·31
		Mean of the errors		+0·17

In this table the first column gives the designation or number of the lines, the second its index of refraction, as determined by a Steinheil prism of 60°, the third the corresponding

---

* Archives du Musée Teyler, vol. i, p. 70.

wave length, according to Van der Willigen, and the fourth the wave length as found by formula (2) by combining B with each line after C in succession.

The mean of the seven values of the wave length of C thus found is 656·70, which is in excess of Van der Willigen's own determination of the value of C by 0·14. From this it appears that the method may be applied with a tolerable degree of approximation, even in the case of a flint glass prism of high dispersive power, and for indices of refraction which refer to lines at considerable angular distances from each other. The increase in the computed values of C, as the intervals between B and the second line of comparison are increased, will however clearly appear from the table. The following results were obtained with the indices of a second Steinheil prism, No. 2, of 46° 52′ 25″·8, also of flint glass.

B	1·60521	B and 8*a*	656·21	—0·35
C	1·60694	B and 11	656·33	—0·23
8*a*	1·60872	B and 13	656·31	—0·25
11	1·60973	B and 16	656·38	−0·18
13	1·60998	B and 17	656·47	—0.09
16	1·61408			
17	1·61495			

The mean of which is 656·28, the error being −0·28. To determine to what extent the method applies, when flint glass prisms are used, and the indices are selected from the more refrangible portion of the spectrum, the following data were assumed:—

F	1·62917	486·39	F and G	438·88	+0.30
35	1·63244	467·00	F and 39	438·76	+0·18
37	1·63828	438·58	F and 38	438·82	+0·24
38	1·63931	434·28	G and 35	438·76	+0·18
39	1·63965	432·74	35 and 38	438·75	+0·17
G	1·64006	431·12	35 and 39	438·67	+0.09

In this table, line 37 is taken as the middle line in applying formula (2), and the results obtained by combining the other lines in pairs are given in columns 4, 5 and 6. It will be seen that, as in the case of the less refrangible portion of the spectrum, the results obtained are with this prism always too high. For the purpose of comparison, I have computed the same wave length from the indices of refraction of the second prism. The data and results are as follows:—

F	1·62332	486·39	F and G	439·07	+0·49
35	1·62657	467·00	F and 39	438·89	+0·31
37	1·63221	438·58	F and 38	438·92	+0·34
38	1·63324	434·28	G and 35	439·00	+0·42
39	1·63358	432·74	35 and 38	438·89	+0·31
G	1·63400	431·12	35 and 39	438·84	+0·26

In the case of the first prism the mean of the errors is +0·21, while for the second the mean of the errors is +0·35. From this it appears that in the more refrangible portion of the spectrum the errors are considerably greater than in the less refrangible portion, even for equal differences of wave length, and further, that the advantage in precision is with the prism having the higher dispersive power. As the probable errors of the measurements of the indices of refraction are not given, it is impossible to determine to what extent the errors in the computed wave lengths are due solely to want of precision in the indices. It is also to be remarked that, while with the second prism the errors in the less refrangible portion of the spectrum are affected with the sign −, in the more refrangible portion they are largely positive. The close agreement in the value of the wave length of 37, as found by Van der Willigen, with the values as found by Ditscheiner and Angström—438·27 and 438·28—proves that the source of error is not an erroneous determination of this quantity. It seems therefore certain that the nearly constant errors noted above are due in part to the fact that the indices of refraction are determined only to five places of decimals, and in part to the high dispersive powers of the prisms employed, which would render it necessary to employ more than two terms in Cauchy's formula to obtain a closer approximation. As the formulas for interpolation would in this way be rendered extremely complicated, it is better, in the case of any series of observations embracing a particular part of the scale, simply to determine the mean of the errors, and to apply this mean with its proper sign to the computed values of the particular wave length to be determined by the measurement of indices of refraction. If we apply such a correction in the cases of the four series of data and results given above, we find for the corrected values of the wave-lengths the following numerical results:—

(1)	(2)	(3)	(4)
656·53	656·49	438·67	438·72
656·54	656·61	438·55	438·54
656·38	656·59	438·61	438·57
656·54	656·66	438·55	438·65
656·59	656·75	438·54	438·54
656·62		438·44	438·49

The true values being respectively 656·56 and 438·56. These results are, I think, sufficient to show that a valuable control for the accuracy of measurements of wave lengths may be obtained even when prisms of high dispersive power are employed, provided that the intervals taken are not too large. It seems at least probable that a greater degree of precision is attainable in measuring indices in the case of substances of high than in those of low dispersive power, partly because the angular deviations to be measured are larger, and partly because the spectral lines are less crowded together.

The following example will serve to illustrate the advantage of taking shorter intervals:—

Lines.	λ	Indices.	λ	Indices.	λ
25*b*	518·63	1·62459	-----	1·61882	-----
26	517·51	1·62472	517.61	1·61895	517·56
27β	517·07	1·62479	-----	1·61901	-----

The data are here also taken from Van der Willigen's measures with the same prisms.

When the angular distances between three spectral lines are not too great, the angular deviation of the lines may, as I find, be substituted for the indices of refraction in formulas (1), (2) and (3). The differences between the angular deviations are of course to be converted into seconds. The following results will show the degree of accuracy attainable by this method, the data being taken from Ditscheiner's* measurements of the indices of a flint prism by Steinheil of refracting angle 60° 4′ 59″.

Kirchhoff's scale.		λ	Angular deviations.	λ	Indices.	λ
B	593	687·06	47° 40′ 55″	-----	1·61358	-----
C	694	655·95	47° 51′ 19″	655·97	1·61537	655.82
--	877	613·57	48° 8′ 8″	-----	1·61824	-----

From this it appears that the error in the determination of the wave-length of the middle line C is only +0·02 when the angular deviations are employed, but amounts to −0·13 when the indices of refraction are taken as the elements of the calculation. Yet the interval between B and 877 is very large.

The following data are taken from another part of the scale, the measurements being made with the same prism:—

Kirchhoff's scale.		λ	Angular deviations.	λ	Indices.	λ
b	1648·8	517·13	49° 4′ 16″	-----	1·62775	-----
	1655·6	516·58	49° 4′ 44″	516·56	1·62782	516·61
	1693·8	514·08	49° 6′ 47″	-----	1·62817	-----

Hence the error in the determination of the wave length of 1655·6 is, when the angular deviations are taken, only −0·02 and when the indices are taken +0·03. It must be borne in mind that in all the above mentioned examples the angles are those of minimum deviation. As the numbers upon Kirchhoff's scale also represent angular though not *minimum* deviations, it seemed worth while to determine how far for a short interval, these could be employed. Taking the three scale numbers of the last example, the error in the wave-length of

---

* Bestimmung der Wellenlängen der Fraunhoferschen Linien des Sonnenspectrums, p. 43.

1655·6 was found to be − 0·38, and when the scale numbers were taken as the sines or tangents of corresponding angles, +0·09.

The following data are taken from the more refrangible part of the spectrum, the measurements being also those of Ditscheiner, and made with the same prism:—

	2822·8	433.34	50° 34′ 57″	-----	1·64287	-----
G	2854·7	430·88	50° 37′ 52″	430·68	1·64334	430·83
	2969·7	429·90	50° 38′ 47″	-----	1·64352	-----

In this case the error in the wave-length of the middle line, 2854·7, is −0·20, as determined from the angular deviations, and −0·05, as determined from the indices. It must be borne in mind that, in this part of the spectrum, the determination both of wave lengths and of indices of refraction is difficult on account of the feeble intensity of the light.

Since only the differences between the angular deviations of the spectral lines are employed in the formulas above given, it follows that in determining wave lengths by the method in question, it is not necessary to employ a spectrometer with a divided circle and appliances for the measurement of large angles. A common spectroscope will be sufficient, if the observing telescope be provided with a filar micrometer by means of which the angular distances of any given line from two other lines of which the wave lengths are known may be measured. The researches of Angström leave nothing to be desired as regards the wave-lengths of standard lines, and the method given may prove a convenient means of determining with all requisite precision the wave lengths of metallic lines.

§ 2.

On liquids of high dispersive power.—Of the liquids which have hitherto been proposed for the construction of prisms, bisulphid of carbon unquestionably presents the greatest advantages. It is cheap, colorless, and unites a moderately high mean refractive to a very high dispersive power. By tacit consent a prism of 60° filled with this liquid has come to be adopted as a sort of standard. The disadvantages of the bisulphid are equally well known, and I have spent no little time and labor in the endeavor to find a liquid with a still higher dispersive power, less volatile, less sensitive optically to changes of temperature, and less offensive in odor. In these efforts I have not been altogether successful, no one liquid examined possessing all the qualities desired. Many organic liquids with high dispersive powers are difficult to prepare in a state of purity and speedily become discolored by absorption of oxygen from the air. Such are oil of cassia, the colorless oil obtainable from balsam of

Peru and others. The thallic alcohol of Lamy[*] is far too costly. The solution of silico-tungstate of sodium,[†] of metatungstate of sodium [‡] and of soluble tungstic acid [§] as obtained by dialysis, all promised good results from their extraordinary densities, but all proved difficult to prepare in a state of purity and extremely easy of decomposition.

A solution of phosphorus in bisulphid of carbon has, according to Messrs. Dale and Gladstone,[‖] a dispersion of 0·225,[¶] or nearly one and a half times as great as bisulphid of carbon alone, but becomes turbid on exposure to sunlight from the formation of amorphous phosphorus. It occurred to me, that, by dissolving sulphur with the phosphorus, the formation of amorphous phosphorus might be prevented, and experiment proved that this was the case. The solution, as thus obtained, has a pale yellow color, but is perfectly clear and undergoes no change by the action of light even when long continued. I have been in the habit of preparing it by dissolving one part of dry flowers of sulphur and two parts of phosphorus, in four or five parts of bisulphid of carbon, and filtering the liquid through a well dried ribbed paper filter, which is easily done. The refractive and dispersive power of the solution will of course vary with the quantity of phosphorus and sulphur dissolved. By a gentle heat the whole, or nearly the whole, of the bisulphid of carbon may be driven off, a liquid compound of sulphur and phosphorus remaining which has so high a mean refractive power that it cannot be employed with prisms having a refractive angle of more than 45°–50°. The same end may, however, also be attained by continually adding phosphorus to a saturated solution of sulphur in bisulphid of carbon, in which phosphorus appears to be soluble without limit.

With a strong and probably saturated solution of sulphur in CS_2 the angle between Li and D was 0° 50′ 10″. When phosphorus was added the angle was 2° 25′ 30″, the refracting angle of the prism being 60°. In this last case the angle between Na_1 and Na_2 was 0° 2′ 20″. The spectrum was perfectly clear, the definition of the dark lines leaving nothing to be desired. In consequence, however, of the yellow color of the liquid there is always a marked absorption of the violet end of the spectrum.

In working with the above described solution I have employed hollow glass prisms with refracting plates cemented on with a mixture of glue and molasses. These were found to

---

[*] Ann. de Chimie et de Physique, 4th series, vol. iii, p. 373.

[†] Ann. de Chimie et de Physique, 4th series, vol. iii, p. 5.

[‡] Scheibler in Journal für prakt. Chimie, lxxxiii, p 273.

[§] Graham in Journal of the Chemical Society, vol. ii, p. 318

[‖] L. and E. Phil. Mag., vol. xviii, p. 30.

[¶] The number 0·225 is the difference between the indices for the extreme red and violet rays.

be perfectly tight and to last for months without change. The great disadvantage in the use of a solution of sulphur and phosphorus consists in the danger of breaking the prisms; the liquid taking fire spontaneously when it has been a few seconds in contact with any porous material like wood or paper. On the other hand, however, the large quantity of sulphur present prevents the fire from spreading, a drop placed upon a piece of wood leaving after combustion only a charred spot. When not in use the prisms should be kept in an iron pot with a tight cover. In this manner I have employed and preserved two during a long and hot summer. The viscid, or rather oily, nature of the solution serves to prevent, to a great extent, the formation of ascending and descending currents from slight changes of temperature, and when the prisms are well shaken before use the definition remains perfect for a long time. In my spectroscope the prisms rest upon a plate of glass instead of upon one of metal.

§ 3.

On an advantageous form of apparatus for the study of the absorption of light in colored liquids.

In his examination of the spectra of colored fluids, Mr. Gladstone employed a hollow wedge of glass, the two refracting surfaces of which made with each other an acute angle. The wedge was filled with the liquid to be studied and so placed that the refracting edge of the analyzing prism was at right angles to the line of intersection of the two faces of the wedge. In this manner a beam of light was obtained which represented different thicknesses of the absorbing liquid, and the resulting spectrum became a complete absorption diagram. In using this apparatus I found the angular deviation produced by the wedge a source of considerable inconvenience. In addition it is easy to see that the wedge itself produces a certain amount of chromatic dispersion. To remedy these defects and at the same time retain the advantages of the method, I have devised what may be termed a double wedge. Two hollow wedges, of glass or metal, are placed together in such a manner that the first and last surfaces of the bounding plates of glass are parallel. The two wedges are separated by a single plate of glass with parallel surfaces. The base of each wedge, or acute-angled prism, is bored for the insertion of a cork or stopper. The construction of the apparatus will be readily understood from the diagram. In using it with a colored aqueous solution one of the hollow prisms, or wedges, is to be filled with distilled water, the other with the aqueous solution to be examined. The incident beam of sunlight is then allowed to fall perpendicularly upon

either surface, and the slit of the spectroscope is placed so as to be perpendicular to the two lines of intersection of the three refracting plates of the double wedge. In this manner a complete absorption diagram is obtained by the various thicknesses of the liquid examined. For substances soluble in alcohol, ether, &c., one of the hollow wedges must be filled with the colorless solvent, whatever be its nature. By this means all angular deviation and prismatic dispersion are avoided, as the coloring matter does not sensibly change the refractive power of the liquid in which it is dissolved, and the incident beam passes through without change in direction. In my apparatus the wedges have acute angles of about 15°. This I find to be quite sufficient for most purposes, as it is easy to increase or diminish the quantity of substance dissolved. When it is wished to examine the absorption produced by a definite thickness of liquid or by different thicknesses in succession, the double wedge is to be so placed that the slit of the spectroscope shall be parallel to the lines of intersection of the faces of the wedges. By moving the wedge to one side or the other all thicknesses of liquid, from 0 to the maximum, obtainable with the apparatus used, may be successively examined.

§ 4.

On tests for the perfection and parallelism of plane surfaces of glass.

When a plano-convex lens of long radius of curvature is placed upon a plane surface of glass and the system is illuminated by an obliquely incident beam of monochromatic light as, for example, by a sodium flame, the well known phenomenon of Newton's rings is observed with remarkable distinctness and perfection of definition. The symmetry of the rings will depend, in part, on the perfection of figure of the lens, in part on that of the plane surface. An extremely minute deviation from a perfect plane will produce a marked distortion of the circular figure of the ring nearest the center. That this distortion is, or is not due to the lens may be determined by rotating the lens round its optical axis normal to the plane. No change of figure will be seen if the lens is perfect in form and the inequality is in the plane surface only. Different parts of the plane surface may of course be tested in succession, by moving the lens from point to point, and if necessary the rings may be observed with a telescope.

Prof. Rood, of New York, has suggested for the observation of Newton's rings a method which permits of the employment of a lens of comparatively small radius of curvature and a microscope. In his arrangement the lens and plate of glass are placed upon the stage of the microscope, the light from beneath being cut off, and monochromatic light is then thrown down upon the system by means of a plate of glass with parallel surfaces inclined to the axis of the microscope at a convenient angle and placed between the objective and the plano-convex lens. In this manner the rings are seen with great distinctness and beauty, and the arrangement is particularly compact and convenient.

The interference bands of Talbot afford a method not merely of observing with great precision the inequalities of surface and want of parallelism of the faces of plates of glass, but also of photographing these defects and obtaining a permanent chart of the glass which may be of material assistance in correcting its figure. It is only necessary for this purpose to place the glass to be examined near to the object glass of the collimator and perpendicular to its axis, so as to intercept that half of the bundle of parallel rays which falls upon the first surface of the first prism nearest its refracting edge. If the plate has perfectly plane and parallel surfaces the interference bands will be sharply defined and parallel in the whole field of view. The slightest inequality of surface, or inclination of the faces, will produce curvature or distortion of the bands, and, if the eyepiece of the observing telescope be removed, the image may be received on a sensitive plate and photographed. The number of prisms to be employed in a particular case will depend upon the thickness of the plate of glass examined and, in general terms, upon its dispersive power. For a piece of French plate glass four millimeters in thickness, two bisulphid of carbon prisms of 60° must be used to produce a sufficient separation of the interference bands to enable them to be seen distinctly. More prisms must be used for thicker plates and in this way a limit is soon reached at which the method ceases to be applicable.

Cambridge, May 1st, 1870.

ART. VIII.—*On the Occurrence of a Peat bed beneath Deposits of Drift in Southwestern Ohio.*

A BED of peat has lately been found one mile east of Germantown, Montgomery county, Ohio, and twelve miles west of south from Dayton—in the occurrence and connections of which there are several facts of unusual interest.

It lies in, and directly above, the channel of Twin creek, a tributary of the Miami river. The general course of the creek is southeasterly, but just above the point where the peat bed is exposed, it has made a sudden change in direction from east to west of south. Its northern and eastern banks for $\frac{1}{4}$ of a mile in each direction from the point of deflection, are precipitous walls of stratified clay and gravel, from 50 to 100 feet in thickness; kept nearly vertical by the constant undermining action of the stream.

Beneath these heavy deposits and occupying 40 rods of the east bank of the creek, the peat bed is found, varying in thickness, in different portions of its extent, from 12 to 20 feet. The amount of the bed that is exposed depends upon the stage of water in the stream. The stream is bedded for 10 or 15 rods upon the peat, but in deeper portions of the channel, upon the eastern bank, an underlying formation of gravel can be detected. The uppermost layers of the peat contain undecomposed sphagnous mosses, grasses and sedges, but in other portions of the bed, the vegetable structure is generally indistinct, with the exception of abundant fragments of coniferous wood, which in many instances can be identified as Red Cedar (*Juniperus virginianus*). At the southern extremity of the bed in particular, there is a great accumulation of wood, in trunks, roots, branches and twigs, much of which has been flattened by the pressure of the 80 feet of clay and gravel that overlie it. Branches that were originally two inches in diameter now afford lenticular sections with no more than a ¼ inch for the shorter axis, while many of the smaller stems have been compressed into ribbons. The berries of the cedar are abundant in the upper layers of the peat. At a point ½ mile higher up the stream, trunks of cedar nearly two feet in diameter, have been taken from beneath these same drift beds and turned to account for fencing posts.

There are indications that the peat bed has a considerable extent to the northward and eastward. A bed of "black earth" was found underlying clay and gravel in digging a well 1¾ miles east of this locality. The bed occurred at a depth of 30 feet and was itself from 10 to 15 feet in thickness. The waters of springs in the same neighborhood are discolored, as if by contact with such deposits.

It may be added in this connection that there is a large amount of wood buried beneath the drift throughout this region generally. It is not a circumstance of infrequent occurrence to meet with it in the digging of wells. There is scarcely a square mile in the thickly settled portions of the adjacent country in which instances of this kind can not be found, and three instances are on record within the limits of a single village.

The wood is in great part coniferous, but not exclusively so; for according to the testimony of intelligent and observing, practical men who deem themselves entirely competent to give a judgment in the case, ash, hickory and sycamore, together with grape-vines and beech leaves, have been found covered with drift deposits.

A stratum of soil, one or two feet in thickness is often associated with these vegetable remains. The soil and the wood occur at various depths, but in the cases already noted, between the limits of 10 feet and 90 feet. A large proportion, however, of the instances on record, have been found at about 30 feet in depth, immediately beneath the yellow clays that constitute the last of the drift series in this region.

Through all portions of the peat above mentioned, sand and pebbles are scattered. The pebbles are mostly of small size, seldom larger than a pea, but occasionally three or four inches in diameter. They agree in general character with the gravel of the country.

At the lower extremity of the peat bed, the formation thins out and the bottom layers are found above the water, resting upon a surface of gravel that slopes downward at an angle of about 30 degrees. All the limestone pebbles which the peat overlies at this point, appear to have been "burned." They are white and soft, as much so as they would have been if they had been converted into hydrates of lime by the ordinary processes. Analysis, however, shows them to be in the state of carbonates.

In the inclined strata, heavy beds of ochreous gravel occur. The ochre is easily separated from the gravel by washing and furnishes a marketable paint of fair quality. The nature and arrangement of the materials of these inclined beds indicate that they were brought from the eastward by a torrent-like stream and deposited over a precipitous bank.

In pockets of the gravel and also in the clay that immediately covers the peat, small quantities of vivianite, "blue earth," or phosphate of iron, are found. From one of the largest accumulations of this substance, a tusk or tooth was taken. It was described as resembling a hog's tusk, except that it was much larger. It may also be added that two mastodon tusks, each measuring eight feet in length, were taken in the spring of 1870, from the northern part of the same drift bed to which the peat belongs and at about the same level.

The reference of the phosphoric acid of the vivianite to vertebrate bones will, therefore, hardly be questioned.

From the above named facts, we seem warranted in concluding that the coniferous wood in question grew in the region where we find it buried. The amount of the wood renders this probable and the nature of the remains forbids any other supposition. In this connection, it is only needful to recall the facts, that cedar berries in notable quantity, and that branching twigs, the veriest spray of the cedar, sometimes still covered with bark, are well preserved in the peat.

We learn furthermore that the date, at which this vegetation grew, was in the closing or Champlain epoch of the Drift period, for it is underlain by stratified drift deposits. A subsidence of the continent below its present level had already occurred, during which these underlying beds were formed, but there would seem to have been a restoration of this southern border of the drift-swept region at least, to dry land once more, and this restoration must have continued through a period of considerable length. It was followed by another movement of depression, during which the highest of the yellow clays, the latest formation of the drift, were deposited. There seem materials in this line of facts for a more orderly division of the later formed deposits of the post-tertiary than has heretofore been recognized.

We also learn that mammalian life was associated with this intercalated period of vegetable growth. The mammoth and the mastodon subsisted on the coniferous wood which is represented so largely here. The series of changes in level already referred to, must have exterminated these earlier representatives of elephantine life, but we find the same species returning to their old dwelling places when the waters of the drift seas had finally abated.

Art. IX. — *On the Theoretical Temperature of the Sun; under the Hypothesis of a Gaseous Mass maintaining its Volume by its Internal Heat, and depending on the Laws of Gases as known to Terrestrial Experiment;* by J. Homer Lane, Washington, D. C.

[Read before the National Academy of Sciences at the session of April 13–16, 1869.]

Many years have passed since the suggestion was thrown out by Helmholtz, and afterwards by others, that the present volume of the sun is maintained by his internal heat, and may become less in time. Upon this hypothesis it was proposed to account for the renewal of the heat radiated from the sun, by means of the mechanical power of the sun's mass descending toward his center. Calculations made by Prof. Pierce, and I believe by others, have shown that this provides a supply of heat far greater than it is possible to attribute to the meteoric theory of Prof. Wm. Thomson, which, I understand, has been abandoned by Prof. Thomson himself as not reconcilable with astronomical facts. Some years ago the question occurred to me in connection with this theory of Helmholtz whether the entire mass of the sun might not be a mixture of transparent gases, and whether Herschel's clouds might not arise from the precipitation of some of these gases, say carbon, near the surface, with their revaporization when fallen or carried into the hotter subjacent layers of atmosphere beneath; the circulation necessary for the play of this Espian theory being of course maintained by the constant disturbance of equilibrium due to the loss of heat by radiation from the precipitated clouds. Prof. Espy's theory of storms I first became acquainted with more than twenty years ago from lectures delivered by himself, and, original as I suppose it to be, and well supported as it is in the phenomena of terrestrial meteorology, I have long thought that Prof. Espy's labors deserve a more general recognition than they have received abroad. It is not surprising, therefore, in a time when the constitution of the sun was exciting so much discussion, that the above suggestions should have occurred to myself before I became aware of the very similar, and in the main identical, views of Prof. Faye, put forth in the Comptes Rendus. I sought to determine how far such a supposed constitution of the sun could be made to connect with the laws of the gases as known to us in terrestrial experiments at common temperatures. Some calculations based upon conjectures of the highest temperature and least density thought supposable at the sun's photosphere led me to the conclusion that it was extremely difficult, if not impossible, to make out the connection in a credible manner. Nevertheless, I mentioned my ideas to Prof. Henry, Secretary of the Smithsonian Institution, when he immediately referred me to a number of the Comptes Rendus, then recently received, containing Faye's exposition of his theory. Of course nothing is further from my purpose than to make any kind of claim to any thing in that publication. After becoming acquainted with his labors I still regarded the theory as seriously lacking, in its physical or mechanical aspect, the direct support of confirmatory observations, and even as being subject to grave difficulty in that direction. In this attitude I allowed the subject to rest until my friend Dr. Craig, in charge of the Chemical Laboratory of the Surgeon General's office, without any knowledge of Faye's memoir, or of my own suggestions previously made to Prof. Henry and another scientific friend, fell upon the same ideas of the sun's constitution, availing himself, precisely as I had done, of Espy's theory of storms. Dr. Craig's ideas were communicated to a company of scientific gentlemen early last spring, and soon after, Prof. Newcomb, of the U. S. Naval Observatory, entered into a general survey of the nebular hypothesis. These communications of Dr. Craig and Prof. Newcomb led me to enter into a renewed examination of the mechanical embarrassment under which I had believed the theory to labor. Not any longer relying on my first rough estimate based on assumed high temperatures at the photosphere, the question was now inverted. Assuming the gaseous constitution, and assuming the laws expressed in Poisson's formulæ, known to govern the constitution of gases at common temperatures and densities, what shall we find to be the temperatures and densities corresponding to the observed volume of the sun supposing it were composed of some known gas such as hydrogen, or supposing it to be composed of such a mixture of gases as would be represented by common air. Pure hydrogen will, of course, give us the lowest temperature of all known substances, under the general hypothesis.

The question was resolved, and the results were communicated in graphical and numerical form in May or June last to two or three scientific friends, but their publication has been delayed by an unavoidable absence of several months from home.

Premising that the unit of density shall correspond to a unit of mass in the cube of the unit of length, the unit of force to the force of terrestrial gravity in the unit of mass, and the unit of pressure or elasticity in the gas to the unit of force on a surface equal to the square of the unit of length:

Let r=the distance of an element of the sun's mass from the sun's center,

t=the temperature of the element,

σt=its atmospheric subtangent, referred to the force of gravity at the earth's surface, or height of the column of homogeneous gas, whose terrestrial gravitating force would equal its elasticity,

ϱ=its density, or mass of its unit volume,

=force of terrestrial gravity in its unit volume,

$\varrho \sigma t$=its elasticity, or elastic force per unit surface,

R=the earth's radius,

M=the earth's mass,

m=the mass of the part of the sun's body contained in the concentric sphere whose radius is r,

$\frac{M\ r^2}{m\ R^2}\sigma t$= the subtangent of the gas under its actual gravitating force in the sun.

The condition of equilibrium between the gravitating force of a thin horizontal layer of gas whose thickness is dr, and the difference of elastic force between its lower and upper surfaces, is expressed by the equation,

$$d \cdot \varrho \sigma t = -\frac{m\ R^2}{M\ r^2}\ \varrho\ d\ r.$$

Under the hypothesis that the law of Mariotte and the law of Poisson prevail throughout the whole mass, and that this mass is in convective equilibrium, we have

$$\sigma = \text{a constant}, \qquad (1)$$
$$t = t_1\ \varrho^{k-1},\text{*}$$

t_1 representing the value of t in the part of the mass where the density is a unit.

The theoretical difficulties which, if the supply of solar heat

---

* k represents the ratio of the specific heat of a gas under constant pressure to its specific heat under constant volume.

is to be kept up by the potential due to the mutual approach of the parts of the sun's mass consequent on the loss of heat by radiation, come in when we suppose a material departure from these laws of Mariotte and of Poisson at the extreme temperatures and pressures in the sun's body, or how far such difficulties intervene, will be considered further on.

By means of the constant value of σ, and the value of t given in (1), the above differential equation is transformed into

$$k\,\sigma\,t_1\,\varrho^{k-2}d\varrho = -\frac{m}{M}\,\frac{R^2}{r^2}\,d\,r,$$

the integral of which gives

$$1-\left(\frac{\varrho}{\varrho_0}\right)^{k-1} = \frac{k-1}{k}\,\frac{R^2}{\sigma M t_1 \varrho_0{}^{k-1}}\int_0^r \frac{m dr}{r_2}, \qquad (2)$$

in which ϱ_0 is the value of ϱ at the sun's center.

We have also

$$m = 4\pi\int_0^r \varrho r^2\,dr = 4\pi\varrho_0 \int_0^r \frac{\varrho}{\varrho_0} r^2\,dr. \qquad (3)$$

If now we put

$$r = \left(\frac{k\sigma M t_1}{4(k-1)R^2\pi\varrho_0{}^{2-k}}\right)^{\frac{1}{2}} x, \qquad (4)$$

we shall have

$$m = 4\pi\varrho_0\left(\frac{k\sigma M t_1}{4(k-1)R^2\pi\varrho_0{}^{2-k}}\right)^{\frac{3}{2}} \mu, \qquad (5)$$

in which

$$\mu = \int_0^x \frac{\varrho}{\varrho_0} x^2\,dx, \qquad (6)$$

and equation (2) becomes

$$1-\left(\frac{\varrho}{\varrho_0}\right)^{k-1} = \int \frac{\mu dx}{x^2}. \qquad (7)$$

In equations (6) and (7) it is plain that upon the value of k alone depends: first the form of the curve that expresses the value of $\frac{\varrho}{\varrho_0}$ for each value of x; secondly, the value of the upper limit of x corresponding to $\frac{\varrho}{\varrho_0}=0$; and thirdly, the corresponding value of μ. These limiting, or terminal, values of x and μ, cannot be found except by calculating the curve, for equations (6) and (7) seem incapable of being reduced to a complete general integral. But when these values have been found for any proposed value of k, they may be introduced once for all into equations (4) and (5), from which the values of ϱ_0, and of $\sigma\,t_1$, are at once deduced.

I have made these calculations for two different assumed values of k, viz., $k=1{\cdot}4$, which is near the experimental value it has in common air, and $k=1\frac{2}{3}$, which is the maximum possible value it can have in the light of Clausius' theory of the constitution of the gases. The calculation of the curve of $\frac{\varrho}{\varrho_0}$, or of $\left(\frac{\varrho}{\varrho_0}\right)^{k-1}$, begins at the sun's center where $x=0$. For the small values of x, integration by series enables us readily to deduce from equations (6) and (7) the following approximate numerical equations:

For $k=1{\cdot}4$,

$$\mu=\tfrac{1}{3}x^3-\tfrac{1}{12}x^5+\tfrac{5}{336}x^7-\tfrac{125}{54432}x^9+\text{ \&c.} \qquad (8)$$

$$1-\left(\frac{1}{\varrho_0}\right)^{\cdot 4}=\tfrac{1}{6}x^2-\tfrac{1}{48}x^4+\tfrac{5}{2016}x^6-\tfrac{125}{435456}x^8+\text{ \&c.} \qquad (9)$$

For $k=1\frac{2}{3}$,

$$\mu=\tfrac{1}{3}x^3-\tfrac{1}{20}x^5+\tfrac{1}{240}x^7-\tfrac{1}{3888}x^9+\text{ \&c.} \qquad (10)$$

$$1-\left(\frac{\varrho}{\varrho_0}\right)^{\frac{2}{3}}=\tfrac{1}{6}x^2-\tfrac{1}{80}x^4+\tfrac{1}{1440}x^6-\tfrac{1}{31104}x^8+\text{ \&c.} \qquad (11)$$

For larger values of x, until $\left(\frac{\varrho}{\varrho_0}\right)^{k-1}$ becomes sufficiently small as there is no need of great precision in these calculations, I have merely developed the values of μ and $\left(\frac{\varrho}{\varrho_0}\right)^{k-1}$ corresponding to $x=1{\cdot}1$, $x=1{\cdot}2$, $x=1{\cdot}3$, &c., by means of differences taken from the differential co-efficients at the middle of each increment of x, and for the same reason have thought it sufficient to begin with $x=1$, in equations (8) and (9) or (10) and (11). After arriving at a sufficiently small value of $\left(\frac{\varrho}{\varrho_0}\right)^{k-1}$ the calculation is finished by aid of the following approximate equations also derived by integration fron (6) and (7).

$$\mu'-\mu=\frac{k-1}{k}\,\mu'^{\frac{1}{k-1}}\,x'^{\,2-\frac{2}{k-1}}\,(x'-x)^{1+\frac{1}{k-1}}\,(1+X) \qquad (12)$$

$$\left(\frac{\varrho}{\varrho_0}\right)^{k-1}=\frac{\mu'(x'-x)}{x'x}-\frac{(k-1)^2}{k(2k-1)}\mu'^{\frac{1}{k-1}}\,x'^{-\frac{2}{k-1}}\,(x'-x)^{2+\frac{1}{k-1}}$$
$$-\frac{(k-1)(2k^2-3k+2)}{k(2k-1)(3k-2)}\,\mu'^{\frac{1}{k-1}}\,x'^{-1-\frac{2}{k-1}}\,(x'-x)^{3+\frac{1}{k-1}} \qquad (13)$$

In these equations x' and μ' are the values of x and μ corresponding to $\frac{\varrho}{\varrho_0}=0$, or the upper limit of the supposed solar atmosphere, and

$$X=-\frac{k(2k-3)}{(k-1)(2k-1)}\frac{x'-x}{x'}+\frac{k(k-2)(2k-3)}{2(k-1)^2(3k-2)}\frac{(x'-x)^2}{x'^2}+\ \&c.$$

$$-\frac{k-1}{2k(2k-1)}\mu'^{-1+\frac{1}{k-1}}x'^{2-\frac{2}{k-1}}(x'-x)^{1+\frac{1}{k-1}}+\&c.$$

With the values of x' and μ' determined, using r' and m' to express in like manner the corresponding values of r and m at the upper limit of the theoretical atmosphere, we find from equations (4) and (5)

$$\varrho_0=\frac{m'x'^3}{4\pi\mu'r'^3}, \qquad (14)$$

$$\sigma t_1=\frac{4\pi(k-1)R^2r'^2\varrho_0{}^{2-k}}{kMx'^2},$$

and by equation (1), $\sigma t=\frac{4\pi(k-1)R^2r'^2\varrho_0}{kMx'^2}\left(\frac{\varrho}{\varrho_0}\right)^{k-1}$, (15)

$$=\frac{k-1}{k}\,\frac{m'R^2x'}{\mu'Mr'}\left(\frac{\varrho}{\varrho_0}\right)^{k-1} \qquad (16)$$

A glance at equation (7) will show that $\frac{\mu'(x'-x)}{x'x}$, equation (13), or $\frac{\mu'}{x'}\;\frac{r'-r}{r}$ may be taken equal to $\left(\frac{\varrho}{\varrho_0}\right)^{k-1}$ throughout the considerable upper part of the volume of the hypothetic gaseous body in which $1-\frac{\mu}{\mu'}$, or $1-\frac{m}{m'}$, is sufficiently small to be neglected. This substitution in the last equation gives

$$\sigma t=\frac{k-1}{k}\;\frac{m'R^2}{Mrr'}(r'-r), \text{ nearly}, \qquad (17)$$

and also $\varrho=\left(\frac{\mu'}{x'}\right)^{\frac{1}{k-1}}\varrho_0\left(\frac{r'-r}{r}\right)^{\frac{1}{k-1}}$ nearly,

$$=\frac{1}{4\pi}\mu'^{-1+\frac{1}{k-1}}x'^{3-\frac{1}{k-1}}\frac{m'}{r'^3}\left(\frac{r'-r}{r}\right)^{\overline{k-1}} \qquad (18)$$

Now the mechanical equivalent of the heat in the mass ϱ of a cubic unit in volume of any perfect gas whose atmospheric subtangent is σt, is $\frac{1}{k-1}\varrho\cdot\sigma t$, and the mechanical equivalent of the heat that it would give out, in being cooled down under constant pressure to absolute zero, is $\frac{k}{k-1}\varrho\cdot\sigma t$. If the density ϱ is taken in units of the density of water, and the unit of length be the foot, this expression is multiplied by $62\frac{1}{2}$ to give for the mechanical equivalent in foot pounds

$$62\tfrac{1}{2}\frac{k}{k-1}\varrho\cdot\sigma t=\frac{62\frac{1}{2}}{4\pi}\mu'^{-1+\frac{1}{k-1}}x'^{3-\frac{1}{k-1}}\frac{m'^2R^2}{Mr'^4}\left(\frac{r'-r}{r}\right)^{1+\frac{1}{k-1}} \tag{19}$$

The mechanical equivalent $\frac{1}{k-1}\varrho\cdot\sigma t$, of the heat in the mass ϱ, viewed in the light of Clausius' mechanical theory of the gases, includes the motions of the separate atoms of each supposed compound molecule relatively to each other, as well as the motion of translation which each compound molecule makes in a straight path through free space till it impinges upon another compound molecule. If we wish to find the mechanical equivalent which would be due to this motion of translation alone, we must put $k=1\frac{2}{3}$ in the factor $\frac{1}{k-1}$ by which $\varrho\cdot\sigma t$ is multiplied, and this gives $\frac{3}{2}\varrho\cdot\sigma t$. To find from this the mean of the squares of the velocities of translation of the compound molecules, we divide by the mass ϱ, and, if the foot be the unit of length, multiply by 64·3, whence we have for the velocity found by taking the square root of this mean of the squares

$$8{\cdot}02\sqrt{\tfrac{3}{2}\sigma t}=8{\cdot}02\left(\frac{3}{2}\,\frac{k-1}{k}\,\frac{m'R^2x'}{\mu'Mr'}\right)^{\frac{1}{2}}\left(\frac{\varrho}{\varrho_0}\right)^{\frac{k-1}{2}} \tag{20}$$

Determination of the curve of density for $k=1{\cdot}4$.—Beginning with $x=1$, in equations (8) and (9), we find $\mu={\cdot}2626$ and $\left(\frac{\varrho}{\varrho_0}\right)^{\frac{4}{10}}={\cdot}8520$. Developing the values of μ and $\left(\frac{\varrho}{\varrho_0}\right)^{\frac{4}{10}}$ for $x=1{\cdot}1$, $x=1{\cdot}2$, &c., by means of differences we arrive at the values $\mu=2{\cdot}145$ and $\left(\frac{\varrho}{\varrho^0}\right)^{\frac{4}{10}}={\cdot}1378$ when $x=4{\cdot}0$. Putting these values into equations (12) and (13) we find

$$x'=5{\cdot}355,\quad \mu'=2{\cdot}188.$$

If we now allow $\frac{1}{22}$d of the radius of the photosphere, or about 20,000 miles, for the height of the theoretic upper limit of the solar atmosphere above the photosphere, and if we take the mean specific gravity of the earth's mass at $5\frac{1}{2}$, and the mean specific gravity of the sun within the photosphere at $\frac{1}{4}$ that of the earth, as it is known to be, these values of x' and μ' give us in equation (14)

$$\varrho_0=28{\cdot}16,$$

so that the density of the sun's mass at the center would be nearly one-third greater than that of the metal platinum.

Curve of density for $k=1\frac{2}{3}$.—For this value of k the numerical coefficients in equations (8) and (9) are replaced by those in (10) and (11). Otherwise, the same process employed with the value $k=1{\cdot}4$, gives, starting with $x=1$, $\mu=\cdot2875$ and $\left(\frac{\varrho}{\varrho_0}\right)^{\frac{2}{3}}=\cdot8452$, and developing for $x=1{\cdot}1$, $x=1{\cdot}2$, &c., brings us to $\mu=2{\cdot}557$ and $\left(\frac{\varrho}{\varrho_0}\right)^{\frac{2}{3}}=\cdot1591$, for $x=3{\cdot}0$, and finally gives us

$$x'=3{\cdot}656,\quad \mu'=2{\cdot}741,$$

and if we now assume the same height as before for the theoretic upper limit of the sun's atmosphere, instead of $\varrho_0=28{\cdot}16$, we find

$$\varrho_0=7{\cdot}11.$$

The new curve of density is found in the same way as the first, and is presented to the eye in the diagram in comparison with it. In the upper part of both curves the scale of density is increased ten fold, and it is, in part only, evident to the eye how immensely different, for the two values of k, becomes the density in the upper parts of the sun's mass. It appears to the eye only in part because the ratio of the two densities multiplies itself rapidly in approaching the upper limit of the atmosphere.

The above was communicated in writing as here given, to the Academy at its late session.* The draft of the following, and a part of the details of its substance, have been prepared since.

Equation (20) gives in feet the square root of the mean square of velocity of translation of molecules $(8{\cdot}02\sqrt{\tfrac{3}{2}\sigma t})$. At the sun's center we find this would be 331 miles per second for the curve of density corresponding to $k=1\tfrac{2}{3}$, and 380 miles per second for the curve of density corresponding to $k=1{\cdot}4$.

In 1838 Pouillet, following the law of heat radiation given by Dulong and Petit, estimated the temperature of the radiating surface of the sun, from observations by himself of the quantity of heat it emits, at from 1461° C. to 1761° C. Herschel, from Pouillet's observations, and his own made at the Cape of Good Hope about the same time, adopts, after allowing one-third for the absorption of our atmosphere, forty feet as the thickness of ice that would be melted per minute at the sun's sur-

---

* I desire here to state that the formulæ which show the relation between the temperature, the pressure, the density, and the depth below the upper limit of the atmosphere, so far as they apply to the upper part of the sun's body, were independently pointed out by Prof. Peirce, in a very interesting paper which that distinguished physicist read before the Academy at the same session, and prior to the presentation of this paper. Also to recall a fact which I first learned from Prof. Peirce's mention of it to the Academy, viz. that Prof. Henry long ago threw out the idea of the atmospheric condition to which Prof. Thomson has more recently given the term convective equilibrium, viz., such that any portion of the air, on being conveyed into any new layer above or below, would find itself reduced, by its expansion or compression, to the temperature of the new layer.

face. The temperature of the radiating surface calculated from this datum by the formula of Dulong and Petit, and with the co-efficient of radiation found by Prof. W. Hopkins for sandstone, the smallest co-efficient he found, is 1550° C. or 2820° Fah. But then the solar radiation is many thousands of times greater than the greatest in Dulong and Petit's experiments, so that these calculations of the temperature of the sun's photosphere have little weight notwithstanding the simplicity and accuracy with which the formula represents the experiments from which it was derived. Nothing authorizes us to accept the formula as more than an empirical one. It seems desirable that experiments similar to those of Dulong and Petit should be made on the rate of cooling of intensely heated bodies, such as balls of platinum not too large. By placing the heated ball in the center of a hollow spherical jacket of water, either flowing or in an unchanged mass, the quantities of heat radiated in successive equal spaces of time will be determined, and the corresponding differences of temperature in the heated ball can at least be estimated with whatever probability we may rely on our knowledge of the specific heat of its material. At present the best means we have of forming any judgment of the probable temperature of the source of the sun's radiation, is perhaps to be found in a comparison between the effects of the hydro-oxygen blowpipe, and the recorded effects of Parker's great burning lens. I am not aware that this method has before been resorted to.

If the angle of aperture at the focus of a burning lens, or combination of lenses, be called $2a$, the radiation received by a small flat surface at the focus will be $\sin^2 a$, if a unit be taken to represent the radiation the same small flat surface would receive just at the sun's surface. Parker's lens, with the small lens added, had, at the focus so formed, an angle of aperture of about 47°. A small flat surface at its focus would therefore receive about one-sixth the radiation that it would just at the sun, making no allowance for absorption by the atmospheres of the earth and sun and rays lost in transmission through the lenses. Pouillet, from the experiments already alluded to made by himself, found his atmosphere in fine weather transmitted, of the sun's heat rays, about the fraction $\frac{4}{5}$ raised to a power whose exponent is the secant of the sun's zenith distance. This, of course, leaves out of view the heat rays of low intensity which are totally absorbed by the atmosphere. He also concluded from comparison with other experiments of his own with a moderately large burning glass, that that glass transmitted $\frac{7}{8}$ of the heat rays incident on it. If we assume the same fraction for each of the two lenses of Parker's combination, and assume further that the sun's zenith distance did not exceed 48° in the experiments made with it, we find for the fractional multiplier expressing the part of the sun's heat radiation which arrived at the focus unintercepted, $(\frac{4}{5})^{1\ 1\cdot2}(\frac{7}{8})^2 = \cdot55$. Hence the radiation actually received by a small flat surface at the focus was ·09, or about one-eleventh, of what it would receive just at the sun. The heat so received by any body so placed in the focus, must, after the body has acquired its highest temperature, be emitted from it at the same rate. The heat so emitted will consist: first, of heat radiated into that part of space toward which the radiating surface of the body looks; secondly, of heat carried of by convection of the air; thirdly, of heat conducted away by the body supporting the body subjected to experiment; fourthly, of heat rays, if any, reflected, and not absorbed, by the body subjected to experiment. Assuming it as a reasonable conjecture that full half of all this* consists of heat *radiated* into the single *hemisphere* looking upon a flat surface, we may conclude that the body, at its highest acquired temperature, radiated not less than $\frac{1}{20}$th as much heat as is radiated by an equal extent of surface of the sun's photosphere, over and above such part of that radiation as may be intercepted by the sun's atmosphere, and such rays of low intensity as are *totally* absorbed by our own atmosphere, the whole of which apparently cannot be great. No allowance seems necessary for the chromatic and spherical dispersion of the lenses, since the diameter of the focus is stated at half an inch, while the true diameter of the sun's image would be not less than one-third of an inch.

Now we are not without the means of forming a probable approximate estimate of this temperature at which the radiation becomes $\frac{1}{20}$th, more or less, of that of the sun's photosphere. We are told that in the focus of Parker's compound lens 10 grains of very pure lime ("white rhomboidal spar") were melted in 60 seconds. We may presume that in that length of time the temperature of the lime, after parting with its carbonic acid, made a near approximation to the maximum at which it would be stationary, a presumption confirmed by the period of 75 seconds said to have been occupied in the fusion of 10 grains of carnelian, and by the considerable period of 45 seconds for the fusion of a topaz of only 3 grains, and 25 seconds for an oriental emerald of but 2 grains, and in fact sufficiently

---

* As to the heat carried off by convection of the air, if its quantity be calculated by the formula given by Dulong and Petit for that purpose, it comes out utterly insignificant in comparison with the heat received from the burning glass. The conjectural allowance of $\frac{4}{5}$ths in all, of this, is likely, therefore, to be much too large. Not much reliance, indeed, can be placed upon the formula here mentioned, at such a temperature as 4000° Fah., yet, as by it the convection is taken proportional to the 1·233 power of the difference of temperature, it seems unlikely that it gives a quantity very many fold less than the truth.

proved, it would seem, by observing that the heat we have estimated to fall at the focus, upon a flat surface, would suffice, if retained, to raise the temperature of a quarter of an inch thick of lime 4000° Fah. in 5 seconds. If, then, we may take the temperature *maintained* at the focus of Parker's lens to have been at the melting point of lime, we may conclude that it is also not far from the temperature given by the hydro-oxygen blowpipe. Dr. Hare, who was the first inventor of this instrument, and the discoverer of its great power, melted down, by its means, in partial fusion, a very small stick of lime cut on a lump of that material, which we understand to have been a very pure specimen. Burning glass and blowpipe seem each to have been near the limit of its power in this apparently common effect. But Deville found the temperature produced by the combination of hydrogen and oxygen under the atmospheric pressure to be 2500° Cent. As the lime in the heated blast would radiate rapidly, its temperature must have been lower than that of combined hydrogen and oxygen, and I have called it 2220° Cent. or 4000° Fah.

The formula of Dulong and Petit, with the co-efficient found by Hopkins, as already mentioned, gives for the quantity of heat radiated in one minute by a square foot of surface of a body whose temperature is $\theta+t$ centigrade, into a chamber whose temperature is θ centigrade, when expressed with the unit employed by Hopkins,

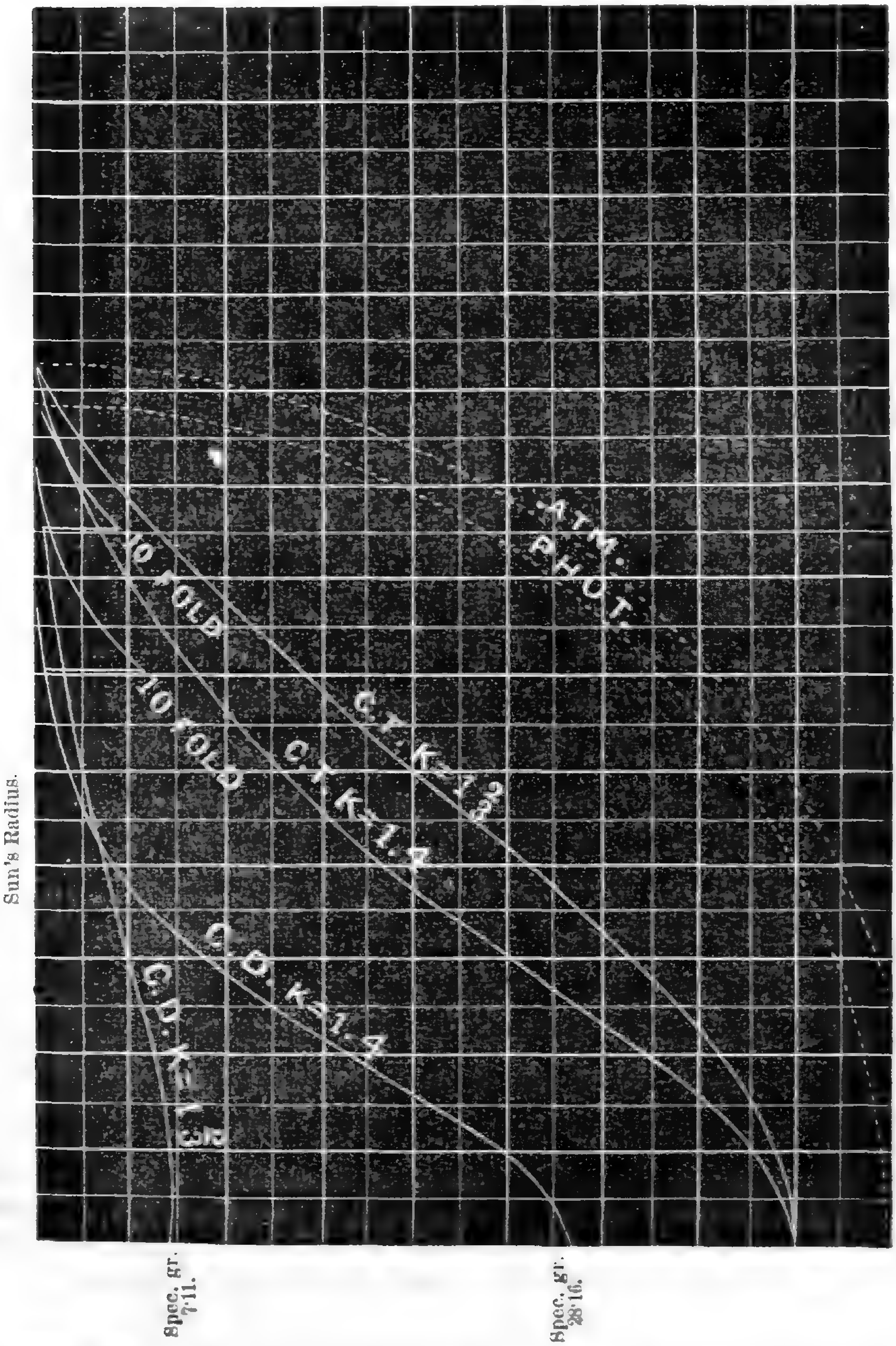
$$8{\cdot}377\,(1{\cdot}0077)^{\theta}\,[(1{\cdot}0077)^{t}-1].$$

It will be convenient, and, in the discussion of the high temperatures with which we are concerned, will involve no sensible error, to use the hypothesis that the space around the radiating body is at the temperature of 0° C. and the formula for the radiation then becomes,

$$8{\cdot}377\,[(1{\cdot}0077)^{t}-1]. \qquad (21)$$

The unit used by Hopkins, in the formula here given, is the quantity of heat that will raise the temperature of 1000 grains of water 1° centigrade. Expressed by the same unit, the quantity adopted by Sir J. Herschel as the amount of the sun's radiation, viz. that which would melt 40 feet thick of ice in a minute (at the sun's surface), is 1,280,000. The $\frac{1}{20}$th of this, or 64,000, expresses, therefore, the quantity which we have estimated the lime under Parker's lens to have radiated, per square foot of its surface, at its estimated temperature of 4000° Fah. If now we calculate its temperature by the above formula, from the estimated radiation, the result is 1166° Cent. or 2130° Fah. This is manifestly much below the real temperature, and so far below that there can be no doubt the formu!a of Dulong and Petit has failed at the melting point of lime. If instead of the co-efficient 8·377 we had used the larger co-efficient 12·808 which Hopkins gives for unpolished limestone, the formula would have been reduced only 53° Cent. It best suits the direction of our inquiry to use the smallest co-efficient which Hopkins' experiments gave, since we are seeking the *highest* temperature which can be plausibly deduced from the sun's radiation. For ease of expression, the curve which we will imagine for representing the actual relation of radiation to temperature, the horizontal ordinate standing for the temperature and the vertical ordinate for the radiation corresponding thereto, may be called the curve of radiation. The course of this curve from the freezing point of water to a point somewhat below the boiling point of mercury is correctly marked out to us by the formula. Beyond that we have but the rough approximation which we can get by means of the above comparison, to the single point of the curve where the radiation is $\frac{1}{20}$th that of the sun's photosphere. The attempt, from these data, to extend the curve till it reaches the full radiation of that photosphere, must be mainly conjectural. As a basis for the most plausible conjecture I am able to make let us assume: first, that the upward concavity of the curve of radiation, which increases very rapidly with the temperature as far as the curve follows the formula of Dulong and Petit, is at no temperature greater than that formula would give it at the same temperature; secondly, that the curve of radiation is nowhere convex upward. If, then, we set out from these two conjectural assumptions—of the degree of probability of which each one must form his own impression—the greatest temperature the sun's photosphere could have consistently with the radiation of 64,000 at the temperature of 4000° Fah., is found by drawing through the point representing that radiation and that temperature a straight line tangent to the curve of the formula. The line so drawn would cross the real curve of radiation in a greater or less angle at the radiation of 64,000 and temperature of 4000° Fah., and at higher temperatures would fall more or less *below* that curve, and its intersection with the sun's radiation of 1,280,000 would be at a temperature greater than that of the curve, that is to say, greater than the temperature of the sun's photosphere. This greater temperature is 55,450° Fah.

A different train of conjecture led me at first to assume a temperature of 54,000° Fah., and this last number I will here retain since it has been already used as the basis of some of the calculations we now proceed to give. It must be here recollected that we are discussing the question of *clouds* of *solid* or at least *fluid* particles floating in non-radiant gas, and constituting the sun's photosphere. If the amount of *radiation*



Explanation.—Atm., Assumed theoretic upper limit of atmosphere; Phot., Photosphere; C.T.K $=1\frac{2}{3}$. Arbitrary Curve of temperature for $k=1\frac{2}{3}$; C.T.K. $=1\cdot4$. Arbitrary Curve of temperature for $k=1\cdot4$; C.D.K. $=1\cdot4$, Absolute Curve of density for $k=1\cdot4$; C.D.K. $=1\frac{2}{3}$, Absolute density for $k=1\frac{2}{3}$.

would lead us to limit the temperature of such clouds of solids or fluids, so also it seems difficult to credit the *existence* in the solid or fluid form, at a higher temperature than 54,000° Fah. of any substance that we know of.

If then we suppose a temperature of 54,000° Fah., what would be the density of that layer of the hypothetic gaseous body which has that temperature, and what length of time would be required, at the observed rate of solar radiation, for the emission of all the heat that a foot thick of that layer would give out in cooling down under pressure to absolute zero? The latter question depends on the mechanical equivalent of this heat for a cubic foot of the layer of gas, and the two questions, together with that of the depth at which the layer would be situated below the theoretic upper limit of the atmosphere, are answered by equations (17), (18), and (19), provided we knew the value of k and the value of σ in the body of gas. The less the atomic weight of the gas the greater the value of σ, and the greater the density of the layer of 54000° Fah. and the greater the quantity of heat which a cubic foot of it would give out in cooling down. I therefore base the first calculation on hydrogen as it is known to us. The value of σ is in that case about 800 feet, and the value of k about 1·4, nearly the same as in common air. These values would give for the layer of 54000° Fah. a specific gravity about ·00000095 that of water, or about one 90th that of hydrogen gas at common temperature and pressure, and the mechanical equivalent of the heat that a cubic foot of the layer would give out in cooling down under pressure to absolute zero would be only about 9000 foot pounds, whereas the mechanical equivalent of the heat radiated by one square foot of the sun's surface in one minute is about 254,000,000 foot pounds. The heat emitted each minute would, therefore, be fully half of all that a layer ten miles thick would give out in cooling down to zero, and a circulation that would dispose of volumes of cooled atmosphere at such a rate seems inconceivable.

It may possibly appear to some minds that the difficulty presented by this aspect of the case will vanish if we suppose the photosphere, or its cloudy particles, to be maintained by radiation at a temperature to almost any extent lower than that of convective equilibrium. This would enable us to place the theater of operations in a lower and denser layer of atmosphere, but the supposition seems to me difficult to realize unless, as the hot gases rise from beneath, precipitation could commence at a temperature many times higher than the 54000° Fah. which we have estimated for the upper visible surface of the clouds, and this, as before intimated, seems to me itself extremely improbable.

I may mention here that my friend Dr. Craig, in an unpublished paper, following the hint thrown out by Frankland, is disposed to favor the idea that the sun's radiation may be the radiation of hot gases instead of clouds. At present I shall offer no opinion on that point one way or the other, but will only state it as my impression that if the theory of precipitated clouds, as above presented, is the true one, something quite unlike our present experimental knowledge, or at least much beyond it, is needed to make it intelligible.

The first hypothesis which offers itself in an attempt to make the theory rational is suggested by one point in Clausius' theory of the constitution of the gases, already alluded to. In forming his theory Clausius found that the known specific heats of the gases are all much too great for free simple atoms impinging on one another, and he therefore introduced the hypothesis of compound molecules, each compound molecule being a system of atoms oscillating among each other under forces of mutual attraction. Now if this were accepted as the actual constitution of the gases it is of course easy enough to conceive that in the fierce collisions of these compound molecules with each other at the temperatures supposed to exist in the sun's body, their component atoms might be torn asunder, and might thenceforth move as free simple molecules. In this case, still retaining the hypothesis of Clausius' theory, that the average length of the path described by each between collisions is large compared with the diameter of the sphere of effective attraction or repulsion of atom for atom, the value of k would reach its maximum of $1\frac{2}{3}$. Experiment has not shown us any gas in this condition, and for the present it is hypothetical. Even in hydrogen the value of k does not materially, if any, exceed the value of 1·4 which it has in air. But if it were found that the hydrogen molecule is compound, and that in the body of the sun the heat splits this molecule into two equal simple atoms, and in fact that all the matter in the sun's body is split into simple free atoms equally as small, then, while the value of k would be $1\frac{2}{3}$, the value of σ would be about 1600 feet. If with these values we repeat the calculation of the density of the layer of 54000° Fah. we find its specific gravity to be 0·000363 of that of water, or 4·35 times that of hydrogen gas at common temperature and pressure and in its known condition, or 8·7 times that which the hydrogen in the hypothetic condition would have if it retained that condition at common temperature and pressure. We find also that the mechanical equivalent of all the heat that a cubic foot of the layer would give out in cooling down, under pressure, to zero, would be no less than 13,500,000 foot pounds. Instead, therefore, of a layer ten miles thick, it would now require only a thickness of 38 feet to give out, in cooling down to zero, twice the heat emitted by the sun in one minute. It will be seen, (equations (17) and (19)), that this thickness, retaining the constant value $k=1\frac{2}{3}$, would diminish with the $2\frac{1}{2}$ power of the masses of the atoms into which the sun's body is hypothetically resolved (the reciprocal of the value of σ), and I leave each to form his own impression how far this view leads towards verisimilitude.

It is important to add that the depth of the layer of 54000° Fah. below the theoretic upper limit of atmosphere, when calculated with value $k=1\cdot4$, $\sigma=800$ feet, comes out only 1107 miles, and with the values $k=1\frac{2}{3}$ and $\sigma=1600$ feet only 1581 miles. This calculation of the depth, unlike the other results above, may be said to be independent of the question of the constitution of the sun's interior mass. It is alike difficult, on any plausible hypothesis, to reconcile a temperature no higher than 54000° Fah. with any perceptible atmosphere extending many thousand miles above, and yet no less an authority than Prof. Peirce has assigned a hundred thousand miles as the height of the solar atmosphere above the photosphere, at the same time, however, pointing out the enormous temperature which, under convective equilibrium, this would imply at the level of the photosphere. But all are not yet agreed that the appearances seen at such distances from the sun are proof of the existence of a true atmosphere there. It will be seen that the numbers I give above were obtained from a first hypothesis of an atmospheric limit 20,000 miles above the photosphere, but for the purpose of this paper it is of no consequence to repeat the calculation from a different limit.

It is, I believe, recognized on theoretical grounds that in an atmosphere containing a mixture of gases of unequal density the lighter gases might be expected to diffuse in greater proportion into the higher parts of the atmosphere and the heavier gases into the lower parts. But perhaps the supposed circulation which the emission of heat maintains within the photosphere must renew mixture at a rate sufficient to mask the rate which theory would assign for diffusion. I have not attempted a theoretic comparison between these two tendences. It will suffice here to repeat that the above numerical results, so far as they may be thought to give countenance to the theory in its mechanical aspect, require that the entire inner mass of the sun shall have, at a mean, (in the supposed state of dissociation), the very small atomic weight specified. We may notice in this connection the uniform proportion of oxygen and nitrogen gases in our atmosphere at the height of four miles or more at which the analysis has been made. Without having gone into a critical examination of the question, I suppose that at that height the proportion of oxygen which the theory of diffusive equilibrium would assign is notably diminished, and that it would be found that the circulation of the air is sufficiently active to mask the theoretic rate of diffusion.

The second hypothesis which might offer itself in an attempt to make the theory rational, but which a very little reflection is, I think, sufficient to set aside, is that which would modify Clausius' theory of the gases by assuming that in the sun's body the average length of the excursion made by each molecule between two consecutive collisions, becomes very short compared with the radius of the sphere of repulsion of molecule for molecule, and with the average distance of their centers at nearest approach. This way of harmonizing the actual volume of the sun with such a temperature as 54000° Fah. in the photosphere, and with the smallest density which we can credit in the photosphere, would involve the consequence that the existing density of almost the entire mass of the sun is very nearly uniform and at its maximum possible, or at all events that any further sensible amount of collapse must be productive of but a very small amount, comparatively, of renewed supplies of heat, for the obvious reason that this hyphothesis carries with it almost the entire neutralization of the force of gravity by the forces of molecular repulsion. In like manner it involves the consequence that in any such small contraction of the photosphere as can have taken place within the history of total eclipses, it is but a very small fraction of the sun's mass, near its surface, that can have taken part in the collapse to any thing like a proportionate extent. Hence it also extremely restricts the period during which we could suppose the sun to have existed under anything like its present visible magnitude in the past, consistently with the production in the way supposed of the supplies of heat it has been sending out. Another thing involved in this second hypothesis is the fact which Prof. Peirce has pointed out to the Academy, viz.: that the existing molecular repulsion in the sun's body would immensely exceed such as would be indicated by the modulus of elasticity of any form of matter known to us.

In conclusion, I do not mean to say that there is any *invincible logical exclusion* of any law of the action of gases different from what is specified or alluded to above. I only mean that, so far as I can see, any theory of heat which is based *simply* and *solely* upon *molecular attraction* and *repulsion* dependent on molecular distance alone, cannot in its application to the sun, escape from the conditions indicated in this paper. It is certainly not absurd to imagine heat to be an agent of some kind so constituted that it cannot be thus represented by the sole conditions of motion and of molecular attraction and repulsion, but yet so constituted that in its effects upon matter it follows the conditions of mechanical equivalency as defined by Joule. In fact, such exceptional cases as the expansion of water in freezing seem to favor such a view, though the range of that phenomenon is very limited. One way of forming a mechanical representation of such a constitution would be by associating molecular motion with the mechanical powers, either with or without molecular attraction or repulsion; the manner in which the imagined mechanical power (or link) attaches itself to the molecules which it connects—so as to make their motion determine their mutual approach or recession or change of relative direction—being dependent on the existing motions and other conditions in such a way as to produce the observed phenomena. The possibility of such a mechanical *representation* is sufficient to show that such a supposed *constitution* is not *logically excluded*, but to accept such a mechanical representation as a physical fact is quite another matter, and, as it seems to me, a very difficult one. Of course this difficulty does not present itself when we suppose that heat is not motion.

ART. X.—*On the mode of observing the coming Transits of Venus;* by SIMON NEWCOMB.*

Transits of Venus over the disk of the sun have more than any other celestial phenomena occupied the attention and called forth the energies of the astronomical world. In the last century they furnished the only means known of learning the distance of the sun with an approach to accuracy, and were therefore looked for with an interest corresponding to the importance of this element. Although other methods of arriving at this knowledge with about equal accuracy are now known, the rarity of the phenomenon in question insures for it an amount of attention which no other system of observation can command. As the rival method, that of observations of Mars at favorable times, requires, equally with this, the general coöperation of astronomers, the power of securing this coöperation does in itself give the transits of Venus an advantage they would not otherwise possess.

Although the next transit does not occur for four years, the preliminary arrangements for its observation are already being made by the governments and scientific organizations of Europe. It is not likely that our government will be backward in furnishing the means to enable its astronomers to take part in this work. The principal dangers are, I apprehened, those of setting out with insufficient preparation, with unmatured plans

---

* Read before the National Academy of Sciences, April 13, 1870.

of observation, and without a good system of coöperation among the several parties. For this reason I beg leave to call the attention of the Academy to a discussion of the measures by which we may hope for an accurate result.

In planning determinations of the solar parallax from the transits of Venus, it has until lately been the custom to depend entirely upon observations of the internal contact of the limbs of the sun and planet, as proposed by Halley. It is a little remarkable that while astronomical observations in general have attained a degree of accuracy wholly unthought of in the time of Halley, this particular observation has never been made with a precision at all approaching that which Halley believed that he himself had actually attained. In his paper he states that he was sure of the time of the internal contact of Mercury and and the sun within a second.* The latest observations of a transit of Mercury, made in November, 1868, are, as we shall presently see, uncertain by several seconds. It is also well known that the observations of the last transit of Venus, that of June, 1769, failed to fix the solar parallax with the certainty which was looked for, the result of the standard discussion being now known to be erroneous by one-thirtieth of its entire amount.

The discrepancies which have always been found in the class of observations referred to, when the results of different observers have been compared, have been generally attributed to the effect of irradiation. The phenomenon of irradiation presents itself in this form: When we view a bright body, projected upon a dark ground, the apparent contour of the bright body projects beyond its actual contour. It may be generalized as follows:—A lucid point, however viewed, presents itself to the sense, not as a mathematical point, but as a disc of appreciable extent, and, usually, of irregular outline. But, for our present purposes the form of the disc may be considered circular. Its outline is necessarily quite indefinite, and its magnitude increases with the brilliancy of the point. A bright body, being composed of an infinity of lucid points, its apparent enlargement is an evident result of this law.

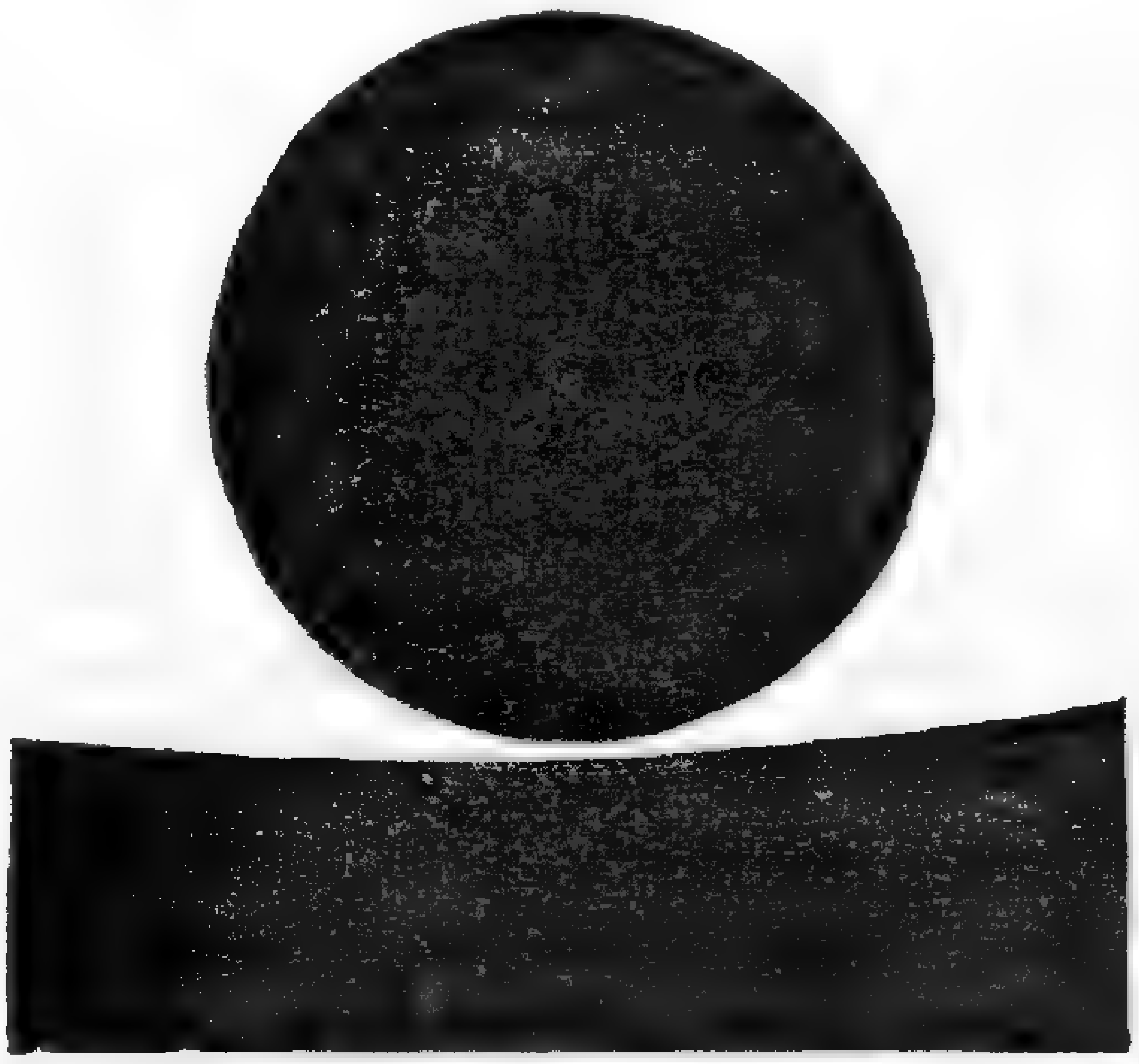
The following diagrams show the effect of this law upon the time of internal contact of a planet with the disk of the sun. The planet being supposed to approach the solar disk, figure 1 shows the geometrical form of a portion of the apparent surface of the sun, or the phenomenon as it would be if there were no irradiation immediately before the moment of internal contact. Figure 2 shows the corresponding appearance immediately after the contact. To indicate the effect of irradiation, or to show the phenomenon as it will actually appear on the theory of irra-

---

* Philosophical Transactions, No. 348, p. 454.

diation, we have only to draw an infinity of minute circles, one from each point of the sun's disk visible around the planet.

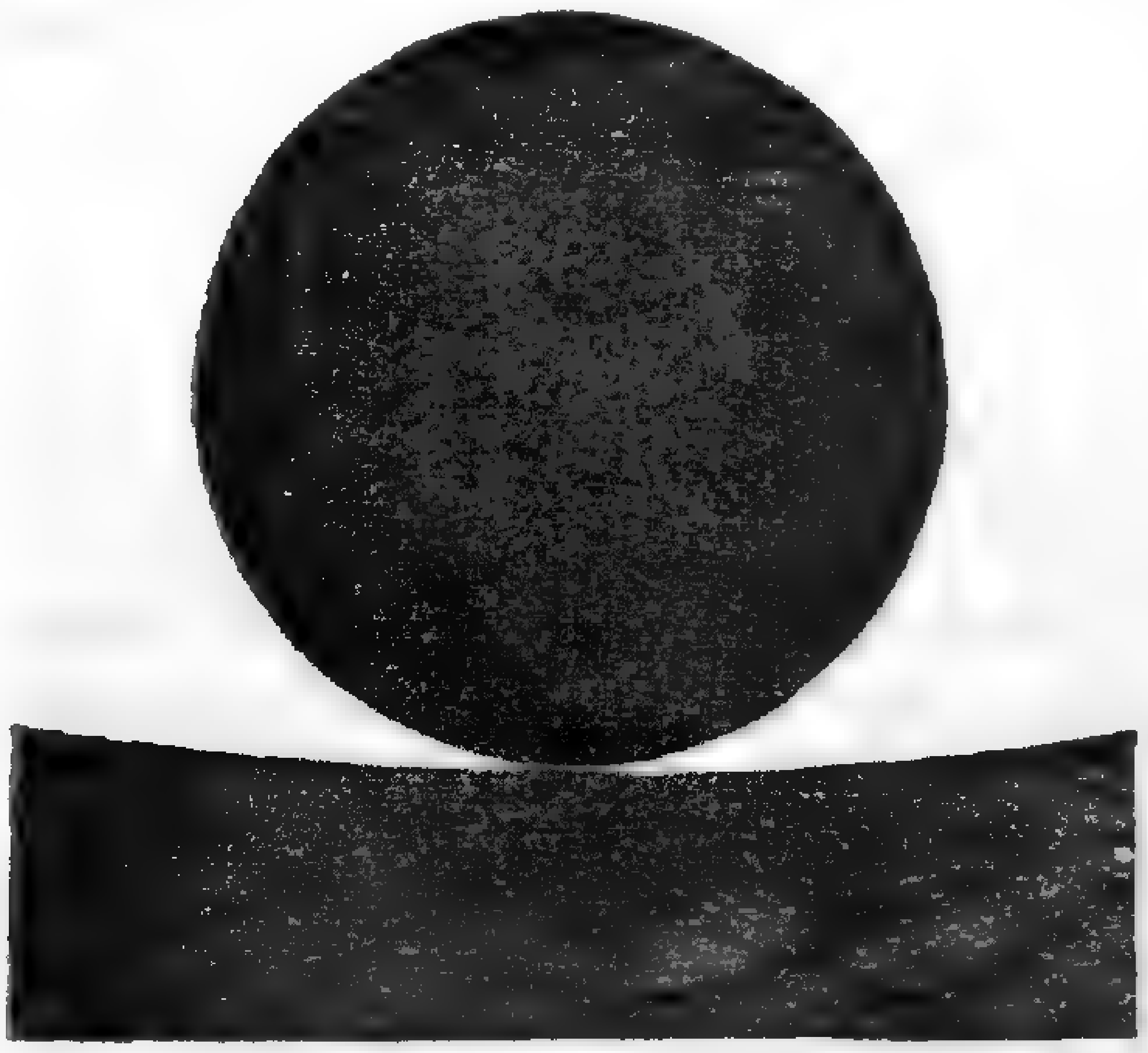
1.



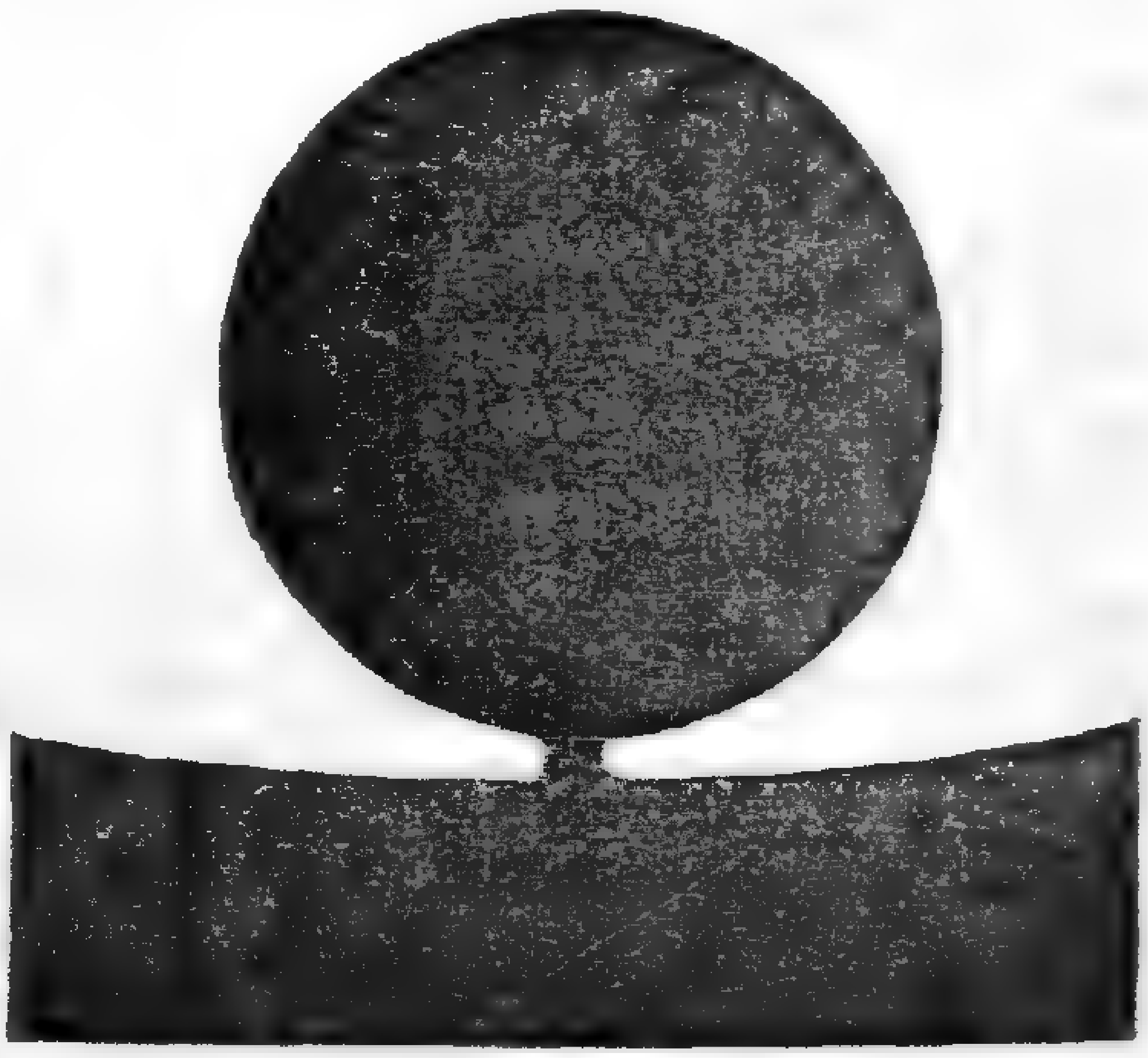
1*a*.

The effect of this is shown in the figures 1*a* and 2*a*. The exceedingly thin thread shown in figure 1 is thus thickened as in 1*a*, and the sharp cusps of figure 2 are rounded off as shown in figure 2*a*. The apparent radius of the planet is diminished by an amount equal to the radius of the circle of irradiation, and the radius of the sun is increased by the same amount. Comparing figures 1*a* and 2*a*, it will be seen that the moment of internal contact is marked by the apparent formation of a ligament, or "black drop," between the limbs of the sun and the planet. This formation is of so marked a character that it

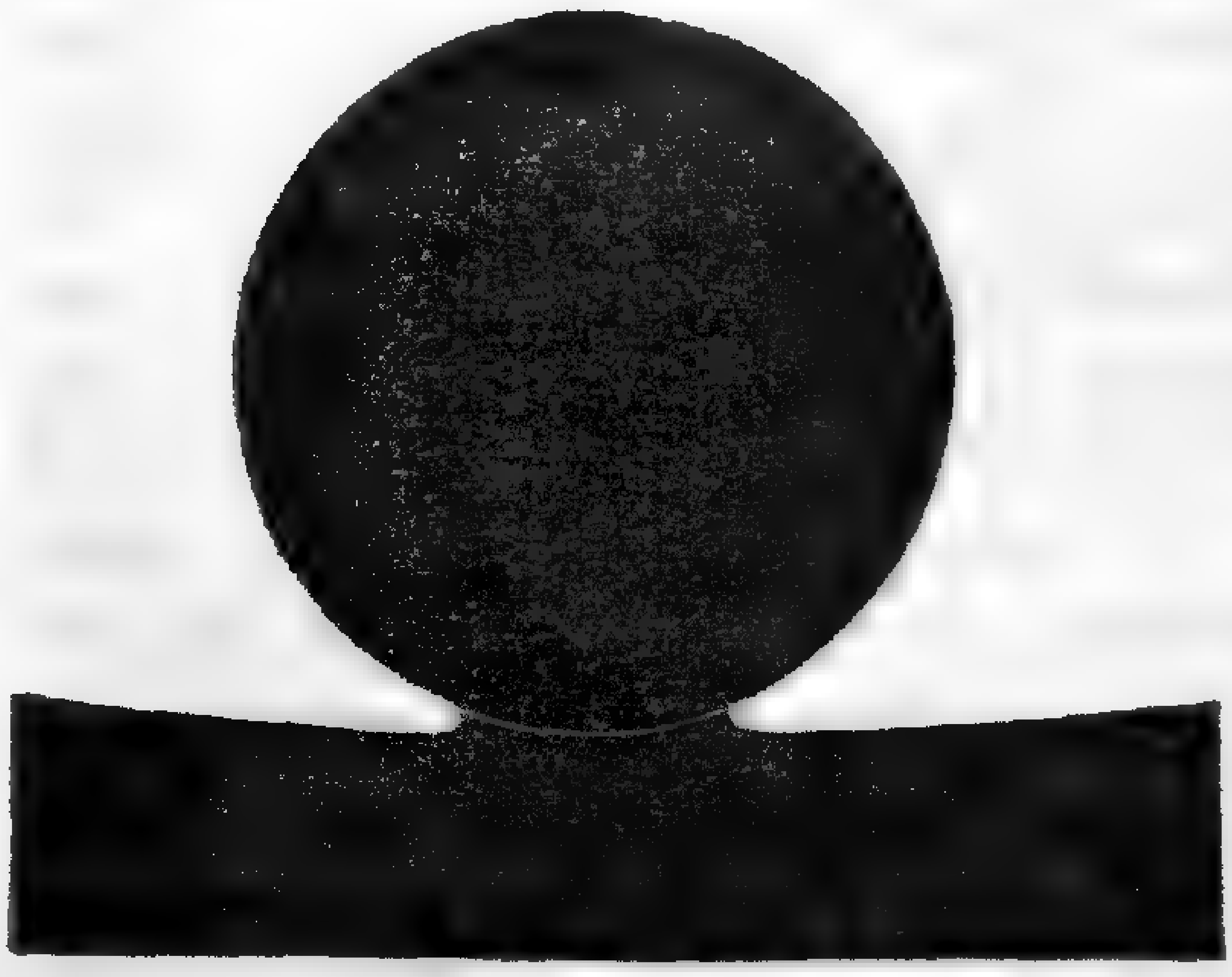
2.



2*a*.



3.



has been generally supposed there could be little doubt of the moment of its occurrence. The remarks of the observers have given color to this supposition, the black drop being generally described as appearing suddenly at a definite moment.

Examining figure 2*a*, it will be seen that the planet still appears entirely within the disk of the sun. The geometrical circle which bounds the latter, and that which bounds the planet, instead of touching, are separated by an amount equal to double the irradiation. If the observer waits until the planet has moved over this space, the two circles will appear in that position in which they would touch if completed. This phase commonly called "apparent contact," is shown in fig. 3. Neither circle is visible at the point of tangency, the black drop, now greatly widened and flattened, extending on both sides this point. The estimate of the moment of contact must therefore be very rough, the means of estimating being far less accurate than those afforded by a common filar micrometer. In the actual case the eye has to continue the two circles to the point of contact by estimation through a distance depending on the amount of irradiation, while measures with a micrometer are made by actual contact of a wire with a disk. Such estimates have, therefore, been generally rejected by investigators, not only from their necessary inaccuracy, but because the time of "apparent contact" depends upon the amount of irradiation, which varies with the observer and the telescope. If there is no irradiation at all, the time of apparent contact and that of true contact will be the same, as shown in figure 2; while, when the cusps are enlarged by irradiation, apparent contact will not occur until the planet has moved through a space equal to double the irradiation.

Let us return to the phenomena at actual contact. According to the theory as it has been presented, the formation or rupture of the black ligament connecting the dark body of Venus with the dark ground of the sky is a well-marked phenomenon, occurring at the moment of true internal contact. This was, I believe, the received theory until Wolf and André made their experiments on artificial transits in the autumn and winter of 1868 and 1869. They announced, as a result of these experiments, that the formation of the ligament was not cotemporaneous with the occurence of internal contact, but followed it at the ingress of the planet, and preceded it at egress. In other words, it appeared while the thread of light was still complete. They furthermore announced that with a good telescope the ligament did not appear at all, but the thread of light between Venus and the dark sky broke off by becoming indefinitely thin.

The result is not difficult to account for. Irradiation has already been described as a spreading of the light emitted from each point of the surface viewed, so that every such point appears as a small circle. The obvious effect of this spreading is a dilution of the light emitted by a luminous thread or point whenever the diameter of the thread is less than that of the circle of irradiation. In consequence of this dilution the thread may be invisible while it is really of sensible thickness, a given amount of light producing a greater effect on the eye the more it is concentrated. Since the thread of light must seem to break when it becomes invisible at its thinnest point, the formation or rupture of the thread marks, not the moment of actual contact, but the moment at which the thread of light becomes so thick as to be visible, or so thin as to be invisible. The greater the irradiation, and the worse the definition, the thicker will be the thread at this moment.

An interesting observation illustrative of this point was made by Liais at Rio Janeiro during the transit of Mercury of November 1, 1868. He had two telescopes, one much smaller than the other. He watched the planet in the small one till it seemed to touch the disk of the sun. Then looking into the large one he saw a thread of light distinctly between the planet and the sun, and they did not really touch until several seconds later.*

Reference to the figures will make it clear that there is no generic difference between the phenomenon commonly called the rupture of the black drop and that of the formation of the thread of light. If the bright cusps are much rounded, as in figure 2*a*, the appearance between them is necessarily that of a drop, while if they are seen in their true sharpness, as in figure 1, the form of the drop will not appear. It has been shown that with different instruments the phenomenon of contact may exhibit every gradation between these extremes. The only well defined phenomenon which all can see is the meeting of the bright cusps and the consequent formation of the thread of light at ingress and the rupture of the thread at egress.

To recapitulate our conclusions—

1. The moment of observed internal contact at ingress is that at which the thread of light between Venus and the sun becomes thick enough to be visible.
2. The least visible thickness varies with the observer and the instrument, and, perhaps, with the state of the atmosphere.
3. The apparent initial thickness of the thread varies with the irradiation of the telescope.

Two questions are now to be discussed. The observed times varying with the observer and the instrument, we must know how wide the variation may be. If it be wide enough to ren-

---

* Astronomische Nachrichten, B. 73, S. 209.

der uncertain the results of observation, we shall inquire how its injurious effects may be obviated.

The first question can be decided only by comparison of the observations of different observers upon one and the same phenomenon. For such comparison I shall select the observations of the egress of Mercury on the occasion of its last transit over the disk of the sun. This selection is made for the reason that this egress was observed by a great number of experienced observers with the best instruments, while former transits, whether of Venus or Mercury, have been observed less extensively or at a time when practical astronomy was far from its present state of perfection, and that the transit in question would therefore furnish much better data of judging what we might expect in future observations. The comparison was made in the following way: I selected from the "Astronomische Nachrichten," the "Monthly Notices of the Royal Astronomical Society," and the "Comptes Rendus" all the observations of internal contact at egress which there was reason to believe related to the breaking of the thread of light, and which were made at stations of known longitude. Each observation was then reduced to Greenwich time and to the center of the earth.

The results are exhibited in the following table. The third column of the table shows the corrected reduced times of contact. Next is given the aperture of the telescope, and the magnifying power employed in the observation. Lastly, we present a classification of the phenomena of contact as described by the observers.

The letters V. C., (vanishing contact,) indicate that the thread of light broke by becoming indefinitely thin, so that there was no irradiation or distortion of the planet.

The letters B. D. (black drop) indicate the appearance of the ligament, as shown in fig. 2*a*.

R. R. Indicates that the time is that of the rupture of the thread, but that it cannot be inferred from the statements of the observer whether the thread had or had not a sensible thickness immediately before breaking.

A. C. Signifies an apparent contact, as shown in fig. 3. When there is no black drop the letters V. C. and A. C. have the same meaning. The letters are therefore employed only when there is reason to believe from the remarks of the observer that the cusps were rounded.

(Inst.) indicates that the observer described the phenomenon as instantaneous.

The letters *b* and *g* refer to the definition, the first indicating that it was bad, the second that it was good.

When there is no indication in this column the observer gives no information respecting the phenomenon.

Observer.	Place.	Reduced Time of Int. Contact.			Aperture.	Power.	Character of Phenomenon.
		h.	m.	s.			
Oppolzer,*	Vienna,	20	59	49·3	4	--	B. D. b.
Oppenheim,	Bonn,			51·8	--	--	b.
Rayet,	Paris,			57·0	5·5	222	V. C.
Le Verrier,†	Marseilles,			57·6	--	--	B. D. (Inst.) g.
Lynn,	Greenwich,			59·2	?	170	B. D.
P. J. Kaiser,	Leiden,			59·2	3	--	R. R. (?) b.
Wolff,	Bonn.,	21	0	1·3	--	--	b.
Liais,‡	Atalaia,			1·5	--	78	V. C. (g.)
Rosen,	Pulkowa,			2·7	5	59	
Stone,§	Greenwich,			4·0	12¼	245	B. D.
Lais,	Rome,			4·6	2	40	R. R.
Kartazzi,	Pulkowa,			4·7	2·9	145	
Wagner,	"			4·7	3·7	148	
Nyren,	"			4·7	7·4	57	
Fuss,	"			4·7	2·4	63	
Dunkin,‖	Greenwich,			4·7	3·7	300	B. D.
André,	Paris,			4·9	5	188	V. C.
Argelander,	Bonn,			5·3	--	--	b.
Oppolzer,	Vienna,			6·3	4	--	A. C.
Dr. Peters,	Altona,			7·3	4	111	b.
Villarceau,	Paris,			8·3	7·3	163	V. C.
Lebedeff,	Pulkowa,			8·7	2·2	70	
Dollen,	"			8·7	2·2	81	
Miroschnit schenko,	"			8·7	3·7	106	
Leskinen,	"			8·7	3·9	117	
Mancini,¶	Rome,			9·5	6	60	R. R.
H. J. Carpenter,	Greenwich,			9·6	?	70	R. R.
Kasavinoff,	Pulkowa,			10·7	1·1	36	
Kaiser,	Leiden,			10·8	7		B. D. b.
Criswick,**	Greenwich,			11·3	4?	170	B. D.
Wolf,	Paris,			11·4	8	200	V. C.
Secchi,	Rome,			11·5	7	200	R. R.
J. Carpenter,	Greenwich,			12·6	3·7	90	B. D. (Inst.)
Prof. Peters,	Altona,			13·3	3	145	b.
Weiss,	Vienna,			13·3	5	40	b.
Struve,	Pulkowa,			13·7	2·5	207	R. R.
Stephan,	Marseilles,			13·7	--	--	R. R. (Inst.)
Merino,	Madrid,			14·1	5	100	R. R. g.
Duner,	Lund,			14·2	9	320	V. C. g.
Stone,	Greenwich,			16·0	--	--	A. C.
Buckingham,	Walworth,			17·3	--	--	B. D.
Sokolov,	Pulkowa,			18·7	2·8	226	
Kam,	Leiden,			19·0	6	--	R. R. (?) b.

* "8^s vor dem Brechen des Lichtfadens schien Mercur gleichsam den Sonnenrand etwas hinauszuchieben so dass an der Stelle des Austrittes ein nach aussen convexer Lichtfaden den Mercur umschloss und dessen Verschwinden ich als den Moment der inneren Berührung notirte."

† Le Verrier used a seven-inch glass with a diaphragm over the objective.

‡ Three seconds before this time the contact was not completed, and three seconds later it was more than completed.

§ A fine ligament seen several seconds before the time recorded.

‖ The planet was suddenly noticed to assume a balloon or pear shape. The apparent contact about 10^s later.

¶ Observed by image in camera obscura, 22 c. m. in diameter.

** Instantaneous. Time perhaps 2^s or 3^s late.

TABLE—continued.

Observer.	Place.	Reduced Time of Int. Contact.	Aper-ture.	Power.	Character of Phenomenon.
		h. m. s.			
Ventosa,*	Madrid,	21·0	1·0	150	B. D. (Inst.)
Kaiser,	Leiden,	23·8	7	--	A. C. b.
Lindeman,	Pulkowa,	24·7	4·2	85	
Prince,	Uckfield,	28·5	--	--	A. C. (?)
Pohl,	Kahlenberg,	29·6	4	135	V. C.
Lynn,†	Greenwich,	31·1	?	170	A. C.
Penrose,	Wimbledon,	52·2	2·2	70	R. R.

In the preparation of this table, the original intention was to exclude all observations unaccompanied with any statement of the phenomena. An exception was afterward made in favor of the observations of Bonn and Pulkowa, so that in this respect there is a lack of homogeneousness in the table. The few observations of "apparent contact," have also been added, in the belief that they would not be devoid of interest.

The principal conclusion, to be drawn from the comparison here exhibited, is that there is no discoverable relation between the time of observation on the one hand, and the size of the telescope, the magnifying power, (so it exceed 50 or 60) or the character of the phenomenon on the other. We find the phenomenon of the apparently instantaneous formation of the "black drop" to range from $20^h\ 59^m\ 58^s$ (Le Verrier) to $21^h\ 0^m\ 21^{\cdot s}0$ (Ventosa). The times of "Vanishing Contact" on which Wolf and André lay so much stress, range from $20^h\ 59^m\ 57^s$ to $21^h\ 0^m\ 29^s$. If we reject Pohl's observation, the range will still be 17^s. I think if the observations of external contact were collected and compared in the same way, it would be found that their agreement was as good as in the case of internal contact. So far as we have data for judging, these differences would seem to be due to the accidental errors of observation. Their amount in arc may be inferred from the fact that 15^s of time correspond to a change of 1″ in the relative position of Mercury and the sun.

I conceive, therefore, that we shall fail if we rely mainly on observations of internal contact. Still, there are two measures by which the reliableness of the determination of ingress and egress may be greatly increased. The first consists in having the observer occupy the entire time of partial ingress and egress in making very careful measures of the distance of cusps with such micrometer as may be best adapted for the purpose. The second consists in bringing the observers at opposite stations together, both before and after the transit, and causing them to make observations on artificial transits with the same instru-

---

* Contact seemed to be established simultaneously at several points.

† Doubtful. Limbs "certainly in contact by at least that time."

ments employed in observing the transit of Venus, in order to determine what correction should be applied to the observations of one to make them comparable with those of the other. It would be a comparatively simple operation to erect an artificial representation of the sun's disk at the distance of a few hundred yards, and to have an artificial planet moved over it by clockwork. The actual time of contact could be determined by electricity, and the relative positions of the planet and the disk by actual measurement. With this apparatus it would be easy to determine the personal errors to which each observer was liable, and these errors would approximately represent those of the observations of actual transit.

Still, it would be very unsafe to trust entirely to any determination of ingress or egress. Understanding the uncertainty of such determinations, the German astronomers have proposed to trust to measures with a heliometer, made while the planet is crossing the disk. The use of a sufficient number of heliometers would be both difficult and expensive, and I think we have an entirely satisfactory substitute in photography. Indeed, Mr. De la Rue has proposed to determine the moment of internal contact by photography. But the result would be subject to the same uncertainty which affects optical observations—the photograph which first shows contact will not be that taken when the thread of light between Venus and the sun's disk was first completed, but the first taken after it became thick enough to affect the plate, and this thickness is more variable and uncertain than the thickness necessary to affect the eye. We know very well that a haziness of the sky which very slightly diminishes the apparent brilliancy of the sun, will very materially cut off the actinic rays, and the photographic plate has not the power of adjustment which the eye has.

But, although we cannot determine contacts by photography, I conceive that we may thereby be able to measure the distance of the centers of Venus and the sun with great accuracy. Having a photograph of the sun with Venus on its disk, we can, with a suitable micrometer, fix the position of the center of each body with great precision. We can then measure the distance of the centers in inches with corresponding precision. All we then want is the value in arc of an inch on the photograph plate. This determination is not without difficulty. It will not do to trust the measured diameters of the images of the sun, because they are affected by irradiation, just as the optical image is. If the plates were nearly of the same size, and the ratio of the diameters of Venus and the sun the same in both plates, it would be safe to assume that they were equally affected by irradiation. But should any difference show itself, it would not be safe to assume that the light of the sun encroached equally upon the dark ground of Venus and upon the sky, because it is so much fainter near the border.

If the photographic telescope were furnished with clock-work, it would be advisable to take several photographs of the Pleiades, both before and after the transit, to furnish an accurate standard of comparison free from the danger of systematic error. There is little doubt that if the telescopes and operators practice together, either before or after the transit, data may be obtained for a satisfactory solution of the problem in question.

To attain the object of the present paper, it is not necessary to enter into details respecting choice of stations and plans of observation. I have endeavored to show that no valuable result is to be expected from hastily-organized and hurriedly-equipped expeditions; that every step in planning the observations requires careful consideration, and that in all the preparatory arrangements we should make haste very slowly. I make this presentation with the hope that the Academy will take such action in the matter as may seem proper and desirable.

ART. XI.—*On the Geology of Eastern New England*; by Dr. T. STERRY HUNT, F.R.S. (From a letter to Prof. JAMES D. DANA.

WHEN, more than twenty years since, my attention was turned to the geology of New England, there was no evidence of the existence between the old gneisses of the Adirondacks and the coal measures, of any other stratified rocks than those of the Huronian series, and the New York system, from the Potsdam formation, upward. It is true that Emmons had, before that time, maintained the presence, in western Vermont and Massachusetts, of a system of fossiliferous sediments, lying unconformably beneath the Potsdam, but the evidence up to this time adduced with regard to these so-called Taconic rocks, has failed to show that they include any strata more ancient than the Potsdam, while most of them are certainly younger. The researches of Sir William Logan, up to 1848, had led him to refer to a period not older than the Lower Silurian the crystalline sediments of the Appalachian region of Canada, between Lake Champlain and Quebec. These form a chain of hills, the continuation of the Green Mountains, and were found by him to be followed immediately, to the southeast, by more or less calcareous and somewhat altered strata, associated with Upper Silurian fossils, and succeeded, across the strike, near the sources of the Connecticut River, by a series, several miles in breadth, of micaceous schists and quartzose strata, occasionally containing chiastolite, garnet and hornblende. These two series of rocks, extending from the base of the Green Mountains to Canaan on the Connecticut, it was suggested by Sir William Logan, in his Report on the Geological Survey, 1847–1848, might be the altered representatives of the rocks of Gaspé, including the Lower Helderberg group, and the succeeding members of the New York system to the top of the Chemung. I then, as now, conceived that these micaceous and argillaceous schists, often holding garnets and chiastolite, were identical with those which make so conspicuous a figure in the White Mountains, and elsewhere in Eastern New England, and when, in 1849, I laid before the American Association at Cambridge, the results of the Geological Survey of Canada (this Jour., II, ix, 19), suggested that to the Gaspé series, as above defined, "may perhaps be referred, in part, the rocks of the White Mountains." Lesley, subsequently, in 1860 (Proc. Philad. Acad. Nat. Sciences, page 363), adduced many reasons for believing that the rocks of these mountains might be strata of Devonian age.* In the large geological map of Canada and the northern United States, lately published by Sir William Logan, no attempt is made to delineate the geology of New Hampshire, but the rocks in question, to the north of the United States boundary, are represented as Upper Silurian, with the exception of a belt of the Quebec group, which has been recognized in that region.

In fact, the schists and gneisses of the White Mountains are clearly distinct, lithologically, from the Laurentian, the Labradorian and the Huronian, as well as from the crystalline rocks of the Green Mountains, and from the fossiliferous Upper Silurian strata which lie at the southwestern base of the Canadian prolongation of the latter. Having thus exhausted the list of known sedimentary groups up to this horizon, it was evident that the crystalline strata of the White Mountains must be either (1) of Devonian age, or (2) something newer (which was highly improbable); or (3) must belong to a lower and hitherto unknown series. In the absence of any proof, at that time, of the existence of such a lower system, the first view, which referred these strata to the Devonian period, was the only one admissible.

---

* In this connection should be recalled the views put forth in 1846, by Messrs. H. D. and W. B. Rogers, in a paper on the Geological Age of the White Mountains, (this Journal, II, i, 411). They there, for the first time, pointed out that the great mass of these mountains consists of more or less altered sedimentary strata, which, upon the evidence of supposed organic remains. they referred. with some little doubt, to the Clinton division of the Upper Silurian. In 1847, however, they announced that the supposed fossils, on which this identification had been founded, were not really such, (this Journal, II, v, 116). Future explorers may, it is hoped, be more successful, and yet discover among the strata of the White Mountains evidences of organic life, probably of primordial Silurian age.

When, however, further investigation showed that the great and progressive thickening which takes place in the paleozoic formations from the west, eastward, is not confined to the augmentations of existing subdivisions, but includes the intercalation of new ones; when the few hundred feet of typical Potsdam sandstone in New York are represented in Vermont, Quebec and Newfoundland, by thousands of feet of strata lithologically very unlike the type; while the Quebec group, not less in volume, appears representing the beds of passage between the Calciferous and Chazy divisions of New York, we begin to conceive that conditions of sedimentation, very unlike anything hitherto suspected in the west, prevailed to the eastward. When, moreover, we find widely separated areas of Labradorian and Huronian rocks,—remaining fragments of great series,—resting upon the Laurentian, from Lake Huron to Newfoundland, we get evidences of a process of denudation in past ages, not less remarkable than the sedimentation.

My observations of last year have led me to a conclusion, which had previously been taking shape in my mind, that there exists above the Laurentian, a great series of crystalline schists, including mica-slates, staurolite and chiastolite-schists, with quartzose and hornblendic rocks, and some limestones, the whole associated with great masses of fine-grained gneisses, the so-called granites of many parts of New England. The first suggestions of this were given me by the observation of Dr. Bigsby, confirmed by specimens since received from the region, that there exists to the northwest of Lake Superior, an extended series of crystalline schists, unlike the Laurentian, and resembling those of the White Mountains. I have already called attention to this resemblance in a review of the progress of American Geology, in 1861 (this Jour., II, xxxi, 395). It was contrary to my notions of the geological history of the continent to suppose that rocks of Devonian age could, in that region, have assumed such lithological characters, and I was therefore led to compare these rocks with a great series of crystalline schists, abounding in mica-slates and micaceous limestones, which occupy considerable areas in the Laurentian region in Hastings county, to the north of Lake Ontario. The distribution of this series has been traced out by Mr. Vennor, who, in 1869, was able to show that, although much contorted, it rests unconformably upon the old Laurentian gneisses, while it is, at the same time, overlaid by the horizontal limestones of the Trenton group. This intermediate series, which attains a thickness of several thousand feet, is terminated by calcareo-micaceous schists, in which *Eozoon Canadense* has been found, both in Madoc and in Tudor. In these localities, as shown by Dawson and Carpenter (this Jour., II, xliv, 367), the calcareous skeleton of the Eozoon, instead of being injected by serpentine or another silicate, is simply filled with impure calcareous and carbonaceous matter. The presence of this fossil serves to connect these rocks with the Laurentian system, with which they had provisionally been classed, although their lithological dissimilarity had long been noticed, and in 1866 Sir William Logan had remarked their resemblance to the mica-slate series found near the sources of the Connecticut River (Report Geol. Survey, 1866, p. 93).

Mr. Alex. Murray's report of his explorations in Newfoundland, published in 1866, throws much light on the history of the rocks immediately succeeding the Laurentian in that region. He found in the great northern peninsula, about the Cloud Mountains and Canada Bay, not less than 5400 feet of strata, referred by him to the Potsdam group. Of these the lower 2500 feet consist of bluish-gray slates, holding near the summit, beds which become conglomerate from the presence of quartz pebbles, and are followed by a mass of purplish amygdaloidal diorite, holding epidote and jaspery red iron ore. Then follow 2000 feet of argillaceous and somewhat micaceous slates, with beds of quartzite and of limestone, generally impure. These contain, besides numerous fucoidal markings, the remains of a Lingula, and of *Olenellus Vermontanus*, a fossil characteristic of the Potsdam group. To this second division succeeds a third, consisting of about 900 feet additional of limestones and slates. Somewhat farther southward, at Great and Little Coney Arms, the lower half of the above series is not observed, but a succession of strata, supposed to represent the upper portion of the Potsdam, is more particularly described. It consists, at the base, of 300 feet of pale bluish-gray mica-slates, with iron stains, "softer, more finely laminated, and more uniform both in color and in texture" than some micaceous strata described by Mr. Murray as occurring in the Laurentian in that region. To these succeeded 430 feet of similar soft bluish-gray mica-slates, holding numerous thin seams of dark colored limestone, and followed by 1000 feet of impure limestones and slates, often micaceous and calcareous, among which are a few beds of white compact marble. No indications of fossils, save fucoidal markings, were met with in this section. At Coney-Arm Head there is seen a series of "whitish granitoid, very quartzose mica-slates," which appear to have a thickness of from 1500 to 2000 feet. The same rock is found in White Bay, where it overlies what is supposed to be Laurentian gneiss. The relations of these whitish granitic mica-slates are still obscure, but Mr. Murray was inclined to regard them as occupying a position beneath the Potsdam group. The latter, in Canada Bay, is immediately followed by the unaltered fossiliferous limestones and shales of the Quebec group. From these investigations of Mr. Murray we learn that between the Laurentian and the Quebec group, there exists a series of several thousand feet of strata, including soft bluish-grey mica-slates and micaceous limestones, belonging to the Potsdam group; besides a great mass of whitish granitoid mica-slates, whose relation to the Potsdam is still uncertain. To the whole of these we may perhaps give the provisional name of the Terranovan series, in allusion to the name Newfoundland.

Imperfect gneisses and micaceous schists are found in several parts of the province of New Brunswick, associated with what has been described as a great granitic belt. These rocks have been examined by Prof. Hind, and by Mr. Robb, on the St. John and Mirimichi rivers; and the former of these observers some years since pointed out the indigenous character of the so-called granites. In the summer of 1869 I had an opportunity of examining, with Prof. L. W. Bailey, the region about St. Stephen, on the river St. Croix, where he had already observed a series of ferruginous quartzites and imperfect gneisses, accompanied by soft bluish mica-slates sometimes holding chiastolite, staurolite, and garnet. These highly crystalline schists are not more than five miles removed from unaltered shales of the Gaspé series, containing fossils of Upper Silurian or Lower Devonian types, and rest unconformably upon older granitoid rocks, which Prof. Bailey regards as probably Laurentian. We subsequently examined the crystalline schists of the St. John, which are apparently identical with those of the St. Croix, and these also overlie, unconformably, an older granitoid gneiss.

More recently Prof. Hind has pointed out that some of the so-called granites of Nova Scotia are ancient gneisses, probably of Laurentian age, and have shown that between these and the gold-bearing slates of that province, there is found, near Windsor, and near Sherbrooke, a series of beds of no great thickness, consisting of imperfect gneisses, quartzites and micaceous schists, which rest unconformably on the Laurentian, and are sometimes wanting altogether. These include mica-schists with chiastolite and garnet, and appear identical with those already observed by Dr. Dawson in other parts of Nova Scotia, which I had already recognized as the same with those of the White Mountains, and those of the St. Croix, just noticed. Prof. Hind, in a late paper, has called these, from their position in Nova Scotia, Huronian; but the Cambrian or Huronian rocks recognized by Messrs. Matthew and Bailey in New Brunswick, where they are widely spread along the north side of the Bay of Fundy, consist of massive diorites and quartzose feldspar-porphyries, with occasional sandstones and conglomerates, and are very unlike the gneissic and micaceous rocks in question, which I believe to belong, like those of the St. Croix and the St. John rivers, to the great Terranovan series. The micaceous and hornblendic schists, with interstratified fine grained whitish gneisses (locally known as granites) which I have seen in Hallowell, Augusta, Brunswick and Westbrook, in Maine, appear to belong to the same series; which will also probably include much of the gneiss and mica-schist of Eastern New England. If this upper series is to be identified with the crystalline schists which, in Hastings County, Ontario, overlie, unconformably, the Laurentian, and yet contain *Eozoon Canadense*, the presence of this fossil can no longer serve to identify the Laurentian system. To this lower horizon however, I have referred a belt of gneissic rocks in Eastern Massachusetts, which are lithologically unlike the present series, and identical with the Laurentian of New York and Canada. To the upper series appear to belong the great endogenous granitic veins so well known to mineralogists as containing beryl, tourmaline and other fine crystallized minerals.

The fine-grained, white granitoid gneisses, often present an apparently bedded structure, which enables them to be removed in large plates or layers, lying at no great angle, and apparently conformable to the present surface of the country. This structure, which I conceive to have been superinduced by superficial changes of temperature, is often quite independent of the bedding, as may be seen in the quarries near Augusta in Maine, and in the cuttings on the Grand-Trunk Railway near Berlin Falls, New Hampshire. It is also observed in exotic or intrusive granites, like those of Biddeford, Maine. This is, in fact, the concentric lamination of granite, long since observed by Von Buch, and, I believe, correctly explained by Prof. N. S. Shaler to be due to movements of contraction and expansion in the mass, caused by variation of temperature during the changes of the seasons. He has not however observed this structure at greater depths than from three to five feet, while in some rocks I have found it penetrating probably twenty feet. (See Shaler's paper, read before the Boston Nat. History Society, Feb. 3, 1869, and published in the Proceedings of the Society, vol. xii, page 289).

While however I admit the existence in the Dominion of Canada and in Eastern New England, of a great series of crystalline schists, distinct from the Laurentian, and apparently the same with those found by Mr. Murray between the Laurentian and the Quebec group in Newfoundland, it is not less certain that we have in these regions rocks of Upper Silurian and Lower Devonian age, holding characteristic fossils. These strata in Maine and New Brunswick are generally but little altered. In the Connecticut valley at Bernardston, Massachusetts, near Lake Memphremagog in Vermont, and further northward in the province of Quebec, fossils of this horizon are found in rocks which, in some localities, are more or less altered and crystalline. I believe however that much of the calcareous mica-slate of Eastern Vermont will be found to belong to the Terranovan series. The extent of these newer rocks, and the limits between them and the more ancient schists, of the ruins of which they are probably in part composed, remain problems for farther investigation. For the solution of these, Prof. C. H. Hitchcock, by his labors in Vermont, is already well prepared, and it cannot be doubted that he, with his able assistants, will in the Survey of New Hampshire, now in progress, throw much light on New England geology. It is worthy of remark, that strata holding fossils of Lower Helderberg age, or thereabouts, are not confined to the shores of Maine and New Brunswick, and the valleys of the Connecticut and St. John rivers, but are found beyond the Green Mountains, in the valley of the St. Lawrence, near Montreal; where, on the island of St. Helen, they rest unconformably on the Utica slate, and at Belœil Mountain, near by, on intrusive diorites, which there break through the shales of the Hudson River group.

The relations of this Terranovan series to the porphyries and diorite rocks which, in New Brunswick, have been called Cambrian and Huronian by Mr. Matthew (first distinguished by him as the Coldbrook group), yet remains to be determined. These rocks are found near to the city of St. John resting directly on what has been regarded as Laurentian, and are overlaid by the uncrystalline schists which contain the primordial fauna now so well known by the descriptions of Prof. Hartt. Rocks which I regard as identical with this same Coldbrook or Cambrian group, are found along the coast of New Brunswick, and constitute the diorites and porphyries of Eastport, Maine. They appear moreover to be the same with those met with near Newburyport, and at Salem, Lynn, and Marblehead, Massachusetts. Farther researches about Passamaquoddy Bay, where the mica-slates are found not far removed from these porphyries, will probably enable us to determine their relations to each other.

It will be remembered that Gümbel has found, in Bavaria, beneath the oldest fossiliferous clay-slates, a mica-schist (and hornblende-schist) series, reposing upon the Hercynian gneiss, which contains crystalline limestones, with graphite, serpentine and *Eozoon Canadense*, and which he has identified with the Laurentian of North America. He distinguishes beneath this a great mass of red gneiss, apparently without limestones, to which he has given the name of the Bojian gneiss. It will however be remembered, that in his studies of the Laurentian system on the

Ottawa, Sir William Logan has shown that this immense series (his Lower Laurentian), some 20,000 feet in thickness, includes four great masses of gneiss and quartzite, divided by three limestone formations, and that it is in the uppermost of these, which is, in some parts, 1500 feet thick, that the *Eozoon Canadense* has been found. Some of the lower gneisses of this vast system may very well represent the Bojian of Gümbel, who has not recognized in Bavaria either the Labradorian (Upper Laurentian) or Huronian series. (See Gümbel on the Laurentian of Bavaria, translated and published in the Canadian Naturalist for December, 1866). Comparative studies of this kind should not be neglected in the investigation of our American rocks.

Montreal, May 10, 1870.

ART. XII.—*Mineralogical Contributions;* by CHARLES UPHAM SHEPARD, Sr.

1. *A new variety (species?) of Columbite.*

THE Columbites of New England deserve a closer examination than they have yet received. Those of New Hampshire, Massachusetts and Connecticut may embrace at least two, perhaps three, mineralogical species. A new locality, recently opened at Haddam (Conn.), has certainly disclosed a variety so different from the productions of the old one on the farm of Mr. Brainard, as to have led to doubts among collectors whether it fairly belongs to the Columbite group.

For my first knowledge of the new variety, I am indebted to several of my pupils who visited Haddam during the past year; and particularly to Mr. Charles H. Ames of the Senior class in Amherst College, who has furnished me with several dozen specimens, mostly isolated crystals, from one ounce in weight, downward, to that of a few grams. They all occur at a single repository, contiguous to the center of the village, and directly in rear of the house of Mr. Nathaniel Cook, the well known collector and dealer in Haddam minerals. The locality was opened by explorers for porcelain stone; but the feldspar proving too ferruginous, being of a somewhat flesh-red color, has led to an abandonment of the enterprise. Although this discovery is a recent one, it is not unlikely that specimens from the same spot had previously found their way into cabinets; and may thus have led to some of the discrepancies existing in the various descriptions of the Connecticut columbites.

The general aspect of the crystals is rather peculiar. Instead of flattened prisms, they are nearly all square and often, through imperfection, decidedly rhombic. With the common goniometer, many of them give angles nearly as oblique as pyroxene. Nevertheless, when more carefully studied by means of the reflective goniometer, accompanied by observations on the faces replacing the lateral prismatic edges, no essential distinction of form can be made out between these crystals and those of columbite or of tantalite. They are all strictly isomorphous. An unimportant, though striking peculiarity is observed in these crystals, proceeding from the unusual development of the octahedral faces, whereby they possess a four-sided pyramidal summit at one, or each extremity. Very rarely are traces of the terminal plane visible; and when seen, the eye or the common goniometer would always report it as slightly wanting in verticality. Its smoothness is insufficient for measuring its position with the reflective goniometer.

The faces of the crystals are wanting in that high luster so frequent in those from other localities. In luster, color and even in general shape, they resemble on first inspection, the average specimens of Elba yenites nearly as much as they do the ordinary Haddam columbite,—the chief exception being, that the narrow faces about the prismatic edges are bright.

No pavonine tints are seen in the present variety. Their absence is so marked, not only on natural planes but also on fractured surfaces, as to afford a ready and almost sure criterion for distinguishing specimens of this locality from those obtained at the old one of Brainards, where every crystal and almost every fragment displays the blue or yellowish tarnish.

The color of the fracture is perfectly black approaching pitchy. It is more firm, and less prone to crumble than the ordinary columbite. The fracture is occasionally small conchoidal. No distinction in hardness can be made out. It is nevertheless more easily ground to a fine powder than the usual mineral; and, when perfectly powdered, presents an almost black color, while columbite is only brownish black. This distinction is so marked, as to be recognized by candle light.

But the most remarkable distinction resides in its lower specific gravity, which may be said to be strictly uniform. Eight examples (varying in weight from 2 to 16 grams) gave 5·31 as the average. Three of these gave respectively, 5·32, 5·34 and 5·35; while four specimens of columbite from the Brainard locality gave 6·02, 6·03, 6·10 and 6·19,—the average being 6·085.

Its powder, strongly heated in an open platinum crucible, lost only ·02 p. c. in weight, but changed in color, by several shades, to a brownish black.

Alone before the blow-pipe, small fragments had their edges decidedly rounded by fusion. Treated in powder in small quantity, in the outer flame with borax on platinum wire, a clear, pale yellow globule (while hot) with faint tinge of brown, was obtained, which on cooling became almost colorless. In the interior flame, no change took place, except that of greater paleness in the globules. The addition of nitre produced the reaction for manganese.

The columbite powder from Brainards' gave similar results, with the difference, of deeper tints to the glass.

With microcosmic salt, the behavior was similar, excepting when the mineral was added in excess; a brownish red globule was then afforded.

Heated with carbonate of soda on charcoal, it yielded, in common with the columbite, minute granules of tin.

It is feebly, if at all, attacked by hydrochloric acid; but is perfectly decomposed by heated sulphuric acid. The action is evinced almost immediately,—the mixture becoming first yellowish, then greenish yellow, and finally deep yellow, which by the affusion of water turns white. The Haddam columbite, on the other hand, is only imperfectly decomposed by the same treatment. Both minerals, however, are speedily decomposed by fusion with bisulphate of potash,* but with a marked difference in the aspect of the fused mass; the new variety yields a honey yellow, transparent glass, while the columbite gives one which is grayish white and opaque.

A portion of the white insoluble residue (after decomposition of mineral with sulphuric acid) on being treated on tinfoil with hydrochloric acid, instantly produced a deep Prussian blue liquid and precipitate. On dilution with water, the color grew paler; and, in half an hour, even the sediment had nearly changed to a grayish blue.

Another portion of the above moist powder was treated with dilute sulphuric acid and a strip of zinc. The metal was instantly coated by a dark blue precipitate, which after two hours also changed to ash-gray.

The original sulphuric acid solution, slightly diluted with water, after separation from the insoluble white precipitate, was boiled; whereupon a considerable white precipitate of the metallic acid, (or acids,) was thrown down. This on being heated was of a faint yellow color, and nearly white after cooling. It tinged borax pale citron yellow while hot, but left it colorless on cooling; and gave similar results with microcosmic salt.

A portion of the metallic acids was fused with caustic soda in a silver crucible. The mass obtained was dissolved in water; and through the solution a stream of carbonic acid was

---

* This easy decomposition, which is the most striking in the new variety, points also to the comparative absence of tantalic acid.

conducted to complete saturation, producing no precipitate, whereby the absence of tantalic acid was inferred.*

Analysis, the decomposition being effected by fusion with potash, gave the composition as follows:

Metallic acids	78·30
Protoxyd of Iron	13·86
Protoxyd of Manganese	7·72
Tin only in traces.	
	100·28

In two processes for the metallic acids, one by the decomposition of the mineral by sulphuric acid, the other by potash, an exact agreement in specific gravity was found, viz.: 4·06; from which, perhaps, it may be just also to infer the absence of tantalic acid, whose density when pure, though varying somewhat according to its mode of preparation, averages fully one-third higher than the present result.

Hermann distinguishes three varieties of columbite, viz.: (1) with gravity above 5·9, (2) with gravity 5·5 to 5·9, and (3) with gravity 5·4; but it is very doubtful whether an example of the mineral under notice has ever fallen into his hands, as this locality is of recent discovery. In conclusion, it may be added that minute coatings of uranochre were observed in patches and specks upon a few of the crystals; though no evidence of the presence of uranium was afforded in the mineral itself.

2. *Unknown mineral (microlite?) in Haddam columbite.*

This mineral I suspect to be new. It has never fallen under my notice until found among the productions of a recent blast-

---

* The following reactions were obtained in testing a solution of the above compound of soda with the metallic acid; with protonitrate of mercury, a curdy, dense precipitate of a pale yellowish white, which after fifteen minutes assumed a green tint; with corrosive sublimate, no precipitate, but by slight evaporation, it thickened, and by drying, still farther, a red-brown oxychlorid of mercury was deposited; sulphate of soda gave no precipitate, but sulphate of potash a feeble one after a little standing; ferrocyanid of potassium, no change at first, but a pale yellowish one afterwards; phosphoric acid rendered the solution feebly opaline at the end of two hours; cyanid of potassium produced immediately a thick, white precipitate; infusion of nut-galls no effect until a drop of hydrochloric acid was added, when a thick, orange-red precipitate appeared; nitric acid gave an abundant precipitate that did not dissolve by heating; hydrochloric acidproduced a voluminous precipitate; a portion of the solution was evaporated to $\frac{1}{8}$th its bulk, whereby a portion of the salt was thrown down, which was redissolved, with exception of a faint opalinity in the liquid; nitrate of silver added to this last solution produced an abundant pale yellow precipitate that was redissolved by boiling, a few drops of ammonia changed its color to brown, a larger quantity dissolved it entirely, and by drying in porcelain, it became first brown, than black. The foregoing reactions are those heretofore supposed to belong to the soda salts of niobium, or columbium; but it is probable that they may be shared to some extent by those of the allied metal, ilmenium.

ing of the columbite at the Brainard locality. It occurs very sparingly (for the most part requiring the use of the microscope for its detection), occupying the sides of open crevices in large crystals of columbite, and also between the surfaces of these crystals and feldspar. At first, I was led to suspect them to be cassiterite; but blow-pipe examination with soda on charcoal only afforded minute traces of tin. The high adamantine lustre next suggested zircon and tungsten. The color was intermediate between that of wöhlerite and monazite; but a closer examination showed a wide divergence in other properties from either of these species.

Apparently equilateral triangular planes were detected; and finally portions of both pyramids, which leave little doubt that the form is that of the regular octahedron, especially as two of the solid angles were each similarly replaced by four planes, resting on the octahedral faces. To verify this conjecture of a cubic primary, will require the breaking up of the only specimens I possess of the crystals, which for the present I defer in the hope of obtaining other examples. The hardness is 5· to 5·5. Brittle, fracture conchoidal, semi-transparent to translucent. Luster of fracture, resinous; that of surface of crystals, adamantine. Streak yellowish white.

Before the blow-pipe, infusible. After long heating, it loses its brown tint, and while still hot, assumes a feeble citron yellow color, which becomes paler on cooling, when it is seen to have lost much of its luster, and is also less translucent. With microcosmic salt in the outer flame, it slowly dissolves into a perfectly colorless, transparent glass; but in the interior flame, it becomes, while warm, slightly milky with a tinge of blue. An intermittent flame produced the same transparency and tinge of blue. Long continued heating produced no trace of brown, violet, amethyst or yellow. The addition of tin gave a bluish white semi-opaque pearl.

It approaches most nearly to the microlite, but differs from it in the absence of all cleavages as well as in luster and in its behavior before the blow-pipe. Its mode of occurrence and properties lead to the suspicion of its being chiefly, if not wholly, composed of one of the metallic acids of the columbium group. The microlite, though occurring within a few yards of the columbite at Chesterfield and in the same ledge of granite, was never seen associated with this species, but invariably presents itself either in the albite or crystals of tourmaline, while the present mineral at Haddam occurs solely in, or upon, the columbite.

3. *New locality of Bismuthine and Bismutite in Haddam.*

About thirty years since I pointed out the existence of both these species and a third bismuthic mineral, the bismucone, as existing together in small quantity, associated with chrysoberyl, yellow beryl, columbite, garnet and a zircon mineral which I afterwards called calyptolite. Mr. Ames has lately procured for me large crystalline masses of bismuthine, more or less coated by bismutite, from a quarryman who had discovered them on the iolite hill, situated about one mile to the southwest of the meeting-house. They had apparently formed a narrow seam in the common orthoclase granite, which had been opened with a view to working as a porcelain-feldspar quarry. One of the specimens weighs about half a pound; and constitutes a deeply striated or channeled crystal. The mineral was supposed to be molybdenite. Examined before the blow-pipe, it affords no indications either of tellurium or selenium.

4. *On the metallic acid in Microlite.*

It having been suggested by Prof. Brush, from blow-pipe experiments* and the higher percentage of the metallic acid in this mineral, that it is the tantalic instead of the columbic, I made the following experiments upon two small crystals whose weight together was 0·350 gram. It was decomposed by fusion with bisulphate of potash, cold water dissolved the mass, which afterwards deposited a white granular precipitate, the clear supernatant liquid not showing even an opaline tendency on ebulition. The white precipitate after ignition gave 72·80 p. c. for the metallic acid. It evinced scarcely the slightest yellow tint when hot; and when cold, was perfectly white.

A portion of it gave with microcosmic salt, a perfectly clear glass, which showed only a faint yellowish tinge while hot. The addition of sulphate of iron produced a blood red tint when heated in the inner flame; on cooling, it changed to dirty yellow. With borax, it dissolves almost without color, being only a faint greenish yellow; but the globule inclines to opacity. No tinge of violet or pink is visible.

Fused with soda, the metallic acid afforded a bluish gray mass. The excess of alkali being removed by cold water, the addition of more water took up the residuum. A portion of this solution on being acidulated with hydrochloric acid, and afterwards treated with solution of nutgalls, gave a very yellow orange precipitate, quite different from that produced by columbic acid, which when contrasted therewith was seen to have a deep reddish tint† as compared with columbic acid.

The chief portion of the sodic solution was then supersaturated with carbonic acid, and gave rise to a decided

---

* Dana's Mineralogy, p. 514.

† There was a want of correspondence also in the reactions of the metallic acid and soda compound, with nitrate of silver, nitrate of mercury, corrosive sublimate and ferrocyanid of potassium.

precipitate. From the foregoing results, it would seem little doubt remains that microlite is a tantalate rather than a columbate of lime.

5. *Redondite.*

I have examined still farther the hydrous phosphate of alumina and iron described by me in a previous number of this Journal (vol. xlvii, p. 338), as occurring so abundantly in the island of Redonda, W. I., and am now of the opinion that it constitutes a species distinct from barrandite, from which it differs in several particulars, and essentially in specific gravity. Barrandite has gr.=2·576. Redondite gives gr.=2·019, which may be presumed to be a little too high, as the three examples used in the determination were found on analysis to give 8·8 p. c. of silica.

The phosphoric acid was determined on the same specimens by the molybdate of ammonia process; and amounted to 40·192 p. c. But as the silica (8·8 p. c.) was obviously accidental, the proportion of phosphoric acid in the pure mineral will stand at 44·07 p. c. The water is 24·73 p. c.

6. *Phosphoric acid in the Diaspore of Chester, Mass.*

Heermann having found phosphoric acid in the diaspores of several localities, I thought it worth while to analyze a good crystal of this mineral from Chester. It was semi-transparent and of a rich hair-brown color with a faint tinge of violet. Sp. gr.=3·343. The phosphoric acid was determined by the molybdate of ammonia. I obtained

Water,	15·80
Phosphoric acid,	0·32
Protoxyds iron, with traces of manganese	0·38
Alumina not determined; but by difference	83·50
	100·00

7. *The Pelham Vermiculite?*

It occurred to me as possibly interesting to make some chemical examination of this rather curious exfoliating mica, described by Mr. Adams in a late number of this journal (vol. xlix, p. 271). It is very abundant, and to the eye apparently as homogeneous as other softened micas.

It loses on an average 7 p. c. of water by ignition. By digestion in hot aqua regia, half its weight comes into solution, leaving behind 50 p. c. of minute, colorless scales, closely resembling margaric acid. They are so remarkable for their uniformity of size, freedom from color, and pearly luster, as scarcely to suggest their inorganic composition. Under the microscope, however, they very nearly give the distinct lateral cleavage lines of mica; nor have they the perfect elasticity of a true mica. They show no tendency to farther exfoliation when heated; before the blow-pipe they melt with difficulty on the edges into a colorless glass. Prof. DesCloizeaux was kind enough to examine some of these scales optically; and found them to be uniaxial, from whence it is probable that they belong to the species biotite.

The solution in aqua regia afforded the following result in reference to 100 parts of the dissolved material.

Alumina	14·00
Magnesia	13·68
Peroxyd of iron	32·00
Silica	24·00

No search was made for alkalies; but from the result obtained it is apparent that the magnesia in part explains the use made by the farmers of this material for a fertilizer, as described by Mr. Adams.

The point, however, which most interested me was the discovery thus accidentally made of the cause of the worm-like exfoliation in the mineral, viz: from the coating of the mica scales with a hydrated, argillaceous mixture, which probably owes its origin to the decomposition of some other species of the micaceous or chloritic family. In subjecting a fragment of the vermiculite of Millbury, Mass., to a similar action of aqua regia, the result was an abundance of brilliant green scales, probably belonging to the species ripidolite. It may therefore be suggested, that many earthy species of minerals (silicates) will under analogous treatment be found to be less homogeneous than has been supposed.

Amherst, May 7, 1870.

Art. XIII.—*Notice of a new Species of Gavial from the Eocene of New Jersey*; by Professor O. C. Marsh, of Yale College.

The Museum of Yale College has recently received some interesting reptilian remains from the Eocene greensand of New Jersey, which indicate a new species of Gavial, considerably smaller than any Crocodilian heretofore discovered. These specimens, which were found together, and are evidently parts of the same skeleton, consist of various fragments of the skull, and ten vertebræ. The coössification of the neural arches of the vertebræ, and the almost entire obliteration in some of them of the sutures, would imply that the individual, although diminutive, was nearly or quite mature.

The few portions of the skull preserved are sufficiently characteristic to show that the animal had an elongated muzzle, and that the upper and posterior parts of the head were of the gavial type. The form of the parietal bone indicates, moreover, that the temporal apertures were large, and near together, with their adjoining sides nearly vertical. The quadrates are elongate, unusually straight on the inner side, and their condylar surfaces deeply and obliquely notched. The pneumatic foramen, on the upper surface of the quadrate near the inner edge, is very large, and characteristic. The teeth of this specimen were unfortunately not secured.

The vertebræ are well preserved, and present marked characters. The articular cup is transversely oval in the cervicals and anterior dorsals, and has its upper margin depressed in the posterior dorsals. The hypapophyses are simple and elongate. The neural canal of the cervical and anterior dorsal series is transverse, and sub-rectangular in outline, and the floor unusually broad and flat. In the posterior dorsals the canal, although still transverse, becomes less rectangular, with the broader portion above.

The principal dimensions of the tenth, or first dorsal, vertebra are as follows:—

Length of centrum, - - - - -	15·	lines.
Transverse diameter of cup, - - -	8·75	"
Vertical diameter of cup, - - - -	7·	"
Width of neural canal, in front, - - -	6·	"
Height of neural canal, in front, - -	3·50	"

The species here described may perhaps prove to be generically identical with the one named by the writer *Thecachampsa Squankensis*, which is the only Crocodilian hitherto found in the Eocene of New Jersey. The genus *Thecachampsa*, however, as proposed by Prof. Cope, cannot yet be regarded as a valid one, since the concentric structure of the dentine, on which it was founded,* is not a character of generic importance; for it occurs in various other Crocodilians, and also in some of the Cetacea. The present remains, therefore, may be placed provisionally in the genus *Gavialis*, and the species be called *Gavialis minor*. It may readily be distinguished from *Thecachampsa Squankensis*, aside from its greatly inferior size, by the posterior dorsal vertebræ, which have the cup and neural canal transverse, instead of vertical, and especially by having the bodies of these vertebræ proportionally more elongate, and without the remarkable constriction, which is one of the most striking characters of the latter species.

---

* Proceedings Philadelphia Acad. Nat. Sciences, 1867, p. 143.

The present specimens indicate that the animal to which they belonged was quite slender, and about six feet in length. They were found by Hugh Hurley, Esq., in the Eocene greensand of Shark River, Monmouth county, New Jersey, and by him presented to the Museum of Yale College.

New Haven, Conn., June 10th, 1870.

ART. XIV.—*On Hydrogenium-amalgam;* by O. LOEW, of the College of the City of New York.*

WHEN zinc-amalgam is shaken with water, a slow decomposition of the latter takes place, recognized by the formation of flocculi of hydrated oxyd of zinc, and the evolution of small bubbles of hydrogen on allowing the mixture to stand for a time. This decomposition of water by zinc is intensified when a small quantity of bichlorid of platinum is present; a spongy body then being formed on the surface of the zinc-amalgam. This body I have found to be an alloy of hydrogenium and mercury.

In order to obtain the hydrogenium-amalgam on a larger scale, zinc-amalgam containing a few per cents of zinc† is shaken thoroughly with about an equal volume of a solution of bichlorid of platinum containing about 10 per cent of the chlorid, care being taken to keep the mixture cool. The zinc-amalgam swells up considerably, precisely as in the ammonium-amalgam experiment, and continues to evolve hydrogen till the decomposition of the amalgam is complete. I have found that the volume of the hydrogen thus developed was, in several experiments from 100 to 150 times that of the mercury employed. This hydrogen possesses a faint but peculiar odor.

When this amalgam of hydrogenium (prepared with about 5 per cent of zinc) is pressed directly after its preparation, between sheets of filtering paper and then spread out in a layer (not too thin) to the air, the temperature soon rises considerably, and vapors of water are formed, which may be condensed in a glass receiver. The finely divided platinum present is obviously the cause of this rapid oxydation of the hydrogenium. In this action of platinic chlorid upon zinc-amalgam oxychlorid of zinc is at the same time formed; and though this may be removed by means of hydrochloric acid, yet by this treatment a part of the hydrogenium-amalgam is destroyed. If, after this, it be

---

* Read before the Lyceum of Natural History, Chemical Section, Apr. 11.

† A zinc-amalgam containing more than 5 per cent of zinc is a solid, and in this state, is not well fitted for use. though it would doubtless produce more hydrogenium in the amalgam. A zinc-amalgam of 5 per cent is readily liquified at a moderate heat and should then be immediately used.

washed with water, it undergoes a very slow decomposition, the volume increases and bubbles of hydrogen escape through the water above. An addition of zinc-amalgam or sodium-amalgam greatly accelerates the decomposition of the hydrogenium-amalgam.

Platinum after perfect amalgamation does not act as energetically as in its nascent state; i. e., when precipitated on the amalgam. When platinum-amalgam is mixed with zinc-amalgam the decomposition of the water by zinc is extremely slow and the hydrogenium-amalgam does not appear for some time. Under certain conditions moreover, the hydrogenium-amalgam is formed without the aid of platinic chlorid. I had at one time about twenty pounds of mercury containing zinc, which was left standing in a bottle with water for three weeks; the hydrogenium-amalgam formed on the surface of the mercury, gradually decomposing above and being renewed from below.

Graham compares hydrogenium to the active modification of oxygen, and no doubt there is much analogy between them. We may distinguish three modifications of hydrogen; namely, (1) common *nascent* hydrogen formed by the action of sodium-amalgam on water, or by that of zinc and hydrochloric acid; it has a strong reducing power but cannot form hydrogenium-amalgam; (2) common gaseous hydrogen which has at common temperatures only a very weak reducing power; and (3) hydrogenium. There are reasons for believing that these differences may be expressed by the following formulæ:

[O] Antozone α oxygen.	[OO] Common oxygen β oxygen.	[OO]O Ozone γ oxygen.
[H] Common nascent or α hydrogen.	[HH] Common gaseous or β hydrogen.	[HH]H Hydrogenium γ hydrogen.

ART. XV.—*The Brachiopoda, a Division of Annelida;*[*] by EDWARD S. MORSE.

AT a meeting of the Boston Society of Natural History, June 1st, 1870, Mr. Edward S. Morse made a verbal communication on the position of the Brachiopoda in the animal kingdom. He referred to the branch of Mollusca as it was understood forty years ago, when misled by external characters, many worms, like *Serpula* and *Spirorbis*, and a group of Crustaceans, the Cirripedia, were included with mollusks, and that from a proper recognition of their characters these diverse forms had

---

[*] Abstract from Proceedings Boston Society Natural History.

been eliminated from time to time, and referred to their proper branches. After long and careful study Mr. Morse was prepared to state that the Brachiopods were true Articulates, and not Mollusks, and that their proper place was among the worms, forming a group near the tubicolous Annelids.

He stated that for the past year he had been deeply engaged in the study of the Brachiopoda, and more particularly their early stages. Beside material from the coast of New England, he had had, through the kindness of Prof. A. E. Verrill, a large lot of *Discina* from Callao, Peru, belonging to the Yale College Museum. From these he had studied their early stages, but as he had in preparation a memoir upon the subject, he would now confine himself to the considerations that follow.

He first spoke of the structure and composition of the Brachiopod shell, and pointed out the relations between the cœcal prolongations of the mantle in *Terebratula* and a similar structure in the test of Crustacea. He had also noticed a marked resemblance between the polygonal cells in the shell of a young *Discina*, and a similar feature in certain lower Crustaceans. The scale-like structure of the test of *Idotaea*, resembled the scale-like structure of *Lingula.* The skin of *Nereis* had similar punctures or dots, as seen in *Terebratula* and also in the peduncle of *Lingula.* He had submitted the shell of *Discina* to chemical tests and believed it to be chitinous. Gratiolet had already given the chemical analysis of *Lingula anatina* and found forty two per cent of phosphate of lime, and only six per cent of carbonate of lime. The position of the valves of all Brachiopoda were dorsal and ventral, and this was a strong articulate character to be compared to the dorsal and ventral plates of the Articulates. The horny setæ that fringe the mantle of Brachiopods was a feature entirely absent in the Mollusca, and peculiar to the worms.

The bristles of worms differ from those of other articulate animals in having sheaths containing muscular fiber, while in other Articulates, the hairs were simply tubular prolongations of the epidermal layer. In Brachiopods the setæ or bristles were secreted by follicles imbedded, or surrounded by muscular fibers, and were moved freely by the animal. In the structure of the setæ he found an identity with that of the worms. He then called attention to the resemblance between the lophophore of the Brachiopods and a similar structure in the tubicolous worms. In *Sabella* the cephalic collar was split laterally, and a portion of it reflected. Let this collar be developed so as to cover the fringed arms, and a representation of the mantle of Brachiopoda would be attained. The thin and muscular visceral walls suggest similar parts in the worms. The circulating system he had not sufficiently studied, though Dr. Gratiolet had stated that in this respect there was a strong resemblance to the Crustacea.

In regard to the respiratory system, Burmeister had shown that there was a resemblance between the soft folds or lamellæ, developed on the internal surface of the mantle of *Balanidæ*; and similar features in *Lingula;* though the existence of these folds in *Lingula* had been questioned, he would presently show that Vogt was right in his observations. In regard to the reproductive system, he called attention to the fact that in one group of Cirripeds the ovaries were lodged in the upper surface of the peduncle, while in another group the same parts were lodged in the mantle. A similar condition existed in the Brachiopods where in one group the mantle holds the ovaries, while in another group they are found in the visceral cavity.

Through Polyzoa also he showed that, in their winter eggs or statoblasts, a relation was seen to the ephippia of *Daphniæ*, and the winter eggs of Rotifers.

Of great importance also, and upon which he laid particular weight, were the peculiar oviducts with their trumpet-shaped openings so unlike the oviducts of mollusks, and as he believed, bearing the closest affinity to the oviducts in many of the worms, namely: a pair of tubes, and in one case two pairs, having their inner apertures with flaring mouths, suspended in the visceral cavity, thus opening a direct communication between the visceral fluids and the surrounding media. He then called attention to what little information we had regarding the embryology of the Brachiopods. Lacaze Duthiers had shown that in Thecidium the embryo was composed of four segments with eye-spots and other strong articulate features. Fritz Müller had given a description, with figures, of the early stage of *Discina*, in which we have not only little cirri projecting from the shell, but a little appendage recalling the plug or operculum in some of the tubiculous worms.

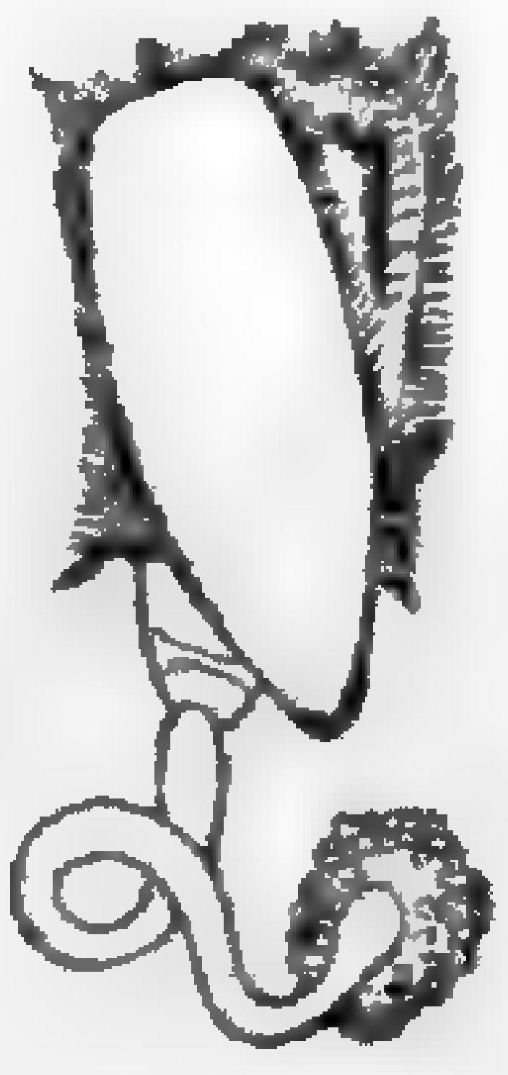
Of great importance also was the fact that in the early stage of *Discina*, Müller observed large bristles, and these were moved freely by the animal. Smitt had shown that in certain Polyzoa (*Lepralia*) the embryo, besides being furnished with cilia, also supported several bristles or setæ which were locomotive, and finally in the worms, Claparède and Mecznikow had figured an embryo of *Nerine* in which barbed bristles were also developed. Mr. Morse referred to his communication before the American Association for the Advancement of Science on the early stages of the Brachiopods, in which he had shown the intimate connections existing between this group and the Polyzoa. Now Leuckart had already seen reasons for placing the Polyzoa with the Annelids, and he would call attention to *Crepina gracilis* and *Phorronis hippocrepia*, admitted to be worms or early stages of them, and their close resemblance in nearly every point of their structure to the hippocrepian Polyzoa. Mr. Morse then stated that in the evidence already given, he had drawn his conclusions from alcoholic specimens of *Terebratula* and *Discina*, and from the papers of Lacaze-Duthiers, Claparède, Mecznikow, Hancock, Huxley, Vogt, Hyatt, Williams, De Morgan and others. He felt the importance of first examining *Lingula* in a living condition before making these announcements and for this reason he had recently visited the coast of North Carolina for the express purpose of finding if possible the rare *Lingula pyramidata* of Stimpson, first discovered by Prof. Agassiz in South Carolina. After nearly a week's fruitless search he had found it, had studied it alive, and had brought with him living examples, which he has the pleasure of exhibiting before the Society.

He would here express his deep sense of gratitude to Dr. Elliott Coues, Surgeon U. S. A. at Fort Macon, N. C., and the Commandant of the Post, Major Joseph Stewart, U. S. A., for the constant aid and sympathy rendered to Dr. A. S. Packard and himself during their visit there. He would not enter into a description of *Lingula* as he had already in preparation a memoir upon the subject, but would call attention simply to the additional evidence in support of the views advanced.

Lingula was found in a sand shoal at lowwater mark, buried just below the surface of the sand. The peduncle was six times the length of the shell, and was encased in a *sand tube* differing in no respect from the sand tubes of neighboring annelids. In many instances the peduncle was broken in sifting them from the sand, yet the wound was quickly repaired, and another sand case was formed.

1

2.



3.

Fig. 1. Peduncle perfect, retaining portion of sand tube.
Fig. 2. Showing valves in motion; peduncle broken and forming new sand-case.
Fig. 3. Peduncle broken close to the body, and forming new sand-case.

He observed that *Lingula* had the power of moving over the sand by the sliding motion of the two valves, using at the same time the fringes of setæ which swung promptly back and forth like a galley of oars, leaving a peculiar track in the sand. In the motion of the setæ he noticed the impulse commencing from behind and running forward.

Within the mantle he found a series of rows of prominent lamellæ in which the blood rapidly circulated, thus confirming the correctness of Vogt's observations. These lamellæ were contractile, however.

The peduncle was hollow, and the blood could be seen coursing back and forth in its channel. It was distinctly and regularly constricted or ringed, and presented a remarkably worm-like appearance. It had layers of circular and longitudinal muscular fiber, and coiled itself in numerous folds or unwound at full length. It was contractile also, and would quickly jerk the body beneath the sand. But the most startling observation in connection with this interesting animal, was the fact that its blood was *red*. This was strongly marked in the gills and various ramifications of the mantle and in the peduncle. At times the peduncle would become congested and then a deep rose blush was markedly distinct. Mr. Morse expressed his gratification in having come to the conclusions in regard to the annelidan characters of Brachiopods a long time previous to his observations on *Lingula*.

He then concluded by stating that the Brachiopods, with the Polyzoa, should be removed from the Mollusca, and placed with the Articulates among the Annelids. That the Brachiopods came near the tubiculous worms, though they were much more highly cephalized. That they exhibit certain crustacean characters, but were widely removed from the Mollusca unless a relation could be traced through the homologues of the Polyzoa to that aberrant group the Tunicates, as pointed out by Allman. He believed the Brachiopods to be a comprehensive type, exhibiting general articulate features, and forming another example of those groups belonging to the last that exhibit the characters of two or more classes combined.

It was interesting in this connection to remark that Lingula, one of the earliest forms created, had yet remained the same through all ages of the earth's history.

A New Comet.—WINNECKE discovered a new telescopic comet at Carlsruhe on the night of the 29–30 of May. The position obtained by him for May 30, is as follows: M. T. at C. $14^h\ 13^m\ 34^s$. R. A. $0^h\ 50^m\ 9{\cdot}55^s$. Decl. $+28°\ 52'\ 18''$. Vogel found for the same comet at the Leipsic observatory, M. T. at Leipsic $13^h\ 2^m\ 28{\cdot}5^s$, R. A. $0^h\ 50^m\ 4{\cdot}09^s$. Decl. $+28°\ 53'\ 17{\cdot}4^s$. We would call the attention of American astronomers to the prizes for the discovery of comets offered by the Austrian Academy. (See this Jour., vol. xlix, p. 442).

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

1. *On the emission, absorption and reflexion of varieties of heat radiated at low temperatures.*—Magnus has communicated an interesting and valuable paper on the heat radiated at low temperatures, and on the absorption and reflexion of such rays. The results of this investigation,—the last we suppose made by the lamented author,—are so far as published, in his own words as follows:

(1.) Different bodies at 150° C. radiate different kinds of heat. These kinds of heat are more absorbed by a substance of the same kind, as the radiating body, than by others, and this absorption increases with the thickness of the absorbent.

(2.) There are substances which emit only one or a few kinds of heat, others, which emit many kinds.

(3.) To the first of these belongs rocksalt when quite pure. Just as its ignited vapor, or that of one of its constituents, sodium, radiates but one color, so rocksalt, even at a low temperature, emits but one kind of heat. It is monothermic, as its vapor is monochromatic.

(4.) Rocksalt even when quite clear emits, together with its peculiar rocksalt-heat, heat which is not more absorbed by a plate of rocksalt 80^{mm} in thickness than by one 20^{mm} in thickness.

(5.) Rocksalt absorbs very powerfully the heat which it radiates. It therefore does not, as Melloni supposed, allow all kinds of heat to pass through it with equal facility.

(6.) The great diathermancy of rocksalt does not depend upon its less power of absorption for different kinds of heat, but upon the fact that it radiates only one kind of heat, and consequently absorbs only this one, and that almost all other substances send out heat containing only a small fraction or none of the rays which rocksalt emits. But all rays which differ from those radiated by any substance, are not absorbed by it, but pass through with undiminished intensity.

From this we may infer that every substance is diathermanous, only because it radiates but few waves of quite definite length, and consequently absorbs only these, allowing all the others to pass through.

(7.) Sylvin behaves like rocksalt, but is not monothermic to the same extent. In the case of this substance also an analogy exists with its ignited vapors, or those of potassium, which as is well known yield a nearly continuous spectrum.

(8.) Fluor spar completely absorbs pure rocksalt heat. We ought therefore to expect that the heat which it emits will be equally absorbed by rocksalt. Nevertheless 70 per cent of this heat pass through a rocksalt plate 20^{mm} in thickness. This may doubtless be easily explained with reference to the proportion of the quantity of heat which fluorspar emits in comparison with that of the rocksalt; still it is possible that fluorspar at 150° emits rays other than those which it absorbs at ordinary temperatures. This behavior is however probably connected with the great reflecting power of fluorspar for rocksalt-heat, of which I shall speak in the second part of this memoir.

(9.) If it were possible to produce a spectrum of the heat radiated at 150° C., the spectrum would, if rocksalt were the radiating body, exhibit only one luminous band. If sylvin were used as a radiator, the spectrum would be much more extended, but would still occupy but a small portion of the spectrum which the heat radiated from lamp-black would form.

In concluding this part of his memoir Magnus makes some remarks on transparency which seem to us very suggestive. If we assume that there is a constant interchange of heat even between bodies having the same temperature, we may fairly assume also that there is such an exchange in the case of light. We cannot observe the light which bodies emit at ordinary temperatures, but they do not absorb light, since this absorption produces their colors. If such an exchange of light takes place at ordinary temperatures, it would follow that transparent bodies either radiate only such rays as are not contained in the light emitted by ignited bodies, and then they absorb none of these rays, or that they emit only one or a few of the wave-lengths of the light which is visible to us; since then they absorb only these, and allow all others to pass through, so that the intensity of the transmitted light is but little less than that of the incident light. We may therefore infer that the transparency of bodies depends upon the fact that they only radiate a few of the wave-lengths, which are contained in the light known to us.—*Pogg. Ann.*, Band cxxxix, p. 431. W. G.

2. *On the breadth of the spectral lines.*—Lippich has given an explanation of the breadth of the spectral lines of luminous gases, which depends upon the now generally received dynamical theory of gases and vapors. The explanation amounts to a mathematical theory of the broadening of the bands, the diminutions in their intensity, the addition of new bands, and the appearance of parts of continuous spectra, all of which appear to depend upon changes of temperature. In the present paper the author treats only of the application of the theory to the widening of the bands. The fundamental assumption is this; if it be necessary to consider a molecule of a gas as a system capable of vibration, the spectrum of an ideal gas in which the molecules would be perfectly free elastic systems, could consist only of a number of different colored bands of absolutely homogeneous light, if the vibratory motions of the molecules alone are taken into account. According to the theory of Krönig and Clausius, the molecules of a gas have progressive motions, with very great velocities, and this circumstance in connection with the well known influence of the motion of a luminous point upon the refrangibility of the emitted rays, makes it possible to explain the widening of the spectral

bands, and to show the dependence of the breadth of the brands upon the temperature and density of the ignited gas. Setting out with these principles, the author arrives at the following law:

The ratio of the difference of the wave-lengths which correspond to the borders of a spectral band, to the mean wave-length of this band, is for one and the same gas constant for all the spectral bands, and in different gases is directly proportional to the square root of the absolute temperature, but inversely proportional to the square root of the density.

The breadths of the bands, as they appear in the spectroscope to the eye, will increase with the refrangibility of the rays somewhat more rapidly than $\frac{1}{\lambda^2}$. The author in the next place shows that in the case of dark bands in a spectrum, produced by inversion, the distribution of the darkness, so to speak, and the breadths of the band will follow the same law as that above given for bright bands. The mere comparison of the relative breadths of the bands may lead to important conclusions. If in any gas-spectrum bands of different breadths lie near together, we might infer the presence of a mixture of gases of different densities, or of different allotropic states of the same gas. Thus the author thinks that the presence of some fine lines in the blue of the oxygen-spectrum may indicate the presence of the denser ozone. Furthermore, the observation of the breadths of these lines may decide whether the appearance of new lines is due to the more intense vibrations of the same molecules, or to their allotropic conditions. For the same gas the breadth of the spectral bands permits a conclusion as to the temperature. This is of special interest in the case of the heavenly bodies, in the spectra of which a widening especially of the hydrogen lines has been observed. The author does not agree with Frankland and Lockyer in ascribing this result solely to the higher pressure of the gas, but thinks that it must also have a much higher temperature. As Huggins and Lockyer have observed a widening of certain dark lines in the spectrum of the umbra of a solar spot, the author infers that the gases and vapors forming the umbra must have a higher temperature than that of the part of the sun's atmosphere, in which Fraunhofer's lines originate. Finally, Lippich suggests that measurements of the breadths of the spectral bands will finally lead not merely to relative but to approximate absolute values of temperature and density. Among other results, it may be possible in this way to furnish an experimental proof of the correctness of the dynamical theory of gases. In conclusion, the author remarks that, strictly speaking, the results which he has obtained apply only to perfect gases. Changes in the spectrum will indicate changes from gases to vapors. New periods of vibration will occur, which will exhibit themselves in the spectrum with the less intensity, the greater their deviation from the periods of vibration of the molecules of the perfect gas. In this manner a spectral band will fade out on both sides, and with increased pressure the illuminated portion will become broader the more the gas deviates from the law of

Gay Lussac and Mariotte. This explanation agrees with Wüllner's experiments, according to which the "washing out" of the bands occurs in the cases of oxygen and nitrogen, under much lower pressures than in the case of hydrogen.—*Pogg. Ann.*, cxxxix, p. 465.
W. G.

3. *On the position of thallium among the elements.*—RAMMELSBERG has contributed some interesting facts in relation to the compounds of thallium, without however furnishing data for determining the degree of atomicity of the metal. The author, in the first place, calls attention to the fact that the isomorphism of thallium in its compounds with potassium and sodium is not sufficient to decide the question. When thallic sesquioxyd is heated with a solution of iodic acid, a normal dithallic salt is formed, which has the formula $\text{Ŧl}(I\Theta_3)_6+3aq.$ The sesquioxyd dissolves easily and completely in chlorhydric acid, forming a colorless solution, which after addition of potassic or ammonic chlorid yields beautiful crystals. The new salts, respectively $\text{Ŧl}Cl_6,6KCl+4aq.$ and $\text{Ŧl}Cl_6,6NH_4Cl+4aq.$, form large colorless transparent crystals, which resemble combinations of the cube, octahedron and dodecahedron, but which really belong to the square prismatic system. They are not decomposed by water, even on boiling. With bromine and potassic bromid, and iodine and potassic iodid, thallium forms the salts, $\text{Ŧl}Br_6,3KBr+3aq.$ and $\text{Ŧl}I_6,3KI+3aq.$, which crystallize in regular octahedra. Rammelsberg remarks that the thallium atom, $Tl=204$, can hardly be regarded as other than monatomic, but as the double atom of the molecule in the dithallic compounds is hexatomic, the single atom would have to be considered as tetratomic. The specific heat of thallium is an evidence that the metal is $Tl=204$, but the isomorphism of two monatomic with one diatomic atom has been established in so many cases that no certain conclusion can be drawn from the isomorphism of thallium with potassium and sodium. It seems probable that the question can only be settled by determinations of vapor density.—*Berichte der Deutschen Chemischen Gesellschaft, Jahrgang*, 3, No. 7, p. 360.
W. G.

4. *On a new method for the volumetric estimation of copper.*—WEIL has given a new method for the determination of copper which appears deserving of attention. It is based upon the following facts. At a boiling heat and in presence of an excess of free chlorhydric acid the least trace of cupric chlorid communicates a very distinct greenish yellow tint to the solution. This tint is the more intense the greater the quantity of acid present. Stannous chlorid instantly reduces under these circumstances cupric chlorid to colorless soluble cuprous chlorid.

$$2\text{Ȼ}uCl_2+\text{Sn}Cl_2=\text{Ȼ}u_2Cl_2+\text{Sn}Cl_4.$$

The reaction is finished when the green solution of cupric chlorid is completely decolorized. A single drop of stannous chlorid in excess is easily detected by a drop of mercuric chlorid, which gives a precipitate of calomel. When the solution contains iron in the form of sesquioxyd as well as copper, the volume of the

solution of tin employed will indicate the sum of the copper and iron. In this case the author precipitates the copper in another portion of the assay by means of zinc coupled with platinum, and then determines the iron by means of potassic hypermanganate. The copper is then easily found by difference. The author determines the litre of his solution of tin by means of pure metallic copper, and preserves it from oxydation under a layer of petroleum. It is of course necessary that the solution of copper should be perfectly free from nitric acid.—*Comptes Rendus*, lxx, p. 997.
W. G.

5. *On the utilization of the secondary products obtained in the manufacture of chloral.*—Dr. A. W. HOFMANN has examined a mixture of secondary products obtained during the manufacture of chloral, and condensed during cold weather. The liquid began to boil at 17°–18°, rising slowly to 30°–31°, where the temperature remained constant a short time, and then rising again to 50°, when nearly all distilled over. The most volatile portions were mixed with three times their volume of alcohol saturated at 0° with ammonia and heated in a water bath for an hour. The liquid was then filtered to separate crystals of sal-ammoniac and the alcohol ammonia and chlorinated ethylic chlorids distilled off. The mass of chlorhydrates of ethyl-ammonias remaining were decomposed with caustic soda, and the separated liquid alkalies dehydrated by caustic soda, and finally distilled. In this manner 5 litres of the secondary products operated on gave 1½ liters of a mixture of anhydrous ethylamines. These could be separated from each other by means of oxalic ether, in the manner already pointed out by Hofmann. The results of this investigation are interesting, from the prospect which they afford of obtaining the ethyl-ammonias as an article of commerce, at a reasonable price, and in comparative abundance.—*Comptes Rendus*, lxx, p. 906. W. G.

6. *On the nature of the secondary products obtained in the manufacture of chloral.*—KRÄMER has studied the other products of the action of chlorine upon alcohol, the existence of a large quantity of ethylic chlorid having been shown by Hofmann. As the ethylic chlorid was in contact with an excess of chlorine, it was natural to expect to find in the less volatile oily products the whole series of chlorinated ethylic chlorids described by Regnault, and experiment showed that several of these substances were present. The most volatile product boiling at 60°, proved to be chlorinated chlorethyl or chlorethyliden, $€_2H_4Cl_2$, identical with the chlorethyliden prepared from aldehyd. A liquid boiling at 85°, proved to be ethylen-dichlorid, the formation of which by the action of chlorine upon ethylic chlorid had not before been observed. The next product was chlorinated ethylen dichlorid $€_2H_3Cl.Cl_2$ boiling at 115°, and the last bichlorinated ethylen, $€_2H_2Cl_2$, boiling at 37°. Other chlorinated products were also observed, but not yet studied. To prove the identity of the chlorethyliden obtained in this manner with that obtained from aldehyd by the action of phosphoric pentachlorid, Krämer heated a portion of it

with alcoholic ammonia to 160° for 12 hours. In this manner an oily base boiling at 180°–182°, and having the characteristic odor of collidine, $\text{Ȼ}_8\text{H}_{11}\text{N}$, was obtained. This base had already been formed from aldehyd-ammonia by Bæyer, and found to be identical with that obtained by Anderson from animal oil.—*Berichte der Deutschen Chemischen Gesellschaft, Jahrgang*, 3, p. 257–262.
W. G.

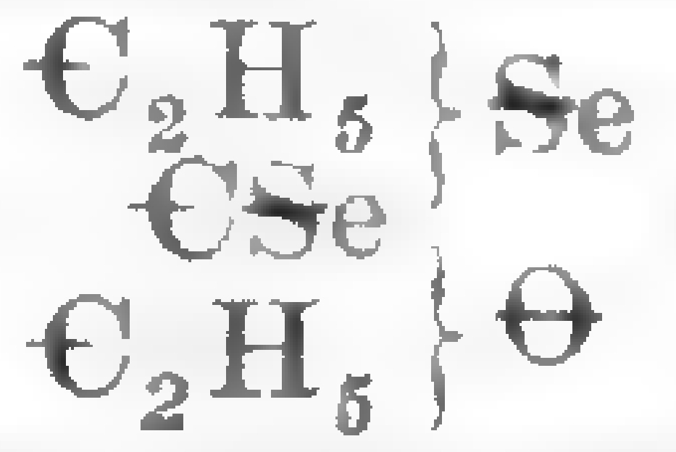
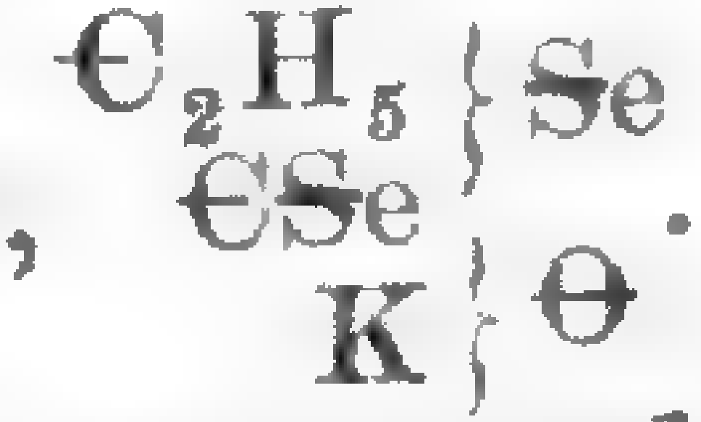
7. *On some properties of iron precipitated by the galvanic current.*—Lenz has given some interesting particulars in relation to the composition and properties of iron as precipitated in the metallic form by the battery. The iron examined was deposited by Klein's process from a solution of the mixed sulphates of iron and magnesium. Weak currents were employed, and the solution was kept neutral by carbonate of magnesia. Iron so thrown down has a beautiful fine-granular structure, showing no traces of crystals under the microscope. Its color is a soft bright gray. Its hardness is very remarkable—not less than 5·5 of the ordinary mineral scale, and it is excessively brittle, so that it may be rubbed to powder between the fingers. When the iron is slowly reduced upon a polished surface, it is free from flaws and has a velvety look. As it becomes thicker, bubbles or pits are formed as small oval depressions. When heated over a fire the iron loses many of these properties in a remarkable degree. Its hardness diminishes and becomes 4·5; its brittleness entirely disappears, and it becomes so flexible and tenacious that it cannot be broken by repeated bending or even by folding and strongly smoothing down the folds. When heated in vacuo the iron changes color and becomes almost as white as worked platinum. The ignited iron rusts very quickly both in air and in previously boiled water; this is not the case with the metal before ignition. In the electric series unignited iron stands nearer to copper than the ignited metal. On analysis by means of Sprengel's pump, the precipitated iron was found to contain various gases—vapor of water, nitrogen, carbonic oxyd, carbonic acid and hydrogen. Levy thinks that the carbonic acid came from the solution from which the iron was precipitated, and that the carbonic oxyd was formed during the ignition of the tube containing the iron in the process of analysis; also that the vapor of water was formed by the union of hydrogen with the oxygen of a small amount of rust in the iron, since it was only given off at 1,600° C. The hydrogen in the iron was always in largest quantity; the whole quantity of gas varied greatly, and sometimes amounted to 185 times the volume of the iron. The absorption of the gases was found to take place mainly in the first layers formed. On warming the reduced iron, the evolution of gas began at temperatures below 100°, but at this temperature chiefly hydrogen was evolved. Ignited galvanically reduced iron decomposes water and absorbs the free hydrogen either wholly or partially.—*Bulletin de l' Académie de Pétersbourg*, xiv, p. 337, *cited in Dingler's Polytechnisches Journal*, cxcvi, p. 44. W. G.

8. *On the preparation of barium chlorate.*—BRANDAU has proposed the following simple method for the preparation of barium chlorate: Commercial crystallized aluminum sulphate, sulphuric acid, and potassium chlorate, in the ratio of one molecule of each of the two former to two of the latter, are mixed with water to the consistence of a thin paste, warmed for half an hour on the water-bath, allowed to cool completely, and treated with alcohol in excess. Upon filtering, and neutralizing with barium hydrate, barium sulphate and some aluminum hydrate are precipitated, and barium chlorate remains in solution. The alcohol is distilled off, and the filtrate on evaporation yields crystals of the pure barium chlorate. The only precaution necessary is to have the aluminum sulphate and the sulphuric acid in slight excess.—*Ann. Ch. Pharm.*, cli, 361, Sept. 1869. G. F. B.

9. *On the properties of Selenium.*—RATHKE has investigated, in the Halle laboratory, the various modifications which selenium, like sulphur, is capable of assuming. Notwithstanding the difference in the behavior of the various forms of each, respectively, to solvents, especially CS_2, Rathke places the insoluble black selenium with the rhombic variety of sulphur, and the red amorphous selenium with the insoluble amorphous form of sulphur. As in the case of sulphur, the latter variety of selenium is produced by the decomposition of selendithionates (solutions of selenium in alkaline sulphites) by acids; by the action of water on selenium chlorid; and by the sudden cooling of fused selenium. So on the other hand, by the slow decomposition of a solution of potassium selenid, distinct crystals of black selenium are produced, precisely as when by the similar decomposition of alkaline sulphids, large rhombic crystals of sulphur separate. Though red selenium is more stable than the corresponding form of sulphur, yet, like this, it passes into the other variety on raising the temperature to 100° C., with a distinct evolution of heat. The specific gravity of selenium in these forms, is as follows: For the black variety, 4·80 to 4·81; the red, crystallized from carbon disulphid, (and corresponding to monoclinic sulphur,) 4·46 to 4·51, the red, amorphous, 4·26. Agreeing here, also, with the corresponding forms of sulphur, as already given. Regarding the behavior of selenium toward carbon disulphid as exceptional, Rathke tried the action of other solvents. Sulphur chlorid saturated with selenium with the aid of heat, deposits on cooling, crystals of sulphur, leaving selenium chlorid, in which selenium dissolves freely, forming when cold a syrupy liquid, from which, on standing, the black variety of selenium separated in small nodules. The solubility of rhombic (octahedral) sulphur in carbon disulphid, led to the supposition that selenium might be equally soluble in carbon diselenid. Various methods for preparing this substance were tried, but, though small quantities were obtained by acting on selenium phosphid with moist vapor of carbon tetrachlorid, heated in a tube, sufficient for the above purpose could not be obtained. Carbon tetrachlorid itself, does not dissolve the black variety of selenium, selenium-ethyl dissolves both,

but in very small quantity. Of the solvents tried, only those which were selenium-compounds dissolved black selenium.

Rathke also examined the compounds which sulphur forms with selenium, hoping to throw some light upon their mutual iso-dimorphism. He first melted the substances together in the ratio of one atom of selenium to two of sulphur, repeatedly extracted the resulting mass with carbon disulphid, and crystallized out the portion dissolved in fractions. The crystals, however, increased progressively in sulphur and diminished in selenium. The precipitate obtained when sulphydric gas and selenium dioxyd gas act upon each other in solution, was then examined. By solution in carbon disulphid and evaporation, well-formed rhombic prisms, in color like potassium dichromate, were obtained, which gave on analysis 63·86 per cent selenium and 35·50 per cent sulphur. These crystals, then, though by no means a mixture of the constituents, do not correspond in composition to the formula SeS_2, which requires 55·42 per cent Se and 44·58 per cent S. The process was then reversed, and the precipitate produced when selenhydric gas reacts upon sulphur dioxyd collected and treated as above. It contained considerable black selenium, not taken up by the solvent. On fractionally crystallizing, three products were obtained, one (*c*) soluble in 57 parts CS_2, the second (*b*) in 263 parts, and the third (*a*) considerably more so than selenium itself. (*a*) consisted of minute dark ruby-red, rounded crystals; (*b*) of brilliant-red prisms; and (*c*) of orange-red tabular prisms. On analysis, (*a*) was found to agree nearly with the formula Se_2S; but (*b*) and (*c*) were intermediate between Se_2S and SeS_2. Rathke regards them as isomorphous mixtures.

Carbon diselenid CSe_2 was prepared but only in small quantity, not even sufficient for analysis. It appeared as a thin, brilliant-brown liquid, of peculiar and disagreeable odor, recalling at first, when dilute, that of carbon disulphid, but being when concentrated, very pungent and irritating to the eyes. It burned with the blue selenium flame, was not very volatile, and was insoluble in all solvents tried. At the same time there was formed ethyl selenxanthate,  $\begin{array}{r}C_2H_5\\ CSe\\ C_2H_5\end{array}\begin{array}{l}\}\,Se\\ \\ \}\,\Theta\end{array}$ and potassium selenxanthate,  $\begin{array}{r}C_2H_5\\ CSe\\ K\end{array}\begin{array}{l}\}\,Se\\ \\ \}\,\Theta\end{array}$. Rathke also also attempted the production of selenium tetra-ethyd but succeeded in getting only tri-ethylselenin chlorid $Se(C_2H_5)_3Cl$, in combination with zinc chlorid, and also with platinum chlorid.—*Ann. Ch. Pharm.*, clii, 181, Nov. 1869. G. F. B.

10. *On the preparation of nitrogen pentoxyd* (*nitric anhydrid.*)—Odet and Vignon employ for this purpose a double U tube, in each bend of which 140 to 150 grams of silver nitrate is placed, the whole being heated to a temperature of 60° C. Phosphoryl chlorid ($P\Theta Cl_3$) is allowed to fall drop by drop, into the first leg of the tube. The nitryl chlorid ($N\Theta_2Cl$) thus produced, reacts upon the silver nitrate in the second bend, to which is attached, by melting, a receiver, in which the product of the reaction collects.

This receiver is immersed in a freezing mixture. No disengagement of oxygen takes place, silver phosphate and chlorid being the only secondary products. The reactions are as follows:—

$$(1)\qquad \left(\left.\begin{matrix}N\Theta_2\\ Ag\end{matrix}\right\}\Theta\right)_3 + (P\Theta)Cl_3 = \left.\begin{matrix}P\Theta\\ Ag_3\end{matrix}\right\}\Theta_3 + (N\Theta_2Cl)_3.$$

$$(2)\qquad \left.\begin{matrix}N\Theta_2\\ Ag\end{matrix}\right\}\Theta + (N\Theta_2)Cl = \left.\begin{matrix}N\Theta_2\\ N\Theta_2\end{matrix}\right\}\Theta + AgCl.$$

—*Comptes Rendus*, lxix, 1142. G. F. B.

11. *On a new method for preparing bromhydric acid.*—Champion and Pellet describe a new and simple mode of preparing bromhydric acid by the action of bromine upon paraffin at a moderately elevated temperature. Two retorts are employed, one for the bromine, the other for the paraffin. The neck of the first retort is prolonged and bent at right angles so as to enter the tubulure of the second—being fixed there by a ground joint—terminating near the bottom. By means of a sand or oil-bath, the paraffin is kept at a temperature of 180° C. and by a saline bath the bromine is maintained at 65° C. As the bromine gradually distils over into the paraffin, the bromhydric acid gas is evolved, and after passing through a bulb tube containing water, and a U tube filled with broken glass and bits of moistened phosphorus, to remove any traces of unchanged bromine, it is collected in the liquid to be saturated with it, placed in a vessel surrounded with ice. The aqueous solution thus obtained saturated at 0° C. has a density of 1·78, and corresponds to the formula $HBr, H_2\Theta$. Each c. c. contains 1·46 grms. HBr.—*Bull. Soc. Ch.*, II, xiii, 197, March, 1870.

G. F. B.

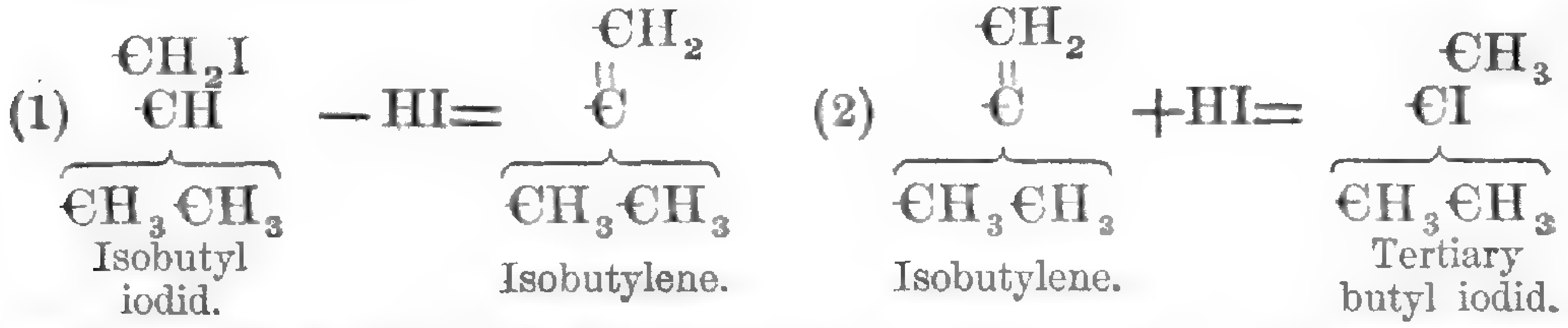
12. *On the recovery of Uranium from the Phosphate.*—In using a solution of uranium for determining volumetrically phosphoric acid, residues of uranium phosphate are obtained, from which it is desirable to recover the uranium. Two methods for doing this have been lately proposed. The first by Heintz, obtains the uranium as nitrate. The phosphate, previously washed, dried and weighed, is dissolved in nitric acid. Half as much pure tin is weighed out, and nine-tenths of it added to the nitric solution, which is then heated until the tin is entirely converted into stannic hydrate; if, in this solution, ammonia gives a precipitate not entirely soluble in acetic acid, the rest of the tin is added, and the process repeated. All the phosphoric acid is contained in the precipitate. The filtrate is diluted, treated with sulphydric gas to precipitate the last traces of tin, again filtered, and evaporated to crystallization.

The second method, proposed by Reichardt, consists in dissolving the uranium residues in either nitric or chlorhydric acid, adding an excess of ferric chlorid, making the solution acid with acetic acid by adding sodium acetate, and after considerable dilution, heating to boiling. All the phosphoric acid, together with the excess of iron, is thus precipitated. Instead of boiling the solution after the addition of the ferric chlorid, it may be treated with an excess of sodium carbonate, and filtered. In presence of free carbonic acid, the sodium carbonate holds the uranium in solution, the phosphoric acid being precipitated with the iron. The filtrate from either of these methods is acidulated with chlorhydric acid, and after boiling to expel the carbonic gas, the uranium oxyd is precipitated with ammonia.—*Ann. Ch. Pharm.*, cli, 216, Aug. 1869. *Zeitschr. analyt. Chem.*, 1869, 116. G. F. B.

13. *On the modifications of sulphur trioxyd* (*sulphuric anhydrid.*)—SCHULTZ-SELLACK has examined the isomeric modifications of sulphur trioxyd. It was prepared by distillation from di-sulphuric acid (fuming,) the vapors being rendered anhydrous by passing them over phosphoric anhydrid. On cooling the liquid thus obtained, the thermometer immersed in it is observed to become stationary at 16° C., the fluid solidifying in long transparent prisms. These melt again at the same temperature, the liquid being obtained again, frequently unaltered. Sometimes, however, white flocks remain in the melted mass, which gradually collect in warty masses of fine white needles, until the entire liquid has become a matted mass of them. The same change takes place when the liquid is kept for some time at a temperature below 25° C.; above 27° C., however, it does not take place. The solid anhydrid thus produced becomes gradually fluid again when the temperature rises to above 50° C.; but this solidification and liquefaction is not a freezing and melting, in the ordinary sense of these terms, since both take place gradually, within certain definite ranges of temperature. The fluid anhydrid has a remarkably high coefficient of expansion by heat; being for temperatures between 25° and 45°, 0·0027 for each degree, more than two-thirds that of the gases. It boils at 46° C. under a pressure of 760^{mm}.; at 20° its vapor-tension is 200^{mm}. In a vacuum the first modification shows no vapor-tension; but gradually vapor is evolved, so that after several days, a tension of 30 or 40^{mm}. at 20° is obtained. The vapor of the solid as well as that of the fluid trioxyd has a normal density, found to be 2·74 to 2·76; SO_3 requires 2·76. From the above facts, the author concludes that sulphur trioxyd exists in two states: 1st, α Sulphuric anhydrid solidifying at +16° in long colorless prisms, which melt at the same temperature; boiling at 46° C. 2d. β Sulphuric anhydrid, produced from the former, at temperatures below 25°, in very fine needles; becoming at temperatures above 50° gradually fluid again, being transformed into α; yielding vapor very slowly at ordinary temperatures, which is like that given by α, but has a less tension. He regards β as a polymer of α, since many polymeric organic compounds, as, for example, cyanuric acid, show similar variations in physical properties, as their molecules are more condensed.—*Ber. Berl. Chem. Ges.*, iii, 215, March, 1870. G. F. B.

14. *On the conversion of isobutyl alcohol into tertiary pseudo-butyl alcohol.*—From purely theoretical considerations, MARKOWNIKOFF was led to believe that isobutyl alcohol (that produced during fermentation) by the loss of water, or its iodid by the loss of

hydriodic acid, would give a butylene identical with the isobutylene of Butlerow. By heating isobutyl iodid with alcoholic potash, a butylene was obtained, which united directly with hydriodic acid, giving a butyl iodid having the boiling point of tertiary pseudo-butyl iodid. On treating this with moist silver oxyd, tri-methyl-carbinol (tertiary pseudo-butyl alcohol) was obtained with its very characteristic properties. The reaction which takes place is as follows:



$$(1)\ \underbrace{\overset{ꞒH_2I}{ꞒH}}_{ꞒH_3ꞒH_3} - HI = \underbrace{\overset{ꞒH_2}{\overset{\|}{Ꞓ}}}_{ꞒH_3ꞒH_3} \qquad (2)\ \underbrace{\overset{ꞒH_2}{\overset{\|}{Ꞓ}}}_{ꞒH_3ꞒH_3} + HI = \underbrace{\overset{ꞒH_3}{ꞒI}}_{ꞒH_3ꞒH_3}$$

Isobutyl iodid. Isobutylene. Isobutylene. Tertiary butyl iodid.

Morkownikoff believes that these facts sustain his general law of the formation of unsaturated compounds; a law which for the homologues of ethylene may thus be formulated: *When water is removed from an alcohol-molecule, that carbon-atom which is directlg united with the carbon-atom holding the hydroxyl, furnish the hydrogen.* A similar law holds in the case of the haloid anhydrids of the monatomic alcohols. *When combination takes place between an unsymmetrically constituted homologue of ethylene* (for example, propylene $ꞒH_3ꞒHꞒH_2$) *and water* (*or an haloid acid*), *the residues into which these latter bodies are separated, are divided between the two carbon-atoms so that the hydroxyl* (*or the corresponding halogen*), *is united to the least hydrogenized carbon-atom in the compound.*—*Zeitschr. Chem.*, II, vi, 29, Dec., 1869. G. F. B.

15. *On the Synthesis of Aromatic Acids.*—Starting from the well known facts that a monobasic acid may be viewed as a hydrocarbon in which an atom of hydrogen is replaced by carboxyl (ꞒΘΘH), and further, that, in general, the basicity of acids depends upon the number of carboxyl groups which they contain, Wurtz proposes to make practical use of them in synthesis. By effecting one or more such replacements in a hydrocarbon, either by the carboxyl group itself or its ethyl-derivative ($ꞒΘΘ(Ꞓ_2H_5)$),—which can be effected by heating the corresponding bromine derivative of the next lower homologous hydrocarbon with ethyl chlorocarbonate and sodium-amalgam,—any desired aromatic acid may be produced. In this way, for example, Wurtz prepared benzoic acid; 90 grams monobromo-benzol (phenyl bromid, $Ꞓ_6H_5Br$) being heated with 60 grams ethyl chlorocarbonate ($ꞒΘΘ(Ꞓ_2H_5)Cl$) and 3·5 kilograms sodium-amalgam containing one per cent of sodium. The action proceeded slowly, requiring many days for completion, even at 110° C. Carbonic and carbonous gases were evolved, sometimes mixed with a gas which burned with a green flame, probably ethyl chlorid. When the fluid had entirely disappeared and the mercury had again recovered its mobility, the saline mass was extracted with ether, and the ether distilled off till the temperature rose to 150° C. The residue, which

contained ethyl benzoate, on being treated with potassium hydrate, evaporating to dryness to remove the alcohol, re-solution in water, and precipitation with hydrochloric acid, afforded benzoic acid abundantly. The reaction, therefore, is as follows:

$$\text{Ꞓ}_6\text{H}_5\text{Br}+\text{Ꞓ}\Theta\left\{\begin{matrix}\Theta\text{Ꞓ}_2\text{H}_5\\ \text{Cl}\end{matrix}\right.+\text{Na}_2=\text{NaCl}+\text{NaBr}+\text{Ꞓ}_6\text{H}_5(\text{Ꞓ}\Theta\Theta\text{Ꞓ}_2\text{H}_5)$$

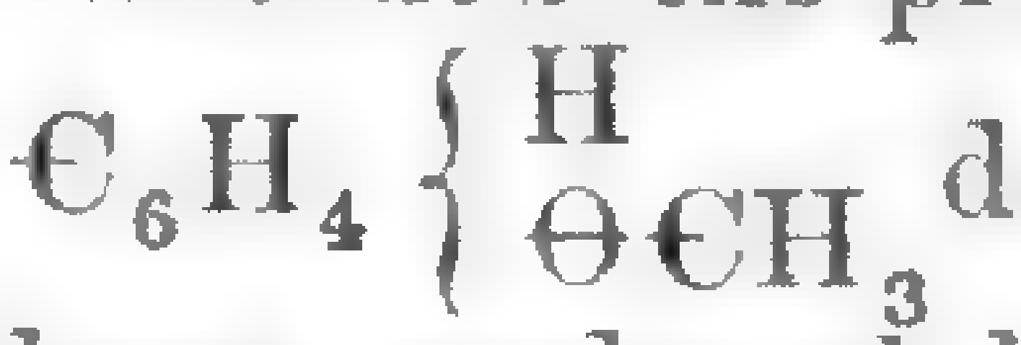
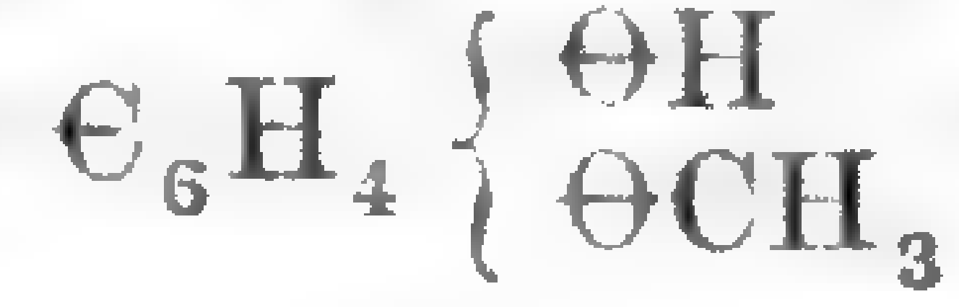
By using mono-bromo-toluol $\left(\text{Ꞓ}_6\text{H}_4\left\{\begin{matrix}\text{ꞒH}_3\\ \text{Br}\end{matrix}\right.\right)$, toluic acid $\left(\text{Ꞓ}_6\text{H}_4\left\{\begin{matrix}\text{ꞒH}_3\\ \text{Ꞓ}\Theta\Theta\text{Ꞓ}_2\text{H}_5\end{matrix}\right.\right)$ with a trace of another acid perhaps isomeric with it, was obtained. With the isomeric benzyl bromid $(\text{C}_6\text{H}_5(\text{CH}_2\text{Br}))$ the reaction yields a more complicated product.

In a more recent paper, Wurtz shows that the acid mentioned above as obtained simultaneously with the toluic, is iso-toluic acid; and that its presence is due to the fact that the bromo-toluol used, contained an isomeric body. The substance obtained by acting upon benzyl bromid or chlorid, he finds to have the composition $\text{Ꞓ}_{15}\text{H}_{14}\Theta_2$, and he gives it the name di-benzyl-carboxylic acid. In a second operation, 252 grams benzyl chlorid, 108 grams ethyl chlorocarbonate and 8,000 grams one per cent sodium-amalgam, were heated on a saline bath, with an upward condenser, till the whole mass was solid. This residue was extracted with ether, the ether distilled off till the temperature rose to 180°, the fluid remaining in the retort decomposed with alcoholic potash, the the alcohol evaporated, the residue dissolved in water, precipitated by hydrochloric acid and recrystallized from water. It separated in drops which solidified to a mass of fine needles. It is almost insoluble in cold water, and but little soluble in hot; alcohol and ether dissolve it readily. At 84° it melts and at a higher temperature distils. Its vapors are aromatic and irritating. To produce it, Wurtz assumes that under the influence of the sodium, the chlorid, by the loss of hydrochloric acid, becomes chloro-di-benzyl $\begin{array}{l}\text{Ꞓ}_6\text{H}_5\text{ꞒH}_2\\ \qquad\ \vert\\ \text{Ꞓ}_6\text{H}_5\text{ꞒHCl}\end{array}$ and that this by the simultaneous action of the sodium and the ethyl chlorocarbonate becomes ethyl di-benzyl-carboxylate $\begin{array}{l}\text{Ꞓ}_6\text{H}_5\text{ꞒH}_2\\ \qquad\ \vert\\ \text{Ꞓ}_6\text{H}_5\text{ꞒHꞒ}\Theta\Theta(\text{Ꞓ}_2\text{H}_5).\end{array}$—*Comptes Rendus*, lxviii, 1298; lxx, 350.

G. F. B.

16. *On the volatile acids of Croton oil.*—Froelich having observed in the Jena laboratory, that by the action of phosphorus pentachlorid upon ethyl-diacetic acid, two metameric chlor-acids were obtained, yielding when treated with sodium-amalgam, two metameric acids of the composition $\text{Ꞓ}_4\text{H}_6\Theta_2$, of which one was solid and identical with that prepared from allyl cyanid, the other was fluid, and supposed to be the same as that described by Schlippe as occurring in Croton oil, and called Crotonic acid, GEUTHER undertook a confirmation of this supposition. Having prepared the volatile acids from four pounds of Croton oil, he finds that no volatile acid of the composition $\text{Ꞓ}_4\text{H}_6\Theta_2$ exists in this oil, and that the solid acid contained in it is not angelic acid; and there-

fore, that Schlippe's statements are entirely erroneous. The volatile fluid acids are essentially acetic, butyric and valeric, mixed perhaps with traces of oenanthic acid and higher members of the oleic series. The solid acid supposed by Schlippe to be angelic acid, is a metamer of it, to which Genther gives the name Tiglic acid. It constitutes more than a third of the volatile acids of Croton oil, and forms a barium salt readily soluble in water, crystallizing in pearly plates, and having the composition $\mathrm{Ȼ}_5H_7Ba'\Theta_2+5H_2\Theta$. It has a remarkably close correspondence in properties with the methyl-crotonic acid of Frankland and Duppa. It is therefore obvious that the name "crotonic" given to the acid $\mathrm{Ȼ}_4H_6\Theta_2$, is a misnomer, since croton oil contains no acid of this composition. Geuther therefore proposes to call the chlor-acid of Froelich, mentioned above, which fuses at 59·5° and boils at 194·8°, monochlor-quartenic acid, and the acid derived from it by the action of sodium-amalgam, which is fluid at 15° and boils at 171·9°, quartenic acid. For the metamer of the chlor-acid, melting at 94° and boiling between 206°–211° with partial decomposition, he proposes the name mono-chlor-tetracrylic acid; and for its derivative $\mathrm{Ȼ}_4H_6\Theta_2$, first prepared from allyl cyanid and till now called crotonic acid, the name tetracrylic acid. Its aldehyd called croton-aldehyd by Kekulé, would therefore be tetracryl-aldehyd.—*Zeitschr. Chem.*, II, vi, 26, Dec. 1869. G. F. B.

17. *On the Rhenish creosote from beech-wood tar.*—Under the direction of Baeyer and Graebe in Berlin, MARASSE has made an investigation of beech-wood creosote, with results far more satisfactory and conclusive than had been previously obtained. The material on which he worked came from the manufactory of Dietze & Company in Mayence; it was colorless, a little thick, heavier than water, in which it was scarcely soluble, and dissolved completely in potassium hydrate solution. On subjecting it to fractional distillation, three separate products were obtained: one boiling below 199°, one between 200° and 203° (by far the larger portion) and one between 216° and 220°. After drying the lightest product, and subjecting it to sixteen fractional distillations, a body was obtained, which boiled between 183° and 184°, solidified on cooling, and had the properties of phenol, which an analysis proved it to be. On distilling the second and largest fraction with zinc-dust and purifying and fractioning the distillate, two products were obtained; the one, boiling between 110° and 112°, proved on analysis to be toluol, $\mathrm{Ȼ}_7H_8$, or  $\mathrm{Ȼ}_6H_4\left\{\begin{matrix}\mathrm{Ȼ}H_3\\ H\end{matrix}\right.$. Since the zinc-dust acts by reducing hydroxyl to hydrogen, the body yielding this toluol must have been  $\mathrm{Ȼ}_6H_4\left\{\begin{matrix}\mathrm{Ȼ}H_3\\ \Theta H\end{matrix}\right.$ or cresol. The other portion boiling at 150° to 155° afforded the properties and composition of anisol. As this anisol  $\mathrm{Ȼ}_6H_4\left\{\begin{matrix}H\\ \Theta\mathrm{Ȼ}H_3\end{matrix}\right.$ does not exist in the creosote as such, it must have been produced by a similar action of the zinc-dust, from the body  $\mathrm{Ȼ}_6H_4\left\{\begin{matrix}\Theta H\\ \Theta CH_3\end{matrix}\right.$ which is guaiacol, the acid methyl

ether of pyrocatechin $Є_6H_4\left\{\begin{matrix}ΘH\\ΘH\end{matrix}\right.$. That the fraction boiling between 200° and 203° was thus composed, Marasse further proved by fusing it with potassium hydrate. Two liquids were thus obtained which on examination proved to be cresol itself $Є_6H_4\left\{\begin{matrix}ЄH_3\\ΘH\end{matrix}\right.$, and pyro-catechin $Є_6H_4\left\{\begin{matrix}ΘH\\ΘH\end{matrix}\right.$, the latter produced by the saponification of its ether, guaiacol, in the experiment. The same result was reached by acting upon this fraction with hydriodic acid; cresol and pyro-catechin being produced as before. And finally, by acting upon this fraction with methyl iodid and potassium hydrate in sealed tubes, the methyl ethers of both cresol (cresyl-anisol $Є_5H_4\left\{\begin{matrix}ЄH_3\\ΘЄH_3\end{matrix}\right)$ and guaiacol ($Є_6H_4\left\{\begin{matrix}ΘЄH_3\\ΘЄH_3\end{matrix}\right)$ were obtained. The last fraction, boiling between 217° and 220°, afforded, after reduction with hydriodic acid and fractioning, phlorol, $Є_8H_{10}Θ$ or $Є_6H_3\left\{\begin{matrix}ЄH_3\\ЄH_3\\ΘH\end{matrix}\right.$ and homo-pyro-catechin $Є_6H_3\left\{\begin{matrix}ЄH_3\\ΘH\\ΘH\end{matrix}\right.$, which last substance was derived from creosol, its acid methyl ether $Є_6H_3\left\{\begin{matrix}ЄH_3\\ΘЄH_3\\ΘH\end{matrix}\right.$ precisely as pyro-catechin was in the previous fraction, from guaiacol. Marasse hence concludes that Rhenish beech-wood creosote is a mixture of compounds belonging to two parallel series, the phenols and the acid methyl ethers of pyro-catechin and its homologues. And since the first members of the series do not coincide in boiling point, the first member of the guaiacol series agreeing with the second member of the phenol series, it is obvious that that portion of creosote which boils at the lowest temperature will consist of the first member of the phenol series, i. e., phenol itself.

	Phenol Series.	Boiling point.		Guaiacol Series.	Boiling point.
Phenol,	$Є_6H_5(ΘH)$	184° C.		----------	----
Cresol,	$Є_6H_4\left\{\begin{matrix}ЄH_3\\ΘH\end{matrix}\right.$	203°	Guaiacol,	$Є_6H_4\left\{\begin{matrix}ΘH\\ΘЄH_3\end{matrix}\right.$	200° C.
Phlorol,	$Є_6H_3\left\{\begin{matrix}ЄH_3\\ЄH_3\\ΘH\end{matrix}\right.$	220°	Creosol,	$Є_6H_3\left\{\begin{matrix}ЄH_3\\ΘЄH_3\\ΘH\end{matrix}\right.$	219°

All the different kinds of beech-wood creosote appear to be identical in composition; those specimens having the highest boiling point, which contain the higher members of these parallel series.—*Ann. Ch. Pharm.*, clii, 59, Oct., 1869. G. F. B.

16. *On Ocean Currents, in relation to the Distribution of Heat over the Globe*, by James Croll of the Geological Survey of Scotland. (Phil. Mag., Feb. 1870.)—I. *The absolute Heating-power of Ocean-currents.*—* * * From an examination of the published sections [of the Gulf Stream] some years ago,* I came to the conclu-

---

* Philosophical Magazine for February, 1867, p. 127.

sion that the total quantity of water conveyed by the stream is probably equal to that of a stream 50 miles broad and 1000 feet deep,* flowing at the rate of four miles an hour, and that the mean temperature of the entire mass of moving water is not under 65° at the moment of leaving the Gulf. I think we are warranted to conclude that the stream, before it returns from its northern journey, is on an average cooled down to at least 40°; consequently it loses 25° of heat. Each cubic foot of water, therefore, in this case carries from the tropics for distribution upwards of 1500 units of heat, or 1,158,000 foot-pounds. According to the above estimate of the size and velocity of the stream, 5,575,680,000,000 cubic feet of water are conveyed from the Gulf per hour, or 133,816,320,000,000 cubic feet daily. Consequently the total quantity of heat transferred from the equatorial regions per day by the stream amounts to 154,959,300,000,000,000,000 foot-pounds.

From observations made by Sir John Herschel and by M. Pouillet on the direct heat of the sun, it is found that, were no heat absorbed by the atmosphere, about 83 foot-pounds per second would fall upon a square foot of surface placed at right angles to the sun's rays.† Mr. Meech estimates that the quantity of heat cut off by the atmosphere is equal to about 22 per cent of the total amount received from the sun. M. Pouillet estimates the loss at 24 per cent. Taking the former estimate, 64·74 foot-pounds per second will therefore be the quantity of heat falling on a square foot of the earth's surface when the sun is in the zenith. And were the sun to remain stationary in the zenith for twelve hours, 2,796,768 foot-pounds would fall upon the surface.

It can be shown that the total amount of heat received upon a unit surface on the equator during the twelve hours from sunrise till sunset at the time of the equinoxes is to the total amount which would be received upon that surface, were the sun to remain in the zenith during those twelve hours, as the diameter of a circle to half its circumference, or as 1 to 1·5708. It follows, therefore, that a square foot of surface on the equator receives from the sun at the time of the equinoxes 1,780,474 foot-pounds daily, and a square mile 49,636,750,000,000, foot-pounds daily. But this amounts to only $\frac{1}{3121870}$ part of the quantity of heat daily conveyed from the tropics by the Gulf-stream. In other words, the Gulf-stream conveys as much heat as is received from the sun by 3,121,870 square miles at the equator. The amount thus conveyed is equal to all the heat which falls upon the globe within 63 miles on each side of the equator. According to calculations made by Mr. Meech,‡ the annual quantity of heat received by a unit surface on the frigid zone, taking the mean of the whole zone, is $\frac{5\cdot45}{12}$ of

---

* The Gulf-stream at the narrowest place examined by the Coast Survey, and the place where its velocity was greatest, was found to be over 30 statute miles broad and 1950 feet deep. But we must not suppose this represents all the warm water which is received by the Atlantic from the equator; a great mass of water flows into the Atlantic without passing through the Straits of Florida.

† Trans. of Roy. Soc. of Edinb., vol. xxi, p. 57. Phil. Mag., S. 4, vol. ix, p. 36.

‡ Smithsonian Contributions to Knowledge, vol. ix.

that received at the equator; consequently the quantity of heat conveyed by the Gulf-stream in one year is equal to the heat which falls on an average on 6,873,800 square miles of the arctic regions. The frigid zone or arctic regions contain 8,130,000 square miles. There is actually, therefore, nearly as much heat transferred from tropical regions by the Gulf-stream as is received from the sun by the entire arctic regions, the quantity conveyed by the stream to that received from the sun by those regions being as 15 to 18.

But we have been assuming in our calculations that the percentage of heat absorbed by the atmosphere is no greater in polar regions than it is at the equator, which is not the case. If we make due allowance for the extra amount absorbed in polar regions in consequence of the obliqueness of the sun's rays, the total quantity of heat conveyed by the Gulf-stream will probably nearly equal the amount received from the sun by the entire arctic regions.

If we compare the quantity of heat conveyed by the Gulf-stream with that conveyed by means of aërial currents, the result is equally startling. The density of air to that of water is as 1 to 770, and its specific heat to that of water is as 1 to 4·2; consequently the same amount of heat that would raise 1 cubic foot of water 1° would raise 770 cubic feet of air 4°·2, or 3234 cubic feet 1°. The quantity of heat conveyed by the Gulf-stream is therefore equal to that which would be conveyed by a current of air 3234 times the volume of the Gulf-stream, at the same temperature and moving with the same velocity. Taking, as before, the width of the stream at 50 miles, and its depth at 1000 feet, and its velocity at 4 miles an hour, it follows that, in order to convey an equal amount of heat from the tropics by means of an aërial current, it would be necessary to have a current about 1¼ mile deep, and at the temperature of 65°, blowing at the rate of four miles an hour from every part of the equator over the northern hemisphere towards the pole. If its velocity were equal to that of a good sailing-breeze, which Sir John Herschel states to be about twenty-one miles an hour, the current would require to be above 1200 feet deep. A greater quantity of heat is probably conveyed by the Gulf-stream alone from the tropical to the temperate and arctic regions than by all the aërial currents which flow from the equator.

* * * * *

The anti-trades or upper return-currents, as we have seen, bring no heat from the tropical regions. After traversing some 2000 miles in a region of extreme cold they descend on the Atlantic as a cold current, and there absorb the heat and moisture which they carry to northeastern Europe. Those aërial currents derive their heat from the Gulf-stream, or if it is preferred, from the warm water poured into the Atlantic by the Gulf-stream. How, then, are these winds heated by the warm water? The air is heated in two ways, viz: by direct *radiation* from the water, and by *contact* with the water. Now, if the Gulf-stream continued a narrow and deep current during its entire course similar to what it is at the Straits of Florida, it could have little or no opportunity of communicating its heat to the air either by radiation or by contact. If the stream was only about 40 or 50 miles in breadth, the aërial particles in their passage across it would not be in contact with warm water more than an hour or two. Also the number of the particles in contact with the water, owing to the narrowness of the stream, would be small, and there would therefore be little opportunity for the air becoming heated by contact. The same also holds true in regard to radiation. The more we widen the stream and increase its area, the more we increase its radiating surface; and the greater the radiating surface, the greater is the quantity of heat thrown off. But this is not all; the number of aërial particles heated by radiation increases in proportion to the area of the radiating surface; consequently the wider the area over which the waters of the Gulf-stream are spread, the more effectual will the stream be as a heating-agent. And, again, in order that a very wide area of the Atlantic may be covered with the warm waters of the stream, slowness of motion is essential. * * *

The quantity of heat conveyed by the Gulf-stream, as we have seen, is equal to all the heat received from the sun by 3,121,870 square miles at the equator. Mr. Findlay, however, as has been stated, thinks that I have doubled the actual volume of the stream. Assuming that I have done so, the amount of heat carried by the stream would still be equal to all the heat received from the sun by 1,560,935 square miles at the equator. The mean annual quantity of heat received from the sun by the temperate regions per unit surface is to that received by the equator as 9·83 to 12.* Consequently the quantity of heat conveyed by the stream, taking Mr. Findlay's estimate of its volume, is equal to all the heat received from the sun by 2,062,960 square miles of the temperate regions. The total area of the Atlantic from the latitude of the Straits of Florida, 200 miles north of the tropic of Cancer, up to the Arctic Circle, including also the German Ocean, is about 8,500,000 square miles. In this case the quantity of heat carried by the Gulf-stream into the Atlantic through the Straits of Florida, to that received by this entire area from the sun, is as 1 to 4·12, or in round numbers as 1 to 4. It therefore follows that one-fifth of all the heat possessed by the waters of the Atlantic over that area, even supposing that they absorb every ray that falls upon them, is derived from the Gulf stream. Would those who call in question the efficiency of the Gulf-stream be willing to admit that a decrease of one-fourth in the total amount of heat received from the sun, over the entire area of the Atlantic from within 200 miles of the tropical zone up to the arctic region, would not sensibly affect the climate of Northern Europe? If they would not willingly admit this, why, then, contend that the Gulf-stream does not affect climate? for the stoppage of the Gulf-stream, taking it at Mr. Findlay's estimate, would deprive the Atlantic of 77,479,650, 000,000,000,000 foot-pounds of energy in the form of heat per day, a quantity equal to one-fourth of all the heat received from the sun by that area.

---

* See Smithsonian Contributions to Knowledge, vol. ix.

Were the sun extinguished, the temperature over the whole earth would sink to nearly that of stellar space, which, according to the investigations of Sir John Herschel* and of M. Pouillet,† is not above —239° F. Were the earth possessed of no atmosphere, the temperature of its surface would sink to exactly that of space, or to that indicated by a thermometer exposed to no other heat-influence than that of radiation from the stars. But the presence of the atmospheric envelope would slightly modify the conditions of things; for the heat from the stars (which of course constitutes what is called the temperature of space) would, like the sun's heat, pass more freely through the atmosphere than the heat radiated back from the earth, and there would in consequence of this be an accumulation of heat on the earth's surface. The temperature would therefore stand a little higher than that of space; or, in other words, it would stand a little higher than it would otherwise do were the earth exposed in space to the direct radiation of the stars without the atmospheric envelope. But, for reasons which will presently be stated, we may in the mean time, till further light is cast upon this matter, take —239° F. as probably not far from what would be the temperature of the earth's surface were the sun extinguished.

Suppose, now, that we take the mean annual temperature of the Atlantic at, say, 56°.‡ Then 239°+56°=295° represents the number of degrees of rise due to the heat which it receives. In other words, it takes all the heat that the Atlantic receives to maintain its temperature 295° above the temperature of space. Stop the Gulf-stream, and the Atlantic would be deprived of one-fifth of the heat which it possesses. Then, if it takes five parts of heat to maintain a temperature of 295° above that of space, the four parts which would remain after the stream was stopped would only be able to maintain a temperature of four-fifths of 295° or 236° above that of space: the stoppage of the Gulf-stream would therefore deprive the Atlantic of an amount of heat which would be sufficient to maintain its temperature 59° above what it would otherwise be, did it depend alone upon the heat received directly from the sun. It does not, of course, follow that the Gulf-stream actually maintains the temperature 59° above what it would otherwise be were there no ocean-currents, because the actual heating-effect of the stream is neutralized to a very considerable extent by cold currents from the arctic regions. But 59° of rise represent its actual power; consequently 59°, minus the lowering effect of the cold currents, represent the actual rise. What the rise may amount to at any particular place must be determined by other means. * * *

---

* 'Meteorology.' Section 36.

† Comptes Rendus, July 9, 1838. Taylor's Scientific Memoirs, vol. ix, p. 44, (1846).

‡ The mean temperature of the Atlantic between the tropics and the arctic circle, according to Admiral FitzRoy's chart, is about 60°. But he assigns far too high a temperature for latitudes above 50°. It is probable that 56° is not far from the truth.

At present there is a difference merely of 80° between the mean temperature of the equator and the poles;* but were each part of the globe's surface to depend alone upon the direct heat which it receives from the sun, there ought, according to theory, to be a difference of more than 200°. The annual quantity of heat received at the equator to that received at the poles, supposing the proportionate quantity absorbed by the atmosphere to be the same in both cases, is as 12 to 4·98, or, say, as 12 to 5. Consequently if the temperatures of the equator and the poles be taken as proportionate to the absolute amount of heat received from the sun, then the temperature of the equator above that of space must be to that of the poles above that of space as 12 to 5. What ought, therefore, to be the temperatures of the equator and the poles, did each place depend solely upon the heat which it receives directly from the sun? Were all ocean and aërial currents stopped, so that there could be no transference of heat from one part of the earth's surface to the other, what ought to be the temperatures of the equator and the poles? We can at least arrive at a rough estimate on this point. If we diminish the quantity of warm water conveyed from the equatorial regions to the temperate and arctic regions, the temperature of the equator will begin to rise and the temperature of the poles to sink. It is probable, however, that this process would affect the temperature of the poles more than it would do that of the equator; for as the warm water flows from the equator to the poles, the area over which it is spread becomes less and less. But as the water from the tropics has to raise the temperature of the temperate regions as well as the polar, the difference of effect at the equator and poles might not, on that account, be so very great. Let us take a rough estimate. Say that, as the temperature of the equator rises one degree, the temperature of the poles sinks about one degree and a half. The mean annual temperature of the globe is about 58°. The mean temperature of the equator is 80°, and that of the poles 0°. Let ocean and aërial currents now begin to cease, the temperature of the equator begins to rise and the temperature of the poles to sink. For every degree that the equator rises the poles sink $1\frac{1}{2}$°; and when the currents are all stopped and each place dependent alone upon the direct rays of the sun, the mean annual temperature of the equator above that of space will be to that of the poles, above that of space, as 12 to 5. When this proportion is reached, the equator will be 374° above that of space, and the poles 156°; for 374 is to 156 as 12 is to 5. The temperature of space we have seen to be —239°, consequently the temperature of the equator will in this case be 135°, reckoned from the zero of the Fahrenheit thermometer, and the poles 83° below zero. The equator would therefore be 55° warmer than at present, and the poles 83° colder. The difference between the temperature of the equator and the poles will in this case amount to 218°.

---

* The mean temperature of the equator, according to Dove, is 79°·7, and that of the north pole 2°·3. But as there is of course some uncertainty regarding the actual mean temperature of the poles, we may take the difference in round numbers at 80°.

Now, if we take into account the quantity of positive energy in the form of heat carried by warm currents from the equator to the temperate and polar regions, and also the quantity of negative energy (cold) carried by cold currents from the polar regions to the equator, we shall find that they are sufficient to reduce the difference of temperature between the poles and the equator from 218° to 80°.

The quantity of heat received in the latitude of London, for example, to that received at the equator is about as 12 to 8. This, according to theory, should produce a difference of about 125°. The temperature of the equator above that of space, as we have seen, would be 374°. Therefore 249° above that of space would represent the temperature of the latitude of London. This would give 10° as its temperature. The stoppage of all ocean and aërial currents would thus increase the difference between the equator and the latitude of London by about 85°. The stoppage of ocean-currents would not be nearly so much felt, of course, in the latitude of London as at the equator and the poles, because, as has been already noticed, in all latitudes midway between the equator and the poles the two sets of currents to a considerable extent compensate each other; viz. the warm currents from the equator raise the temperate, while the cold ones from the poles lower it; but as the warm currents chiefly keep on the surface and the cold return-currents are principally under-currents, the heating effect very greatly exceeds the cooling effect. Now, as we have seen, the stoppage of all currents would raise the temperature of the equator 55°; that is to say, the rise at the equator alone would increase the difference of temperature between the equator and that of London by 55°. But the actual difference, as we have seen, ought to be 85°; consequently the temperature of London would be lowered 30° by the stoppage of the currents. For if we raise the temperature of the equator 55° and lower the temperature of London 30°, we then increase the difference by 85°. The normal temperature of the latitude of London being 40°, the stoppage of all ocean and aërial currents would thus reduce it to 10°. But the Gulf-stream raises the actual mean temperature of London 10° above the normal. Consequently 30°+10°=40° represents the actual rise at London due to the influence of the Gulf-stream over and above all the lowering effects resulting from arctic currents. On some parts of the American shores on the latitude of London, the temperature is 10° below the normal. The stoppage of all ocean and aërial currents would therefore lower the temperature there only 20°. * * *

[The author next shows that the climate of the arctic must be affected by the Gulf-stream; and also that the low temperature of the southern hemisphere is owing to the Ocean-currents].

Without Ocean-currents the Globe would not be habitable.—All these foregoing considerations show to what an extent the climatic condition of our globe is due to the thermal influences of ocean-currents.

As regards the northern hemisphere, we have two immense oceans, the Pacific and the Atlantic, extending from the equator to near the north pole, or perhaps to the pole altogether. Between these two oceans lie two great continents, the eastern and the western. Owing to the earth's spherical form. by far too much heat is received at the equator and by far too little at high latitudes to make the earth a suitable habitation for sentient beings. The function of these two great oceans is to remove the heat from the equator and carry it to temperate and polar regions. Aërial currents could not do this. They might remove the heat from the equator, but they could not, as we have already seen, carry it to the temperate and polar regions; for the greater portion of the heat which aërial currents remove from the equator is dissipated into stellar space: the ocean can alone convey the heat to distant shores. But aërial currents have a most important function; for of what avail would it be, though ocean-currents should carry heat to high latitudes, if there were no means of spreading the heat thus conveyed over the land? The function of aërial currents is to do this. Upon this twofold arrangement depends the thermal condition of the globe. Exclude the waters of the Pacific and the Atlantic from temperate and polar regions and place them at the equator, and nothing now existing on the globe could live in high latitudes.

Were these two great oceans placed beside each other on one side of the globe, and the two great continents placed beside each other on the other side, the northern hemisphere would not then be suitable for the present order of things: the land on the central and on the eastern side of the united continent would be by far too cold. * * *

II. GEOLOGY.

1. *Sun-Pictures of Rocky Mountain Scenery;* by Dr. F. V. HAYDEN, Prof. of Mineralogy and Geology in the University of Pennsylvania.—We have been permitted to make the following extracts from a work of the above title, by Dr. Hayden, which is soon to be published by Julius Bien of New York.* The volume will contain thirty photographic views along the line of the Pacific railroad from Omaha to Sacramento, and besides a description of the geographical and geological features of the country, by Dr. Hayden, whose researches over the Rocky Mountains have often been chronicled in this Journal. We understand that the views selected for publication are generally those that illustrate the geographical and geological features of the region, and are interesting to the student of science as well as to the artist and the lovers of the picturesque in nature.

Communications on the Fossil Plants, Mammals, Fishes, Insects, &c., have been contributed by Messrs. Newberry, Leidy, Cope, and

---

* The work will be issued to subscribers only, at $25 per copy, and subscriptions should be addressed to Julius Bien, lithographer, 16 and 18 Park Place, New York City.

Scudder, and our extracts are from these papers, and from some of the notes of Dr. Hayden, furnished us by him.

Photograph No. 9, illustrates a peculiar feature of the surface near the junction of Hilter Creek with Green river. The formations are composed of thin layers of fine sand, clay and sandstones or chalky limestones readily disintegrating on the surface, and, as it were, breaking down, so that the water has worn an almost unlimited series of furrows or small ridges with considerable uniformity; sometimes on the summits of the hills or ridges harder portions are left, which wear into castellated forms. This peculiar appearance of the surface is not uncommon all over the west where the Cretaceous and Tertiary formations prevail, especially where they are composed of rather soft clays and sands. The black plastic clays of the Cretaceous beds on the Upper Missouri are subject to this style of weathering, as well as much of the country usually termed the "Bad Lands."

This appearance of the surface carries with it, also, the aspect of desolation. There is little vegetation but the wild sage, Chenopodiaceous shrubs, and other plants which love the alkaline soils. Little depressions occur here and there in which the surface waters accumulate in wet weather; but in the dry season the water evaporates and the surface is left with a thick incrustation of salts of soda, magnesia, &c. I have called the formation along Green river, the Green river shales, from the fact that the sediments are arranged in regular layers, mostly thin like shale, varying, however, in thickness, from that of a knife blade to several feet. This laminated character with the slight variations in color give to the hills the peculiar branded appearance as shown in Photographs 11 and 12 of "Citadel Rock" and "Castle Rock."

Photograph No. 10, illustrates a cut along the railroad through thin layers of a sort of cream-colored chalky limestone. Some of the layers are of a dark brown color and so saturated with petroleum that they burn with a good deal of freedom. This cut is usually called the "Burning Rock cut" from the fact that during the progress of the work, the men built a fire by the side of one of the walls, and the rocks ignited, burning for some days, illuminating the labors of the workmen by night, and filling the valley with a dense smoke by day. In the distance may be seen the banks of Green river formed of similar rocks, which are made up for the most part of silica, lime, and some clay, excellent material for the preservation of organic remains. Besides, all the rocks are more or less impregnated with the oily substance which no doubt originated from the vast quantities of animals which existed in this lake, the remains of which are found in the greatest abundance every where. One of these excavations along the railroad bears the name of the "Petrified Fish Cut," on account of the thousands of beautiful and perfect impressions of fishes which are shown on the surface of the thin slabs or layers of rock. Sometimes a dozen or two of these little herring are found on an area of a square foot. Insects, water plants, and a remarkable specimen of a *feather of a bird* has been found here. The feather Prof. Marsh regards as a unique specimen, forming a most interesting addition to the bird remains of North America. It is the distal portion of a large feather, with the shaft and vane in such excellent preservation that it may perhaps indicate approximately the nature of the bird to which it belonged.

My collection of *fossil fishes* from these shales was very large, and my success was mostly due to the kind aid of Mr. A. W. Hilliard, a gentleman of intelligence, who superintended the excavations on the line of the railroad, and preserved from time to time such specimens of value as came to his observation.

Prof. E. D. Cope, a distinguished comparative anatomist of Philadelphia, has kindly prepared the following *account of the petrefied fish remains* which were submitted to him for examination, especially for this work. Prof. Cope says:

"The fishes placed in my hands for determination by you, consist of four species, viz: one Acanthopterygian, *Asineops squamifrons* Cope, and three Malacopterygians, *Clupea humilis* Leidy, *Clupea pusilla* Cope, *Cyprinodon levatus* Cope. Those named by the writer were not previously known, and the Asineops represents also a genus not before brought to the notice of scientific men. In ordinary language the last mentioned fish is a perch, but in no degree similar to the white and yellow brought to our markets. The nearest resemblance in general structure is to be found in the black bass of the Ohio and Tennessee rivers, but a closer similarity in form exists in the Red Eye or Goggled Eyed perch of western and southern rivers, Ambloplites of Naturalists. Zoologically it is not very nearly related to either, for it combines with some of their characters, others now existing only in marine fishes of other families. It is an aberrant form of the family of Chætodons, which embraces marine fishes only, and which chiefly abound at the present time in the Indian and Pacific Oceans. But the form and proportions of its fins and scales remind one very much of the swamp and the tide water sun perch of New Jersey, (*Acantharchius pomotis* Baird, *Bryttus* sp. etc.), and suggest a similarity of habit. The teeth are fine and the dentition brush-like as in the fresh water and many marine perch, and its food was probably much like theirs. As far as zoological evidence goes, there is nothing to indicate whether this species belonged to fresh or salt water; its unarmed character constitutes a pecularity much more prevalent among fresh water than marine fishes, while its zoological affinities, so far as known, are altogether with marine forms. In size this fish exceed the red eye, and was less than the black bass, averaging about as the yellow perch.

"The Clupeas are herring of small species, considerably less than the herring of our coasts. One of the blocks contains the remains of two small shoals of the fry, probably of *Clupea humilis* which were caught suddenly by a slide or fall of calcareous mud, and entombed for the observation of future students. They must have been taken unawares, since they lie with their heads all in one direction as they swam in close bodies. One or two may have had a moment's warning of the catastrophe, as they have turned a little aside, but they are the exceptions. The fry are from one-half to three quarters of an inch long and upwards.

"The herring, or those without teeth, are chiefly marine, but they run into fresh water to deposit their spawn in the spring of the year, and then return to salt waters. The young run down to the sea in autumn and remain there till old enough to spawn. The size of the fry of the Rocky Mountain herring indicates that they had not long left their spawning ground, while the abundance of adults suggests that they were not far from salt water, their native element. To believe then that the locality from which the specimens were taken, was neither far from fresh nor far from salt water, is reasonable, and this points to a tide or brackish inlet or river. Lastly, the species of Cyprinodon inhabit also tide and brackish waters. Most of the species of the family, as well as of the genus, are inhabitants of fresh water, but they generally, especially the Cyprinodons proper, prefer still and muddy localities, and often occur in water really salt. This habitat distinguishes them especially from Cyprinidæ (minnows and suckers) and Pike. The materials which compose the shales indicate quiet water, and not such as is usually selected by herring for spawning in, while the abundance of adult Clupeas indicate the proximity of salt water. This is far from a satisfactory demonstration of the nature of the water which deposited this mass of shales, but it is the best that can be attained with such a meagre representation of species.

"As to geological age, the indications are rather more satisfactory. The genus Clupea ranges from the Upper Eocene upward, being abundant in the slates of Lebanon and Monte Bolca, while Cyprinodon has been found in neither, but first appears in the middle or lower Miocene in Europe. The Asineops resemble very closely, and I believe essentially, the Pygaeus of Agassiz, of Eocene age, from Monte Bolca. The peculiarities presented by the genus found by Dr. Hayden are of such small significance as to lead me to doubt the beds in question being of later than Eocene in age, though the evidence rests chiefly on this single new and peculiar genus.

"The position of these fishes, 7,000 feet above the level of the sea, furnishes another illustration of the extent of the elevations of regions once connected with the ocean, and the comparatively late period of geological time at which, in this case, this elevation took place."

The *fossil Insects* were examined by Mr. SAMUEL H. SCUDDER, of Boston, and he has kindly prepared the following short but very interesting note. The insects were found in the same locality with the fishes.

"The fossil insects found by Dr. Hayden in the tertiary shales of Green river, belong to three species, one being an ant, the others flies. The ant is rather poorly preserved and must he examined with great care before its precise character can be determined.

The larger fly, of which we can distinguish almost the whole of the body, though but little of the wings, evidently belonged to some species of *Syrphidæ*; it is nearly three quarters of an inch in length and seems to have had a bright colored abdomen, banded with black. In their perfect state, all of this family are very fond of flowers, but the typical species are particularly interesting from the peculiar habits of the larvæ; these are footless grubs which feed on plant lice, piercing them one by one, and sucking out their juice; it is more likely, however, that our species belonged to one of the genera whose larvæ live in the water, or about decaying vegetable substances; larvæ, which from their size and general form might well produce such a fly as this, were found abundantly by Professor Denton; they evidently inhabited the water.

The other fly, judging from the neuration of the wings, which is pretty well preserved, seems to belong to the great family of *Muscidæ*, of which the common house fly is an example; it may probably be referred to a section in which they are nearly or quite wanting, and where the larvæ ordinarily feed upon dead animal matter or upon decomposing plants. Belonging to the same group and perhaps already allied, are the species of *Ephydra* which live in salt marshes, and frequent salt pans to such an extent as to be very troublesome; they have been discovered in the saline waters of some Nevadan lakes, and Professor Denton states that dipterous larvæ, probably of a similar kind, are found in great numbers in lakes impregnated with petroleum. It is probable that the shales, in which these remains occur, were deposited in such a lake."

About a mile west of the "Petrified Fish Bed," is a cut along the railroad which passes through a moderate thickness of buff, chalky limestone filled with impressions of leaves of deciduous trees. These rocks hold a position about one hundred feet above the petroleum shales which contain the fish remains, and therefore the date of their existence may be regarded as somewhat subsequent, though belonging to the same basin. Prof. J. S. Newberry has given these plants a partial examination, and communicated the following interesting notes in the form of a letter.

"I have examined the plants from the Green River beds with as much care as the limited time at my command would permit, and am surprised in not finding among them a single species contained in any of your other great collections at the far west. They, thus far, afford no certain criteria for collating the Green River Tertiaries with those of other localities where you have studied them. The plants from the rocks enclosing the coal at Marshall's mine are more significant, as they include species (*Platanus Haydeni*, which is certainly different from *P. aceroides*), such as were found by you at Carbon Station, and at the mouth of the Yellow Stone. Every collection of fossil plants received from the Tertiary of the west brings to light many new species, and the great diversity which they exhibit proves either a number of plant-bearing horizons, or great localization of the species in the Tertiary flora.

"Among your Green River plants, are only some half dozen species, so well preserved as to be capable of satisfactory identification or comparison, but they form a very interesting group. Among them I find two palms, both quite unlike anything before found on this continent. One is a new *Phenicites*, resembling Heer's *Manicaria formosa*. The other but an imperfect fragment, is yet altogether new and strange to me. The most abundant species contained in the collection is a *Magnolia* allied to *M. terminervis* Lesqx., but more elongate and acute—also an oak resembling *Quercus Saffordi* Lesqx. There is another oak in the collection, a laurel (probably), fragments of two ferns too imperfect for determination. On the whole, these plants resemble most those described by Lesquereux from Mississippi, and I am inclined to suspect that they are of the same age. This would make the Green River beds older than you have thought them, and I should want more material before venturing anything more than a suggestion to that effect. I trust you will be able to make other collections from these plant-beds during the present season.

"The specimens contained in the bluff marly limestones of the Green River series are generally not well preserved, and yet I think careful search at the locality where these plants sent me were obtained would result in the discovery of some fine things. I would especially urge a search for fruits.

"The aspect of the small group of plants now before me from Green River, is more tropical than any you have brought from the west, and, as we have reason to believe that our Eocene climate was warmer than the Miocene, and that from the Eocene epoch to the Glacial period a progressive depression of temperature took place, the Green River beds would seem to be of early rather than of late Miocene."

2. *On the Graphite of the Laurentian of Canada;* by J. W. Dawson. (Jour. Geol. Soc., 1869, 112).—In this paper Dr. Dawson sustains the view that the graphite of the Canada Laurentian is of organic origin, and shows that the amount of "graphite in the Lower Laurentian Series is enormous." A limestone in the township of Buckingham on the Ottawa, which is 600 feet or more thick, with some three intercalated bands of gneiss, is in some parts *one-fourth* graphite, and the whole is not less than 20 or 30 per cent graphite. In the adjoining township of Lochaber, a band of limestone 25 to 30 feet thick is so reticulated with graphite, that it is mined for it; and another bed in the same district, 10 to 12 feet thick, yielding 20 per cent of the pure material, is worked. It occurs in equal abundance at other horizons through beds of limestone which have, according to Logan, an aggregate thickness of 3500 feet. In view of the facts Dr. Dawson adds "it is scarcely an exaggeration to maintain that the quantity of carbon in the Laurentian is equal to that in similar areas of the Carboniferous system."

On the mode of occurrence and origin the author observes:

"The beds of graphite near St. John, some of those in the gneiss at Ticonderoga in New York, and at Lochaber and Buckingham and elsewhere in Canada are so pure and regular that one might fairly compare them with the graphitic coal of Rhode Island. These instances, however, are exceptional, and the greater part of the disseminated and vein graphite might rather be compared in its mode of occurrence to the bituminous matter in bituminous shales and limestones. We may compare the disseminated graphite to that which we find in those districts of Canada in which Silurian and Devonian bituminous shales and limestones have been metamorphosed and converted into graphitic rocks not dissimilar to those in the less altered portions of the Laurentian.* In like manner it seems probable that the numerous reticulating veins of graphite may have been formed by the segregation of bituminous matter into fissures and planes of least resistance, in the manner in which such veins occur in modern bituminous limestones and shales. Such bituminous veins occur in the Lower Carboniferous limestone and shale of Dorchester and Hillsborough, New Brunswich, with an arrangement very similar to that of the veins of graphite; and in the Quebec rocks of Point Levi, veins attaining to a thickness of more than a foot are filled with a coaly matter having a transverse columnar structure and regarded by Logan and Hunt as an altered bitumen. These paleozoic analogies would lead us to infer that the larger part of the Laurentian graphite falls under the second class of deposits above mentioned, and that, if of vegetable origin, the organic matter must have been thoroughly disintegrated and bituminized before it was changed into graphite. This would also give a probability that the vegetation implied was aquatic, or at least that it was accumulated under water.

"Dr. Hunt has, however, observed an indication of terrestrial vegetation, or at least subaërial decay, in the great beds of Laurentian iron-ore. These, if formed in the same manner as more modern deposits of this kind, would imply the reducing and solvent action of substances produced in the decay of plants. In this case such great ore beds as that of Hull, on the Ottawa, 70 feet thick, or that near Newborough, 200 feet thick,† must represent a corresponding quantity of vegetable matter which has totally disappeared. It may be added that similar demands on vegetable matter as a deoxydizing agent are made by the beds and veins of metallic sulphids of the Laurentian, though some of the latter are no doubt of later date than the Laurentian rocks themselves."

He concludes as follows:

"We may sum up these facts and considerations in the following statements:—First, that somewhat obscure traces of organic structure can be detected in the Laurentian graphite; secondly, that the general arrangement and microscopic structure of the substance corresponds with that of the carbonaceous and bituminous matters in marine formations of more modern date; thirdly, that if the Laurentian graphite has been derived from vegetable matter,

---

* Granby, Melbourne, Owl's Head, &c., 'Geology of Canada,' 1863, p. 599.

† Geology of Canada, 1863.

it has only undergone a metamorphosis similar in kind to that which organic matter in metamorphosed sediment of later age has experienced; fourthly, that the association of the graphitic matter with organic limestone, beds of iron ore, and metallic sulphids greatly strengthens the probability of its vegetable origin; fifthly, that when we consider the immense thickness and extent of the Eozoonal and graphitic limestones and iron-ore deposits of the Laurentian, if we admit the organic origin of the limestone and graphite, we must be prepared to believe that the life of that early period, though it may have existed under low forms, was most copiously developed, and that it equalled, perhaps surpassed, in its results, in the way of geological accumulation, that of any subsequent period.

"In conclusion, this subject opens up several interesting fields of chemical, physiological and geological inquiry. One of these relates to the conclusions stated by Dr. Hunt as to the probable existence of a large amount of carbonic acid in the Laurentian atmosphere, and of much carbonate of lime in the seas of that period, and the possible relation of this to the abundance of certain low forms of plants and animals. Another is the comparison, already instituted by Professor Huxley and Dr. Carpenter, between the conditions of the Laurentian and those of the deeper parts of the modern ocean. Another is the possible occurrence of other forms of animal life than *Eozoon* and Annelids, which I have stated in my paper of 1864, after extensive microscopic study of the Laurentian limestones, to be indicated by the occurrence of calcareous fragments, differing in structure from *Eozoon*, but at present of unknown nature. Another is the effort to bridge over, by further discoveries similar to that of the *Eozoon bavaricum* of Gümbel, the gap now existing between the life of the Lower-Laurentian and that of the Primordial Silurian or Cambrian period. It is scarcely too much to say that these inquries open up a new world of thought and investigation, and hold out the hope of bringing us into the presence of the actual origin of organic life on our planet, though this may perhaps be found to have been Prelaurentian. I would here take the opportunity of stating that in proposing the name *Eozoon* for the first fossil of the Laurentian, and in suggesting for the period the name 'Eozoic,' I have by no means desired to exclude the possibility of forms of life which may have been precursors of what is now to us the dawn of organic existence. Should remains of still older organisms be found in those rocks now known to us only by pebbles in the Laurentian, these names will at least serve to mark an important stage in geological investigation.

3. *On Laurentian Rocks in Nova Scotia;* by T. STERRY HUNT, F.R.S.—The American Journal of Science for May, 1870, contains a paper by Prof. H. Y. Hind on the Geology of Nova Scotia, in which it is said that Dr. Honeyman, in the autumn of 1868, discovered in the Arisaig district, "syenites, diorites, and crystalline limestones, with serpentine, which he then supposed to be of Lau-

rentian age, as he informed me subsequently to the publication of my preliminary report on the Nova Scotia Laurentian. Specimens were sent to Montreal for examination, and instructions were given by Dr. Hunt, who also shared Dr. Honeyman's opinion, to the lapidary, to prepare sections of the serpentinous rock for microscopic examination. By some mischance this was neglected, and the specimens remained unexamined, and indeed forgotten until quite recently, as Dr. Hunt informs me, under date of Feb. 3, 1870. When submitted to the microscopic test the *Eozoon Canadense* was distinctly seen, and Dr. Dawson has confirmed the observations." Prof. Hind further says that the discovery of this fossil "enables geologists to recognize the truth of Dr. Honeyman's opinions, although, by accident, these opinions were not made known or confirmed until after the publication of my report." He again refers to "the just though tardy recognition of the correctness of Dr. Honeyman's views with reference to the age of the limestones and diorites of Arisaig." Pages 354, 355.

Prof. Hind has been deceived in this whole matter, and as he is now, and has been for some months absent in England, I feel called upon, in the interest of truth, to state the facts in the case. In the spring of 1869, Dr. Honeyman, previously employed as an explorer by the Geological Survey of Canada, showed me, in Montreal, a series of specimens collected by him the autumn previous, in the Arisaig district, and including besides syenites and diorites, crystalline limestones, sometimes mixed with a pale green serpentine. These were at once noticed by Mr. Murray of the Geological Survey of Newfoundland, who was present, and myself, as having a close lithological resemblance to the Laurentian rocks, and we mentioned the fact to Dr. Honeyman; while I at once suggested to the lapidary of the Survey, who was in the room, that they were so like the Eozoon-limestones of the Ottawa that it would be well to prepare slices for examination. Meanwhile Dr. Honeyman never made to myself, Sir William Logan, or Dr. Dawson, any suggestions as to the geological age or relations of the rocks in question, and in his official report, handed to Sir William some days later, not only neglects to mention the name of Laurentian, but forgets to make any allusion whatever to the diorites, limestones and serpentines in question. Farther, in summing up his report, he concludes that the district examined by him includes all the rocks between the coal-measures and the gold-bearing slates, thus, by implication, excluding anything lower in the geological series.

I see no reason to believe that the name of Laurentian, first applied by me to these rocks from Arisaig, in Dr. Honeyman's presence, conveyed to his mind, at the time, any notion of geological age, position, or succession, or that he attached any importance to the specimens in question, except as ornamental stones, until the appearance of Prof. Hind's first notice called his attention to the meaning of the term, and to the published descriptions of Laurentian rocks. Were it otherwise, his total silence on the subject from the autumn of 1868 to some time last winter, is simply incomprehensible.

To Prof. Hind belongs the credit of having declared that the old granitoid rocks of the region are clearly stratified gneisses, and at the base of the series in that region; as they are well known to be on the north side of the Bay of Fundy, where they have been described as Laurentian, and examined by Dr. Dawson, Matthew and Bailey. I had, however, sought in vain for Eozoon in the serpentine-limestones of New Brunswick, but on the receipt of Prof. Hind's preliminary notice, recalled the Arisaig specimens, and recognized in them a form of Eozoon, which, however, according to Dr. Dawson, is specifically distinct from *Eozoon Canadense.*

Montreal, June 7, 1870.

4. *The Ornithosauria: an elementary study of the bones of Pterodactyles;* by H. G. Seeley of St. John's College, Cambridge: with twelve plates. 132 pp. 8vo, 1870. (Deighton, Bell & Co., Cambridge; Bell & Dalby, London). *Index to the Fossil Remains of Aves, Ornithosauria, and Reptilia, from the Secondary system of strata, arranged in the Woodwardian Museum of the University of Cambridge.* By H. G. Seeley, of St. John's College, Cambridge; with a prefatory notice by the Rev. Adam Sedgwick, LL.D., F.R.S., Woodwardian Professor and Senior Fellow of Trinity College.—The author of these works, the assistant of Prof. Sedgwick at Cambridge, states in his preface that they are portions of the Catalogue of the Woodwardian Museum. This Museum, through the labors mainly of Prof. Sedgwick, is rich in remains of Reptilians from the lower Cretaceous, and is unsurpassed in those of the Cambridge upper Greensand, which has afforded large numbers of bones of Ornithosaurs (Pterosaurs). In the first of these works the able author, Mr. Seeley, reviews briefly what had before been done in connection with the subject of Pterosaurs, discusses their classification and relations, gives the details with regard to the various bones in the collections, and describes and names the several species to which they belong. The author finds that the pneumatic perforations in the bones (seen in the lower jaw, the whole vertebral column, the bones of the fore limb, the scapula and coracoid, the femur and tibia, etc.) are situated as in birds, and indicate a similar system of air-circulation from the lungs, and he argues that this implies the existence of a double heart as in birds. He also points out a relation to birds in the form of the brain, this organ having a very large cerebrum, and, as seen from above, a very small cerebellum abutting against it and pressing to either side the optic lobes (instead of having, as in ordinary Reptiles, the cerebellum behind separated from the cerebrum by the part called the optic lobes). Hence he concludes that the Ornithosaurs were hot-blooded, and makes of them a class of Vertebrates distinct from both Reptiles and Birds. The subject is illustrated by twelve plates.

5. *First Annual Report of the Geological Survey of Indiana, made during the year* 1869; by E. T. Cox, State Geologist, assisted by Prof. Frank H. Bradley, Dr. Rufus Hammond, and Dr. G. M. Levette. 240 pp. 8vo. Indianapolis, 1870.—This first annual Report of the Geological Survey of Indiana, under Prof. Cox, consists of a General Report by the head of the survey, and special Reports by Prof. Bradley on Vermillion Co., and Dr. Hammond on Franklin Co. Prof. Cox has begun a good work with reference to the coal formation, and has already proved that the number of coal beds, and the subdivisions of the formation, as laid down by Owen and others in Indiana and Kentucky, are wholly erroneous. As one example he shows that the "Mahoning Sandstone" and the "Anvil Rock Sandstone" are actually the same rock. Prof. Cox states that he is not yet prepared to present his own conclusions, but he shall make it a special object, in the course of the Survey, to ascertain the precise facts on this important subject. Much valuable information is presented by him respecting the coal formation and other rocks and products of Clay county. Prof. Bradley describes with considerable detail the Carboniferous rocks of Vermillion county, mentioning many of their fossils, and giving some account of the beds of iron ore and fire clay which they contain. He says that in this county the "boulder clay" has a depth near Perrysville of about 100 feet before reaching the underlying quicksand; and that in some places it is 125 feet thick. It contains boulders of limestone and of metamorphic rocks, which are sometimes striated, besides occasional rolled masses of galena and native copper. There is much in the volume we might quote, with interest to our readers, if space allowed. The report closes with a list of the Mammals and Birds of Franklin Co. It is illustrated by three maps, and a large plate of sections.

6. *Meek on Crinoids.*—The views of Mr. Meek on the anatomy of of the Paleozoic Crinoids, presented in a paper reprinted from the Proceedings of the Academy of Nat. Sciences of Philadelphia, in vol. xlviii of this Journal (at p. 23), are fully endorsed in a letter written by Prof. Sars of Norway, the profound investigator of species of recent Crinoids, a few days only before his death. Prof. Sars observes, judging from the photographs of the specimens which had reached him, that Mr. Meek's conclusions seem to him to be perfectly founded, and to result with logical necessity from the investigations;" and that "they spread unexpected light upon these curious extinct Crinoids." Letters from Prof. Wyville Thomson, of Belfast, and Dr. Lütken, of Copenhagen, also approve entirely of his conclusions.

7. *The lifted and subsided Rocks of America, with their influences on the oceanic, atmospheric, and land currents and the distribution of Races;* by Geo. Catlin. 228 pp., 12mo. London, 1870. (Trübner & Co.)—The writer of this work, well known for his travels among the American Indians, here treats of mountain drainage, upheavals, metamorphism, making of mountain chains, sinking of mountains, and of the Indian races of America. He

presents his geological views and criticisms with great positiveness, which is consistent with the fact of his limited knowledge of the subject.

8. *Geological Survey of Iowa.*—The State of Iowa has ordered the printing of three thousand copies of the Report of the Geology of the State by Prof. White. The work will consist of two royal octavo volumes, in the style of Hall's Geology of Iowa and of the Illinois Geological Report, and will be well illustrated. We regret to have to add that the Legislature has discontinued the Survey.

III. ZOOLOGY.

1. *Die bis jetzt bekannten Schildkröten, u. d. bei Kelheim u. Hannover neu aufgefunden altesten Arten derselben;* von Dr. G. A. Maack. Cambridge, Mass. (Cassel, 1869,—from H. von Meyer's Palæontographica).—This useful work, a 4to of 146 pp., is contributed by Dr. Maack, whose arrival in the United States and occupation in the Museum of Comparative Zoology, we take pleasure in noticing. It embraces a synopsis of the species of extinct Testudinata, arranged in the order of geological succession, rather than according to structural affinity. The number thus enumerated is 192, of which 26 are assigned to the Testudinidæ, 114 to the Emydidæ, including the Pleurodira; 27 to the Trionychidæ, and 25 to the Cheloniidæ. The species are not nearly all described, but their enumeration forms an invaluable hand-book to the student of the subject. The stratigraphical table given, adds to its value. From it we perceive that the Tertiaries embrace the majority of the species, and the Cretaceous and Jurassic periods successively fewer. The upper Jurassic of Switzerland, Bavaria and Hanover have furnished the oldest known Testidinata, unless the Chelytherium of the Würtembergian Trias belong to the order, a point still doubtful. The number of Jurassic species known was 15, to which Dr. Maack adds 3, based on remains mostly from Hanover, from a stratum of prior deposit to those of Switzerland and Bavaria.

Before noticing the types of Cretaceous and Jurassic Tortoises, it must be observed that the system of Strach which Dr. Maack adopts, is a very defective one, and far behind the requirements of modern zoology and paleontology. The structural features defining the suborders and families are overlooked in this. For example, one of the primary divisions of the order, the Pleurodira, is included among the Emydidæ, whereas it embraces a series of families distinguished by features quite similar to those defining the remaining families from each other.* In consequence several conclusions are reached which require modification. The genus *Platemys* as adopted may be cited. It embraces 9 species according to the present work, the genus Pleurosternum of Owen being referred to it. This is done because the additional pair of thoracic bones which characterizes it is found in a rudimental

---

* See Proc. Acad. Nat. Sci., 1868, p. 282.

condition in *Platemys Bowerbankii*, and *Platemys Bullockii* of Owen presents the intermarginal scuta of *Pleurosternum*, and because of the general resemblance in specific characters between the latter and the *Pl. concinnum*. To us, however, the genus *Pleurosternum* appears to be Cryptodire not Pleurodire, as it lacks the integular scutum of the latter suborder, and to represent a peculiar family of that group characterized by the possession of ten instead of eight sternal bones. *Platemys Bullockii*, *P. Bowerbankii* and *Emys lævis* Ow. and Bell appear on the other hand to be Pleurodira, and to be referable to two families of that suborder. The *Pl. Bullockii*, on account of its five pairs of sternal bones, to the Sternothæridæ, and on account of its intermarginal scuta, to a new genus which I have called *Digerrhum*. The last two, in their rudimental fifth pair of sternals resemble many Pleurodira, and cannot be distinguished from the genus *Podocnemis* now living in the Amazon. The *P. sulcatus* Leidy is near to Podocnemis also, but represents a distinct genus, characteristic of the Cretaceous, which I call *Taphrosphys;* there are six species in North America. After these deductions the only Platemydes that remain are *P. Mantellii* and *P. Dixonii* of Owen.

The new forms described by Maack are of much interest. His *Chelonides Wittei* is one of the group found in both Jurassic and Cretaceous strata in Europe and North America, which combines *Chelydroid* and *Chelonioid* characters so as to render it difficult to be assured as to which group they truly represent. The characters of the carapace in most, and of the plastron in many, are those of the latter, while those of the limbs, the crucial test in this case, are those of the former. Two of the North American genera add one or two costal bones, a character of importance and one not hitherto met with in the order; these may be regarded as the type of a peculiar family with the name of the *Propleuridæ*, including the genera *Osteopygis* and *Propleura*. The family with eight costals includes *Chelonides* Maack, which seems to be near *Chelonemys* Jourd., as he has placed it,—with *Platychelys*, *Hydropelta*, *Idiochelys* and some other European forms which, with *Catapleura* and *Lytoloma* from North America, are nearer *Chelydra*, and I cannot at present find characters which distinguish them as a family from the existing forms. In *Stylemys* Maack, the second new genus introduced into the present work, the sternum is without fontanelles, and resembles entirely that of *Adocus* from the American Cretaceous, while the carapace and femur are of the type of *Osteopygis*. Until further investigated it should remain as an *Emydoid*, as placed by Dr. Maack. Two species are described, *S. Lindenensis* and *S. Hannoveranus*. The use of the name *Stylemys* is a *faux pas*, since it must probably be used for a genus of *Emydidæ* described by Leidy from the Miocene and Pliocene of Nebraska and Dakota. True, it was originally established on untenable characters, and reunited by its proposer with *Testudo*. But I have been able to point out (Trans. Amer. Phil. Soc., 1869, 123) that the species so originally named are really *Emydidæ* having Testudinine characters, and requiring a distinct generic name, for which *Stylemys* should perhaps be adopted. Dr. Maack will therefore be necessitated to find another for that which he describes.

This and other rectifications relating to North American species not having been published prior to Dr. Maack's work, he was unable to take advantage of them. I therefore append the following list as supplementary to it.

Postpliocene Species.

Emydidæ.

Cistudo eurypygia Cope.
Emys petrolei Leidy.

Pliocene and Miocene Species.

Emydidæ.

Stylemys Nebrascensis Leidy.
S. Niobrarensis Leidy.

Trionychidæ.

Trionyx lima Cope.
T. Buiei Cope.

Cheloniidæ.

Puppigerus grandævus Leidy.

Eocene Species.

Trionychidæ.

? *Trionyx pennatus* Cope.

? *Cheloniidæ.*

Lembonax polemicus Cope.
Puppigerus parviscutatus Cope.

Cretaceous Species.

Emydidæ.

Emys turgidus Cope.
E. petrosus Cope.
E. firmus Leidy.
E. Stevensoni Leidy.
Adocus beatus Leidy.
Adocus agilis Cope.
A. pravus Leidy.
A. Vyomingensis Leidy.
A. pectoralis Cope. (*Pleurosternum* olim.)

Chelydridæ.

Lytoloma Jeanesii Cope.
L. angusta Cope.
Catapleura repanda Cope.
Pentresius ornatus Leidy.

Propleuridæ.

Euclastes platyops Cope.
Propleura sopita Leidy.
Osteopygis emarginatus Cope.
Osteopygis platylomus Cope.
O. chelydrinus Cope.

Trionychidæ.

Trionyx halophilus Cope.
T. priscus Leidy.
Trionyx guttatus Leidy.

Podocnemididæ.

Bothremys Cookii Leidy.
Taphrosphys molops Cope.
T. sulcatus Leidy.
T. longinuchus Cope.
Taphrosphys Leslianus Cope.
T. strenuus Cope.
T. nodosus Cope.

Thirty-eight species. *Puppigerus* embraces all of the Chelones described by Owen and Bell from Sheppey. To *Lytoloma* perhaps belong the two Chelones described and figured by Faujâs and Cuvier from the Cretaceous of Maestricht named *Ch. cretacea* by Keferstein and *Ch. Faujasii* by Maack. E. D. C.

2. *On Discosaurus and its allies;* by Dr. J. Leidy. (Proc. Acad. Nat. Sci. Philad., 1870, p. 18).—On the 5th of April, Dr. Leidy made remarks on Discosaurus and its allies, additional to those published at p. 392 of the last volume of this Journal; and in the course of it discusses the relations of *Cimoliasaurus*, as originally established, to *Discosaurus.* On this point he observes:

"In the first place, by comparison with the skeleton of the Kansas saurian, we observe that the position in the column assigned to the vertebral bodies of *Cimoliasaurus* was incorrect, and this probably contributed to mislead Prof. Cope in his examination of the skeleton of the Kansas saurian.

"The vertebral specimens referred to *Cimoliasaurus* consisted of two sets of specimens, from two different individuals, both from the greensand of Burlington Co., N. J. They are described in 'Cretaceous Reptiles,' page 25, and characteristic ones presented in plates v and vi.

"The eleven vertebræ considered as lumbar, and represented by figs. 17–19, pl. v, and 16–18, pl. vi, are evidently cervicals. Those considered as dorsals on page 26, and represented in figs. 13–16, pl. v, are at least in part posterior cervicals. Of the fourteen vertebræ referred to on page 27 as dorsals and lumbars, those described and represented in figs. 1–5, pl. vi, are alone dorsals, while the others described and represented in figs. 6–9 are posterior, and those of figs. 10–18 more anterior cervicals.

"The cervicals of *Cimoliasaurus* are so different in their proportions from those of the Kansas saurian that there can be no question as to the distinction of the two animals, at least as species.

"Do all the remains originally referred to *Discosaurus* belong to this genus as distinct from *Cimoliasaurus?* I suspect that those from New Jersey belong to the latter. The animals indicated by all the fossils which have been under consideration are Plesiosauroid, and, as in recognized species of *Plesiosaurus*, there is much variability in the number, proportions, and other characters of the cervicals without a corresponding extent of variation in other parts of the vertebral column, we would be prepared to find in *Cimoliasaurus* nearly the same kind of caudals as in *Discosaurus.*

"Prof. Cope, in his 'Synopsis of the Extinct Batrachia and Reptilia,' pt. i, 1869, p. 56, describes two vertebral specimens from the lower bed of the Cretaceous green sand of Gloucester and Monmouth counties, which he attributes to a species with the name of *Elasmosaurus orientalis.* The specimens described as caudals are seen, by comparison with the Kansas skeleton, to be cervicals."

The species referred to in this paper—all Cretaceous—are finally as follows: 1. *Discosaurus vetustus* Leidy (Cimoliasaurus magnus, and C. vetustus, of *Cope*), from Alabama. 2. *Disc. grandis* (Brimosaurus grandis *Leidy*, Cim. grandis *Cope*), from Arkansas. 3. *Disc. carinatus* (Elasm. platyurus, and Disc. carinatus of *Cope*) from Kansas. 4. *Disc. magnus* (Cim. magnus *Leidy*, Disc. vetustus in part? *Leidy*), from New Jersey. 5. *Disc. planior* (Disc. vetustus in part, *Leidy*), from Mississippi. 6. *Disc. orientalis* (Elasm. orientalis *Cope*), from New Jersey.

3. *On Elasmosaurus platyurus* Cope; by Prof. E. D. Cope. (Communicated by the author).—I observe, in the last number of this Journal, that Prof. J. Leidy criticizes my determination of the structural characters and generic relationships of the above reptile, stating that I have reversed the direction of the vertebral column, describing the cervical as the caudal series and vice versa, and that it is the same as his previously described genus *Discosaurus*. On these points I would make the following observations.

First, as to the direction of the vertebral column, I have little doubt that Prof. Leidy is correct in his determination, especially since I have already pointed out, that, assuming the direction I gave it to be true, the vertebral articulations, and the scapular and pelvic arches, appeared to be the reverse of those of Plesiosaurus. Prof. Leidy does not, however, allude to the principal cause of this error, which was the similar reversal of the vertebral column in his descriptions of his genus *Cimoliasaurus*, first published in 1851, and re-published with 2 plates in 1864. Having my mind pre-occupied with this determination and not suspecting the error, I arranged *Elasmosaurus* in accordance with it. The great size of the clavicle, and lack of special characters of the scapular arch, as mesosternal, etc., and consequent close resemblance to many reptilian fishes, rendered the error more easy, while the coincident discovery of several reptilian forms with zygosphene articulation, attracted my atention to that character.

It might be added that the description and restoration are correct in the "Synopsis Extinct Batr. Reptilia, etc., North America," the error having appeared in a few extra copies only. Also that the accounts already given will require scarcely any modification, the caudal region like the cervical being very long, and less depressed than in *Cimoliasaurus*, etc.

Second, from the identification of *Elasmosaurus* with *Discosaurus*, I entirely dissent. Dr. Leidy, having assumed the cervicals of *Cimoliasaurus* to be lumbars, and stating it as "probable that part of the series described as lumbars may be regarded as representing sacrals and caudals,"* referred the true caudals of the same genus, to another supposed genus, under the name of *Discosaurus*. Anterior caudals of *Cimoliasaurus magnus* he regarded as cervicals of the new genus. But, entertaining a suspicion that the two genera might be one, he says that in this case, they "represent cérvicals, dorsals and lumbars of *Discosaurus*," (i. e. *Cimoliasaurus*, the name earliest given). Having shown the identity of the two forms in accordance with the structure of *Elasmosaurus*, I failed to reverse the arrangement adopted by Prof. Leidy. Had I

---

* Cretaceous Reptiles, N. America, 29.

done so, *Cimoliasaurus* and *Discosaurus* would have become synonyms of *Plesiosaurus*, since no characters are known by which to distinguish them. As it is, I preserve the first, on the supposition that its scapular arch, will be found to present the peculiarities belonging to *Elasmosaurus.*

There are, however, several species included in Leidy's last description of *Discosaurus vetustus*, as he suggests, but believing that "the material was not sufficient to justify a separation," he allowed them to remain together. A portion of this material from New Jersey belongs undoubtedly to *Cimoliasaurus magnus*, and the other specimens (two vertebræ), which present a few peculiarities, are recorded in my Synopsis Extinct Batrachia, Reptilia, etc., as *Cimoliasaurus vetustus.* I presume that it is on these two vertebræ that Leidy bases his reference of *Elasmosaurus* to *Discosaurus.* If the evidence furnished by these was "insufficient to justify their separation" from *C. magnus*, it is certainly insufficient to justify their reference to another genus. The proximal caudals of *Elasmosaurus* and *Cimoliasaurus* are identical, but the median and distal caudals of the two are quite distinct. In *E. platyurus* they present a deep median groove beneath and a rib-like elevation on each side. No such vertebræ have been described as referable to *Cimoliasaurus*, and there is no evidence to prove that the slightly angulate caudals among those referred to *C. vetustus* by Prof. Leidy did not belong to the medial caudal region of a *Cimoliasaurus.*[*] In a little notice furnished to LeConte's report on the Geology of the Union Pacific R. R., southern division, written almost as soon as I received the fossil, I temporarily referred the caudals to *Discosaurus*, not being generally willing to establish a new genus on caudal vetebræ or other distal portions.

In conclusion, it may be summarily stated that: 1, *Discosaurus* was erroneously constituted; 2, that characters separating it from *Plesiosaurus* were not adduced; 3, that it was not distinguished from *Cimoliasaurus;* 4, that *Discosaurus vetustus* embraces at least two species, one of which is *Cimoliasaurus magnus ;* and, 5, the other cannot be proven to be an *Elasmosaurus*, but scarcely differs from corresponding parts of *Cimoliasaurus magnus.*

4. *A new species of Tapir, from Guatemala ;* by Prof. THEODORE GILL. (Extract from a letter to one of the Editors).—The Smithsonian Institution has now the skulls of four adults and one young of the Tapir of Guatemala, and strange as it may appear, they seem conclusively to prove that the Tapir of that region is a different species from that of Panama, belonging, however, to the same genus—*Elasmognathus.* The most obvious differences are in the development of the nasal and frontal bones, but those are confirmed by the differences in the dentition, especially in the form of the first premolar of each jaw. The nasal bones of the young, compared with those of the corresponding age of *E. Bairdii*,

---

[*] It can hardly be doubted that the median and distal caudals of Cimoliasaurus are angulate beneath, to produce terminal planes for the chevrons, in accordance with the structure of Plesiosaurus.

are wider, especially in front of the "pits," and exhibit basilar processes recurrent forward along the frontal bones, like those of *Tapirus*, but less developed, and the grooves for the nasal cartilages are deeper. As the animal advances in age, however, the frontals would appear to grow forward and force apart the nasals, which apparently do not increase, or even diminish in size, and sooner or later are fused with the frontals! This strange outgrowth of the frontal bones has been verified on four adults, and consequently the natural suggestion, that it was a monstrous individual feature, is rendered improbable. The first premolars of the Guatemalan form, in comparison with those of *E. Bairdii*, are in the upper jaw much narrower and divided into halves, the anterior of which is compressed and of almost uniform width, while the inner face of the posterior half bulges abruptly inward; in the lower jaw, they are also narrower, and the anterior cusp is separated by a vertical groove on the inner face of the tooth. All the specimens were obtained by Capt. John M. Dow, who "was told by the party who gave [him] the skulls that the young *are not* marked on the body with longitudinal light colored stripes, like *E. Bairdii*," and he believes that "this want of marking is evidently constant in the young of the species found in Guatemala." This, if confirmed by Capt. Dow himself from autopsy, would furnish a remarkable exception in the family; but without attaching undue importance to the statement, the differences already enumerated seem sufficient to establish its specific rank, and I shall describe it at length under the name *Elasmognathus Dowii.* I may add that Prof. Baird from the first has insisted on its distinctness.

5. *Contributions to the Theory of Natural Selection: A Series of Essays;* by ALFRED RUSSELL WALLACE, author of the Malay Archipelago, etc. 384 pp. 8vo. London, 1870. (Macmillan & Co). —Mr. Wallace, the author of this work, has the credit of being an independent originator of the doctrine of "Natural Selection." His first memoir, published in 1855, led to the publication by Darwin of his first work (in 1859) on the Origin of Species, as Darwin states in his preface. Darwin's views had been partly in manuscript for more than ten years, and had been made known to Lyell in 1844. Wallace's work will be read with interest by all who would acquaint themselves with the arguments and facts upon which the hypothesis of Natural Selection rests, and also for its original and curious observations on the geographical distribution and habits of some animals. The author differs widely on some points from Darwin and Huxley. He has a chapter, evincing profound and earnest thought, on the Limits of Natural Selection as applied to Man, in which he shows that Man could not have been made through Natural Selection, appealing for proof to the size of his brain; his naked and soft skin; his feet and hands; man's mental faculties, and his moral sense. He speaks of man as an ever-advancing, spiritual being, the ultimate aim of all organized existence; of his will, as not a product of nature's chemistry, but above

nature; and he says that the inference which he draws from the facts reviewed is that "a superior intelligence has guided the development of man in a definite direction, and for a special purpose, just as man guides the development of many animal and vegetable forms." He also argues that "all force is probably will-force;" and thus, that "the whole universe is not merely dependent on, but actually *is*, the WILL of higher intelligences, or of one Supreme Intelligence." We cite a few paragraphs from the part of his argument based on a comparison of the brains of Man and the Man-apes.

"The collections of Dr. J. B. Davis and Dr. Morton give the following as the average internal capacity of the cranium in the chief races:—Teutonic family, 94 cubic inches; Esquimaux, 91 cubic inches; Negroes, 85 cubic inches; Australians and Tasmanians, 82 cubic inches; Bushmen, 77 cubic inches. These last numbers, however, are deduced from comparatively few specimens, and may be below the average, just as a small number of Finns and Cossacks give 98 cubic inches, or considerably more than that of the German races. It is evident, therefore, that the absolute bulk of the brain is not necessarily much less in savage than in civilized man, for Esquimaux skulls are known with a capacity of 113 inches, or hardly less than the largest among Europeans.* But what is still more extraordinary, the few remains yet known of pre-historic man do not indicate any material diminution in the size of the brain case. A Swiss skull of the stone age, found in the lake dwelling of Meilen, corresponded exactly to that of a Swiss youth of the present day. The celebrated Neanderthal skull had a larger circumference than the average, and its capacity, indicating actual mass of brain, is estimated to have been not less than 75 cubic inches, or nearly the average of existing Australian crania. The Engis skull, perhaps the oldest known, and which, according to Sir John Lubbock, "there seems no doubt was really contemporary with the mammoth and the cave bear," is yet according to Professor Huxley, "a fair average skull, which might have belonged to a philosopher, or might have contained the thoughtless brains of a savage." Of the cave men of Les Eyzies, who were undoubtedly contemporary with the reindeer in the South of France, Professor Paul Broca says (in a paper read before the Congress of Pre-historic Archæology in 1868)—"The great capacity of the brain, the development of the frontal region, the fine elliptical form of the anterior part ot the profile of the skull, are incontestible characteristics of superiority, such as we are accustomed to meet with in civilized races." * * * We cannot fail to be struck with the apparent anomaly, that many of the lowest savages should have as much brains as average Europeans. The idea is suggested of a surplusage of power; of an instrument beyond the need of its possessor. In order to discover if there is any foundation for this notion, let us compare the brain of man with that of animals. The adult male

---

* While the largest Teutonic skull in Dr. Davis's collection is 112·4 cubic inches, there is an Araucanian of 115·5, an Esquimaux of 113·1, a Marquesan of 110·6, a Negro of 105·8, and even an Australian of 104·5 cubic inches.

Orang-utan is quite as bulky as a small sized man, while the Gorilla is considerably above the average size of man, as estimated by bulk and weight; yet the former has a brain of only 28 cubic inches, the latter, one of 30, or, in the largest specimen yet known, of 34½ cubic inches. * * *

"We see, then, that whether we compare the savage with the higher developments of man, or with the brutes around him, we are alike driven to the conclusion that in his large and well-developed brain he possesses an organ quite disproportionate to his actual requirements—an organ that seems prepared in advance, only to be fully utilized as he progresses in civilization. A brain slightly larger than that of the gorilla would, according to the evidence before us, fully have sufficed for the limited mental development of the savage; and we must therefore admit, that the large brain he actually possesses could never have been solely developed by any of those laws of evolution, whose essence is, that they lead to a degree of organization exactly proportionate to the wants of each species, never beyond those wants—that no preparation can be made for the future development of the race—that one part of the body can never increase in size or complexity, except in strict co-ordination to the pressing wants of the whole. The brain of pre-historic and of savage man seems to me to prove the existence of some power, distinct from that which has guided the development of the lower animals through their ever-varying forms of being."

IV. ASTRONOMY.

1. *Cordova Observatory under the direction of* Dr. B. A. Gould.—Dr. Gould has, at our request, furnished us with the following statement with regard to the Cordova Observatory which has been placed under his charge. He sailed with his family for South America in the latter part of May.

The Argentine Congress voted to establish a national observatory at Cordova, at the instance of President Sarmiento, and through the exertions of the present Minister of Public Instruction, Dr. Avelleneda,—who invited me to organize and take charge of it, knowing my desire to extend the catalogue of the southern heavens beyond the limit of 30° to which the zones of Argelander extend. Bessel went through the region from 45° N. to 15° S. with systematic zone-observations at Königsberg, which have since been reduced and published in two catalogues by Weisse of Cracow. Argelander carried the same systematic scrutiny with the Meridian Circle, from Bessel's Northern limit to the pole, and afterwards from Bessel's southern limit to 30° S.

Since then Gilliss has observed a series of zones for 30° around the south pole; but the reduction of these, although very far advanced, was not completed at the time of his death, and the Ms. is now stored somewhere in Washington. Let us hope that it may at some time be recovered, the work completed and given to the world.

My hope and aim is to begin a few degrees North of Argelander's southern limit, say at 26° or 27°, and to carry southward a system of zone observations to some declination beyond Gilliss's northern limit, thus rendering comparisons easy with both these other labors, and permitting the easy determination of the corrections needful for reducing positions of any one of the three series to corresponding ones for the other. It is of course impossible to arrange in advance the details of such an undertaking, but my expectation is to go over the region in question in zones 2° wide, (except in the vicinity of the Milky Way where the width would be but one-half as great,) up to a declination of about 55°, after which the width would be gradually increased as the declinations became greater. Within these zones all stars seen as bright as the 9th magnitude would be observed, so far as possible, moving the telescope in altitude when no bright star is in the field until some one becomes visible, according to the well known method of zone-observations.

For reducing the observations, differential methods will probably be employed, inasmuch as the time now assigned for my absence from home would be inadequate for proper discussion of the corrections required for nice determinations of an absolute character. Still it is my present purpose, so far as possible, to make such subsidiary determinations as might hereafter be needed in any attempt at computing the observations absolutely. But as I hardly venture to anticipate any opportunity of making a thorough determination of the constants of refraction, or of the errors of graduation, it seems best to arrange for a differential computation at least at first.

It is improbable that a sufficient number of well determined stars will be found available even for this differential reduction, and the necessity may thus be entailed of determining the comparison-stars myself, this determination, however, itself depending upon standard star places. So far as possible I propose employing those heretofore determined by me, and published by the coast survey, which form the basis of the star places of the American Nautical Almanac.

With these observations of position it is my hope to combine others of a physical character to some extent; but in the presence of a plan implying so much labor and effort, it would be unwise to rely upon the possibility of accomplishing much more than the zone-work.

The Meteorological relations of the place are very peculiar, but I dare not undertake any connected series of observations bearing upon these, without self-registering apparatus, which is beyond my means.

Cordova is one of the oldest cities, and contains the oldest university, of the Western hemisphere. It is situated in 31°½ S. latitude, on the boundary of the Pampa, where the land begins to rise toward the group of mountains known as the Sierra de Cordova. It is connected with Rosario, on the Parana, by the Central Argentine railway, which has probably been already opened to travel through its entire length of about 250 miles, although information to that effect has not yet reached this country.

The two largest instruments will be a Repsold meridian-circle of 54 inches focal length and 4½ inches aperture, and an equatorial by Alvan Clark & Sons, provided with the 11 inch object-glass, by Fitz, lately in the posession of W. Rutherfurd who has supplied its place by one of 13 inches. A photometer by Ausfeld of Gotha, according to Zöllner's latest form, has been constructed under the supervision of Prof. Zöllner himself; a spectroscope will be furnished by Merz of Munich, and a clock by Tiede of Berlin.

The Scientific institutions of the U. S. have afforded the expedition every possible assistance. The Coast Survey lends a circuit-breaking clock, a chronograph, and a portable transit; the Smithsonian Institution lends a zenith telescope; the American Academy of Arts and Sciences of Boston [probably] a photometer and spectroscope; the Washington Observatory and the Nautical Almanac have greatly aided the undertaking by gifts of books and by a manuscript copy of Gilliss's catalogue of Standard Stars; and from the astronomers of England, Germany and Russia important assistance has been freely and effectively contributed, in the order and supervision of instruments and apparatus, and by the gift of books, as well as by important and valuable suggestions.

Four assistants will accompany me, Messrs. Miles Rock, John M. Thome, Clarence L. Hathaway and William M. Davis, Jr. We hope to reach Buenos Ayres not later than the middle of August.

The building is now under construction in Boston. The means available proved inadequate for its construction according to the original plan, which was in the form of a cross, with four square rooms about its center, and turrets at its four extremities. One half of it will be first erected; and it is hoped that the remaining portion will speedily be added.

2. *Recent Auroral displays in the United States.*—We are now near the period of a maximum of the solar spots, and if auroral displays observe a similar periodicity, we must also be near an auroral maximum. The reports made by the observers of the Smithsonian Institution as published with the monthly Reports of the Department of Agriculture have been examined to ascertain the number of auroras recorded upon successive years. Only an occasional observation of the aurora appears in these Reports until the year 1869. It is presumed that the Department of Agriculture did not regard the auroral observations of the preceding years as possessing sufficient general interest to authorize their publication. The following are the dates upon which auroras were reported by at least one of the observers of the Smithsonian Institution for the year 1869, and for three months of the year 1870.

1869,	Jan. 3, 5, 6, 7, 10, 13, 17, 18, 19, 20, 21, 22, 30,	Total	13	days
	Feb. 2, 3, 4, 5, 6, 8, 9, 10, 11. 15, 17. 18, 19,	"	13	"
	March 1, 2, 4, 5, 6, 7. 8, 9, 10, 11. 12, 13, 14, 16, 17, 18, 30, 31,	"	18	"
	April 1, 2, 3. 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16, 17, 19, 22, 23, 28, 29, 30,	"	23	"

1869,	May 3, 4, 5, 6. 7, 8 9, 10. 12, 13. 24, 27, 28, 29,	Total,	14	days.
	June 4, 5. 6, 7, 8, 12. 15. 16, 19. 20, 23. 25, 26, 27. 28, 29. 30,	"	17	"
	July 2, 3, 5. 10, 11, 12, 13, 14, 15, 16, 17, 18, 19, 20, 25, 26, 28, 29,	"	18	"
	Aug. 1, 3, 5, 6, 7, 10, 14, 15, 16, 22, 24 27, 28, 29. 30,	"	15	"
	Sept. 1, 2. 3, 4, 5. 6, 8. 9, 10, 11, 12, 13. 14, 15, 16, 19, 24, 26, 27, 28, 29. 30,	"	22	"
	Oct. 1. 2, 4, 5, 6, 8. 9, 12, 21, 22. 24, 25, 26, 27, 28, 30, 31,	"	17	"
	Nov. 1, 6, 8, 9, 10, 12, 25, 26, 29,	"	9	"
	Dec. 4, 5, 6, 7, 13, 14, 15, 18, 22, 25, 26, 29, 30,	"	13	"
	Total for the year 1869,		192	days.
1870,	Jan. 1, 2, 3, 4, 5, 6, 7, 8, 9, 16, 18, 24 25, 26 27, 28, 29, 30. 31,	Total	19	days.
	Feb. 1, 2, 3, 4. 5, 6, 10. 11, 12. 13, 20, 21, 23. 24, 25, 26, 27, 28,	"	18	"
	March 1, 2, 3. 4, 6, 8, 9, 10, 11, 13, 19, 20, 21, 22, 23, 24, 25, 26, 27, 28, 29, 30, 31,	"	23	"
	Total for three months of 1870,		60	"

These observations indicate a very remarkable number of auroras, but it should be remembered that they are derived from the reports of 350 observers spread over a territory embracing 56 degrees of longitude and 20 degrees of latitude. In order that we may be able to make a comparison with former years, it is desirable to have a similar summary of the reports of all the Smithsonian observers; and it is to be hoped that the Secretary of that Institution will furnish us with such a summary for each year since the Smithsonian system of meteorological observations was commenced.

The following summary of the auroral observations made for five years at Depauville, N. Y. (lat. 44° 7′ N. long. 76° 3′ W.) has been furnished by Mr. Henry Haas.

	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Year.
1865	2	3	3	5	1	5	5	5	4	5	2	2	42
1866	1	1	4	3	7	2	3	4	6	5	4	2	42
1867	2	1	4	3	5	2	3	6	10	5	2	1	44
1868	3	3	10	4	6	4	3	3	2	1	0	1	40
1869	4	1	4	6	5	2	4	4	8	1	2	3	44

The number of auroras reported for the different years is remarkably uniform, and similar results have been found at other stations where the annual number of auroras rises as high as fifty or upwards; but in lower latitudes where auroral displays are less frequent, the indications of periodicity are unmistakable.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Variations in the Eccentricity of the Earth's Orbit.*—In vol. xlvi of this Journal (1868), Prof. J. N. Stockwell has a paper, with a chart, illustrating the variations in the eccentricity of the Earth's orbit for the past one million of years, starting from a point 175,000 years back of the present time; and in an earlier memoir "On the Secular Equation of the Moon's mean motion" published at Cambridge in 1867, he has given a similar chart for the million of years following. His calculations were made for intervals of 10,000 years, and hence they are much more exact than the earlier chart

of Croll, whose intervals were, for the major part of it, 50,000 years. The main point put forward by Prof. Stockwell in his Cambridge memoir has not been accepted by astronomers, but this does not affect his calculations of the eccentricity. According to his results for past time, a low maximum occurred about 100,000 years back; a higher, 200,000 years; a lower, 300,000; a rather low minimum 410,000; a low maximum, 475,000; a very low minimum, 520,000; a maximum, 570,000; two maxima, the second 750,000; a very low minimum 800,000; an extreme maximum, 850,000; another very low minimum, 900,000; a high maximum, 950,000; etc.—In future time, there will be a very low minimum, 24,000 years on; a low maximum, 150,000 years; another low maximum, 250,000; a very low minimum, 300,000; a low maximum, 400,000; a very high maximum, 515,000; a minimum, 560,000; an extreme maximum, 610,000 years; and so on. Some of the minor undulations, and most of the minima, are not here noted.

2. *Note on an Electrification of an Island;* by F. JENKIN.—A curious discovery has been made by Mr. Gott, the superintendent of the French company's telegraph station at the little island of St. Pierre Miquelon. There are two telegraph stations on the island. One, worked in connection with the Anglo-American company's lines by an American company, receives messages from Newfoundland and sends them on to Sydney, using for the latter purpose a powerful battery and the ordinary Morse signals.

The second station is worked by the French Trans-atlantic Company, and is furnished with exceedingly delicate receiving instruments, the invention of Sir William Thomson, and used to receive messages from Brest and Duxbury. These very sensitive instruments were found to be seriously affected by earth-currents; i.e., currents depending on some rapid changes in the electrical condition of the island; these numerous changes caused currents to flow in and out of the French company's cables, interfering very much with the currents indicating true signals. This phenomenon is not an uncommon one, and the inconvenience was removed by laying an insulated wire about three miles long back from the station to the sea, in which a large metal plate was immersed; this plate is used in practice as the earth of the St. Pierre station, the changes in the electrical condition or potential of the sea being small and slow, in comparison with those of the dry rocky soil of St. Pierre. After this had been done, it was found that part of the so-called earth-currents had been due to the signals sent by the American company into their own lines, for when the delicate receiving instrument was placed between the earth at the French station and the earth at the sea, so as to be in circuit with the three miles of insulated wire, the messages sent by the rival company were clearly indicated, so clearly indeed, that they have been automatically recorded by Sir William Thomson's syphon recorder. Annexed is a facsimile of a small part of the message concerning the loss of the steamship Oneida, stolen in this manner [here omitted.]

It must be clearly understood that the American lines come nowhere into contact, or even into the neighborhood of the French line. The two stations are several hundred yards apart, and yet messages sent at one station are distinctly read at the other station; the only connection between the two being through the earth; and it is quite clear that they would be so received and read at fifty stations in the neighborhood all at once. The explanation is obvious enough: the potential of the ground in the neighborhood of the stations is alternately raised and lowered by the powerful battery used to send the American signals. The potential of the sea at the other end of the short insulated line remains almost if not wholly unaffected by these, and thus the island acts like a sort of great Leyden jar, continually charged by the American battery, and discharged in part through the short insulated French line. Each time the American operator depresses his sending key, he not only sends a current through his lines, but electrifies the whole island, and this electrification is detected and recorded by the rival company's instruments. * * *

All owners of important isolated stations should use earth-plates at sea, and at sea only. This plan was devised by Mr. C. Varley many years ago to eliminate what we may term natural earth-currents, and now it should be used to avoid the production of artificial earth-currents which may be improperly made use of.—*Nature*, May 5th, 1870.

3. *Baron von Richthofen's Explorations in China.*—The Baron v. Richthofen, formerly of the Geological Survey of Austria, accompanied Mr. J. Ross Brown to China two years ago, and since then has occupied his time in making geological explorations in China. His investigations have been encouraged by the generous subscription of 16,000 dollars, made by the American merchants of China to aid him in prosecuting such exploration. An interesting feature of this subcription is that he is at liberty to use it as he deems best for the interests of science, without regard to immediate commercial or economic results, and in addition to his geological work, he purposes to institute meteorological observations at several points.

Already many important additions have been made to the knowledge of the geology of Northern China and Manchuria. Some of his observations made between Shanghai, and Han-kau were published in the Proceedings of the American Academy of Boston, vol. viii. Since these were written other letters have been received by Prof. J. D. Whitney, from which we hope to make extracts for a future number of this Journal.

4. *Thermal Units.*—Prof. T. Muir proposes in "Nature" of April 14th, the introduction of the word *therm* for a thermal unit, making that unit *the quantity of heat necessary to raise the temperature of* 1 *gram of water from* 0° C. *to* 1° C. *Therm*, *hectotherm*, *kilotherm* would be consequently the expressions for respectively 1,100, 1000 therms, suggestively corresponding to gram, hectogram, kilogram in name as well as in nature."

5. *Astronomer at the Cape of Good Hope.*—Mr. E. J. Stone, F. R.S., of the Greenwich Observatory, has received an appointment to this position.—*Nature*, June 23.

6. *American Association.*—The American Association for the Advancement of Science will hold its next meeting in Troy, commencing on the first Wednesday in August. Prof. William Chauvenet is the President for the year.

7. *British Association.*—The next meeting of the British Association will be held at Liverpool on Wednesday, the 14th of September, under the Presidency of Prof. Huxley.

8. ADMIRAL RUSSELL HENRY MANNERS, President of the Astronomical Society of London, died recently, aged 70 years.

IV. MISCELLANEOUS BIBLIOGRAPHY.

1. *A concise analytical and logical Development of the Atmospheric System as God made it; and the Elements of prognostication by which the weather may be forecasted; designed as a Weather-book for the practical mind of the Country;* by THOMAS B. BUTLER. Hartford, 1870. 404 pp. 8vo.—This work on Meteorology will be read with profit by all interested in the subject. It presents a large number of facts of the Author's own observations, together with others from various sources, and is illustrated with many woodcuts. The title is calculated to excite a prejudice in scientific minds against the volume, and many would at once substitute for "The Atmospheric System as God made it," "The Atmospheric System as Thomas B. Butler makes it." The subject is too large a one to be discussed in a book notice; and we refer, therefore, to the volume for the views, which, according to the motto on the title page, is, in the author's opinion, "the truth [that] is mighty and will prevail."

2. *Synopsis of the Extinct Batrachia and Reptilia of North America*: Part 2d. By EDWARD D. COPE.—This second part of Prof. Cope's important work on the Extinct Batrachia and Reptilia of North America includes the synopsis of the remainder of the Dinosauria, the Testudinata, the Pterosauria, the Pythonomorpha, and the Ophidia. The Testudinata are divided into two groups: the Cryptodira, represented by thirty-four species which are referred to fourteen genera, and the Pleurodira, represented by ten species of four genera. *Rhabdofelix longispinis* Cope is the only known North American representative of the Pterosauria. Of the Pythonomorpha, the author mentions twenty-seven species. Three species of the Ophidia are given. Several additional Reptilian species are remarked upon in the Appendix, and one new genus of the Cheloniidæ is defined. Prof. Cope, through his labors, is throwing great light on the ancient vertebrate life of this continent.

3. *First Principles of Chemical Philosophy;* by JOSIAH P. COOKE, JR., Erving Professor of Chemistry and Mineralogy in Harvard College. 534 pp. text and 22 tables and index, 12mo.—The first part of Prof. Cooke's Chemical Philosophy was noticed in a former volume of this Journal. That portion of the work, it will be remembered, was devoted to the development of the fundamental principles of chemical science. In the second part of his treatise, the author gives a succinct, but very comprehensive, summary of the more important elements and compounds, exhibiting

their constitution and relation by means of formulæ and reactions, illustrated by problems calculated to direct the attention of the student to the more important facts and principles of the science.

By his method of developing the subject, Prof. Cooke succeeds in giving to chemistry a high degree of logical sequence and mathematical exactness, and his work, taken in connection with a good book of laboratory practice, will be found a most efficient aid to both teacher and pupil in the study of chemistry.

4. *Neue Untersuchungen über den elektrisirten Sauerstoff;* von Dr. G. Meissner. Mit zwei lithographirten tafeln. Göttingen, 1869. 4to, pp. 110.—This brochure, re-published from the Transactions of the Royal Academy of Sciences in Göttingen, contains the experiments made by the author, with the aid of his previous experience, in support of his opinion that the so-called atmizone or antozone fog consists only of electrified oxygen and water; an opinion stated in his previous research published in 1863. An abstract of this supplementary memoir of Meissner will appear in our next number.

5. *Great Outline of Geography: a Text-Book to accompany the Universal Atlas;* by Theodore S. Fay. 238 pp. 12mo. New York, 1869. (G. P. Putnam & Son).—Mr. Fay has made an important contribution to our means of instruction in geography in the Atlas and its accompanying text, lately published by Putnam & Son. The work has been long in preparation, the map having been prepared by Hassenstein of Berlin, during the author's long residence as American Minister at European courts. A fac-simile of a letter received by the author from Humboldt expressing his approval of it, is given in the volume. The number of plates constituting the atlas is eight, the most of them double and all well executed. They give, as far as the limited space allows, the chief astronomical, political and historical features of geography, together with some of the physical, and are presented in a clear and striking manner. The text consists partly of brief explanations of terms and principles, and of historical, geographical and other observations, but mainly of lists of places to be looked out on the maps. The plan of instruction proposed in the work is for the teacher to read the lesson from the book while the pupil before him "follows every word upon the plates," looking for the cities, rivers, &c., as the names are mentioned; and this exercise, which is not to exceed, at any one time, an hour, is study enough. The pupil who goes through with the work under the guidance of a patient teacher cannot fail to become well acquainted with the subject.

6. *Geology and Revelation; or the ancient history of the earth considered in the light of Geological facts and revealed religion, with Illustrations;* by the Rev. Gerald Molloy, D.D., Professor of Theology in the Royal College of St. Patrick Meynooth. 418 pp. 12mo. London, 1870. (Longmans, Green, Reader and Dyer).—No book on this subject has appeared from the theological side that is more worthy of respect than this by Dr. Molloy. The author's discussion of the principles of geology, with reference to

their bearing on the Mosaic history of Creation evinces great familiarity with the science and the necessities of the unscientific reader, and thorough knowledge of the sacred record. In treating of the antiquity of the earth he enforces his argument by copious quotations from the Christian Fathers, showing that long before geology had any existence as a science, and of course, when the discussions and doubts it has excited were unknown, the essential points respecting *time* and the order of Creation had received careful attention from devout thinkers, and that the conclusions, at which they arrived on purely theological grounds, were in most cases much the same as those which the best writers of our time deduce from geological evidence. Dr. Molloy closes his arguments as follows:—"We have then two distinct systems of interpretation according to which the vast antiquity of the earth asserted by geology may be fairly brought into harmony with the history of creation recorded in Scripture. One allows an interval of incalculable duration between the Creation of the Heavens and the Earth, and the work of the six days; the other supposes each one of the six days to have been itself an indefinite period of time." The questions touching the antiquity of the human race the author proposes to discuss separately at a future time.

We understand that this work will soon be issued from the publishing house of G. P. Putnam & Son of New York.

7. *Reliquiæ Aquitanicæ, being contributions to the Archæology and Palæontology of Périgord and the adjoining Provinces of Southern France;* by E LARTET and HENRY CHRISTY, edited by THOMAS RUPERT JONES, Prof. Geol., etc., Roy. Mil. College, Sandhurst. Part X, for February, 1870, carries the work to pages 140 and 132 in its two parts, the plates to A XXXII, and B XVIII.

Report of the Commissioner of Agriculture for the year 1868. 671 pp. Washington, 1869.

Annual Report of the Trustees of the Museum of Comparative Zoology, at Harvard College. 42 pp. 8vo. Boston, 1870.

First Annual Report of the American Museum of Natural History. [Museum in the New York Park.] 30 pp. 8vo. Jan. 1870.

Transactions of the Edinburgh Geological Society, Vol. I, Parts I, II, III, 372 pp. 8vo. with many plates. 1868–1869. Edinburgh.

Tables for the determination of Minerals; by Thomas Egleston, Jr., Prof. Min. and Met., in the School of Mines of Columbia College. 26 pp. 8vo. New York, 1870.

A Treatise on Elementary Geometry, with appendices containing a Collection of Exercises for students and an introduction to Modern Geometry; by William Chauvenet, L.L.D. Prof. of Math. and Astr. in Washington Univ. 368 pp. 8vo. Philadelphia, 1870. (J. B. Lippincott.)

On Eozoon Canadense, by Professors William King and T. H. Rowney, of Queen's Univ., Ireland. 48 pp. 8vo., with 4 colored plates. From the Proc. R. Irish Acad., July, 1869. [Facts and arguments against the animal nature of the Eozoon, from which we may cite at another time].

Report of the British Association for 1869. cv, 434 and 260 pp. London, 1870.

Grundzüge einer allgemeinen Theorie der Oberflächen in synthetischer Behandlung, von D. Ludwig Cremona, Prof. Berlin. 1870.

Annali di Mathematica pura ed applicata, diretta da F. Brioschie L. Cremona, Ser. II, Milano.

The American Colleges and the American Public; by Noah Porter, D.D., Professor in Yale College. 282 pp. 12mo. New Haven, Conn., 1870. (C. C. Chatfield & Co.)

Miscellaneous Bibliography.

We have received the following works from George P. Putnam & Son, publishers, New York.

The Great Architect: Benedicite; or Illustrations of the Power, Wisdom and Goodness of God, as manifested in his works, by G. CHAPLIN CHILD, M.D. Two vols. in one; reprinted from the London edtion. 376 pp. 12mo. 1869.

A manual of Political Economy, by E. PESHINE SMITH. 269 pp. 12mo. 1869.

Elements of Geology, intended for the use of students, by SAMUEL ST. JOHN, Prof. of Chemistry and Geology in College of Physicians and Surgeons, New York. Eleventh edition. 334 pp. 12mo. 1869.

Astronomy without Mathematics, by EDMUND BECKETT DENISON, L.L.D., Q.C., and F.R.S., author of the Rudimentary Treatise on Clocks and Watches and Bells, Lectures on Church Building, etc. From the fourth London Edition. Edited with corrections and notes, by PLINY E. CHASE, A.M. 357 pp. 12mo. 1869.

Lectures on Natural Theology or Nature and the Bible, delivered before the Lowell Institute, Boston, by P. A. CHADBOURNE, A.M., M.D., Prof. of Nat. Hist. in Williams College, author of Lectures on the Relations of Natural History, etc. 320 pp., 12mo. 1869.

Drawing without a Master. The Cavé method for learning to draw from memory, by Madame MARIE ELIZABETH CAVE. Translated from the fourth Paris edition. Revised, corrected and enlarged by the author. 130 pp., 12mo. 1868.

The Cavé Method of Drawing. 2d part, Color: for teaching painting in oils and water colors. Translated from the third French edition. 110 pp. 12mo. 1869.

The New West, or California in 1867–8, by CHARLES LORING BRACE, author of Races of the Old World, Home Life in Germany, Hungary in 1851, etc. 373 pp. 12mo. 1869.

Wonders of the Deep, a Companion to Stray Leaves from the Book of Nature, by M. SCHELE DE VERE. 351 pp. 12mo. 1870.

Our Admiral's Flag Abroad. The Cruise of the Franklin. 464 pp. 1869.

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. XVI.—*Comparison of the mean daily range of the Magnetic Declination, with the number of Auroras observed each year, and the extent of the black Spots on the surface of the Sun;* by ELIAS LOOMIS, Professor of Natural Philosophy in Yale College.

IN 1826, M. Schwabe of Dessau in Germany commenced a series of observations of the solar spots which he has continued to the present time. For each year he has kept a record of the number of days of observation, the number of groups of spots observed, and the days on which the sun was free from spots. These observations decidedly indicate a periodicity in the number of the solar spots, a maximum recurring at an interval of from 7 to 13 years. In 1849, Dr. Rudolf Wolf, of Zurich, Switzerland, commenced a series of observations for the same object as those of Schwabe, but upon a plan somewhat more precise and thorough. For each day of observation he recorded two numbers; the first showed how many groups or isolated spots were seen with a four-feet Fraunhofer telescope and a magnifying power of 64; the second showed the total number of visible spots for that day. In order to deduce from these observations a series of numbers which shall be proportional to the amount of spotted surface of the sun, he multiplies the number of groups for each day by ten, and adds to this product the total number of spots. Thus, if on a certain day he counts 9 groups and 31 single spots, he obtains 121 (9×10+31) which he calls the relative number. The mean of all the numbers thus obtained for a month, is the relative number for that month; and the mean of the number thus obtained for a year is the relative number for that year. Dr. Wolf has continued these observations to the present time, and they show clearly a minimum in 1856 and another in 1867, the interval being 11 years.

Dr. Wolf has made a most thorough examination of the records of Astronomical observers, since the invention of the telescope in 1608, and has aimed to deduce a similar series of numbers which shall be proportional to the amount of spotted surface of the sun for a period of 260 years. In this attempt he has been remarkably successful, and he claims to have determined the date of every maximum and every minimum of the solar spots since 1608. He has also published a table of relative numbers for each year since 1700, in which, however, we find 36 years marked with an interrogation point, indicating that the numbers for those years are specially unreliable, or were derived by interpolation. Since 1749 only 12 years are thus marked with an interrogation point. These years are 1774, 1792, 1793, 1801 to 1807, and 1814–5. From 1749 to 1825, the numbers for 37 years are claimed to be specially reliable.

The following is a list of all the important observations since 1700 from which these numbers are derived. All the references are to "Vierteljahrschrift der Naturforschenden Gesellschaft, in Zurich."

Obs. from 1700–1748 by Kirch, vol. 12, p. 142.
" 1705–1726 by Plantade, vol. 5, p. 258.
" 1718–1726 by Rost and Alischez, vol. 5, p. 261.
" 1742–1751 by Hagen, vol. 4, p. 250.
" 1749–1799 by Staudacher, vol. 2, p. 277.
" 1754–1760 by Zucconi, vol. 2, p. 285.
" 1769 by Horrebow, vol. 10, p. 281.
" 1773–1777 by Mallet, vol. 3, p. 394.
Obs. from 1794–1830 by Flaugergues, vol. 6, p. 433.
" 1800–1818 by Heinrich, vol. 4, p. 85.
" 1813–1836 by Stark, vol. 3, p. 373.
" 1816–1825 by Tevel, vol. 4, p. 239.
" 1819–1823 by Adams, vol. 6, p. 449.
" 1826-1868 by Schwabe, vol. 1, p. 266; vol. 5, p. 1, etc.
" 1849–1868 by Wolf, vols. 1, to 14.
" 1854–1860 by Carrington, vol. 9, p. 248.

I have no reason to doubt that the relative numbers which Dr. Wolf has derived from these observations are generally as accurate as can be deduced from the materials employed, but I think his numbers need some correction for the years 1793 and 1794 and probably also for 1795. His numbers from 1787 to 1795 are

1787	92·8*	1790	75·2	1793	20·7 ?
1788	90·6*	1791	46·1	1794	23·9
1789	85·4	1792	52·7 ?	1795	16·5

I propose to examine into the accuracy of these numbers for 1793 and 1794.

The relative numbers from 1787 to 1793 depend almost entirely upon the observations of Staudacher. On p. 283, vol. 2, Wolf, by comparing the average number of spots on each day of observation, deduces the following numbers :

1787.	1788.	1789.	1790.	1791.	1792.	1793.
46·4	45·3	39·4	37·6	22·8	21·7	17·0

The relative numbers which he gives for 1787 and 1788 are obtained by doubling the corresponding numbers here given for those years; and applying the same rule to 1793 we obtain 34 as the relative number for that year. If we combine with Staudacher's observations, the six observations made by Huber, Hahn, and Bode we shall obtain almost exactly the same result. I, therefore, adopt 34 as the most probable relative number for 1793.

The relative number for 1794 depends chiefly upon the observations of Flaugergues, and I will compare his observations for 1794 with those for 1816, for which year Wolf considers the relative number as well determined; and I will also include 15 days (Sept. 12 to 27, 1794) as being without spots. The following is a summary of the results:

	Groups.	Spots.	Days.	Average daily No. of Groups.	Average daily No. of Spots.	Relative No.
1794,	41	148	35	1·17	4·23	15·9
1816,	62	112	51	1·22	2·20	14·4

The relative number which Wolf adopts for 1816 is 45·5. The above result for 1794, reduced to the same scale, becomes 50·2. If we combine with the observations of Flaugergues, observations on 15 days made by Ende, Herschel, Bode and Staudacher, this relative number will be somewhat increased. I therefore adopt 50 as a number not too great for the relative number of 1794, in place of 23·9 given by Wolf. A similar analysis applied to the observations of 1795 would give 35 as the correct relative number instead of 16·5. The maximum for 1804, Dr. Wolf estimates at 70; but this number is altogether conjectural, since observations are reported for only two days of that year. The observations of the magnetic declination indicate that this number is too great, and I have accordingly reduced it to 50, and have reduced the numbers for the three preceding and following years in the same ratio. The entire series of relative numbers from 1740 to 1868 will then be as follows. An asterisk (*) denotes a result considered specially trustworthy; an interrogation point (?) denotes a result considered specially untrustworthy.

Table of Relative Number of Solar Spots Each Year.

Year.	Number of Spots.	Year.	Number of Spots.	Year.	Number of Spots.	Year.	Number of Spots.	Year.	Number off Spots.
1740	60·0 ?	1766	17·5	1792	52·7 ?	1818	34 1*	1844	13·0*
1741	35·0 ?	1767	33·6	1793	34 0 ?	1819	22·5*	1845	33·0*
1742	18·3	1768	52·2	1794	50·0	1820	8·9*	1846	47·0*
1743	14·6	1769	85 7*	1795	35.0	1821	4·3*	1847	79·4*
1744	5·0 ?	1770	79·4*	1796	9·4*	1822	2·9*	1848	100·4*
1745	10·0 ?	1771	73 2*	1797	5·6*	1823	1 3*	1849	95·6*
1746	20·0 ?	1772	49·2	1798	2·8*	1824	6·7	1850	64·5*
1747	35·0 ?	1773	39·8	1799	5.9*	1825	17·4	1851	61·9*
1748	50·0 ?	1774	47·6 ?	1800	10·1*	1826	29·4*	1852	52·2*
1749	63·8*	1775	27·5	1801	22·1 ?	1827	39·9*	1853	37·7*
1750	68·2*	1776	35·2	1802	27·4 ?	1828	52·5	1854	19·2*
1751	40·9*	1777	63·0	1803	35·7 ?	1829	53·5*	1855	6·9*
1752	33·2*	1778	94·8	1804	50·0 ?	1830	59·1*	1856	4·2*
1753	23·1	1779	90 2	1805	35·7 ?	1831	38·8*	1857	21 6*
1754	13·8*	1780	72·6	1806	21·4 ?	1832	22·5*	1858	50·9*
1755	6·0*	1781	67·7	1807	7·1 ?	1833	7·5*	1859	96·4*
1756	8·8*	1782	33·2	1808	2·2	1834	11·4*	1860	98·6*
1757	30·4*	1783	22·5	1809	0·8	1835	45.5*	1861	77·4*
1758	38·3	1784	4·4	1810	0·0*	1836	96·7*	1862	59·4*
1759	48·6	1785	18·3	1811	0·9*	1837	111·0*	1863	44·4*
1760	48·9*	1786	60·8*	1812	5·4*	1838	82·6*	1864	47·1*
1761	75·0*	1787	92 8*	1813	13·7*	1839	68·5*	1865	32·5*
1762	50·6*	1788	90·6*	1814	20·0	1840	51·8*	1866	17·5*
1763	37·4*	1789	85·4	1815	35·0	1841	29·7*	1867	8·0*
1764	34·5*	1790	75·2	1816	45·5*	1842	19·5*	1868	40·2*
1765	23·0*	1791	46.1	1817	43·5*	1843	8·6*	1869	84·1*

The curve line at the bottom of the accompanying Plate, shows the fluctuations in this series of numbers, and exhibits a succession of maxima and minima which occur at the following dates:

Maxima.

Year.	Interval.	Year.	Interval.
1750			10 years.
	11 years.	1804	
1761			12·5 "
	8 "	1816·5	
1769			13·5 "
	9 "	1830	
1778			7 "
	9 "	1837	
1787			11 "
	7 "	1848	
1794			12 "
		1860	

Minima.

Year.	Interval.	Year.	Interval.
1744			11·5 years.
	11·5 years.	1810	
1755·5			13 "
	10·5 "	1823	
1766			10 "
	9 "	1833	
1775			10 "
	9 "	1843	
1784			13 "
	9 "	1856	
1793			11 "
	5·5 "	1867	
1798·5			

The maxima occur at intervals which vary from 7 to 13·5 years, the average interval being exactly ten years. The minima occur at intervals which vary from 5·5 to 13 years, the average interval being 10¼ years. Dr. Wolf overlooks the maximum of 1794 and counts but one period from 1787 to 1804. This interval amounts to 17 years, which is the same as the interval from the maximum of 1761 to that of 1778 (including two periods); and the interval from the maximum of 1830 to that of 1848 (including also two periods) is only 18 years. I think, therefore, we should count 1794 as a year of maximum, equal in amount to the following maxima of 1804 and 1816, but almost blended with the preceding maximum of 1787, in consequence of an unusual prevalence of spots during the intermediate interval. From 1750 to 1860, Dr. Wolf reckons only 10 periods, which would make the average length of one period 11 years; but if we reckon 11 periods (as I think we ought) we shall find the average length of one period to be 10 years.

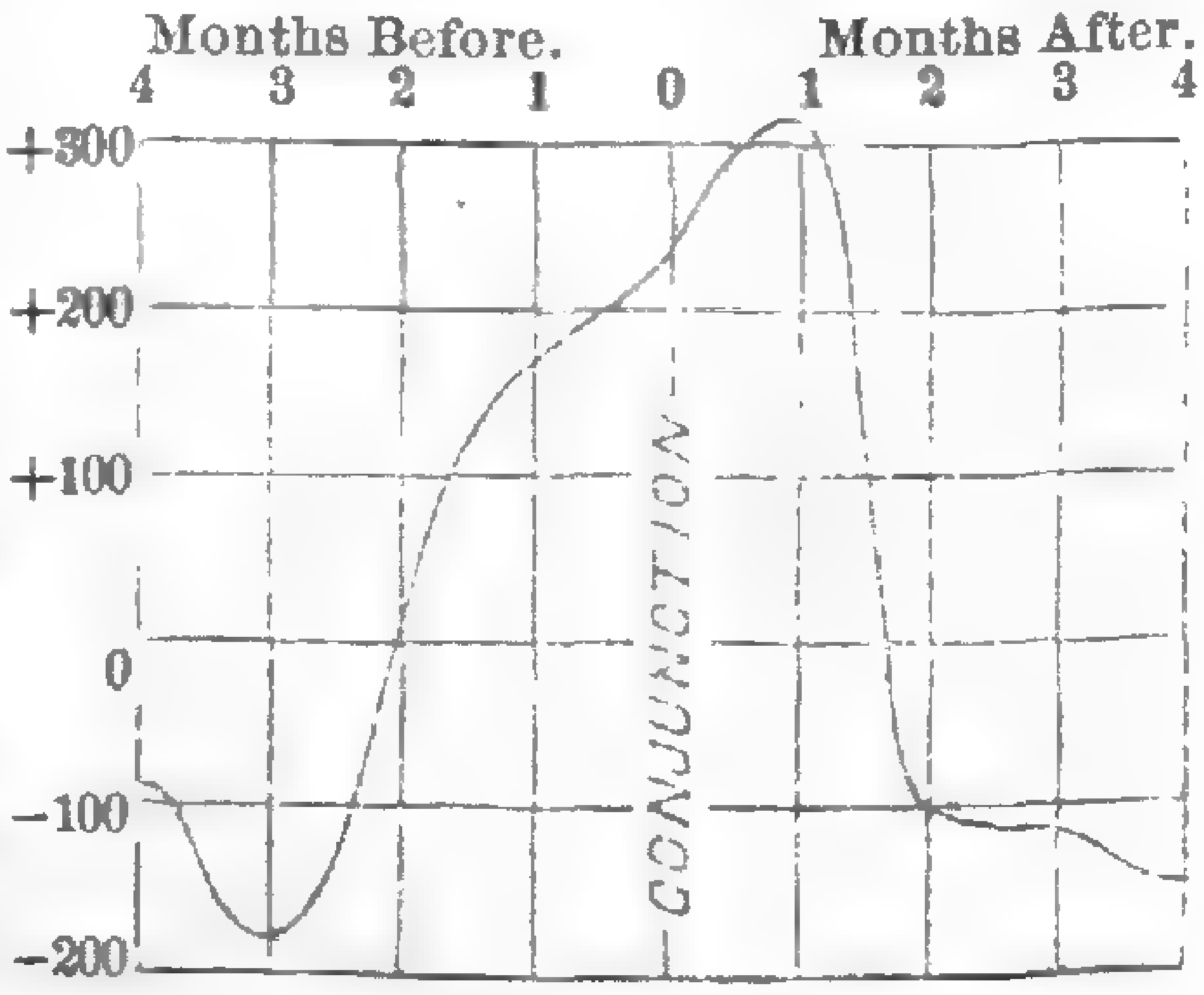
If now we attempt to explain the fluctuations in the sun's surface by ascribing them to the influence of the planets, we find that the planet whose period approaches nearest to the period of 10 or 11 years, is Jupiter, whose time of revolution is 11·86 years, which is nine months greater than Wolf's period, and almost two years greater than the period above found. But the interval between two successive heliocentric conjunctions of Jupiter and Saturn is 19·86 years. Once, therefore, in 9·93 years, Jupiter and Saturn are either in conjunction or opposition; and if we suppose that the action of these planets upon the sun has some analogy to that of the moon upon the earth in raising a tide, then we shall have a cause whose period corresponds quite accurately with the mean period of the maxima of the solar spots. It remains, however, to assign a cause why these periods are alternately increased and diminished by 3 or 4 years, and why the successive maxima are so variable in amount. This cause may, perhaps, be found in part in the position of Jupiter and Saturn in their orbits at the time of conjunction and opposition; but to render the explanation complete we seem obliged to admit that the magnetic condition of the sun has undergone a decided change within the past century; for whether we compare the times of maximum and minimum of the solar spots with the heliocentric longitudes of Jupiter and Saturn, or with their conjunctions and oppositions, we find large differences in the apparent influence of these planets in successive revolutions, and these differences go on steadily increasing for several revolutions of Jupiter; in other words the action of Jupiter and Saturn does not appear to be always the same in the same part of their orbits, nor does the maximun of the spots always occur at the same interval from conjunction or opposition of the planets.

That the mean period of the solar spots is determined mainly by the conjunctions and oppositions of Jupiter and Saturn, is rendered probable by a similar action of Venus and the earth. The observations of Mr. Carrington from 1854 to 1860, have been carefully reduced by Messrs. De La Rue and Stewart (Researches on Solar Physics, second series, 1866) so as to show the actual area of the sun's spotted surface for each day of observation for seven years. In order to eliminate the effect produced upon the amount of spotted surface by the ten yearly period, Messrs. D. and S. have taken the difference between the actual amount of spotted surface for each month, and the amount indicated by the normal ten yearly curve. These differences are mainly the effect of causes whose period is less than 10 years. If now we arrange these numbers in appropriate columns regulated by the times of conjunction of Venus and the earth, and take the averages of the different columns, we shall find that the results follow (in the main) an obvious law, as will be seen in the following table, showing the

Spotted surface of the sun dependent upon the conjunctions of Venus and the Earth.

	Months Before.				Con-	Months After.			
1854 Feb. 28. Inf. conj.	4	3	2	1	junc.	1	2	3	4
" Dec. 12. Sup. conj.				−198	− 34	− 47	+ 10	−147	−107
1855 Sept. 30. Inf. conj.	− 88	+ 5	− 73	+311	− 75	+369	+114	− 50	− 93
1856 July 20. Sup. conj.	− 63	− 48	− 50	− 38	− 35	+ 35	− 2	+ 40	0
1857 May 9. Inf. conj.	0	+ 15	0	+ 9	+ 20	+138	− 30	− 48	− 55
1858 Feb. 28. Sup. conj.	− 5	− 30	−165	+ 16	+240	− 83	+ 20	−160	+280
" Dec. 13. Inf. conj.	+ 45	−177	− 90	+419	+ 62	+494	−364	−214	− 58
1859 Sep. 27. Sup. conj.	−167	+183	+443	+488	+285	+960	−235	− 65	−670
1860 July 18. Inf. conj.	−115	−460	+625	+640	+900	+750	+135	−300	−670
	−305	−900	−500	−180	+720	+235	−565	− 25	+100
Mean	− 87	−177	+ 24	+163	+231	+317	−102	−108	−141

The curve of the accompanying figure represents these average results, and shows that the principal part of these differences may be represented by a simple curve whose period is 9·7 months; being the period between the successive conjunctions of Venus and the sun, or the heliocentric conjunctions and oppositions of Venus and the earth.



The preceding facts are thought to warrant the following propositions:

1. The mean interval between the successive maxima of the solar spots is almost exactly ten years.
2. The value of the maximum of the solar spots is variable, there being generally two or three successive maxima of unusual magnitude, followed by two or three maxima which are smaller than the average magnitude.
3. The ten yearly period of the solar spots is determined by the heliocentric conjunctions and oppositions of Jupiter and Saturn.
4. The principal fluctuations in the amount of the sun's spotted surface from the normal ten-yearly curve, are determined by the heliocentric conjunctions and oppositions of Venus and the earth.
5. The fluctuations in the period of the solar spots from 7 to 13 years, and the great fluctuations in the amount of the suc-

cessive maxima cannot be explained simply by the configuration of the planets, without also admitting a secular change in the magnetism of the sun analogous to the secular change which has been observed in the magnetism of the earth.

Diurnal inequality of the magnetic Declination.—In comparing the diurnal inequality of the magnetic Declination with the amount of spotted surface of the sun, I have taken the observations at Prague as the standard, because these observations are at least as suitable for this purpose as those of any other observatory, and because they are the observations which Professor Wolf has chiefly employed in his comparisons. During the eighteenth century, observations of the magnetic declination were made at Montmorency, France; at Mannheim, Baden; at Paris and London. Those of Montmorency extend from 1777 to '79, and were published in the Connaissance des temps for 1780, etc., and are copied in the Zürich Vierteljahrsschrift, v. 5, p. 241; those of Mannheim extend from 1781 to 1790, and were published in the Palatine Ephemerides, and are copied in the Vierteljahrsschrift, v. 6, p. 427; those of Paris were made by Cassini, and extend from 1784 to '88, and are copied in the Vierteljahrsschrift, v. 2, p. 291; and those of London extend from 1786 to 1805, were made by Gilpin, and were published in the London Philosophical Transactions for 1806, p. 416. During the present century, before the commencement of the Prague observations, we have Beaufoy's observations at London from 1813 to '20; Arago's observations at Paris from 1821 to '31; and the Göttingen observations from 1834 to 1840.

In order that all these observations may be properly represented by a continuous curve, a correction must be applied, not only for difference of locality, but also for a difference in the hours of observation, and for a difference in the mode of deducing from the observations the mean diurnal change. At Prague, the mean diurnal change for the year is obtained by taking the difference between the mean declinations at 8 A. M. and 2 P. M. But the greatest declination usually occurs at 1 P. M., so that the diurnal change appears greatest at those observatories which select the hours of 8 A. M. and 1 P. M. Also the critical hours vary somewhat with the season of the year, so that we shall obtain a still greater value of the diurnal change if we combine the hours of maximum and minimum for each month separately. Moreover, at Greenwich it is customary, in making these determinations, to reject those observations which were made in times of great magnetic disturbance. If such observations are retained, their effect will be to increase considerably the amount of the mean diurnal change. It is difficult to decide what correction should be applied to the different series of observations above enumerated, in order to reduce them to the Prague standard. Fortunately, however, this correction scarcely at all affects the times of maximum and minimum, but simply removes what would otherwise appear as anomalies in the amount of the diurnal fluctuation for different years. The corrections which I have actually applied are the following:

The observations at Montmorency have been increased by one-fifteenth part.
" " " Mannheim diminished by one-fifteenth.
" " " Paris, 1784–88, diminished by one-fifth.
" " " London, 1786–1820, diminished by one-tenth.
" " " Paris, 1821–31, diminished by one-fourth.
" " " Göttingen, 1834–40, diminished by one-fifteenth.

The following table presents a summary of all these observations: [See page 161.]

These observations, after being reduced to the Prague standard in the manner already stated, are represented by the middle curve traced on the accompanying Plate. This curve bears an obvious resemblance to the curve of solar spots, and in general may be said to be parallel with it. The differences between the two curves prior to 1834, may be suspected to arise in part from errors in the observations of one or both of the phenomena represented. Since 1834 the general correspondence of the two curves is remarkably close, but real differences are indicated in the years 1836–8, in 1842–4, and in 1864.

This comparison seems to warrant the following propositions:

1. A diurnal inequality of the magnetic declination, amounting at Prague to about six minutes, is independent of the changes in the sun's surface from year to year.

2. The excess of the diurnal inequality above six minutes as observed at Prague, is almost exactly proportional to the amount of spotted surface upon the sun, and may therefore be inferred to be produced by this disturbance of the sun's surface, or both disturbances may be ascribed to a common cause.

Periodical recurrence of great auroral displays.—In attempting to decide whether auroral displays exhibit the character of a true periodicity, a careful discrimination is necessary in selecting our data for comparison. Travelers who have wintered in some parts of the Arctic regions, have reported that auroral displays were witnessed almost every clear night when the light of the sun did not interfere, even on those years when in the middle latitudes auroras were least frequent. If then we construct a complete catalogue of all recorded auroras, including the 142 auroras observed at Fort Enterprise in 1820–1, the 143 auroras observed at Bossekop in 1838–9; the 141 auroras observed at

Moose Factory in 1850–1; and the 131 auroras observed at Point Barrow in 1852–3, etc., we introduce into our series of numbers an anomaly which results simply from a change of the station of observation, and our numbers do not represent the relative frequency of auroras on different years for any single station or region of the earth. All observations then from the polar regions which are limited to occasional years, and do not form a continuous series, should be entirely discarded in the present comparison. We desire to know

Diurnal inequality of the Magnetic Declination.

Year.	Montmorency.	Mannheim.	Paris.	London.	Reduc'd to Prague.	Year.	Paris.	Gottingen.	Reduced to Prague.	Prague.
1777	11·′2	------	------	------	12·′0	1828	11·′52	------	8·′64	------
1778	10·0	------	------	------	10·7	1829	13·74	------	10·31	------
1779	8·5	------	------	------	9·1	1830	12·40	------	9·30	------
1781	------	9·′12	------	------	8·51	1831	11·75	------	8·81	------
1782	------	8·11	------	------	7·57	1832	------	------	------	------
1783	------	8·77	------	------	8·19	1833	------	------	------	------
1784	------	6·98	9·′65	------	7·12	1834	------	8·′47	7·91	------
1785	------	8·56	10·80	------	8·32	1835	------	9·57	8·93	------
1786	------	12·01	14·00	14·′40	11·79	1836	------	12·34	11·52	------
1787	------	------	15·14	14·98	12·80	1837	------	12.27	11·45	------
1788	------	------	13·48	13·81	11·61	1838	------	12·74	11·89	------
1789	------	8·75	------	11·87	9·43	1839	------	11·03	10·29	------
1790	------	8·33	------	11·95	9·27	1840	------	9·91	9·25	8·′84
1791	------	------	------	11·42	10·28	1841	------	------	------	7·43
1792	------	------	------	9·12	8·21	1842	------	------	------	6·34
1793	------	------	------	8·43	7·59	1843	------	------	------	6·57
1794	------	------	------	7·25	6·53	1844	------	------	------	6·05
1795	------	------	------	6·94	6·25	1845	------	------	------	6·99
1796	------	------	------	7·20	6·48	1846	------	------	------	7·65
1797	------	------	------	7·52	6·77	1847	------	------	------	8·78
1798	------	------	------	7·28	6·55	1848	------	------	------	10·75
1799	------	------	------	7·16	6·44	1849	------	------	------	10·27
1800	------	------	------	6·74	6·07	1850	------	------	------	9·97
1801	------	------	------	7·68	6·91	1851	------	------	------	8 32
1802	------	------	------	8·22	7·40	1852	------	------	------	8·09
1803	------	------	------	9·18	8·26	1853	------	------	------	7·09
1804	------	------	------	8·12	7·31	1854	------	------	------	6·81
1805	------	------	------	8·16	7·34	1855	------	------	------	6 41
1813	------	------	------	6·83	6·15	1856	------	------	------	5·98
1814	------	------	------	7·62	6·86	1857	------	------	------	6·95
1815	------	------	------	7·66	6·89	1858	------	------	------	7 40
1817	------	------	------	8·55	7·70	1859	------	------	------	10·44
1818	------	------	------	8·81	7·93	1860	------	------	------	10 05
1819	------	------	------	7·77	6·99	1861	------	------	------	8·46
1820	------	------	------	7·79	7·01	1862	------	------	------	7·48
1821	------	------	9·10	------	6·82	1863	------	------	------	7·33
1822	------	------	8·83	------	6·62	1864	------	------	------	6·72
1823	------	------	8·18	------	6·14	1865	------	------	------	------
1824	------	------	8·20	------	6·15	1866	------	------	------	------
1825	------	------	9·67	------	7·25	1867	------	------	------	6·47
1826	------	------	9·76	------	7·32	1868	------	------	------	7·27
1827	------	------	11 31	------	8·48	1869	------	------	------	------

whether auroral displays exhibit a periodicity in any given locality or region, and for this purpose we desire a long series of faithful observations at a single station or a limited number of stations; or if satisfactory observations of this kind cannot be obtained, then we must employ observations from a limited region of country where the records have been the most complete and continuous.

The observations at New Haven and Boston combined (Smith. Report, 1865, p. 225,) form a tolerably complete series from 1742 to 1854, and these numbers correspond in a remarkable manner with the fluctuations in the solar spots; but since during a considerable portion of this period no systematic watch for auroras was maintained, it is desirable to combine with this series, observations made at other localities. The observations at St. Petersburg (Smith. Rep. 1865, p. 227) are continuous from 1726 to 1811; and the observations at Berlin (Heis Wochenschrift, Jan. 1870, p. 47) are continuous from 1700 to 1800. I have combined these observations with those at New Haven and Boston, and the correspondence of the resulting numbers with the fluctuations of the solar spots is quite satisfactory. Inasmuch, however, as I have not been able to obtain a continuation of the Petersburg and Berlin observations to the present time, I have thought it best to extend the area of observation, and include all published observations from Europe south of the parallel of 55°. By this method of comparison we eliminate to a considerable extent the anomalies introduced into the observations of any single station, by cloudy weather which conceals many auroras from view; and we also partially eliminate the anomalies resulting from the unequal diligence of the many different observers who must necessarily participate in a series of observations extending over more than a century.

In the Vierteljahrsschrift, vol. 10, p. 232, is given a very complete catalogue of European auroras classified by parallels of latitude, and I have used this as my principal basis in the subsequent comparisons. In the table on page 163, column first shows the year of observation; column second the number of auroras recorded in Europe south of the parallel of 55°; column third the auroras at New Haven and Boston; column fourth the sum of the numbers in the two preceding columns. In order to eliminate still further the effects of irregular and non-periodic causes, I have taken the averages of the numbers in column fourth for each successive period of three years, and the numbers thus resulting are given in column fifth.

For a few years preceding and following 1787, column second shows a very remarkable increase in the number of observed auroras sufficient to excite a suspicion that these numbers are relatively too great. The Palatine Meteorological Society, which

Number of Auroras observed on different years.

Year.	Europe south of 55°.	Boston and New Haven.	Sum of do.	Mean for three years.	Year.	Europe south of 55°.	Boston and New Haven.	Sum of do.	Mean for three years.	Year.	Europe south of 55°.	Boston and New Haven.	Sum of do.	Mean for three years.
1739	46		46		1783	73	22	58	47	1827	16	7	23	17
1740	23		23	39	1784	51	4	29	39	1828	12	6	18	21
1741	49		49	39	1785	42	9	30	56	1829	20	2	22	25
1742	42	2	44	43	1786	110	55	110	84	1830	28	6	34	25
1743	33	2	35	31	1787	133	47	113	108	1831	17	2	19	20
1744	14	0	14	20	1788	124	38	100	105	1832	6	2	8	13
1745	10	0	10	16	1789	103	51	102	84	1833	8	3	11	11
1746	17	7	24	22	1790	77	13	51	68	1834	4	9	13	12
1747	23	10	33	27	1791	78	12	51	46	1835	7	6	13	15
1748	18	6	24	27	1792	60	6	36	37	1836	14	5	19	32
1749	13	10	23	33	1793	15	8	23	23	1837	22	41	63	42
1750	34	17	51	31	1794	8	2	10	14	1838	5	39	44	60
1751	14	5	19	30	1795	6	2	8	7	1839	27	47	74	65
1752	19	2	21	17	1796	2	0	2	8	1840	32	44	76	80
1753	11	1	12	15	1797	13	0	13	5	1841	34	56	90	67
1754	12	0	12	11	1798	1	0	1	6	1842	18	18	36	53
1755	10	0	0	10	1799	5	0	5	4	1843	24	10	34	37
1756	7	0	7	9	1800	6	0	6	6	1844	28	13	41	39
1757	4	6	10	7	1801	8	0	8	6	1845	18	24	42	48
1758	1	4	5	14	1802	2	2	4	6	1846	25	35	60	54
1759	22	5	27	16	1803	1	5	6	6	1847	35	25	60	76
1760	11	6	17	23	1804	4	4	8	14	1848	46	62	108	72
1761	20	5	25	22	1805	23	4	27	14	1849	26	23	49	69
1762	16	7	23	19	1806	3	4	7	13	1850	15	36	51	49
1763	2	6	8	16	1807	2	2	4	4	1851	20	26	46	65
1764	4	12	16	11	1808	1	0	1	2	1852	33	64	97	67
1765	1	7	8	8	1809	0	2	2	1	1853	20	39	59	63
1766	0	0	0	5	1810	1	0	1	1	1854	7	27	34	35
1767	4	4	8	9	1811	0	0	0	0	1855	0	12	12	20
1768	12	7	19	30	1812	0	0	0	1	1856	5	8	13	11
1769	46	18	64	40	1813	2	0	2	4	1857	1	7	8	21
1770	22	14	36	41	1814	6	3	9	5	1858	11	30	41	37
1771	7	15	22	24	1815	2	1	3	5	1859	47	16	63	50
1772	6	7	13	26	1816	2	0	2	5	1860	28	17	45	48
1773	26	17	43	33	1817	11	0	11	7	1861	24	12	36	36
1774	24	20	44	33	1818	4	4	8	11	1862	23	5	28	28
1775	7	5	12	22	1819	9	6	15	10	1863	19	2	21	29
1776	5	4	9	26	1820	4	2	6	8	1864	33	6	39	33
1777	41	15	56	38	1821	2	0	2	4	1865	36	2	38	34
1778	30	18	48	69	1822	2	1	3	2	1866	21	5	26	31
1779	98	4	102	70	1823	0	0	0	1	1867	9	20	29	22
1780	34	25	59	67	1824	0	0	0	1	1868	3	7	10	32
1781	68	25	59	57	1825	2	2	4	4	1869	28	28	56	
1782	58	24	53	57	1826	9	0	9	12	1870				

embraced a large number of zealous observers, was organized in 1780 and continued to publish an annual volume of observations till 1792. During this period, auroras were made a special subject for observation, and it cannot be doubted that they were recorded with more fidelity than for most of the preceding and following years. I have therefore reduced the European observations during this interval by one-half, and have combined this result with the New England observations in completing the numbers in the last two columns.

The Boston observations published by Prof. Lovering in the Memoirs of the American Academy, vol. ix, p. 101, and copied in the Smithsonian Report for 1865, p. 227, extend only to 1848. In order to obtain a continuation of this series to the present time, I have carefully consulted the Meteorological Journal kept at Cambridge (Mass.) Observatory, and have obtained the following results :

Number of Auroras recorded each year at the Cambridge Observatory.

Year.	No.	Year.	No.	Year.	No	Year.	No.	Year.	No.
1841	27	1847	9	1853	27	1859	16	1865	1
1842	11	1848	17	1854	25	1860	12	1866	0
1843	4	1849	4	1855	12	1861	6	1867	20
1844	6	1850	15	1856	8	1862	0	1868	6
1845	7	1851	17	1857	7	1863	1	1869	28
1846	15	1852	44	1858	30	1864	1		

When auroras were recorded at New Haven which were not recorded at Cambridge, I have added them to the preceding numbers, and the result is given in column third of the table on page 163. In the years 1860, 1 and 2, no record of auroras was preserved at New Haven, and to supply this omission I have inserted for these years the observations made at Middletown, Conn., by Prof. John Johnston. The European observations published in the Vierteljahrsschrift close with 1861. I have continued the table to the present time by means of the observations published each year in Heis Wochenschrift, confining myself to such observations as were reported south of lat. 55°.

The numbers given in column fifth have been projected in a curve represented at the top of the accompanying Plate. This curve shows an unmistakable resemblance to the curve of the solar spots, and to that of the magnetic declination. To each maximum of the two latter curves there corresponds a maximum of the auroral curve, not always simultaneously, but not differing from it more than a single year except in one or two instances. The following table shows the departures of the auroral maxima from the maxima of the solar spots.

Date.	Diff.	Date.	Diff.	Date.	Diff.
1750	−1 year.	1787	0 year.	1837	+3 years.
1761	0 "	1804	0 "	1848	−1 "
1769	+1 "	1816·5	+1·5 "	1860	−1 "
1778	0	1830	−1 "		

The correspondence in the times of minimum is still more remarkable as is shown in the following table.

Date.	Diff.	Date.	Diff.	Date.	Diff.
1744	+1 year.	1784	0	1833	0
1755·5	+1·5 "	1798·5	unc'n.	1843	0
1766	0 "	1810	0	1856	0
1775	0 "	1823	0	1867	0

There are only two instances in which there is any discrepancy in the times of minima. In one of them (1744) the observations of the solar spots are confessedly very uncertain, and in the other (1755) the change in the number of observed auroras from 1755 to 1757 was only *three*, so that we seem authorized to assert that there is an invariable coincidence in the times of minima of the solar spots and of auroral displays.

The range of the maxima and minima of auroral displays is considerably greater than that of the solar spots, and the observations of magnetic declination seem to indicate a similar peculiarity. There seems then to be no room for doubt that auroral displays exhibit the ten yearly period of solar spots, but the range of the changes on different years is subject to influences which may be independent of the sun.

In order to decide whether this periodicity is exhibited in the higher latitudes, I have selected all those observations with which I am acquainted which show more than a hundred auroras in a year. The following is the table.

Date.	Place.	Lat.	Long.	Numb. Auroras.	Authority.
1820–1	Cumberland House,	53° 56′	102° 16′	142	Force (Smith. Cont.), p. 6.
1820–1	Fort Enterprise,	64 28	113 6	142	Force (Smith. Cont.), p. 24–35.
1833–5	Great Slave Lake.	62 46	109 1	105	Capt. Back.
1838–9	Bossekop,	69 58	23 34E	143	Voyage en Scandinavie.
1848–9	Fort Confidence,	66 54	118 49	122	Athabasca obs. p. 324–350.
1850–1	Moose Factory,	51 10	81 0	141	Am. Jour. Sci., [2], XIV, 156.
1850–1	Athabasca Lake,	58 43	111 18	109	Am. Jour. Sci., [2], XIV, 156.
1852–3	Point Barrow,	71 21	156 15	131	Phil. Trans. 1857, p. 497.

These places are all situated within the zone of greatest auroral frequency, Am. Jour., vol. XXX, p. 89. The observations of 1820–1 and 1833–5 were made near the time when the disturbance of the sun's surface was a minimum; while those of 1848–9 were made when the disturbance of the sun's surface was a maximum; and those of 1838–9, and 1850–1 were made when the disturbance was greater than the average. The average number of auroras recorded at the first two dates is nearly the same as at the last three dates. So far as any conclusion is warranted from so limited a number of observations, the inference is that within the zone of greatest auroral frequency, the number of auroras is about the same for all years, independent of the disturbed condition of the sun's surface. It is presumed that there must be a periodical change in the *brilliancy* of the auroral displays, but the observations are not sufficiently numerous and precise to justify us in pronouncing positively upon this point. As we recede from the zone of greatest auroral frequency, the influence of the sun is shown in a periodical change in the number of auroras; and this periodicity in Europe appears to be the most distinctly marked near the parallel of 50° latitude; and in New England near the parallel of 42° latitude.

It will be observed that the change from a maximum to a minimum of the solar spots is not uniform, and frequently the inequality is so marked as to assume the form of a distinct secondary maximum. Such maxima occurred in 1774, 1792, 1794 (if this is not to be regarded as a primary maximum) and 1864; and in several other cases the inequality in the progress from a maximum to the succeeding minimum is decided. The mean daily range of the magnetic declination exhibits also secondary maxima, and generally the small inequalities in the curve of magnetic declination bear some correspondence to the small inequalities in the curve of the solar spots. The auroral curve shows still greater irregularities, and it is remarkable that an irregularity in the auroral curve often has a decided corresponding irregularity in one of the other two curves. Thus both the auroral and sun-spot curves show a decided maximum in 1774; also in passing from the maximum of 1778 to the following minimum the undulations of the three curves bear a marked resemblance; in the descent from the maximum of 1787, there is a secondary maximum of the magnetic curve corresponding to that of the solar-spot curve; in the descent from the maximum of 1848 there are corresponding undulations of the sun-spot curve and the magnetic curve, while the auroral curve shows at the same time a well-marked secondary maximum; and nearly the same remark is applicable to the descent from the principal maximum of 1860.

The following conclusions are thought to be warranted by the preceding observations.

1. Within the zone of greatest auroral frequency, auroras are of almost daily occurrence in all years, and it is doubtful whether in this region there is any decided periodicity in the number of auroral displays, although there may be periodical changes of brilliancy.

2. At places where the average number of auroras is about 20 or 25 annually, the ten-yearly period of the solar spots can be distinctly traced.

3. The times of maximum and minimum of the solar spots correspond in a remarkable manner with the times of maximum and minimum in the frequency of auroral displays in the middle latitudes.

4. The successive maxima of auroral displays are more variable than those of the solar spots, so that the ten-yearly period might be easily overlooked, and it might be inferred that the maxima only occurred at intervals of about 60 years.

5. The most remarkable irregularities in the auroral curve in the progress from a maximum to a minimum, correspond to similar though generally smaller inequalities in the curve of solar spots or of magnetic declination.

Magnetic storms compared with the prevalence of solar spots.

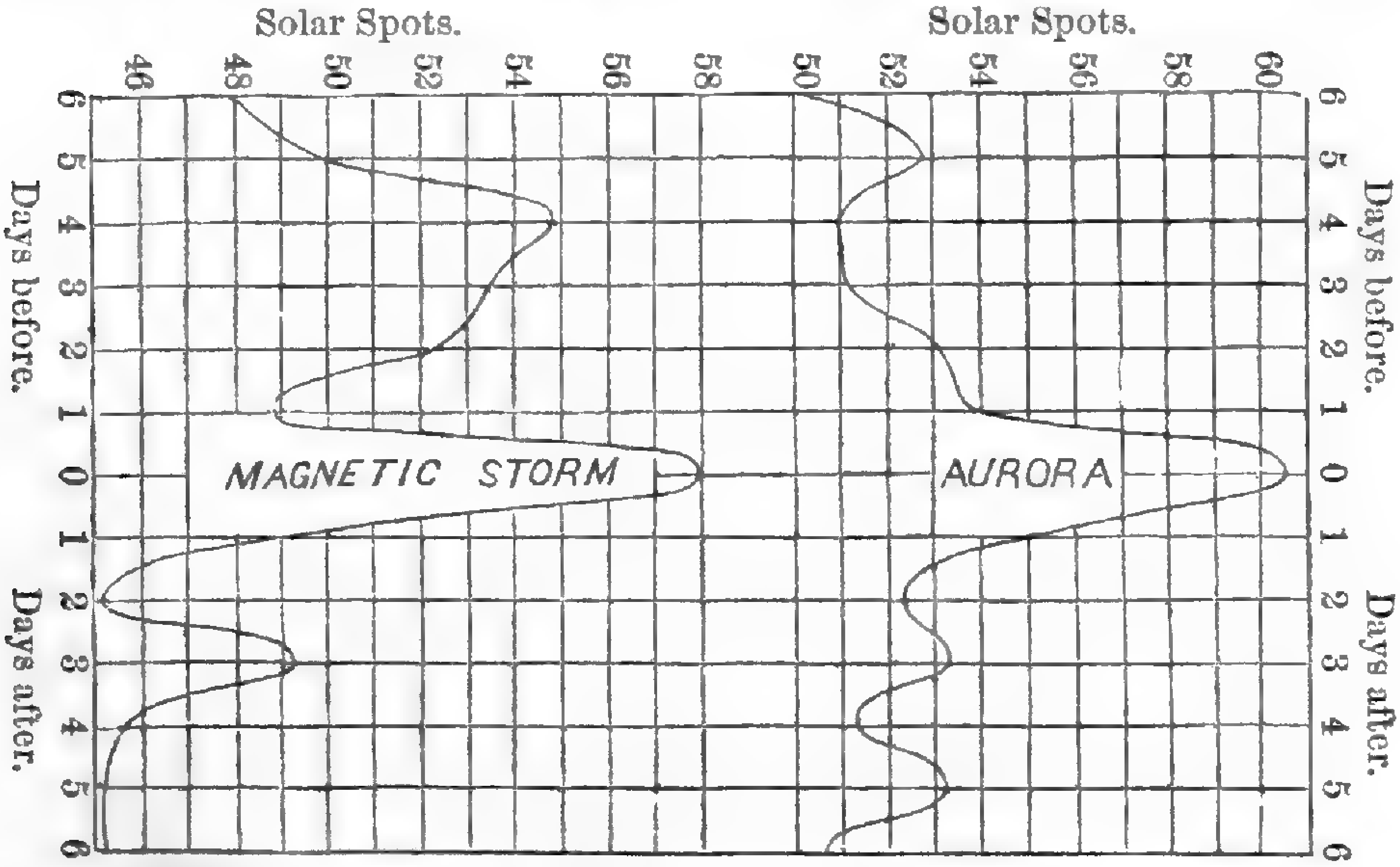
Since the mean daily range of the magnetic needle is greatest on those *years* in which the sun's surface is most disturbed, we could not be surprised if the range of the magnetic needle should prove to be the greatest on those *days* on which the solar spots are most extensive. In order to test this question, I have compared the extent of the solar spots for 6 days preceding and 6 days following each of the great magnetic disturbances at Greenwich for a period of 23 years. In the Greenwich observations for 1862 is given an abstract of the magnetic observations on 177 days of great magnetic disturbance from 1841 to 1857; and in the observations for 1867 are enumerated the days of great magnetic disturbance from 1858 to 1863, amounting in number to 45. These 23 years furnish 222 days of great disturbance, and I have compared Wolf's numbers representing the extent of the solar spots near the time of these dates. Having prepared a table with 13 vertical columns, I insert in the middle column, Wolf's relative number for one of the days of great magnetic disturbance, and I also insert in their appropriate columns the relative numbers for the 6 preceding and 6 following days. In this manner I have treated all the days of great magnetic disturbance at Greenwich, with the exception of those cases in which very few observations of the solar spots were made, and a few cases in which there were two or three successive days of great magnetic disturbance, in which cases I have generally selected the day of greatest disturbance and made but one entry of that period in the table. The cases of disturbance which I have thus employed amount to 135. The following table (see page 168) exhibits these cases for ten years from 1848 to 1857, and at the bottom of the table are given the averages of these numbers for each of the 13 columns; and in another line are given the corresponding averages for the whole number of 135 cases of magnetic disturbance.

The details for the remaining 74 days of observation are omitted in order to avoid incumbering the pages of this Journal. The resulting averages for the 135 cases are represented by the lower curve line in the figure, page 169, where the middle vertical line represents the day of a magnetic storm at Greenwich, and the other vertical lines represent 1, 2, 3, etc., days *before* the storm, and also 1, 2, 3, etc., days *after* the storm. The horizontal lines indicate the amount of disturbance of the sun's surface as measured by Wolf's relative numbers extending from 45 to 58. From this curve it will be seen that the disturbances of the sun's surface about the time of a magnetic storm, bear an analogy to the waves in our own atmosphere about the period of a violent winter storm. We find a well-

Spotted surface of the sun near the time of a magnetic storm.

		Days before the storm.						Magnetic Storm	Days after the storm.					
		6	5	4	3	2	1		1	2	3	4	5	6
1848.	Jan. 28,			100		133	147	151	135	127	105	90		107
	Feb. 21,	116	90		70		20	103	50	106		60	50	
	March 17,	80	106	76		101	60	113	101	75	95	93	79	78
	May 18,	50	91	96	90	54	113	99			85	72	88	68
	July 11,	93	105	112	160	99	122	170	132	126	70	50	77	60
	Oct. 23,		150			70	70	142	94	60	90	40	74	88
	Nov. 17,	20	79		136	163	156		80	60	60		70	54
	Dec. 17,	129	141	133	133	100		105	142	100	135	125	90	170
1849.	Oct. 30,	58	44			34	61	118	66	66	111	40	71	
1850.	Feb. 22,	58	73	80	122	154	55	115	54	60	56	78	75	69
	March 31,	57	23	59	39	67	37	39	27	27	37	0	24	0
	May 7,	25	86	14	36	60	22	80	46	50	74	101	56	92
	June 13,	46	15	132	133	147	129	67	51	34	60	48	56	80
	Oct. 2,		25		45	45	35	55	40	34	39	59	40	22
1851.	Jan. 19,	58	87	27	57	20	20	62	45	23		37	13	11
	Feb. 18,	79	78	46	108	73	105	78	101	105	47	122	116	86
	Sept. 4,	35	46	38	102	40	24	103	66	35	45	69	70	52
	Oct. 28,	101	94	63		82	36	50	54	40	20	37	48	22
	Dec. 6,	79	87	81	46	47	38	26	13	38	36	53	41	
	Dec. 28,	76	60	30	10	71	23	96	94	47	117	82	82	59
1852.	Jan. 4,	94	47	117	82	82	59	52	70	61	83	6	50	66
	Jan. 19,	6	7	60	40	6	96	127	106			14	74	76
	Feb. 15,	2	48			77	77		144	62	16	15	101	10
	Feb. 20,	77		144	62	16	15	101	10	5	77	54	46	15
	April 20,	62	64	34	53	51	38	47	49	28	16	13	50	50
	May 20,	38	46	36	37	11	11	29	28	31	42	57	64	59
	June 11,	57	66	56	44	1	0	11	30	28	4	4	30	24
	June 16,	0	11	30	28	4	4	30	24	25	22	33	48	26
	July 10,	51	65	65	70	11	56	61	34	24	0		25	0
	Nov. 11,	48	10	105	14	81	81	73	61	11	47	34	11	30
1853.	Jan. 10,	25	39	29	27		11	11	23	12	12	11	24	26
	March 7,	15	17	15	12	0	0	11		14	11	11	22	23
	May 3,	26	0	11	14	41	27	31	26	21	16	14	22	13
	May 24,	30	35	47	48	71	46	20	49	59	54	23	29	10
	June 22,	65	26	67	56	56	51	11	20	11	11	11	11	11
	July 12,	35	45	48	75	69	47	48	71	35	63	79	49	47
	Sept. 1,	53	67	33	33	37	56	57	44	68	55	23	34	
	Oct. 25,	48	49	50	46	30	44	44	40	32	26	36	25	
	Nov. 9,			25	25	25		22	22	34		35	25	23
	Dec. 6,	13	11	12	17	17	14	17	17			29	38	18
	Dec. 21,		32	20	31		22	11			11	14	13	
1854.	Jan. 8,	19	27	10	17	10	25	25	0	17	19	19	17	13
	Jan. 20,	13	14	15	17	17	16	14	11	0	0	0	0	0
	Feb. 16,	26	29	35	28	17	10	22	25	17	17	11	0	0
	Feb. 24,	17	17	11	0	0		12	0	0	0	0	12	12
	March 6,	0	12	12	0	0	0	0	0	0	11	34	45	44
	March 15,	11	34	45	44	39	33	33	23	26	27	33	33	27
	March 28,	25	11	13	11	29	22	35	11	0	11	12	12	36
	April 10,	37	38	49	49	56	54	54	55	55	36	35	14	12
	April 23,	12	13	24	22	25	12		11	11	0	0	0	0
	May 25,	13	11	11	13	13	11	0	0	0	0	0	0	14
1855.	March 12,	29	12	38	23	11	16	12	11	14	11	0	11	0
	April 4,	12	16	17	17	12	0	11			13	0		0
	Oct. 18,	15	12	0	0	0	0	0	12	17	11	12	13	11
1857.	March 13,	0	0	0	11	13	12	0	11	11	13	11	13	0
	May 7,	17	16	16	15	15	25	24	20	25	25	26	22	20
	May 10,	15	15	25	24	20	25	25	26	22	20	42	53	60
	Sept. 3,	39	27	27	26	37	38	35	27	48	50	40	29	58
	Nov. 12,	32	26			24	24	24	24	24	36	60	38	58
	Nov. 16,	24	24	24	24	24	36	50	38	58	50	23	45	46
	Dec. 16,	11					71	79	68	54	68			42
Mean of 61 days obs.		40·6	44·4	46·5	46·1	45·8	42·4	52·4	46·2	38·8	40·0	36·6	40·8	38·2
Mean of 135 d'ys obs.		47·9	50·1	54·8	53·5	52·3	48·9	57·9	49·0	45·1	49·3	45·6	45·2	45·3

marked maximum on the day of the magnetic storm; a second maximum four days before the storm, which is also pretty distinctly marked; and a third maximum, more uncertain, three days after the storm. The entire fluctuation within a period of two days is from 45·1 to 57·9, or 28 per cent of the smaller quantity, an effect which is so large and derived from a comparison of so many cases as to indicate a law of nature. Hence we seem authorized to conclude



1. Great disturbances of the earth's magnetism are accompanied by unusual disturbances of the sun's surface on the very day of the magnetic storm; and are, therefore, due to some influence which emanates immediately from the sun.

2. The great disturbance of the sun's surface which accompanies a terrestrial magnetic storm, is generally heralded by a smaller disturbance three or four days previous, succeeded by a comparative calm which immediately precedes the magnetic storm.

Auroral displays and Solar spots compared.

Since the average disturbance of the sun's surface is greatest on those *years* in which auroras are most prevalent, we could not be surprised if the disturbance of the sun's surface should prove to be the greatest on those *days* on which auroras prevail in the middle latitudes. In order to test this question, I have proceeded in the same manner as in the case of magnetic storms; and in order to avoid the suspicion of having selected a few cases for the purpose of confirming a preconceived theory, I have taken the entire series of observations made by Messrs. Herrick and Bradley at New Haven, from 1837 to 1854. These observations are 514 in number, and I have employed them all with the following exceptions:

Spotted surface of the Sun near the time of an Auroral display.

		Days before the Aurora.							Days after the Aurora.					
		6	5	4	3	2	1	Aurora	1	2	3	4	5	6
1850.	Jan. 18,	79	62	73	27	74	65	20	20	15	33		37	56
	Feb. 3,	15	14	79	54	87	75	113	86	86	22	20	30	102
	Feb. 22,	58	73	80	122	154	55	115	54	60	56	78	75	69
	March 3,	56	78	75	69	93	71	97	45	94	56	65	51	168
	March 10,	45	94	56	65	51	168	98	89	65	69	78	70	64
	March 31,	57	23	59	39	67	37	39	27	27	37	0	24	0
	April 7,	27	27	37	0	24	0	12	27	11		30	31	15
	May 7,	25	86	14	36	60	22	80	46	50	74	101	56	92
	May 12,	22	80	46	50	74	101	56	92	50	11	52	34	33
	July 12,	0	12	11	24	46	12	76	33	110	105	110	77	40
	Aug. 9,	77	71	35	47	42	65	60	13	59	47	13	12	51
	Sept. 4,	54	13	82	81	126	105	107	86	104	106	98	86	106
	Sept. 10,	107	86	104	106	98	86	106	74	118	78	58	76	52
	Sept. 30,	46	50		25		45	45	35	55	40	34	39	59
	Oct. 2,		25		45	45	35	55	40	34	39	59	40	22
	Oct. 27,	46	62	40	40		58		86	15	27	68	14	20
	Dec. 26,	81	66	71	60	40	27	58	48	35	39			55
1851.	Feb. 18,	79	78	46	108	73	105	78	101	105	47	122	116	86
	March 22,	31	46	23	47	57	64	81	62	38	54	40	27	35
	March 29,	62	38	54	40	27	35	35	53	54	28	45	12	34
	June 5,	36	81	95	74	27	49	51	44	37	31	42	37	39
	June 27,	108	96	40	34	49	64	61	41	71	41	35	11	12
	July 17,	45	36	34	34	66	45	30	33	52	55	54	35	54
	Sept. 4,	35	46	38	102	40	24	103	66	35	45	69	70	52
	Sept. 7,	102	40	24	103	66	35	45	69	70	52	76		79
	Sept. 10,	103	66	35	45	69	70	52	76		79	80	45	60
	Oct. 1,	40	38	30				14	14	47	109	14	43	32
	Oct. 20,	48	51	15	20	51	64	54	136	101	94	63		82
	Oct. 23,	20	51	64	54	136	101	94	63		82	36	50	54
	Oct. 28,	101	94	63		82	36	50	54	40	20	37	48	22
1852.	Jan. 19,	6	7	60	40	6	96	127	106			14	74	76
	Feb. 16,	48			77	77		144	62	16	15	101	10	5
	Feb. 20,	77		144	62	16	15	101	10	5	77	54	46	15
	March 7,	5	36	2	44	1	39	40	73	56	67	79	81	86
	April 10,	61	77	94	113	117	113	114	90	79	77	62	64	34
	April 15,	113	114	90	79	77	62	64	34	53	51	38	47	49
	April 22,	34	53	51	38	47	49	28	16	13	50	50	81	84
	May 17,	41	25	2	38	46	36	37	11	11	29	28	31	42
	June 5,	4	97	80		5	72	57	66	56	44	1	0	11
	June 11,	57	66	56	44	1	0	11	30	28	4	4	30	24
	June 16,	0	11	30	28	4	4	30	24	25	22	33	48	26
	July 6,	74	49	34	51	51	65	65	70	11	56	61	34	24
	July 10,	51	65	65	70	11	56	61	34	24	0		25	0
	Sept. 6,	49	37	47	34	35	38	35	61	4	38	2	29	38
	Sept. 17,	29	38	26	11	11	0	15	24		57	2	48	54
	Sept. 21,	11	0	15	24		57	2	48	54	44	41	2	33
	Sept. 29,	54	44	41	2	33	34	36	26	3	26	37	42	7
	Oct. 5,	36	26	3	26	37	42	7	37	26	3	27	28	42
	Oct. 20,	69		52	63	76	87	89	109	128	146	40	8	97
	Nov. 11,	48	10	105	11	81	81	73	61	11	47	34	11	30
	Dec. 5,	55	38		40	33	43	58	42	43	29	27	35	33
	Dec. 13,	43	29	27	35	33	27	28	26	14	28	24	10	15
1853.	Feb 14,			28	36	54	44		68	63	43	60	58	85
	March 14,		14	11	11	22	23	24	23		36	64	60	40
	March 27,	34	53	56	68	56	65	63	56	47	58	45	35	34
	April 6,	45	35	34	58	49	20	77	72	27	34	49	40	42
	May 2,	26	26	0	11	14	41	27	31	26	21	16	14	22
	June 1,	59	54	23	29	10	20	38	27	15	28	19	50	44
	June 9,	15	28	19	50	44	40	52	69	80	52	40	47	73
	July 12,	35	45	48	75	69	47	48	71	35	63	79	49	47
Mean of 60 days obs.		48·9	49·3	47·4	49·5	52·5	52·3	56·3	53·2	47·1	48·6	47·5	41·8	47·6
Mean of 251 days obs.		50·3	52·7	51·0	51·2	53·1	53·7	60·5	54·8	52·5	53·3	51·4	53·3	50·7

1. Those cases in which the observers were not entirely confident that there was any auroral display.
2. Those cases in which the corresponding observations of the solar spots were very incomplete; and
3. Those cases in which auroras were observed on two or three successive days, when I have generally selected the most remarkable aurora, and made but one entry for that period.

The number of auroras which I have thus discussed is 251. In order to furnish a specimen of these numbers, the table on page 170 is given, in which the arrangement is the same as in the table on page 168. At the bottom of the table are given the averages of these numbers for each of the 13 columns; and in another line are given the corresponding averages for the whole number of 251 auroras.

These final averages are represented by the upper curve line in the figure page 169, from which it will be seen that there is a well-marked maximum of solar disturbance corresponding to the date of an auroral display. The small fluctuations during the preceding and following days, bear some resemblance to the fluctuations attending magnetic storms, but they are so small in amount that no importance is attached to them. The entire fluctuation within 6 days extends from 50·3 to 60·5, or 20 per cent of the whole quantity; a number so large and derived from so many cases that it is thought to indicate a law of nature. Hence we conclude that

Auroral displays in the middle latitudes of America are generally accompanied by an unusual disturbance of the sun's surface on the very day of the aurora, and are, therefore, subject to some influence which emanates immediately from the sun.

These conclusions may be modified by a comparison of a longer series of observations, and especially by more accurate observations which furnish for each day an exact measurement of the extent of the sun's spotted surface. Such observations have been made for several years at the Kew observatory, and it is hoped that when published they will furnish the materials for the desired comparison. The observations for 1862 and 1863 have already appeared in the Philosophical Transactions for 1869, pp. 23–44; and it is expected that the observations for the subsequent years will soon follow.

ART. XVII.—*On a new Period in Chronology, called the Precession Period;* by J. W. FRENCH. (In a letter to the Editors.)

I PROPOSE in the place of the Julian Period in chronology, another which I will call the Precession-Period.

This latter will be found to have all the advantages of the other without its defects, and beyond these comparative utilities, to have chronological uses of its own which are great and various, both for the subdivisions of history, and for the vast cycles contemplated by science.

The Julian Period in chronology consists of 7,980 Julian years, that number being formed by the continual multiplication of 28, 19 and 15; that is to say, of the cycle of the sun, the cycle of the moon, and the cycle of indiction. The first year of the Christian Era is made the 4,714th of the Julian Period. By such an arrangement we can find for any year its golden number, its number for the solar cycle, and that for its Roman Indiction. Also, we have a fixed period reaching back in history among the local and broken cycles of different peoples and countries. These certainly are great advantages, and have secured for that Period acceptance and commendation.

But some of its defects are these. 1. It has an artificial element, that of the Roman Indiction, instead of having its foundation wholly in astronomy. 2. It is soon exhausted, and in the past, it does not reach far enough even for the Septuagint Chronology in history. 3. It furnishes no unit nor cycle for science.

The period which I propose is founded wholly on astronomy, is exhaustless by being recurrent, has its initial point sufficiently far back for any conceivable historical purposes, gives to science a worthy unit for the vast durations it contemplates, and with these inestimable advantages, has the two practical utilities of the Julian Period;—that of giving the elements needed in the almanac for every year, and that of extending into the past a long and unaltering standard for time.

I propose to take, as the chronological unit, the time for the precession of the equinoxes, 25,872 *years.* By a singular felicity, that number can be formed from the factors 28, 84, and 11. Now, 28 is the number of the solar cycle; 84 is a lunar cycle employed formerly by the Jews, and having peculiar uses in estimating long series of lunations. The other number, 11, is a lunar cycle employed by the early Christians, the errors of which almost exactly counteract those of the cycle of 84 years. These three multiplied together form 25,872. A subordinate felicity will at once be seen by a complete mathematician. *The number* 25,872 *can be divided and subdivided to the last, without a remainder, by a large number of divisors.*

Making the years Gregorian, and calling the initial year of the Christian Era, 0, *I place the beginning of the Precession Period at* 12,693 *B. C.*

Instantly we have by that arrangement, the advantages which have brought the Julian Period into favor.

We can find the solar cycle, the golden number, the Roman Indiction (correctly as we would from the Julian Period) by the following simple rules:

1. To express any year before or after Christ in the correspondent number in the Precession-Period: For any year before Christ, deduct from 12,693, the figures of the year B. C.: For any year after Christ, add to the same number (12,693) the figures of the year A. D.

Thus 752 B. C. (753 *historical reckoning*) is 11,941 P. P. The present year 1870 A. D. is 1,870+12,693=14,563 P. P. (We use P. P. as abreviation for Precession-Period.)

2. To find the Golden Number, Solar Cycle, and Roman Indiction for any year before or after Christ: 1. Turn the year by the first rule into the correspondent number of Precession Period: 2. Divide that number by 19 for the golden number, by 28 for the solar cycle, and by 15 for the Roman Indiction. In the three remainders you have the answer.

Thus the present year 1870 A. D., is 14,563 P. P. Dividing this latter number by 19, I find a remainder of 9; by 28, a remainder of 3; and by 15, a remainder of 13. Opening the Almanac for this year, I find the answer correct. It gives the Golden Number as 9, the Solar Cycle as 3, the Roman Indiction as 13.

The Precession-Period has then in this particular, equal utility with the Julian.

The other advantage, that of a fixed standard of time extending back in past history, it possesses, and adds the great benefit of adequate length in both directions, to 12,693 B. C., and on to 13,179 A. D.; the whole forming one precession. And this precession is not like the Julian Period, an arbitrary straight line stretched over a small portion of duration. It is a definite circle marked on the face of the heavens. The precession begins at 12,693 B. C. with the *point of the vernal equinox in the Zodiac near Spica Virginis*, a brilliant star, forming a good point of departure. When the whole circle of the Zodiac is swept, and the first degree of celestial longitude is again by *Spica Virginis*, the period is completed.

Surely it is better to adopt such a period for our almanacs and histories than the inferior one called the Julian.

But beyond the historical and chronological uses in the *subdivisions* of the Precession-Period, are the advantages of that period as a unit for Astronomy and Geology. We want a unit larger than the year. A precession is a good one. Instead of cumbering a line of page with bewildering cyphers we can say 40, 100, or 1,000 precessions. Thus 40 precessions (and forty is a number easily remembered) would, by another felicity of this period, make a million of years, with a little over. We might call it a millionade, and give that again multiplications to form an *age*. An "age" might be 400 precessions.

Should we adopt any thing like this plan, we should have the same delightful surprise which we often experience, by finding that ancient races and nations have been along the same pathway which we imagined ourselves to be for the first time breaking and exploring. Six days, such as are His, who in His times as in His nature, must be what others are not; six immense periods are reckoned in the Creative work not only in Genesis, but in Oriental Records. Take 100 precessions. Divide them by that number, six. You have the very periods recognized by the Brahmins of India, each about 430,000 years.

West Point, June 7, 1870.

ART. XVIII.—*Upon the Atomic Volumes of Solid Compounds;* by FRANK WIGGLESWORTH CLARKE, S.B.

IN studying the atomic volumes of solid compounds, the materials at my command have been in some respects quite copious, and in others quite limited. Having been unable, through lack of opportunity, to make any new determinations of specific gravities, I have been forced to content myself with the data which are scattered through the various scientific publications. These, apart from the views expressed in nearly a hundred papers written by various chemists upon atomic volumes, consisted of about 1,900 determinations of the specific gravities of 912 different solids of definite constitution, exclusive of alloys. Much of this material had to some extent been already worked over, although the larger part of it had never been examined in this direction. In some cases there are data covering a whole series of compounds, in others only one or two members of a series have been studied, while again, for many important substances no determinations of specific gravity have ever been taken. Again, for some compounds there are many determinations by different authorities, and these bodies can be studied with much certainty; while for other compounds only single observations of specific gravity have been made, and these often with no pretence to rigid accuracy.

Here then at the very outset is a difficulty. If a substance has a very low specific gravity, and a very high atomic weight, a minute error in the determination of the first value must, in calculating the atomic volume, become greatly multiplied; so that a single experimental observation only, may often lead to wholly erroneous results for the atomic volume. Thus, for instance, with Melene, $C_{30}H_{60}$, an error of only 0·04 in the specific gravity will alter the atomic volume about 24·00. And it is very common to find the determinations of specific gravity for a single compound differing more than 0·04. And, as I have already stated, many of the specific gravities observed have never had more than approximate accuracy claimed for them. Hence, one of the chief difficulties in comparing atomic volumes lies in deciding how great variations can be safely ascribed to experimental error. For this purpose, instead of directly comparing the actual atomic volumes with the results of theory, I have preferred to compare the specific gravities calculated from the latter with those really determined by experiment. Even here much care is needful, since errors are more likely to occur with some compounds than with others. Thus, there is much more danger of error in determining the specific gravity of anhydrous magnesic chlorid, than in taking that of baric sulphate.

Furthermore, many different modes of taking specific gravity are represented by the values from which I have to calculate. Some compounds have been examined in the form of powder, and others have been crystallized; some have been studied near their melting point, and others distant therefrom. Nevertheless, in spite of all these possibilities of error and chances of irregularity, certain curious series of relations between atomic volumes are easily demonstrated; which, taken together, hint strongly at a rather more general theory of atomic volumes than has hitherto been enunciated.

But, to begin with, we need a brief resumé of certain points which have been demonstrated by other observers.

First comes the axiomatic statement of Schröder that the atomic volume of a compound must equal the sum of the atomic volumes of its constituent parts. This at once suggests questions of interest. Although many compounds, especially certain sulphids, selenids, and tellurids, have atomic volumes equalling the sums of those of the *free* elements composing them, many other compounds have values lower than such sums, thereby indicating condensation. And some iodids have atomic volumes greater than the sums of those of the metal and the iodine. So here arises the question,—when a compound possesses a greater or less volume than the elements contained in it do in the free state, do those elements condense or expand in equal or in different ratios? It is really upon this point that the study of atomic volumes hinges; so that, stripped of all metaphysical notions of atoms and spheres of heat, the subject simply concerns the distribution of the changes of volume undergone by the various elements in uniting to form compounds.

Second. It has been shown, most definitely by Schröder and Kopp, that, in many cases, when from the atomic volumes of certain series of salts, oxyds, &c., we subtract the atomic volumes of the respective metals contained in them, we obtain constant remainders which may be regarded as representing the values of the various radicals and of oxygen. Thus, if from the atomic volumes of one class of oxyds we remove the values of the metals, we obtain the remainder 2·6. With other oxyds we get 5·2, and with still others, 10·4. These are provisionally regarded as the atomic volumes of oxygen in its solid compounds. These different values for a single element are of course natural consequences of the fact that the various compounds containing it, undergo, in their formation, different degrees of condensation from the free elements composing them. But, in assuming these three numbers to be the atomic volumes of oxygen, one assumption is made which is by no means allowable, viz: that the metals entering into these compounds undergo no condensation themselves, all the change in volume taking place in the oxygen. But, some oxyds have atomic volumes lower than those of the metals contained in them. And yet the remarkable multiple relation between the three values quoted above cannot be due to accident. These points will be referred to again.

Third, we come to some results which I myself published rather more than a year ago.* In my last paper upon this subject I pointed out some curious multiple relations connecting the atomic volumes of similar solid elements. For the proofs of these relations I must refer to the above mentioned paper, but the values for the elements themselves I am obliged to cite here for further reference. I give the values for solids only. Li 11·4, Na 22·8, K 45·6, Rb 57·0. These stand to each other as 1 : 2 : 4 : 5. I 25·6, Tl 17·2, Ag and Au 10·2.

Here arises an interesting question. The atomic volumes of Ag and Li are quite near together. But the specific gravity of lithium was determined comparatively near the melting point of that metal, while that of silver was taken at a temperature greatly removed from the degree at which it fuses. Now *melted* silver, according to Playfair and Joule, has the specific gravity 9·206; and consequently an atomic volume of 11·7, nearly that of solid lithium. May not these two metals then, under similar circumstances, and at strictly comparable temperatures, be supposed to have equal atomic volumes?

O 2·6, 5·2, 10·4. S 10·4, 15·6. Se 10·4, 15·6.(?) Te 20·8. This value for S is that of the octahedral variety, prismatic

---

* This Journal for March and May, 1869.

sulphur having 16·3–16·7. Amorphous selenium has an atomic volume of 18·6.

Sr, 34·3. Ba, 34·2.
Ca, 25·8. Pb, 18·2.

As and P 12·9, Sb 17·2, Bi 21·5. These stand as 3 : 4 : 5. This value for P is that of the so-called "metallic" variety. Common phosphorus has an atomic value of 16·9–17·0, (perhaps 17·2, like Sb?) and the ordinary red modification 13·9–14·5. Amorphous arsenic has the value 15·9. Bo 4·1. Vanadium in my last paper I calculated theoretically from the atomic volume of one of its oxyds, making it equal to As and P. Since then, however, this idea has been overthrown, Roscoe having determined the specific gravity of the metal itself. This, 5·5, gives an atomic volume of 9·4, which differs widely from my supposititious value, and bears no definite relation which I can see, to those of As and P.

C (graphite), 5·5, Si 11·0, Ti 11·0 (?), Sn 16·5. These stand as 1 : 2 : 2 : 3. C (diamond) 3·4. Zr 21·7 (perhaps 22·0, or 5·5×4.)

Cr, Mn, Fe, Co, Ni, U,* and Cu,	6·9	These four values stand as 3 : 4 : 5 : 6.
Zn, Pt, Ir, Os, Pd, Ru, and Rh,	9·2	
Mo and W,	11·5	
Cd, Mg, and Hg, (solid)	13·8	

Gl 43,† Al 10·1–10·6, Th 30·4–30·9, Ce 16·7, In 10·2.

Before proceeding farther, it now becomes necessary for me to allude briefly to some regularities in atomic volumes which have already been traced by others. First, the alums have equal atomic volumes. To the evidence which has been cited by other investigators in proof of this, may now be added the atomic volumes of rubidium and cæsium alums, whose specific gravities, taken by Redtenbacher, show that they follow the regular rule. Second, a number of similar carbonates, especially those of Zn, Mg, Fe, and Mn, have equal atomic volumes. Third, a similar relation connects the vitriols with 7 aq. This series is especially important, since it contains the sulphates of Fe, Ni, Co, Mg, Zn, and Cd. Fourth, the oxyds allied to Gahnite, with the general formula MO, M_2O_3, are, with one or two exceptions, equal in atomic volume. Fifth, many corresponding phosphates and arsenates have equal values. Other series have been traced here and there, but these are perhaps the most striking.

Now, in the first place, we may lay it down as a general rule that when two similar elements have equal atomic volumes, the

---

* Possibly this may not belong in this group, but its equal atomic volume be merely a coincidence. The values of some of its compounds suggest this idea.

† Calculated from the lower atomic weight for Gl. In my last paper I used the higher.

atomic volumes of their corresponding compounds will also be equal. Of the truth of this I will cite a few examples, and point out some exceptions. For want of space I cannot adduce all the cases I have accumulated, however, but will merely attempt to illustrate the principle, and afterwards bring the exceptions under another rule.

Barium and strontium. Sr Cl_2, sp. gr. 2·8033, Karsten. Ba Cl_2, 3·704, Karsten. At. vols., 56·5 and 56·2.

SrI_2 4·415, Bödeker. BaI_2 4·917, Filhol. At. vols., 77·3 and 79·5. Sr H_2O_2, 8 aq., 1·396 Filhol, Ba H^2O_2, 8 aq., 1·656, Filhol. At. vols., each 190·2.

The sulphates and nitrates, bromids, carbonates, and simple hydrates, are either doubtful or exceptions. In all these cases the compounds of Ba have the highest values.

Iron group. Here examples are so abundant that I will only mention a few. MnO sp. gr., 5·38, P. & J.* NiO 5·597, P. & J. CoO 5·597−5·750, P. & J. UO 10·15, Ebelmen. CuO 6·130, Boullay. At. vols., respectively, 13·0, 13·4, 13·0−13·4, 13·4, 12·9.

Cr_2O_3 4·950, P. & J. Fe_2O_3 4·679−5·135, P. & J. Ni O_3 4·814, P. & J. Co_2O_3 4·814, P. & J. At. vols., 30·7, 31·1−34·2, 34·4, 34·4. Mispickel, FeS_2, $FeAs_2$ 6·0−6·4 Gersdorffite, NiS_2, $NiAs_2$ 5·6−6·9. Cobaltite, CoS_2, $CoAs_2$, 6·0−6·3. At. vols., 50·9−54·3, 48·1−59·2, 52·6−55·3.

FeS 5·035, P. & J.: NiS 5·650, Rammelsberg; 4·601 Kenngott. CoS 5·45. At. vols., 17·5, 16·1–19·7, 16·7. Exceptions, MnS and CuS, having sp. grs. 3·95, Dana's Min., and 4·163, Karsten; and at. vols., 21·8 and 22·9. It is worth noting that these two sulphids stand at the ends of the series; CrS and US, being (if they exist), wholly unstudied in this direction.

One more striking exception may be found by comparing FeS_2, MnS_2, and CoS_2, whose atomic volumes are very dissimilar.

Platinum group. 2KCl, $PtCl_4$; and 2KCl, $IrCl_4$. Sp. grs. Bödeker, 3·586 and 3·546. At. vols., 136·2 and 137·9.

PdP_2 and PtP_2 have unequal values.

Molybdenum and tungsten. It has been proved by other authorities that MoO_3 and WO_3 have equal atomic volumes.

Cadmium group. CdO 6·9502, Karsten. HgO 11·344, P. & J. At. vols., 18·4 and 19·0. MgO has an exceptional value which will be studied in another connection.

Phosphorus and arsenic. Cu_3P 6·59, Hvoslef. Cu_3As 7·62, Genth. At. vols., 33·6 and 34·9.

P_2O_5 2·387, Brisson. As_2O_5 4·25 Filhol; 3·7342, Karsten. At. vols., 59·5 and 54·1−61·6.

---

* Playfair and Joule. This is the only abbreviation of the sort which I shall have occasion to employ. When I give no authority for a specific gravity, it will be found in "Dana's Mineralogy," last edition.

Phosphates and arsenates have been compared by others, Where their values differ, that of the arsenate is the highest.

Silicon and titanium. The acids of this group will be cited farther along. Fayalite, $2FeO\ SiO_2$ 4·006. $2FeO\ TiO_2$ 4·370 Hautefeuille. At. vols., 50·9 and 51·7.

The only definite exception is found in comparing $2KF\ SiF_4$; sp. gr. 2·6652, Stolba; and $2KF\ TiF_4$, 2 080, Bödeker. At vols., 82·5 and 116·3.

These examples will suffice for the present, although I shall have occasion to cite others to illustrate another point. Upon comparing all the material which I have collected, I find the exceptions to be quite rare, although, perhaps, I have given them undue prominence here.

Second, similar compounds of similar metals often have equal atomic volumes, even when those of the metals themselves are unequal. The corresponding compounds of the iron, platinum, and cadmium groups often exemplify this most strikingly. The exceptions to the previous rule are probably due to this. Thus, although Mg and Cd have equal atomic volumes, their oxyds show no such equality; that of the first metal having shaded off into approximate uniformity with those of its kindred iron group.

Of the rule under consideration, however, the most striking case hitherto adduced is that of the vitriols. As I have already stated, the sulphates with 7 aq. of Fe, Ni, Co, Zn and Mg, have equal atomic volumes, while the double salts of the same class, containing in addition to the above metals Cu and Cd, follow the same rule. A similar regularity connects the anhydrous sulphates of most of these metals. But an equally remarkable series is formed by the chlorids of the same metals, which, of the formula MCl_2, have, as far as they have been studied, atomic volumes which are very nearly equal. If we present these chlorids in tabular form, taking the average of their observed atomic volumes as the real value, we shall find that the variations from their average are wholly within the limits of experimental error.

$FeCl_2$.	Sp. gr.	2·528, Filhol.	At. vol.,	found,	50·2	Mean, 47·6.	Sp. gr.	calc.,	2·668
$NiCl_2$	“	2·560, Schiff.	“	“	50·7		“	“	2·726
$CoCl_2$	“	2·937, P. & J.	“	“	44·2		“	“	2·726
$CuCl_2$	“	3·054, “	“	“	44·0		“	“	2·825
$ZnCl_2$	“	2·753, Bödeker.	“	“	49·4		“	“	2·857
$PtCl_2$	“	5·8696. “	“	“	45·7		“	“	5·638
$MgCl_2$	“	2·177, P. &J.	“	“	43·6		“	“	1·996
$CdCl_2$	“	3·6254, Bödeker,	“	“	50·5		“	“	3·844
$HgCl_2$	“	5·4032, Karsten.	“	“	50·1		“	“	5·693

Now, if we take into account the difficulty of obtaining perfectly accurate determinations of sp.gr. for some of these substances; and also bear in mind that for most of them only single observations have been made, it is clear that every variation from equality may be ascribed to errors in experiment. The extreme difference in atomic volume is from a minimum of 43·6 to a maximum of 50·7. It is not rare for an equal divergence to occur between different determinations for a single substance.

Again, although chlorids, bromids, and iodids have unequal atomic volumes, those of similar chlorates, bromates, and iodates are equal,—at least as far as we have any data.

$NaClO_3$, sp. gr., 2·289, Bödeker. $NaBrO_3$, 3·339, and $NaIO_3$, 4·277, Kremers. At. vols., 46·5, 45·2, and 46·3.

$KClO_3$, sp. gr., 2·325, Buignet. $KBrO_3$, 3·271, and KIO_3, 3·979, Kremers. At. vols., 52·7, 51·1, and 53·8.

Most similar compounds of silver and sodium have equal atomic volumes. This holds true of the chlorids, bromids, and iodids (as will be shown in another connection hereafter), and of the sulphates, chlorates, nitrates, and probably carbonates. The sulphids and oxyds are exceptions.

Some compounds of As, Sb, and Bi, have equal values.

As_2O_3, sp. gr., 3·695, Guibourt; 3·884, Filhol. Sp_2O_3, 5·11, Terreil; 5·78, Boullay. Bi_2O_3, 8·079, P. & J.; 8·450, Leroyer & Dumas. At. vols., 51·0—53·6; 50·5—57·1; and 55·4—57·9.

In the carbon group compare certain oxyds.

SiO_2, 2·663, Deville. TiO_2 (artificial anatase) 3·700, Hautefeuille. SnO_2 6·720, Daubrée. At. vols., 22·5, 22·2, and 22·3.

Would space permit, I might go on multiplying examples to an almost indefinite extent; but my object at present is merely to illustrate certain principles. There are other regularities yet to be noticed. The first of these is comparatively unimportant, and I will merely state it as it is, without adducing any evidence. All sulphates have atomic volumes which are lower than the sum of the metallic oxyds and the SO_3. That is, the value for $MnSO_4$ is less than the sum of those of MnO and SO_3. To this rule I have found no exceptions. Whether a similar rule holds good for chromates, molybdates, and tungstates, I am not certain; the materials at my command being too limited to settle the question.

Another regularity is more remarkable. It is noteworthy that certain oxyds and sulphids have atomic volumes which are lower than those of the metals contained in them. Now, in a number of cases, I find that those atomic volumes equal *half* the sum of those of the metal and the other element, provided that $S=15·6$, and O receives its highest value, 10·4. I give oxygen this value, for this reason. Many sulphids have atomic volumes, as I have already stated, equal to the sum of those of the metal and sulphur in the free state; so that it is probable that some oxyds follow the same rule. 10·4 being the highest number for oxygen, renders it likely that that is the true value for this element in those oxyds corresponding to the above mentioned sulphids.

Now, two sulphids appear to follow this rule, viz: Na_2S and K_2S, whose sp. gr., given by Filhol, are respectively 2·471 and 2·130. At. vols., 31·6 and 51·6. Now $\frac{2(22·8)+15·6}{2}=30·6$; and $\frac{2(45·6)+15·6.}{2}=53·4$. If these theoretical atomic volumes are true, then the sp. gr. of Na_2S will be 2·549, or 0·078 greater than the value found; while that of K_2S will be 2·059, varying 0·071 from Filhol's number. These variations are wholly within the limits of error for such compounds.

But the results obtained with four oxyds are more striking.

MgO.	Sp. gr.	3·200, Karsten.	At. vol.	12·5.	$\frac{13·8+10·4}{2}=12·1.$	Error	0·4
CaO.	“	3·161, “	“	17·7.	$\frac{25·8+10·4}{2}=18·1.$	“	0·4
SrO.	“	4·611, Filhol.	“	22·4.	$\frac{34·3+10·4}{2}=22·35.$	“	0·05
Al_2O_3.	“	3·928, Ebelmen.	“	25·9.	$\frac{2(10·2)+3(10·4)}{2}=25·8.$	“	0·1

The oxyds of barium, sodium, and potassium, do not follow this rule.

One more regularity traced, and I am done. If my views concerning multiple relations are correct, and all the values for oxygen and sulphur are multiples of the lowest, then we must expect that compounds formed by the union of these elements will have atomic volumes which are also multiples. Now, sulphuric anhydrid, SO_3, has, according to Buff, the sp. gr. 1·909 at 25°, and according to Baumgartner 1·975. Its atomic volume, then, is from 40·5 to 41·9. And 41·6 is precisely four times 10·4!

The chlorids, bromids, and iodids of the alkali metals and silver, seem to afford a similar example. In one of my previous papers I showed that Kopp's values for Cl, Br, and I, in their liquid compounds were almost exact multiples of his number for H, 5·5 Consequently, judging from analogy, it is likely that these elements in their solid compounds would follow a similar rule. Now the metals Li, Na, K, and Ag, have atomic volumes which do not vary greatly from multiples of 5·5. And, in accordance with what we should expect, their chlorids, bromids, and iodids have atomic volumes which are either exact, or so nearly exact, multiples of 5·5, that the circumstance cannot be ascribed to accident.

I present a tabular view of this regularity.

LiCl.	Sp. gr.	1·998, Kremers.	At.	vol. found,	21·3.	Calc.,	22·0.	Sp.	gr. calc.,	1·932
NaCl	“	2·145, Buignet.	“	“	27·2	“	27·5	“	“	2·127
KCl	“	1·945, Kopp.	“	“	38·3	“	38·5	“	“	1·935
AgCl	“	5·130, Herapath.	“	“	27·9	“	27·5	“	“	5·218
NaBr	“	3·079, Kremers.	“	“	33·4	“	33·0	“	“	3·121
KBr	“	2·672, P. & J.	“	“	44·5	“	44·0	“	“	2·704
AgBr	“	5·8–6·0.	“	“	32·3–32·4	“	33·0	“	“	5·697
NaI	“	3·450, Filhol.	“	“	43·5	“	44·0	“	“	3·409
KI	“	3·056, Filhol.	“	“	54·3	“	55·0	“	“	3·018
AgI	“	5·350, Schiff.	“	“	43·9	“	44·0	“	“	5·341

On comparing these numbers, it will at once be noticed, not only that the variations from theory are remarkably slight, but also (which has been previously stated) that the corresponding compounds of Na and Ag have nearly equal, or equal atomic volumes, that the values of potassium compounds exceed those of the sodium compounds by 11·0, (5·5×2), that bromids exceed chlorids by 5·5, and that iodids have atomic volumes 11·0 greater than bromids. These regularities in difference between chlorids, bromids, and iodids, however, do not appear so distinctly in other series of them.

Now, to sum up the important relations traced in this paper, bearing in mind that in many cases exceptions exist.

First. When similar metals have equal atomic volumes, those of their similar compounds will also be equal.

Second. Metals whose atomic volumes are unequal, but simply related to one another, often form similar compounds having equal values.

Third. Some compounds have atomic volumes which stand in very simple relations to the sums of those of the free elements which form them.

Fourth. Compounds formed by the union of similar elements, have atomic volumes which are multiples of the lowest for that group.

And, fifth, we may add the multiple relations traced in my former papers, which not only connect the atomic volumes of different elements, but the various values for each single element.

Now, what do these regularities mean. Are we justified either in drawing any direct conclusions from them, or in basing upon them any general theory of atomic volumes? To this I must answer, that, although no generalization is absolutely established by them, it seems to me that one is decidedly hinted at. May we not say that in all compounds, the atomic volume of every element will be either a perfect multiple of the lowest value for that element, or of the lowest value in the group to which it belongs? Although this theory cannot be regarded as entirely proved, it certainly possesses a considerable degree of probability, and seems to harmonize well with the regularities which I have pointed out. But why an element should have a higher value in one compound than in another, remains to be accounted for, although upon this point, perhaps, Buff's idea that the different degrees of quantivalence of an element in its various compounds, cause the differences in its atomic volumes, may prove correct.*

But at all events, whether the theory which I put forward turns out true or false, it may, perhaps, by lending some system to the study of atomic volume, pave the way for something of greater value.

Boston, May 30th, 1870.

ART. XIX.—*Considerations on the apparent inequalities of long period in the mean motion of the Moon;* by SIMON NEWCOMB.

[Read to the National Academy, April, 1870.]

THE problem of determining the motion of the moon around the earth under the influence of the combined attraction of the sun and planets has, more than any other, called forth the efforts of mathematicians and astronomers. Nearly every great geometer since Newton has added something to the simplicity or the accuracy of the solution, and, in our own day we have seen it successfully completed in its simplest form, in which the earth, the moon, and the sun are considered as material points, moving under the influence of their mutual attractions. The satisfactory solutions are due to the genius of Hansen and of Delaunay. Working independently of each other, each using a method of his own invention more rigorous than had before been applied, they arrived at expressions for the longitude of the moon which, being compared, were found to exhibit an average discrepancy of less than a second of arc. No doubt could remain of the substantial correctness of each.

The solutions here referred to exhibit only inequalities of short period in the motion of the moon. But, it has long been known, from observation, that the mean motion of the moon is subject to apparent changes of very long period, and especially to a secular acceleration by which it has been gradually increasing, from century to century, since the time of the earliest recorded observations. If we inquire into the problem of these inequalities of long period, we shall find it seemingly no nearer a final solution than it was left by La Place, observation having since added more anomalies than theory has satisfactorily shown to exist.

The first inequality in the order of discovery was the secular acceleration. This was discovered about the middle of the last century by a comparison of ancient eclipses with modern ob-

---

* See Buff's paper in the Annalen d. Chem. u. Pharm., 4th supplement vol., 1865–6. Or, see his "Grundlehren der theoretischen Chemie."

servations. Its cause was first discovered by La Place, who showed that it was due to the effect of the action of the planets in changing the eccentricity of the earth's orbit.

The results of his computations agreed substantially with observations, and was therefore received with entire confidence until less than twenty years ago. The question being then taken up by Mr. John C. Adams, this eminent mathematician was led to the conclusion that La Place's result was nearly twice too large.

The same conclusion was reached independently by Delaunay, and gave rise to a remarkable discussion, the history of which is too familiar to be now recounted. It is now conceded that the value found by Adams and Delaunay is theoretically correct.

The new result no longer agreeing with observation, the difference is now accounted for by an increase in the length of the day. That this length is increasing is also known from theoretical considerations, but the data for its accurate determination are wanting.*

In the third volume of the *Mécanique Céleste* (Seconde Partie, Livre vii, Chapitre v) La Place discusses an apparent inequality of long period in the motion of the moon. The discussion is mainly empirical. The existence of the inequality is inferred from observations, these showing that the mean motion of the moon during the half century following 1756 was less than during the half century preceding. He then assumed that the inequality was due to the fact that twice the mean motion of the moon's node, plus the motion of its perigee, minus that of the sun's perigee was a very small quantity, less than two degrees per annum, and determined the coefficient of the varying angle solely from the observations. The result was that these might be satisfied by supposing the inequality of mean longitude

$$\delta l = 47''\cdot 51 \ [\text{or } 15''\cdot 39] \sin\left(2\,\Omega\text{☽} + \pi\text{☽} - 3\pi\odot\right)$$

If, in this expression, we substitute Hansen's values of the elements, it becomes

$$\delta l = 15''\cdot 39 \sin\left[173^\circ\ 26' + (1^\circ\ 57'\cdot 4)\,(t - 1800)\right].$$

When in 1811 Burckhardt constructed his tables of the moon,

---

* The time and place when the discordance referred to was first distinctly attributed to the tidal retardation of the earth having been a subject of discussion, the following extract from an article on "Modern Theoretical Astronomy" in the North American Review for October, 1861 (vol. 93, p. 385), may not be devoid of interest.

"It seems to be well established that the new theory is inconsistent with the observations of ancient eclipses, and if it should prove to be correct, we may be driven to the conclusion, that a portion of the acceleration proceeds from some other cause than the attraction of gravitation, or that the length of the day is gradually increasing to an extent which has become perceptible from the cause to which we have already referred [the tidal retardation, p. 374]. If, as centuries roll by, the day should gradually increase, the moon would move a little farther in the course of a day than if no such increase should take place. Since, in our calculations, we suppose the day constant, the apparent acceleration would be greater than the real—precisely the effect observed. The difference can be entirely accounted for by supposing an increase of something less than one thousandth of a second per century in the length of the day, and a corresponding diminution in the lunar month."

he omitted the sun's perigee from this argument by the authority of La Place, himself, who now attributed the inequality to a difference of compression between the two hemispheres of the earth. The function was also changed from *sin* to *cos* and the coefficient altered. The adopted term thus became

$$\delta l = -12''{\cdot}5 \cos\,[291^\circ\ 57' + (2^\circ\ 0'{\cdot}45)(t-1800)]$$
$$= \ \ 12''{\cdot}5 \sin\,[201^\circ\ 57' + (2^\circ\ 0'{\cdot}45)(t-1800)]$$

Succeeding investigators have regarded the theoretical coefficients of both of these terms as insensible. It does not seem likely that there is any such difference between the two terrestrial hemispheres as could produce the second, but I am not aware that the coefficient of the first has ever been shown to be insensible by any published computation. This coefficient is of the ninth order and the argument is,

In Delaunay's notation, $3D-2F-l+3l'$;
In Hansen's, $w-3w'$.

The period is 184 years, and the large value of the ratio of this period to that of the moon itself might render the coefficient sensible. Both Hansen and Delaunay pronounce it insensible, but neither publish their computations of its magnitude.

These terms have ceased to figure in the theory of the moon since Hansen announced that the action of Venus was capable of producing inequalities of the kind in question. So far as I am aware, Hansen's first publication on this subject is that found in No. 597 of the Astronomische Nachrichten (B. 25, S. 325.) Here, in a letter dated March 12, he alludes to La Place's coefficients, and says he has not been able to find any sensible coefficient for La Place's argument of long period. But on examining the action of Venus on the moon he found, considering only the first power of the disturbing force, the following term in the moon's mean longitude:

$$\delta l = 16''{\cdot}01 \sin\,(-g-16g'+18g''+35^\circ\ 20').$$

g, g' and g'' being the mean anomilies of the moon, the earth and Venus respectively. As this expression still failed to account for the observed variations of the moon's longitude he continued the approximation to the fourth power of the disturbing force, and found that the terms of the third and fourth order increased the coefficient to $27''{\cdot}4$, the angle remaining unchanged, so that the term became

$$27''{\cdot}4 \sin\,(-g-16g'+18g''+35^\circ\ 20'),$$

But this increase made the theory rather worse, and the term depending on the argument of Airy's equation between the earth and Venus was then tried with the result—

$$\delta l = 23''{\cdot}2 \sin\,(8g''-13g'+315^\circ\ 30').$$

The introduction of this term seemed to reconcile the theory with observation.

Hansen finally remarks that these values of the coefficients are still subject to some uncertainty from his not having employed decimals enough in his computation.

In a letter to the Astronomer Royal, published in the Monthly Notices of the Royal Astronomical Society for Nov. 1854, Hansen gives a statement of the elements employed in his tables of the moon, and refers to the subject of these inequalities in the following terms:—

"The accurate determination of these two inequalities by theory is the most difficult matter which presents itself in the theory of the moon's motion. I have on two occasions and by different methods sought to determine their values, but I have obtained results essentially different from each other. I am now again engaged with their theoretical determination by a method which I have simplified, and hope to bring the operation to a definitive close. I have also applied to my tables some coefficients which are not free from empiricism but which I can justify by the circumstance that they represent the ancient as well as the modern observations with great exactness, and it may be expected that they will represent the future observations equally well."

Hansen's lunar tables were published in 1857.

The terms of long period finally adopted are

$$15''\cdot34 \sin(-g-16E+18V+30^\circ\ 12')$$
$$+21\cdot48 \sin(8V-13E+274^\circ\ 14'),$$

V and E representing the mean longitudes of Venus and the earth. Changing them to mean anomalies the terms become

$$15''\cdot34 \sin(-g-16g'+18g''+33^\circ\ 36')$$
$$+21\cdot47 \sin(8g''-13g'+4^\circ\ 44').$$

It appears that while the first term has been restored to what was substantially its original value, when only the first power of the disturbing force was included, the argument of the second term has been changed by 50°, the coefficient being but slightly changed.

In a letter to the Astronomer Royal, dated 1861, Feb. 2d, found in the Monthly Notices for March, 1861, Hansen again refers to this second term with the statement that its coefficient is one of those somewhat empirical. At the same time he has found the coefficient, by his last theoretical determination of it, by no means insensible, like Delaunay. He adds that in the comparison with observation he has never gone beyond Bradley, nevertheless his tables satisfactorily represent the ancient observations.

A well marked feature of Hansen's published works is the copiousness and perspecuity with which his theoretical calculations are laid down. But, so far as I am aware he has never published any computation of these inequalities except that part of the first inequality which depends on the first power of the disturbing force. This computation is found in vol. xvi of the Memoirs of the Royal Astronomical Society. In the second part of his "Darlegung" we find a general method of treating inequalities of long period, but—unless I have overlooked it—no computation of any particular inequality. Nor do we find any statements of the numerical results of Hansen's various computations except those already quoted.

The only geometer besides Hansen who has attacked the problem of these inequalities is Delaunay. His researches are published in full in the Additions to the Connaissance des Temps for years 1862 and 1863. For the first approximation to the first inequality his result is

$$16''\cdot02 \sin(-l-16l'+18l''+35° \; 20'\cdot2)$$

a result practically identical with that of Hansen. The ulterior approximations change it to

$$16''\cdot34 \sin(-l-16l'+18l''+35° \; 16'\cdot5),$$

so that they increase the coefficient instead of diminishing it as in Hansen's theory. The difference is however so small that the results may be regarded as identical.

But, in the case of the second inequality instead of reproducing the result of Hansen, he finds a coefficient of only $0''\cdot27$, a quantity quite insignificant in the present state of the question. We have thus an irreconcilable difference on a purely theoretical question.

I propose to inquire whether we have in either theory an entirely satisfactory agreement with observation. As a preliminary step to this inquiry I have prepared the following table of the mean longitude of the moon from the tables of Burckhardt and of Hansen respectively, for a series of equidistant dates, the interval being 3652·5 days, and the epoch 1800 Jan. 0, Greenwich mean noon. These dates are marked by the year near the beginning of which they fall. Column L_0 gives Burckhardt's mean longitude on the supposition of uniform motion, from the data given on the fifth page of the introduction to his tables. Next is given the acceleration of the mean longitude deduced from Table XLVIII. The inequality of long period is from Table XLIX. The sum of these three quantities gives the corrected mean longitude.

Hansen's mean longitude and secular acceleration are deduced in the same way from the elements given on page 15 of his *Tables de la Lune.* His terms of long period are deduced from Tables XLI and XLII, the constants being subtracted and the remainder reduced to arc by being multiplied by the factor $0''\cdot004703$. The last column of the table gives the correction to Burckhardt's mean longitude to reduce it to that of Hansen. That this difference is really the mean difference between the longitudes of the moon deduced from the two tables is shown by its agreement with the known difference at particular epochs. At the end of the British Nautical Almanac for 1862 is found a comparison of the two tables, from which it appears that Burckhardt's mean longitude was then greater than Hansen's by about 14″·2. The general agreement between 1750 and 1800, when both tables agreed with observations, shows that the difference of mean motion is certainly affected with no sensible error.

Year.	Burckhardt.								Hansen.							H.–B.
	L₀			Sec. Var.	Long Period.	Corr. Mean Longitude.			L₀		Sec. Var.	Long Period.	Mean Longitude.			
	°	′	″	″	″	°	′	″	′	″	″	″	°	′	″	″
1630	100	19	28·0	+ 4·9	− 8·0	100	19	24·9	18	14·4	+38·5	−21·4	100	18	31·5	−53·4
40	347	6	45·4	+ 3·6	−10·8	347	6	38·2	5	36·3	+34·1	−20·0	347	5	50·4	−47·8
50	233	94	2·7	+ 2·5	−12·3	233	53	52·9	52	58·3	+30·0	−17·2	223	53	11·1	−31·8
60	120	41	20·1	+ 1·6	−12·3	120	41	9·4	40	20·3	+26·1	−13·1	120	40	33·3	−36·8
70	7	28	37·4	+ 0·9	−10·8	7	28	27·5	27	42·2	+22·5	− 8 1	7	27	56·6	−30·9
80	254	15	54·8	+ 0·4	− 8·0	254	15	47·2	15	4·2	+19·2	− 2·3	254	15	21·1	−26·1
90	141	3	12·1	+ 0·1	− 4·2	141	3	7·8	2	26·1	+16·1	+ 3·9	141	2	46·1	−21·7
1700	27	50	29·5	+ 0·0	+ 0·2	27	50	29·7	49	48·1	+13·3	+10·0	27	50	11·4	−18·3
10	274	37	46·8	+ 0·1	+ 4·4	274	37	51·3	37	10·0	+10·8	+15·6	274	37	36·4	−14·9
20	161	25	4·2	+ 0·4	+ 8·3	161	25	12·9	24	32·0	+ 8·5	+20·5	161	25	1·0	−11·9
30	48	12	21·5	+ 0·9	+11·0	48	12	33·4	11	59·9	+ 6·5	+24·2	48	12	24·7	− 8·7
40	294	59	38·9	+ 1·6	+12·4	294	59	52·9	59	15·9	+ 4·8	+26·4	294	59	47·1	− 5·8
50	181	47	56·2	+ 2·5	+12·2	181	47	10·0	46	37·9	+ 3·3	+26·9	181	47	8·1	− 2·9
60	68	34	13·6	+ 3·6	+10·6	68	34	27·8	33	59·8	+ 2·1	+25·7	68	54	27·6	− 0·2
70	315	21	30·9	+ 4·9	+ 7·8	315	21	43·7	21	21·8	+ 1·2	+22·9	315	21	45·9	+ 2·2
80	202	8	48·3	+ 6·4	+ 3·9	202	8	58·6	8	43·7	+ 0·5	+18·5	202	9	2·7	+ 4·1
90	88	56	5·6	+ 8·1	− 0·4	88	56	13·4	56	5·7	+ 0 1	+12·8	88	56	18·6	+ 5·2
1800	335	43	23·0	+10·0	− 4·7	335	43	28·4	43	27·7	0·0	+ 6·1	335	43	33·8	+ 5·4
10	222	30	40·4	+12·1	− 8·3	222	30	44·2	30	49·6	+ 0·1	− 1·1	222	30	48·6	+ 4·4
20	109	17	57·8	+14·4	−11·0	109	18	1·2	18	11·6	+ 0·5	− 8·4	109	18	3·7	+ 2·5
30	356	5	15·2	+16·9	−12·4	356	5	19·7	5	33·5	+ 1·2	−15·4	356	5	19·3	− 0·4
40	242	52	32·5	+19·6	−12·2	242	52	39·9	52	55·5	+ 2·1	−21·6	242	52	36·0	− 3·9
50	129	39	49·9	+22·5	−10·6	129	40	1·8	40	17·5	+ 3·3	−26·5	129	39	54·3	− 7·5
60	16	27	7·2	+25·6	− 7 6	16	27	25·2	27	39 4	+ 4·8	−29·8	16	27	14·4	−10·8
70	263	14	24·6	+28·9	− 3·8	263	14	49·7	15	1·4	+ 6·5	−31·3	263	14	36·6	−13·1

Burckhardt's tables have been selected for this comparison because they have been extensively compared with observations made before 1700. The additions to the Connaissance des Temps for 1824 contain a paper by Burckhardt himself giving a comparison of his tables with observations of occultations made by Flamstead, Hevelius and others, between 1637 and 1700. The general result of this comparison is that the mean longitude of his tables could hardly have been more than a very few seconds in error in the year 1670. But, the preceding table shows that for this epoch Hansen's mean longitude is 30″ less than Burckhardt's. Therefore, unless we suppose Burckhardt's investigation to be affected with some egregious systematic error we must admit that the mean longitude of Hansen's tables for the epoch 1670 is about 30″ too small.

Desiring an independent test of this conclusion I have selected certain observations which, with the data available, seemed well fitted to answer this purpose and compared them directly with Hansen's Tables.

They are

1. Occultation of Aldebaran, 1680, Sept. 13, observed at Greenwich by Flamstead.
2. Occultation of the same star 1680, Nov. 7, observed at Greenwich by Flamstead, and at London by Halley.
3. Total eclipse of the sun 1715, May 3, observed at London, Greenwich and Wanstead by Halley, Flamstead and Pound.

To compute the occultations of Aldebaran the mean position for 1680·0 was derived from Le Verrier's Tables (Annales de l'Observatoire, Tome II) correcting the right ascension by $+0^s{\cdot}01$, and was as follows:

$$\alpha(1680) = 4^h\ 17^m\ 37^s{\cdot}01$$
$$\delta\ \ldots\ldots +15^\circ\ 49'\ 11''{\cdot}8$$

The corrections for reduction to apparent place are

for Sept. 13, $\Delta\alpha = +2^s{\cdot}90$; $\Delta\delta = +1''{\cdot}1$
Nov. 7, $\Delta\alpha = +4{\cdot}18$ $\Delta\delta = +2{\cdot}4$

The following geocentric positions of the moon were derived from Hansen's Tables.

Date (Julian Cal.)	Sept. 13.		Nov. 7.	
	h m s	h m s	h m s	h m s
Gr. Mean Time,	15 0 53	16 12 53	7 50 39	8 48 15
☽'s Longitude,	64° 54′ 24″·3	65° 37′ 20″·4	64° 33′ 11″·6	65° 9′ 49″·6
" Latitude,	–4 45 29·8	–4 48 10·6	–4 39 26·9	–4 40 48·0
" Parallax,	0 59 30·0	0 59 28·8	1 1 18·5	1 1 17·8

From these data we derive the following times for the immersion and emersion of Aldebaran for the dates in question. The observed times have been concluded from the observed altitudes and clock times given by Flamstead in the *Historia Celestis*, kindly furnished me by Prof. Winlock. They differ but little from the results of Flamstead himself, when the latter are corrected for the equation of time.

	Computed.			Observed.			C—O.
	h	m	s				s
Sept. 13, Immersion,	15	2	49	15	0	53	+116
Emersion,	16	10	5	16	9	12	+ 53
Nov. 7, Immersion,	7	51	47	7	50	43	+ 64
Emersion,	8	48	16	8	47	12	+ 64

The great difference between the results of the two phases of the first occultation gives rise to a suspicion of error in the observations or the data of reduction. The second observation is confirmed by that of Halley in London, he having observed the immersion at $7^h\ 50^m\ 9^s$, and noticed that the star was "newly emerged" at $8^h\ 47^m\ 1^s$. His place of observation was probably twenty-five or thirty seconds west of Greenwich, and therefore his observation agrees well with that of Flamstead. The discordance between the observed and computed times, of this second occultation indicates a correction of about +32″ to Hansen's mean longitude at the epoch 1680, and the first may be considered as confirming this correction in direction, if not in amount.

For the eclipse of May 3, 1715 we have the following computed and observed times. I have assumed Halley's station to be in latitude 51° 31′ and longitude 25^s west. Pound's is taken in accordance with his own statement to be in latitude 51° 34′, and longitude 8^s west. These agree pretty well with Flamstead's statements that Wanstead is seven or eight miles N. by E. from Greenwich,* and that Crane Court is half a minute of time West of Greenwich.

Halley at London.

	Computed.			Observed.			C—O
	h	m	s	h	m	s	s
First contact,	20	2	35	20	2	37	— 2
Beginning of Totality,	21	5	52	21	5	39	+13
End of "	21	9	3	21	9	2	+ 1
End of Eclipse,	22	16	55	22	16	37	+18

Pound at Wanstead.

	Computed.			Observed.			C—O
	h	m	s	h	m	s	s
Eclipse first perceived,	20	3	18	20	3	15	+ 3
The total immersion,	21	6	38	21	6	6	+32
The emersion,	21	9	48	21	9	26	+22
The just end of the eclipse.	22	17	42	22	17	10	+32

The only information I have respecting Flamstead's observations is contained in a letter of his found in Baily's 'Life and Correspondence of Flamstead, p. 315, from which it appears that his times differ only a few seconds from Halley's, instead of differing by the half minute required by the difference of meridians. An obvious slip of the pen, (*later* being written instead of *earlier*) makes it doubtful in which way the "few seconds" are to be counted. It can, however, be fairly inferred from his statement that his observations diverge from the tabular times as much or more than Pound's.

The discordance of the results of first and last contact may be attributed to this cause: that with their imperfect telescopes the observers did not begin to see the moon until several seconds after the actual commencement of the eclipse, and lost sight of it a few seconds before the actual end. The discordance in the duration of totality indicates with a high probability that the computed shadow path falls a few miles too far north. In this case the mean of the results for beginning and end of totality

---

* **Baily's Flamstead, p. 316, p. 328.**

will be about right, and we have for the excess of computed times

Halley's observations,	+ 7^{s}
Pound's,	+ 27
Flamstead's,	+ 30 ±

I infer from these results that the correction to Hansen's mean longitude at the epoch 1715 is about +11″.

Comparing the corrections thus found for the epochs 1680 and 1715, we find they are substantially those required to reduce Hansen's mean longitude to Burckhardt's. I conclude, therefore, that no egregious systematic error has crept into the researches by which Burckhardt sought to show that the epoch of his tables was substantially correct during the latter half of the seventeenth century, and that the difference between the mean longitude of Hansen and Burckhardt during that period represents approximately, at least, errors of Hansen's mean longitude.

The observations of the moon made at the observatories of Greenwich and Washington during the last ten years, indicate a tabular deviation of a remarkable character. From 1850 to 1862 we find the moon slowly running ahead of the tables, until the latter required a correction of plus two seconds in longitude to make them agree with observation. But this correction, instead of continuing to increase as all analogy would have led us to anticipate, suddenly began to diminish, so that since 1862 the moon seems to have been falling behind the tables at the rate of a second a year. This is shown by the following exhibit of the corrections to Hansen's mean longitude, or right-ascension, deduced from the meridian observations of the two observatories.

Year.	Correction given by Greenwich.	Correction given by Washington.	Mean.	Corr. mean.
1850	+0″·3	−1″·3	0″·0	+1″·0
51	+1·5	+0·6	+1·3	+2·7
52	+0·9	----	+0·9	+2·4
56	+1·0	----	+1·0	+1·4
57	+1·5	----	+1·5	+1·4
58	+2·0	+1·5	+1·8	+1·3
62	+2·4	+2·4	+2·4	+0·9
63	+2·2	+1·2	+1·7	+0·5
64	+0·1	−1·0	−0·4	−1·2
65	−1·1	−2·4	−1·7	−2·1
66	−2·2	−2·5	−2·4	−2·4
67	−3·9	−4·1	−4·0	−3·6
68	−4·4	−4·5	−4·5	−3·6
69	----	−5·5	−5·5	−4·3

The corrections here given as those of Greenwich are, previous to 1859, derived from the comparison found in the Greenwich observations for 1859. From 1863 forward they are derived from a paper by Mr. Dunkin in the Monthly Notices of the Royal Astronomical Society for April, 1869. The work of only the four principal observers is therefore included in the comparison. The object of this comparison being not so much to determine the absolute correction to the epoch of the tables as to show the changes of this correction, it is better to reject the results of the observers whose labors were discontinuous. In the case of the Washington observations, such a selection could not be made: the results given are therefore an indiscriminate mean of all. The systematic personal differences are however found to be very small.

That these corrections are real will not, I conceive, be disputed. To suppose them due to errors of observation, would be to suppose that six or eight long practiced observers divided between the two hemispheres, all progressively changed their habits of observing in the same way, and to nearly the same amount, through a period of seven or eight years.

A portion of the observed discordance may arise from a small error in Hansen's value of the coëfficient depending on the ellipticity of the earth, which is more than a second greater than the values derived by previous investigators, either from theory or observation. The last column of the preceding table shows what the correction would be if Hansen's coëfficient were $1''\cdot5$ smaller than it is.

From all these comparisons it would appear that the problem of the inequalities of long period in the moon's mean motion is really no nearer such a solution as will agree with observation, than when it was left by La Place. By a partially empirical correction, Hansen has succeeded in securing a very good agreement during the period 1750–1860, but, if the results of the preceding examination are correct, this has been gained only by sacrificing the agreement for the century previous to 1750, and for the years following 1860. This failure to reconcile theory with observation must arise from one of two sources. Either:

(1) The concluded theory does not correctly represent the mean motion of the moon. Or:—

(2) The rotation of the earth on its axis is subject to inequalities of irregular character and long period.

The first hypothesis admits of two explanations. We may suppose either that the mean motion of the moon is subject to change from some other cause than the gravitation of the known bodies of the solar system, or that the effect of this gravitation is incorrectly calculated, and that theory and observation will be reconciled by a correct calculation.

There are difficulties in the way of accepting either of these explanations. In reference to the first it may be remarked that anomalies of mean motion cannot be accounted for by a deviation from the received law of gravitation inversely as the square of the distance, because the anomalies produced by such deviation would be regularly progressive, and would be most sensible in the secular motion of the moon's perigee. The comparison of the theoretical and observed values of this motion is, perhaps, the severest test to which the Newtonian law has yet been subjected. That the anomalies proceed from the attraction of unknown bodies passing through the system seems extremely improbable, since, if they were distant, they would affect the earth and planets more than the moon, while the closer passage of bodies could scarcely escape detection. Still, this explanation does not admit of being mathematically disproved. If we attribute the deviation to the impact of meteoric matter, we must suppose the moon to have encountered such matter in quantities nearly incredible.

These three causes exhaust those on which we can base the first explanation, unless we invalidate the third law of motion. For, by that law; matter moves only by the influence of other matter. Other matter can affect the motion of the moon only by impact and gravitation. The gravitation of known bodies. the gravitation of unknown bodies, and the impact of matter is therefore an exhaustive enumeration.

We pass now to the second explanation of the first hypothesis, namely, errors or omissions in the theoretical computation of the effect of gravitation. The wide difference between the conclusions of Hansen and Delaunay suggests the possibility that there may be inequalities still overlooked. We have however the assurance of Hansen that there are none. and we shall find it extremely difficult to introduce any periodic terms whatever which will represent the observed deviation of the moon from the tables during the past ten years, without discordance during the century previous, when the agreement of Hansen's tables with theory is believed to be quite close. It is however hardly worth while to dwell upon this explanation until we have a more rigorous theory of the inequalities of long period produced by gravitation.

Considering that the reconciliation of theory and observation is not very probable, the second hypothesis may become worthy of serious consideration. If we accept it we must admit that between the years 1860 and 1862 the rotation of the earth was so accelerated that our reckoning of time is already eight or ten seconds ahead of what it would have been had the day remained invariable. Such an acceleration could proceed only from a change in the arrangement of the matter of the earth. The possibility of this effect being produced by changes in the quantity of ice accumulated around the poles has, I believe, been pointed out by geologists. But the effect of this cause could scarcely be sensible. But, if we admit that the interior of the earth is a fluid, and also admit that general changes in the arrangement of this fluid are possible, we have all that is necessary to account for considerable changes in the rotation of the outer crust. That this fluid, admitting its existence, is not in a state of entire quiescence is rendered probable by the phenomena of volcanoes and earthquakes. If we suppose a large mass of it to move from the equatorial regions to a position nearer the axis, a mass from the latter position taking its place, the following effects will follow:—

1. A diminution in the angular velocity of the surface of the fluid, accompanied by a corresponding increase in the velocity of the axial portion. The velocity of the outer crust will then be gradually retarded by friction.

2. The gradual transmission of the increased rotation of the central mass to the surface by friction and viscosity. The motion of the crust will then be gradually accelerated. The velocity of rotation finally attained will be greater or less than the original velocity, according as the radius of gyration of the fluid mass is diminished or increased by the change in the arrangement of the fluid.

I conclude, from this discussion, that we have reason to suspect that the motion of rotation of the crust of the earth is subject to inequalities of an irregular character, which, in the present state of science, can be detected only by observations of the moon. This suspicion can be neither confirmed nor removed until we have more positive knowledge than we now have of the possible inequalities which may be produced in the mean motion of the moon by the action of gravitation.

The operation of calculating these inequalities, though complicated and difficult, is certainly within the powers of analysis. When it is completely and thoroughly done, we may ascertain whether the result can be made to represent observations. If so, well; the length of the day is not variable, and the future positions of the moon can be safely predicted. If not, it will follow either that the motion of the moon is affected by other causes than the gravitation of the known bodies of the solar system, or the day is irregularly variable.

By the end of the present century, if not sooner, we shall have an independent test of the latter hypothesis, in the agreement of the observed and theoretical times of the transits of Mercury and Venus. If the hypothesis is a true one, the irregularities may range over half a minute of time in the course of a century, and this quantity might be detected even by meridian observations of the planets in question.

ART. XX.—*Researches in Electro-Magnetism*; by ALFRED M. MAYER, Ph.D.

THE refined experiments of Weber, Tyndall and Knoblauch having fully established the fact of the reversed polarity of a bar of bismuth, when subjected to the magnetizing influence of a helix, the attention of natural philosophers is naturally directed to the necessity of subjecting Ampère's theory of magnetism to as severe deductive tests as can be applied. Thus, from new experiments and lines of research suggested by theoretic deduction, we may hope for such addition to our knowledge as will evolve a theory which will embrace both magnetic and diamagnetic phenomena, as completely as Ampère's beautiful generalization contains the explanation of the magnetization of iron.

Among other experiments, thus suggested, was the following: Ascertain the relative forces of two electro-magnetic cores; one composed of *insulated* wires, the other of the same number of similar wires *uninsulated.*

Theory indicates that the insulated bundle will be found the weaker; for, in this case, we have not only the reaction of the molecular currents, but also, the reaction of the infinitely larger currents flowing around each insulated wire. Also, admitting the truth of the molecular hypothesis, we have in the uninsulated wire-core the interaction of exceedingly small currents separated by distances, great when compared to their size; while in the insulated wire-core we have, in addition, very large currents reacting at distances very small when compared to their diameters.

It was the attempt to solve this problem which led to the invention of the experimental method described in this paper; for, it will appear further on, that it could not have been attacked by methods heretofore used. The satisfactory solution of this question and the ascertained delicacy and precision of the apparatus encouraged me to make other determinations, which I here present; not as a finished and neatly rounded piece of work, but as showing what may be expected from a more complete discussion of the method used, and from that perfect experimental control of the apparatus which a more extended experience will give.

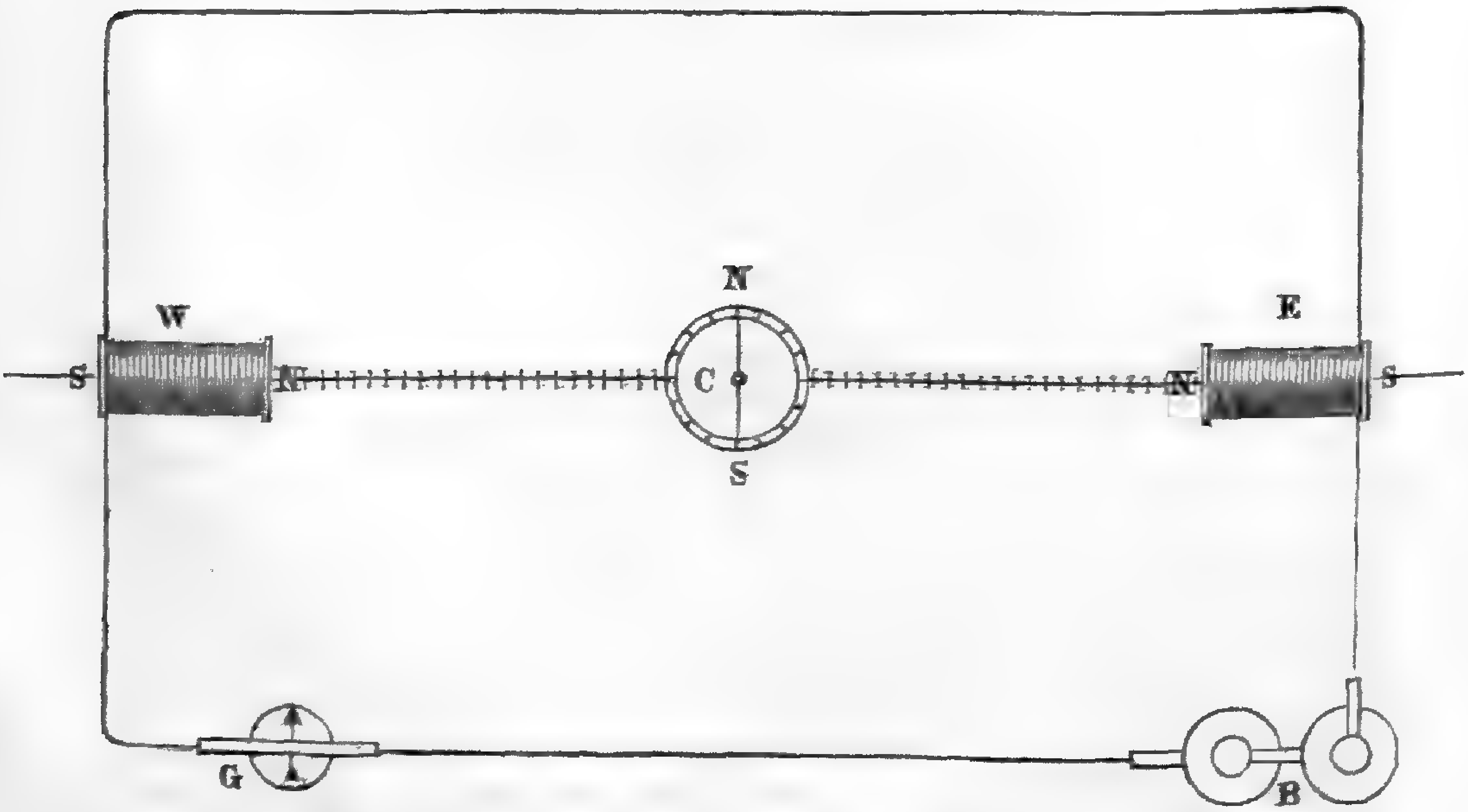
In my first experiments I adopted the plan of Müller (Poggendorff's Annalen, Bd. LXXIX and LXXXII) and tried the forces of the different cores, magnetized in one and the same helix, by their action on a distant magnetic needle; keeping the current, as far as possible, of the same intensity during the two comparisons. This method I found could not serve the purpose of measuring forces differing but slightly in intensity; and the impossibility of obtaining a current so constant as not to produce continual motion and vibration of the needle caused me to devise the following method, which I have found from experience to be both sensitive and precise.

Apparatus for the comparative measures of electro-magnetic forces.—On a table 10 ft. long was drawn a center line and divided into fractions of an inch. This line was then accurately placed at right angles to the magnetic meridian. Two helices, which I designate as E and W, were placed 8 ft. apart with their axes in the same vertical plane as the above line. A surveyor's compass, with a sensitive needle 5·85 in. long was so arranged that the point of suspension of the needle could be moved between the helices in the line of their axes. The same battery current passed through both helices, and in such direction that the N. pole of each was facing the needle; by reversing the current, the S. poles could be opposed to each other.

Both helices were composed of 10 layers of ·1 inch "extra covered" copper wire, wrapped on copper spools of $8\frac{5}{8}$ inches long, 1·82 inches diam., and having flanges at the ends 1·25 inches high. These spools, with their flanges, were split in the direction of their length by an opening of $\frac{1}{16}$ inch. Each layer of coils was saturated with a thick solution of shellac in alcohol and covered with thick paper, coated with shellac, before the succeeding layer was wrapped.

Helix E contains 557·5 feet of wire in 696 turns, and on account of its better insulation and greater number of turns is superior in strength to helix W. Helix W contains 551 feet of wire in 688 turns.

1.



The accompanying diagram shows the arrangement of the apparatus. Helix E to the east of the compass; helix W to the west; at C the compass; and at G the tangent galvanometer sufficiently removed from E and W, not to be affected by the magnetized cores. The Bunsen battery is shown at B.

Each end of the compass needle is thus subjected to the powerful action of two opposing forces and a magnetic couple is thus formed whose equilibrium, shown by the needle, is readily disturbed by a change in the relative forces of the cores or by a change in the distance of the needle relatively to the two helices.

It was found that when the needle was placed at such a position on the line between the helices that it stood at 0° when the circuit was open or closed, and a current passed from 12 cells of a Gaiffe bichromate battery, which was rapidly decreasing in electro-motive force, the needle remained at 0° for over 20 minutes without change or tremble.

Now if we can find the law which shows the variation of the intensity of action of the electro-magnet on the needle with a change of its distance from the magnetized core, we will have a system of measurement for the electro-magnetic forces which will exceed in accuracy and in delicacy, though be similar in arrangement, to the best photometric processes.

The advantages of this method, as far as my knowledge of it extends, is that you can thus subject the needle to two opposing actions of great intensity and thus any minute difference in the causes which alter their relative intensities will, on account of the nearness of the needle to the helices, give a deflection which would be inappreciable in the methods heretofore used; and above all, the same current traversing both helices will, even if inconstant and fitful, affect both proportionately and the needle, as seen above, will preserve a fixed position at 0° under these circumstances.

The law of variation of the intensity of the force with a change of distance.—To find the law of variation of the force with the distance from the pole of the core (which as is well known exists in the plane of the end-coils of the helix) the following experiments were made.

A core 10 in. long and 1·64 in. in diameter composed of 400 soft well annealed iron wires, was placed in helix E and the plane of the ends of the wires facing the needle were brought into the plane of the end coils of the helix. The center of the compass needle was placed at distances of 4, 5, 6, 7 and 8 ft. from the end of the core; the needle brought to 0° at each of these positions by the action of terrestrial magnetism and the current passed from seven new, freshly amalgamated Bunsen-cells; as soon as the needle had come to rest the angle of deflection was read and at the same time the tangent galvanometer was noted.

The following table gives the results, each number being the mean of four experiments.

Column (1) gives the distance of the center of needle from core; (2) the readings of the tangent galvanometer; (3) the deflection of the compass needle for the distances given in column (1); the nat. tangs. of the angles of (3); (5) the products of the nat. tans. of the angles of deflection by the cubes of the distances in (1); these products being obtained by multiplying the nat. tan. of each angle of deflection by the corresponding distance from the core raised to the cube, should, if the law followed the reciprocals of the cubes, be equal; column (6) gives the products of the nat. tangs. of the needle deflections into their corresponding distances from the core raised to the 2·7404 power; (7) contains the differences of each product of (6) from ·788336 the nat. tan. of 38° 15′. If the experiments had been more extended these differences would have been smaller; as it is, they are not great, and greater approximation cannot be obtained with three decimals in the exponent of the power, and this degree of accuracy is all the determinations will allow. The sum of the + departures equals ·008857, that of the − departures equals ·008651, their difference is +·000206.

(1) Dist. from Core.	(2) Tan. Gal.	(3) Deflection.	(4) Nat. Tan. Defl.	(5) $\frac{1}{d^3}$	(6) $\frac{1}{d^{2\cdot7404}}$	(7) Dif.
4 ft. or 1	51° 45′	38° 15′	·788336	·788336	·788336	·000000
5 " " 1·25	51° 45′	23°	·424475	·829042	·782392	·005944
6 " " 1·5	51° 30′	14° 30′	·258618	·872835	·785629	·002707
7 " " 1·75	51° 45′	9° 45′	·171831	·917888	·796385	·008049
8 " " 2	51° 30′	6° 44′	·118063	·944504	·789144	·000808

We can therefore assume that *the intensity of the force* (for this apparatus) *varies inversely as the* 2·7404 *power* (*or as* $d^{\frac{1}{2\cdot7404}}$) *of the distance from the core.*

Experiments on the comparative strengths of cores formed of insulated and uninsulated wires.—The wire used in these experiments was of the best soft iron and of two diameters, $\frac{1}{10}$ and $\frac{1}{20}$ inch. The wire was cut into lengths of 10 in. and then placed in iron tubes, (to keep them from the action of the air), and heated to bright redness; the tubes were then covered thickly with lime and allowed to cool slowly. This process was twice repeated on all the wire and iron tubes and cylinders used in this investigation, and they were found after magnetization to have very little residual magnetism. The caution however was taken always to present the same end of a core toward the needle; thus all the cores were placed in the same circumstances. Three cores were made of each size wire. Each core of the $\frac{1}{10}$ wire contained 100 pieces, while each core of the $\frac{1}{20}$ wire consisted of 400 wires. The wires of one core of each size wire, (after having been tested, as in Ex. 7, that they were equally affected in the same circumstances of magnetization), were insulated by dipping them separately into parafine, melted in a tube about 12 in. long, and were thus uniformly coated with a firm layer of that substance. They were then closely tied together into bundles by means of silken thread. The helices, in all the experiments on the cores, were placed 8 feet apart.

To abbreviate the descriptions of the experiments we will always place the E helix east of the compass and the W helix west, and to indicate the deflection of the needle we will use such expressions as "S. 17° W." which means, S. *end* of *needle* deflected 17° W. or toward helix W. In the case of N. poles facing the compass the above expression would show that the core of helix W was stronger than that of helix E. If the current was reversed it would indicate that the E helix was the stronger. When not otherwise stated it will be understood that the N. poles of the helices face the needle, and therefore the expression given of the deflection will always show which of the cores has the greater action on the needle. To designate the different wire-cores we will use "100 or 400 UC" or "100 or 400 I C," which respectively mean, *the uninsulated core* of 100 or 400 wires and *the insulated core* of 100 or 400 wires. T. G. stands for tangent galvanometer.

Experiment 1. Helices alone, without cores. Needle placed midway between helices and brought to 0°. Current passed from 10 Bunsen cells. Needle S. 4° E. T. G. 58°.

Ex. 2. Reversed current S. 4° W. T. G. 58°.

Ex. 3. Moved needle toward W. helix so that it stood at 0° when circuit was open or closed. Distance from E helix = 52·77 in. T. G. 58. Expts. 1, 2 and 3 show that E helix is the stronger.

Ex. 4. Three cores of 400 wires each, one of insulated wires, were compressed to exactly the same diameter of 1·64 ins. An uninsulated core was placed in each helix, and the needle brought to 0°, midway between the helices. Current passed S. 27° E. T. G. 50°.

Ex. 5. Current reversed S. 27° W. T. G. 50°.

Ex. 6. Other circumstances remaining as in (4) moved needle toward W helix until it stood at 0° when circuit was open or closed. Needle distant from E helix 55·6 in. T. G. 46°. Expt. repeated many times, with current direct and reversed, with the same result.

Ex. 7. Interchanged the cores in the helices. Current passed. Needle still at 0°. T. G. 48°. Current reversed. Needle at 0°. This shows that the cores were equally affected in the same circumstances.

Ex. 8. Replaced E core by the *insulated* core of 400 parafined wires. Needle having remained in same position as in (6) and (7). Current passed. S. 55′ W. T. G. 46°.

Ex. 9. Current reversed S. 55′ E. T. G. 46°.

Expts. 8 and 9 repeated several times with same results, showing that the insulated core was the weaker.

Ex. 10. Moved needle toward E helix, which contained the 400 insulated wires, until it stood at 0° when circuit was open or closed. In this position needle was 55·4 in. from E helix.

The 100 wire-cores substituted for the 400 wire-cores gave similar results only differing in degree.

It is very important to remark that errors may be introduced into such determinations as the above, by not taking care to have the various cores which we compare of *exactly the same diameter.* To show the effect of a change of diameter in the same bundle of wires the following experiment was made.

Ex. 11. The two cores of 400 uninsulated wires of 1·64 in. diameter were placed in the helices and needle brought so that it stood at 0° when circuit was open or closed. The E core was now so firmly compressed that its diameter was reduced to 1·48, and replaced in E helix, otherwise the circumstances were the same. Current passed. S. 1° 15′ W., showing a considerable diminution in the magnetic force of the compressed core; and also, beautifully exhibiting the *surface* action of magnetic force.

It will be observed, in the above comparative experiment, that the two uninsulated wire-cores were of exactly the same strength when equally magnetized, (Ex. 7.) and that one of them remained in the W helix as *unit of comparison* to the uninsulated and insulated cores placed in the E helix. This method is necessary when the magnetizing effect of the helices are not the same. The uninsulated core which remains in the W helix stands in the place of the "unit-candle" in Bunsen's method of photometry, and the needle which is moved to such a position between the helix that it remains at 0° when the circuit is open or closed is the equivalent of the paper screen with its central translucent circle. But if it should happen that when the needle is placed midway between two similar helices that it remains at 0° when the circuit is open or closed, then two cores would answer the purpose.

Calculation of the relative forces of the uninsulated and insulated cores.—The data for the calculation are given in Experiments 6 and 10.

W.UC : E.UC :: $40{\cdot}4^{2{\cdot}7404} : 55{\cdot}6^{2{\cdot}7404}$: 1 : 2·39925.
W.UC : E.IC :: $40{\cdot}6^{2{\cdot}7404} : 55{\cdot}4^{2{\cdot}7404}$: 1 : 2·34374.

Hence *Uninsulated Core* : *Insulated Core* :: 1· : 0·9768.

The insulated core is therefore ·02318 weaker than the uninsulated.

In Expts. 8 and 9 it was seen that the above difference of ·02318 in magnetic force caused a deflection of 55′ in the compass needle, and as 3′ or even less of deflection can be accurately read it follows that a difference of $\frac{1}{1000}$th in the magnetic force of the two cores can be detected and measured by this method. This example will convey an idea of the delicacy and accuracy of this plan of measurement, which no doubt can be increased by the careful selection of the proper force of current, distance of helices and other related functions of the apparatus.

It thus appears that this result sides with the theory of Ampère, and also, to my mind, points to that postulate of the atomic hpyothesis which states that the atoms of bodies are separated from each other by distances which are great when compared to the size of these atoms.

Attempts to determine what thickness of tube in fraction of its diameter, is equal to a solid cylinder of the same diameter.—According to the theory of Ampère, the magnetization of a bar of steel, or of an iron core subjected to a helical current of electricity, is due to the surface currents, which are the resultants of the interaction of all the molecular currents in the interior of the bar. It is therefore exceedingly interesting in a theoretic point of view to determine accurately what thickness of tube is equivalent to a solid cylinder of the same length and diameter. The experiments of Barlow, Harris, de Haldat, and of Du Moncel have shown the truth of this deduction.

There is evidently a certain thickness required to develope an equivalent action, but a certain portion of the interior of an electro-magnetic core can be removed without diminishing its force *when in a condition of "saturation."* This determination will therefore inform us, (accepting the hypothetical theory of Ampère), what central currents are prevented by their interaction from affecting the resultant surface currents of the bar; and it appears to me that we should express the result in terms of the central cylinder which can be removed and not of a certain thickness of tube which remains.

Out of well annealed soft iron a series of tubes, 10 in. long, were turned so that they fitted closely into each other; into the innermost tube slid a solid iron core of ·83 in. diameter. The diameter of the outer tube was 1·68 in. With this construction of core, tubes of various thicknesses could be rapidly produced and compared in effect. Two such cores were constructed.

Ex. 12. 400 uninsulated wire-core in W helix; in E helix was placed the combination of tubes and core. Needle brought to such a position that it stood at 0° when circuit was open or closed. T. G. 46°.

Ex. 13. Removed inner core and tubes, and drilled out the innermost remaining tube until the thickness of the combination tube was $\frac{1}{6}$ of its diameter. Current passed. Needle remained at 0°. On further decreasing the thickness the S. end of the needle was deflected toward the W helix. T. G. 45°.

It thus appears that a tube of this size having a thickness of $\frac{1}{6}$ of its diameter is, when magnetized with this strength of current (8 cells, T. G. 46°) equal in effect to a solid cylinder of the same diameter.

Ex. 14. Another tube was constructed having a diameter of 1·3 in. and a thickness of $\frac{1}{6}$ the diam. Into this tube slid a solid core. Placing the uninsulated wire-core of 400 in W. helix and tube with inner core in E. helix, the needle was placed so that it stood at 0° when circuit was open or closed. T. G. 46°.

Ex. 15. Withdrew inner core. Current passed. S. 40′ W. T. G. 46°. Thus a tube of the above diameter, having a thickness of $\frac{1}{6}$ of its diameter is not equal to a similar solid cylinder. This subject will therefore have to be the object of further investigation.

Thinking that in the case where a current did not saturate the above tube it would equal the solid cylinder, the following experiments were made.

Ex. 16. The tube used in (14) was placed with inner core in E. helix, and a similar tube and core in W. helix, and the needle placed so that it stood at 0° when circuit was open or closed. A current from 4 cells was passed. T. G. 32°.

Ex. 17. Solid core removed from tube in helix E. Needle remains at 0°. Hence for this strength of current (T. G. 32°) the tube is equal to an equivalent solid cylinder.

Ex. 18. Other things remaining as in (17) a current from 5 cells, T. G. 37°, was passed through helices. Needle S. 2′ W.

Ex. 19. Current passed from 6 cells. T. G. 40°. Needle S. 15′ W.

Ex. 20. Current passed from 7 cells. T. G. 42°. Needle S. 20′ W.

Ex. 21. Current passed from 8 cells. T. G. 44°. Needle S. 40′ W. It is clearly seen from above experiments that with a current of T. G. 32° the tube is equal to the similar solid cylinder, but on gradually increasing the current up to T. G. 44° that the tube falls more and more below the strength of the cylinder, and the needle is again deflected S. 40′ W. toward the cylinder with that current. Therefore in determining what portion of the interior of a cylinder can be removed so that what remains shall equal the solid cylinder we should see that the tube is "saturated," otherwise the comparison does not solve the problem. Whether this condition was fulfilled in Expt. 13 was not ascertained.

In Daguin's *Traité de Physique*, vol. iii, p. 615, Paris, 1861, we read: "M. Du Moncel finds that it is necessary, in order that the magnetization [of a tube] may remain the same as a solid cylinder, that the thickness should be at least $\frac{1}{4}$ that of the radius of the cylinder." I have not seen Du Moncel's research on this subject and do not know the method he adopted in comparing the magnetization, but I think that his estimated fraction of $\frac{1}{8}$ of the diameter is too low, as Ex. 13 clearly shows. The cause of this error, I imagine, resides in the fact that he used a current not sufficient to saturate the tube and cylinder, and thus his determination would be explained by Expts. 15 to 21.

This subject is worthy of careful research, and its theoretical bearings on the subject of magnetism are very interesting. My experiments thus far cause me to doubt if any *constant* fraction of the diameter will be found, but that this fraction will depend upon the diameter of the tube; being larger as the tube is smaller.

Experiments on a Tube slit longitudinally.—To determine the influence, if any, of destroying the continuity of the exterior surface of a tube by a longitudinal slit, I obtained a tube of well annealed iron, 1·68 inch in exterior diameter, ·16 in. thick and 10 ins. long, and slit it in its whole length by an opening $\frac{1}{20}$ inch wide. This opening could be closed at pleasure by placing in it a tightly fitting piece of iron 10 in. long.

Ex. 22. The above tube, with the slit closed, was placed in the E. helix and the 400 U. C. in the W. helix, and needle brought to such a position between them that it stood at 0° when circuit was open or closed. Eight cells of Bunsen. T. G. 46°.

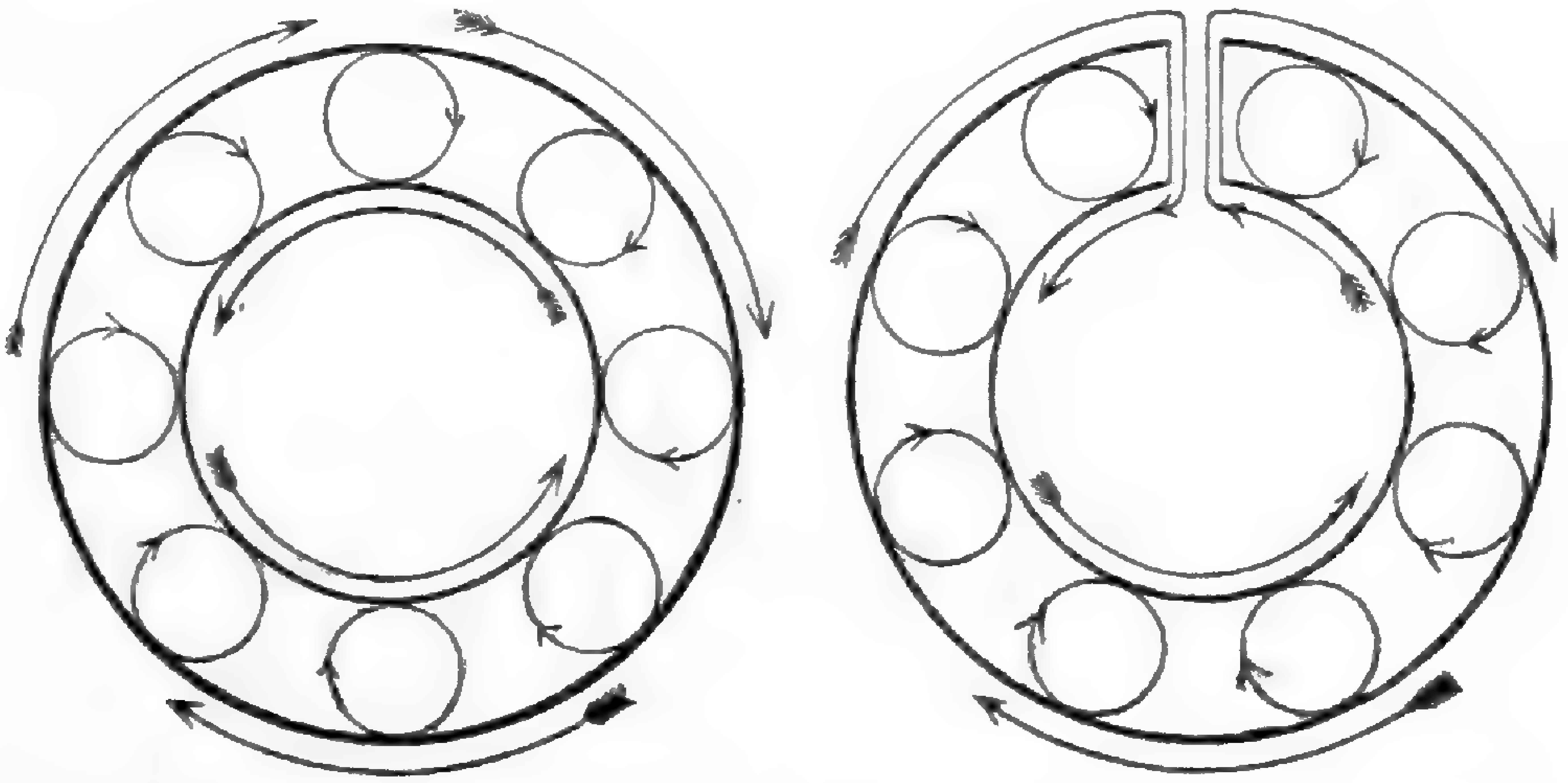
Ex. 23. Slit opened by removing the piece of iron. The needle remains at 0°. T. G. 46°.

Expts. 22 and 23 repeated several times with the same result.

In the accompanying diagrams is shown, according to the theory of Ampère, the direction of the circulation of the exterior and interior surface-currents. It is seen that in both cases the interior surface current circulates in an opposite direction to the exterior current; and that when the tube is slit longitudinally the circulation is not cut off but *facilitated* by the joining of the exterior and interior surfaces. It is therefore natural to suppose that on account of this facility afforded to the circulation that a *quicker* magnetization and demagnetization will occur in the split than in the closed tube; but no difference is found in the magnetic force of the two tubes, for the loss in surface by the slit seems made up in the greater facility of circulation.

I would therefore suggest that the cores of the electro-magnets of astronomical chronographs and of telegraph instruments be of soft well annealed Norway iron, made into tubes having a thickness of $\frac{1}{5}$ of their diameters, and slit longitudinally by a narrow opening.

2.



On the magnetic condition of the interior surface of a tube.—Referring to fig. 2 it will be seen that the Ampèrean currents on the interior surface of a tube flow in a direction contrary to those on the exterior. The magnetic condition of this surface was therefore an extremely interesting point for investigation; it has been the subject of over two hundred experiments, and is by far the most difficult research mentioned in this paper.

In my first experiments, steel rods and iron cores were introduced into soft iron tubes which were then placed in helix E. The current was then passed and the tube with enclosed core very slowly withdrawn. The steel or iron core was then removed from the tube and tested by placing it to the W. of the center of a compass needle. The magnetic force of the core was then compared with what it had before it was introduced into the tube. This method was thoroughly tested by withdrawing tube and core both before and after contact was broken in the helix, but no decisive conclusions could be drawn because the effect of the inductive action which took place when the core was withdrawn from the tube changed the magnetic relations which existed while the core was in the tube.

The following method was then devised. Two tubes of very soft iron were constructed, having both an internal diameter of ·84 inch, and one an external diam. of 1·03 inch, the other of 1·31 inch. Both tubes were 9 in. long. One hundred and twenty well annealed iron-wires a little less than 9 in. long were tightly bound together by silken cord and then wrapped in six thicknesses of writing paper, so that the wire core fitted neatly the interior of the tubes.

The wire core was now deprived as far as possible of magnetism by passing a weak magnet over it, and could thus be so reduced that it caused a deflection of only 1° when placed 7·5 in. W. of the center of a surveyor's compass needle. By now inverting the core in the line of the dipping-needle this small residual magnetism could be reduced to 10′ or 15′ deflection, and in some experiments even a reversed magnetization was thus produced. The core was now handled very carefully to prevent all vibrations or blows, and placed in an E. and W. horizontal line.

The tube was placed in helix E and the current passed. It was then *very slowly and steadily* withdrawn, so as gradually to let down its magnetism, the tube removed several feet from the helix and the current broken. This operation was repeated until the tube had attained as high a degree of magnetization as could be given to it with the same constant current in the helix, and a further repetition of the process caused no increased deflection in the needle. The tube was then carefully placed in an E. and W. horizontal line and the core slowly introduced, and so that the S. pole of the core was in the S. end of the tube. It is important in all these manipulations to keep the core and tube in a horizontal plane, for if they are inclined to the horizon, during the experiment, the magnetic condition is changed and the operations have to begin anew. The degree of susceptibility of very soft iron to a change of inclination can only be appreciated by those who make the trial.

The tube with enclosed wire-core was now introduced into the helix and the current passed; then withdrawn, and this repeated (as in the above process on tubes alone), until the needle-deflection showed no increase of magnetic force. This deflection was then compared with that previously produced by the tube acting alone.

Now if the interior electric currents really flow as Ampère's theory states, then when the core is in the tube and the maximum magnetization reached, the combined effect of tube and enclosed core on the needle should be less that given by the tube alone without the central core; for, in the former case the core has a magnetism given it the *reverse* of the tube; i. e., supposing marked end of tube S and marked end of introduced core S, then, after magnetization in the helix, the marked end of core is N and will therefore neutralize a portion of the tube's action on the needle. Now this was found to be *invariably* the case in all the experiments made; and how can it otherwise be explained. It is known that the combined force of two magnets with like poles placed together is less than the sum of the forces of the separate bars separately measured; but the force of the two together is always greater than that of one alone. But we here have a tube and enclosed insulated core with like poles together giving, after magnetization (in the same conditions *exactly* as that given to the tube alone) a less effect than that given by the tube alone. Therefore these facts show, to my mind, that the deduction from Ampère's theory is verified by experiment; and that we have succeeded in giving to a core in the interior of a tube a polarity the reverse of that given to the tube itself.

We here present the following experiments, which are typical of all made; the others only differing in quantity with a change of distance from the needle and a change in the intensity of the current in the helix.

The marked ends of tubes were S. poles, and S. 10° E. means that when tube or core or both were placed W. of the center of the compass-needle that the S. *end of needle* was deflected 10° E. If the same end is deflected W. then the marked end is a N. pole.

Ex. 24. Experiments with tube of 1·03 in. diam. is given under column A; those with tube of 1·31 in. diam. under column B. Tangent galvanometer in all the experiments was at 30°. Tube and core placed 7½ inches W. of needle.

	A	B
Marked end of tube placed in S. pole of helix and withdrawn (as descibed above). Repeated 6 times	S. 17° 18′ E.	S. 21° 40′ E.
Introduced tube again and withdrew	“ 17 30 “	“ 21 40 “
“ “ “ “ “	“ 17 30 “	“ 21 40 “
Core placed W. of needle	“ 0 33 “	“ 0 18 “
Core slowly and steadily introduced into tube so that S. end of core was in S. end of tube; then tube and core introduced into helix E. and withdrawn 6 times	“ 16 2 “	“ 20 18 “
Introduced and withdrew tube and core again	“ 16 13 “	“ 20 18 “
“ “ “ “ “ “ “	“ 16 15 “	“ 20 18 “
“ “ “ “ “ “ “	“ 16 15 “	“ 20 18 “
While tube remained in position W. of compass the core was slowly withdrawn from the tube. Tube now alone deflected	“ 17 40 “	“ 21 38 “
Core now alone deflected	“ 0 50 “	“ 0 10 W.

Referring to Expts. A and B it is seen that when the tubes were magnetized alone as fully as can be by the current (of T. G. 30°) through the helix, they deflect the needle in Exp. A 1° 15′ more and in Exp. B 1° 22′ more than when the tubes with enclosed cores acted together on the needle after having been magnetized in exactly the same circumstances; also, that when the tubes, remaining undisturbed in their position, W. of the needle, had the cores withdrawn, the needle was deflected 1° 25′ more in Exp. A and 1° 20′ more in Exp. B than when the tubes with enclosed cores both acted upon the needle.

In the break-circuit experiments the effects are more complicated by reason of the instantaneous and intense extra-current in the helix which causes a sudden inductive action on the tube and core, and thus other actions are introduced which are foreign to the object of the investigation. I have therefore only given examples of the make-circuit experiments, though the others showed even a greater difference between the action of the tube alone and the tube and core together; this difference amounting in some experiments to 5°.

After the results of the above experiments, it occurred to me to investigate the action of *a helix introduced into the tube*, and to ascertain if a magnetism could be given to the tube the reverse of that which is induced when the helix surrounds it. The former experiments show that the tube can give a reversed magnetism to an enclosed core, and we will now see that a helix can effect a reverse magnetism on a surrounding tube.

The first Expts. were made with opposing helices, one being helix E with the 400 wire core, the other a tube of soft iron 9 in. long, 1·3 in. external diam. and ·12 thick, around which was wrapped a helix of one layer of 148 turns of $\frac{1}{20}$ inch wire, and in its interior fitted a helix also of one layer 148 turns of the same wire wrapped upon a cylinder of wood. This last I will call the inner helix, the other the outer helix.

Ex. 25. The inner helix was placed in the circuit (near the battery, far removed from the compass) and the compass was so placed that its needle stood at 0° when circuit was open or closed. The inner helix was now placed inside the tube and the current passed through helix E and the outside helix; and also through the inside helix in a direction the same as the current in the outer helix; and the needle was noted. The current was now reversed in the inner helix and needle again noted; but as the opposing helices were 8 ft. apart, and the current not strong, no effect (after allowance made for direct action of inner helix on needle) was obtained from the action of the inner current.

Ex. 26. I then devised the following experiment. The opposing helix E with 400 wire-core was removed and the compass-needle brought to 0° by the action of the earth's magnetism. The iron tube was then placed 1 foot W. of center of needle with its S. pole toward the compass. The deflection produced by its magnetism was S. 4° 40′ E. The inner helix was now placed in the position W. of the compass previously occupied by the tube and the current passed so that its S. pole was opposed to the needle; the deflection was S. 3° 13′ E. I now placed the helix in the tube and both 1 ft. W. of needle and passed current so that S. end of helix was in S. end of tubes. The deflection produced was S. 4° 50′ E.

From above it is seen that when tube and inner helix act together on the needle the deflection is only 10′ greater than when the tube acts alone, although the helix acting alone causes a deflection of 3° 13′. The neutralization of such a quantity of magnetism can only be accounted for by the fact that the greater part of the magnetism of the tube was reversed by the action of the heliacal current on the inner surface of the tube.

Ex. 27. To obtain directly a reversed magnetism by the action of the *outside of the helix* the following experiments were made. Several wires 9 in. long and $\frac{1}{20}$ inch diam. were heated in a cupel furnace to bright redness and slowly cooled; one end of each wire was then marked. A wire was then taken and placed with its marked end 2·75 in. W. of the center of the compass-needle and the deflection produced was S. 18′ E., showing that the marked end was a feeble S. pole. A current was now passed through the inner helix and the wire laid upon it so that its marked or S. end was upon the S. pole of the helix, after remaining here for a few moments it was conveyed to the compass and placed, as before, with its marked end 2·75 in. W. of the center of the needle. The deflection was now S. 28′ W., showing that the magnetism of the wire had been reversed and that a wire placed on the *outside* of a helix has given to it a magnetism the reverse of that given when it is placed *inside.*

Ex. 28. Twelve wires were now placed with their marked ends 3·25 in. W. of the needle, and the deflection being S. 8′ E. showed that their marked ends had a slight S. magnetism. They were now removed and placed with their marked ends on the N. pole of the helix at equal distances from each other around the helix, through which a current was passing; they were then removed from the helix before the current was broken and again placed in their former position 3·25 W. of the compass-needle. The deflection was now S. 55′ E., showing that their marked ends were of south magnetism. They were again placed on the helix with their marked ends on its S. pole and removed, as before; they now deflected the needle S. 20′ W., showing that their marked ends were now N., their magnetism having been reversed.

Ex. 29. The inner helix alone was placed 1 foot W. of needle and the current passed so that its S. pole was opposed to the needle. Deflection S. 3° 30′ E. T. G. 20°. Reversed current. Deflection 3° 15′ W. By holding a bar of soft iron in the line of the dip and passing the wires over its end I succeeded in rendering them without action on the helix when put in the place of the helix in the above experiment. The twelve wires were then tied around the helix equidistant from each other and separated about ·2 inch. The current was then passed so that the S. pole of the helix was opposed to the needle. Deflection S. 2° 40′ E.. Reversed the current. Deflection S. 2° 30′ W. We therefore have in the 1st experiment 50′ less and in the 2d 45′ less action on the needle when the wires are around the helix than when they are away. These results correspond to the above experiments on the tube, the helix giving the wire a polarity the reverse of its own.

Beccaria, Coulomb and Faraday have, by their well known experiments, proved that frictional electricity when *at rest* only exists on or just within the outer surfaces of bodies, and Prof.

Joseph Henry, by many experiments, has shown that this is also true when the same species of electricity, of high tension, is *in motion.* The following beautiful experiment, so conclusive in its evidence, appears so little known, that I will here give it as stated by that philosopher. "A copper wire, of the size usually employed for ringing door bells, passed through the axis of an iron tube, or a piece of gas pipe, about three feet long. The middle of this wire was surrounded with silk, and coiled into a magnetizing spiral, into which a large sewing needle was inserted. The wire was supported in the middle of the tube by passing it through a cork at each end, covered with tin-foil, so as to form a good metallic connection between the copper and the iron. On the outside of the tube and opposite each other were placed two other magnetizing spirals, their ends soldered to the iron. When these two spirals were also furnished with needles, and a discharge from a Leyden jar sent through the apparatus, as if to pass along the wire, the needle inside of the iron tube was found to exhibit no signs of magnetism, while those on the outside presented strong polarity. This result conclusively shows that, notwithstanding the interior copper wire of this compound conductor was composed of a material which offered less resistance to the passage of the charge than the iron of which the outer portion was formed, yet when it arrived at the tin-foil covering of the cork, it diverged to the surface of the tube, and still further diverged into the iron wire forming the outer spirals. We must not conclude, however, from this experiment, that the electricity actually passes on the outside of the tube. On the contrary, we must infer from the following fact, that it passes just within the surface. If the iron be coated with a thin coating of sealing wax, the latter will not be disturbed when a moderate discharge is passed through it, though with a large discharge in proportion to the conducting power of the rod, the outward pressure may become so great as to throw off the stratum of sealing wax."

Barlow and Harris have made experiments which show that magnetism is also a surface action; and in Exp. 11 of this paper we saw that when the surface of a wire-core was diminished by compressing the bundle the magnetism diminished with it. To show that this diminution of force was not, in major part, owing to the increased repulsion produced between the bars when brought nearer together, the following experiments were made.

Ex. 30. About 200 $\frac{1}{20}$ inch wires were pressed together as tightly as could be by binding them in a bundle with silken cord, and the deflection they caused in the needle, when magnetized in the helix, was noted; they were now taken apart and bound as tightly as before around a wooden cylinder about 1 inch in diameter; and being magnetized again in the helix with the same strength of current, the bundle caused a far greater deflection in the needle than when it acted without the central wooden cylinder. I consider this experiment as very conclusive of the surface action of magnetism, for in the two measures we used one and the same mass of metal, subjected to exactly the same magnetizing influence, and only differing in the extent of exterior surfaces existing during the two experiments. That the increase of force with the surface was not owing to a change of distance of the wires from the interior surface of the helix is conclusively shown in the next section.

But there are differences to be made between these analogous phenomena of frictional electricity and of magnetism; in magnetism a *considerable thickness* of metal is required to develope this action at the surface; so that (Ex. 13) a tube must have a thickness of about $\frac{1}{6}$ of its diameter to equal a solid cylinder of the same length and diameter, both being, when compared, "saturated" with magnetism; also, it appears (Ex. 26) that a magnetic action can be effected on the interior surface of a tube, while no similar action can be obtained with frictional electricity.

Experiments to determine whether a change of positon of a bar in the interior of a helix causes a change in the intensity of its magnetization.—Theory indicates that no change in the degree of magnetization will follow a change of position of a bar in the interior of a helix, and the following experiments conclusively prove the truth of this deduction.

Ex. 31. In the interior of helix E resting on the bottom of the opening was placed a cylinder of soft iron ·83 inch diam. and 9 ins. long. Opposed to this helix was helix W, (in the same circuit) with the 400 wire-core. Needle was brought to such a position that it stood at 0° when the circuit was open or closed. T. G. 44½°.

Ex. 32. The axis of the iron cylinder was now made to coincide with the axis of the helix. Current passed. Needle still at 0°. T. G. 44½°. Thus showing that the change of position makes no difference in the intensity of the magnetization.

Experiments on the comparative magnetizing effects of a helix and of a combination of spirals, formed of similar wire, and containing the same number of turns, arranged in a length equal to that of the helix.—According to the theory of Ampère the currents which encircle a magnetized iron bar or a steel magnet are in planes at right angles to the axis of the bars; and it seemed to me interesting to determine with this sensitive apparatus what difference, if any, existed between the magnetizing effects of a helix whose turns were inclined in the successive layers alternately in opposite and equal angles with the axis, and the effects of a combination of spirals composed of an equal number of turns of wire as the helix and existing in the same length.

Fifty spirals, each composed of 14 ft. 8·06 ins. of $\frac{1}{20}$ inch wire in 20 turns, were made by a process to be described in a subsequent communication. These spirals were placed vertically in a frame at equal distances from each other so that they formed a cylinder 9 ins. in length, 3·9 ins. in exterior diameter and with a cylindrical axial opening of 1·68 ins. The turns of the spirals were carefully insulated from each other by saturating the covering of the wires with melted parafine.

A helix was constructed of the same wire, wrapped in 20 layers, each layer consisting of 50 turns. The wire in each layer was wrapped parallel with two lengths of twine so that the 50 turns in a layer occupied a length of exactly 9 ins. This gave a "pitch" to the turns of the helix of ·18 ins. and the innermost turns of the helix formed an angle of 1° 57′·25, and the outside turns formed an angle of 50′·5 with the axis of the helix, alternately to the right and to the left, as it was wrapped. Each layer of turns of wire and twine was carefully saturated with melted parafine of a high temperature, so that the copper was seen through the saturated covering after the parafine had solidified.

Ex. 33. The helix and combination of spirals were placed 8 feet apart and an uninsulated 400 wire-core placed in each. The compass was placed midway between them and the needle brought to 0°. The current was now passed so that the N. pole of the cores faced the compass. The S. end of the needle was slightly deflected toward the helix, showing that the core of this was somewhat stronger than that of the spirals.

The greater strength of the helix could not be attributed to the excess of wire it contained over the spirals, for this only amounted to about 2 inches; thinking that the intense inductive action of the spirals on each other might have some influence, the following experiments were made.

Ex. 34. Placed the needle between helix and spirals so that it stood at 0° when the circuit was open or closed. Then I introduced between the spirals 49 copper discs having central openings a little smaller than those of the spirals. This arrangement, as Prof. Joseph Henry has shown, so effectually cut of the mutual inductive action of the spirals that, on passing a current through them and breaking a mercury contact, the spark of the "extra-current" was (on account of the greater resistance of the spirals) less than when only the circuit of the battery wires was similarly broken. With the exception of the interposed copper discs things remained as in Exp. 33. Current passed. Needle remained at 0°. Showing that the inductive action had no influence on the intensity of the magnetizing effect. I am therefore of the opinion that the increased effect of the helix was due to superior insulation.

Ex. 31. The combination of 50 spirals was separated into two, each containing 25 spirals. Between the spirals of one combination were introduced the 25 spirals of the other, so that every alternate spiral belonged to the same combination. If the current is now passed through one of the combinations and the two terminal wires of the other combination joined, Faraday has shown, that the "extra-current" in the first combination is entirely given up to second. The spirals thus arranged, with the terminals of the interposed spirals separated, were placed opposite the helix E, and distant 8 feet, and the needle so placed between them that it stood at 0° when the circuit was open or closed. On connecting the terminals of the interposed spirals and passing the current, the needle remained at 0°. Thus conclusively showing that *in a combination of spirals or in a helix the inductive action of the wire on itself or of adjoining spirals or turns on each other has no effect on the power of their magnetization and therefore no effect on the intensity of the current passing through them.*

I do not remember ever having seen a solution of this question, and these experiments have given it under conditions of a very strong inductive action, and with a very delicate apparatus for detecting any effect which might have been produced. The result is one which has an important theoretic bearing on dynamical inductive action, but I reserve for another communication my views on that point.

In bringing this research to a conclusion I think I may safely say that these results and experiments have shown the delicacy and precision of this method of comparing and measuring the electro-magnetic forces; and at a future time I propose using it to solve the problems which relate to the variation of the intensities of cores with their diameters and with their surfaces, and to examine the varying magnetizing effects of helices of different lengths, diameters, and number of turns of wire, and traversed by currents of various intensities.

Some of the above experiments, which relate to the inverse polarity given to cores placed inside of magnetized tubes and wires placed outside of helices, are very suggestive as to an explanation of diamagnetism, but I reserve for the present the hypothetical notions which they have originated in reference to those phenomena.

South Bethlehem, Pa., June 2, 1870.

ART. XXI.—*Abstract of the Second Series of Professor Meissner's Researches upon Electrized Oxygen;** by GEORGE F. BARKER.

IN the first series of Dr. Meissner's researches upon oxygen, published in 1863—an abstract of which, by Professor Johnson, appeared in this Journal, volume xxxvii, page 325, and volume xxxviii, page 18—he arrived at the remarkable conclusion, that oxygen, under the influence of electrical tension, was converted not only into ozone, but also into another modification which always appeared simultaneously, and which formed, when brought into contact with watery vapor, especially after the absorption of the ozone, a peculiar dense mist. This second modification of oxygen Meissner identified with Schönbein's antozone. So remarkable were these results, and so important their bearing, if true, not only upon our theories of ozone itself, but also upon the philosophy of chemistry, that Meissner desired to repeat his experiments, studying particularly the character of the antozone-mist and the effects of electrical tension upon the volume of the oxygen submitted to the discharge. The results of these experiments constitute the paper now referred to.

I.

Section I is devoted to "Electrized Oxygen." The experiment which is to be critically investigated is thus described: "Oxygen is submitted to the action of electricity in a Siemens's or von Babo's apparatus, is then passed into a receiver containing a concentrated solution of potassium iodid, in which the ozone is completely absorbed, and finally through water contained in a second receiver; the gas as it issues from the water, forms above it a thick white mist, which also appears in a less degree over the solution of potassium iodid, but which is denser, the less concentrated the solution and the more favorable the ozonizing conditions." To prove that this mist consists solely of electrized oxygen and water, Meissner proposes to show:—1st, that no other gas but oxygen is in any way concerned in the production of the phenomenon; particularly no nitrogen, chlorine, hydrogen, or carbonic acid. 2d, that for the production of the result, the presence of aqueous vapor in the electrizing tube is not necessary. And 3d, that the potassium iodid solution used for the absorption of the ozone, has nothing whatever to do with the appearance of the phenomenon, further than is implied in effecting the removal of the ozone from the current of electrized oxygen.

---

* Neue Untersuchungen über den elektrisirten Sauerstoff. Von Dr. G. Meissner. Mit zwei lithographirten Tafeln. Aus dem vierzehnten Bande der Abhandlungen der Königlichen Gesellschaft der Wissenschaften zu Göttingen. 4to, pp. 110. Göttingen, 1869.

The apparatus used to establish the first point, consists of two pieces, the first of which is intended for the production of chemically pure oxygen, the second for the electrization of this oxygen, and its subsequent analysis. We have space here for only a general description of these marvellously accurate specimens of Geissler's glass-work, referring those specially interested to the original plates. The oxygen is evolved by electrolysis from acidulated water contained in a U tube, and then passes through a second U tube filled with bits of glass moistened with sulphuric acid, then through a straight tube filled with oxydized copper-turnings to which heat can be applied, then through a second U tube containing glass and sulphuric acid, and finally through a horizontal tube a foot in length, containing anhydrous phosphoric acid: being collected, after passing an ingeniously-constructed mercury-valve, in a receiver over mercury. All these tubes are joined, either by fusion or by the most carefully ground joints. By means of the heated copper oxyd, the hydrogen, which, by diffusion or mechanically, may be mixed with the oxygen,—as well as the ozone produced by the electrolysis, and the vapor of hydrogen peroxyd—if any there be—are destroyed. The entire apparatus is first washed out with several liters of pure oxygen chemically prepared, then with 15 liters of the electrolytic oxygen; after which the oxygen, being assumed pure, is collected. The second piece of apparatus was made in duplicate; one has a tube filled with sulphuric acid between the receiver and the electrizing tube; the other has a tube filled with anhydrous phosphoric acid in this position. Moreover, in the former, Siemens's tube, in the latter von Babo's, is used for the electrization. With these exceptions they are alike; a description of one suffices. The gas passes from the receiver through the tube filled with the anhydrous phosphoric acid, to free it from any traces of moisture it may have acquired in the receiver, then into the electrizing tube, thence into two small flask-shaped receivers, the first containing potassium-iodid solution, the other water—and then to the Jolly's mercury pump. Between the drying tube and the electrizing tube is a barometer tube by which the exhaustion may be regulated. After thoroughly washing out the entire apparatus with pure oxygen, it is thoroughly exhausted, the cock beyond the electrizing tube closed, and the one connected with the receiver gradually opened, the electrizing tube thus filled with oxygen, and this submitted to the action of the silent electric discharge.* The electrized oxygen now passes successively through the solution of potassium iodid and the water, upon the surface of which latter liquid the dense anto-

---

* The electrizing tube of von Babo is described in Prof. Johnson's abstract above alluded to.

zone-mist appears, increasing momentarily in density until the flask, two inches in diameter, becomes perfectly opake. This experiment, performed in precisely this way, was repeated twelve times, and always with the same result; Dr. Meissner believes therefore, that neither hydrogen nor nitrogen is in any way concerned in the production of this peculiar mist.

The second point, that the presence of aqueous vapor in the electrizing tube is not necessary to the result, was established by filling this tube with pure oxygen dried over anhydrous phosphoric acid, then electrizing it, and, after intermitting the current, passing it through the solutions. The result was the same as before.

Nitrogen having been shown to be without effect on the production of the mist, the especial apparatus for the electrolytic preparation of oxygen was given up, and this gas was obtained from potassium chlorate, and washed with a solution of potassium iodid. In the next experiment this oxygen passed from the gasometer through a wash-bottle filled with potassium hydrate solution, then through a U tube filled with calcium chlorid, then through another wash-bottle containing sulphuric acid, and finally over anhydrous phosphoric acid, to the electrizing tube, being now free from chlorine, carbonic acid, ammonia, and moisture, though containing perhaps a trace of nitrogen. On passing a stream of this electrized oxygen, first over phosphoric anhydrid, then into iodid of potassium solution, and afterward through water, the dense cloud appeared as before. Meissner believes therefore "that these experiments, not once only or a few times repeated, but performed very frequently, prove the following point: that the mist formed by de-ozonized electrized oxygen with aqueous vapor, appears when neither chlorine, nitrogen, ammonia, hydrogen, carbonic acid nor watery vapor is present in the tube where the electrizing occurs; and that the presence or aid of neither of these substances is necessary afterward for its formation. That in other words, the mist-phenomenon requires only dry electrized oxygen, the potassium iodid used for de-ozonization, and the vapor of water, for its production."

The third point, that the potassium iodid acts simply by absorbing the ozone, and thus setting the mist-forming oxygen free, Meissner proves by replacing this substance by a great variety of other bodies, differing widely in chemical properties and agreeing only in the property of absorbing ozone. Sodium pyrogallate,—which must be free from even a trace of gallic acid—potassium ferrocyanid, potassium manganate, the higher sulphids of potassium and sodium, barium sulphid—the hydrosulphuric gas being removed by passing the de-ozonized oxygen through a solution of cupric sulphate before entering the receiver of water—ammonium sulphid, hydrosulphuric acid, sulphurous acid—either free or as sodium sulphite—and ammonio-ferrous tartrate, were all used with good results, the ozone being entirely removed. It is not necessary however, to absolutely de-ozonize the oxygen, in order to produce the mist; partial absorption will produce it, though it is weaker in proportion to the amount of ozone remaining. By using potassio- or sodio-ferrous tartrate, instead of the ammonia salt, such a partial absorption is effected. The same is true of certain metals, when moistened with water; lead, zinc, copper, iron, tin, cadmium, antimony, aluminum, and thallium all giving the mist, though lead, zinc and aluminum are the best for this purpose. Meissner compares the mist rising from these metallic bars to the cloud which rises from moistened phosphorus. Even carbon, in the form of gas-carbon, and sometimes animal charcoal, effects this absorption. The noble metals gold and platinum remain completely unaltered in both moist and dry ozonized oxygen. Chemically pure silver in the dry gas remains unchanged except at single points which show a beautiful greenish play of colors, without a trace of black peroxyd, into which, in the presence of moisture, it is so rapidly converted. Mercury, perfectly dry and still, remains unaltered, though on the least agitation a film appears on its surface, by which it adheres strongly to the glass. On replacing the ozonized by common oxygen, the mercury becomes again mobile, a black powder—of mercurous oxyd probably—being left. If the mercury be moist, reddish mercuric oxyd results. With none of these latter metals, however, is any mist formed.

Though these experiments prove conclusively that no single substance can be yielded by such widely different de-ozonizing substances, to form the mist, yet may not each yield one, which, though unlike the others in all else, may have, in common with them, this mist-forming property? This hypothesis, Meissner considers disproved by the fact that the mist is chemically identical, whatever the absorbing agent. It may be agitated with, or passed through water, not only without losing, but often with actual increase of, its properties. It may be passed through dilute sulphuric acid or alkaline solutions unchanged. "In a word, the mist which appears in my experiments under the given conditions, is neither acid nor alkaline, consists of a body neither soluble nor insoluble in water, but is solely a mechanical or adhesive combination of oxygen and water, which, when washed and collected in a gas-holder, gradually disappears, fine fluid drops collecting upon the walls of the vessel which when examined are found to be pure water, containing possibly under certain conditions, a trace of hydrogen peroxyd." When passed through strong sulphuric acid, or over calcium chlorid or phosphoric anhydrid, the cloud disappears because of the removal of the water; but it reappears again, though weaker, when brought again in contact with water. The unwashed mist, indeed, contains impurities coming from the absorbent employed; as iodine and traces of iodic acid from the potassium iodid, ammonia, hydrosulphuric gas, and probably, also, traces of ammonium nitrate. But all de-ozonizing substances do not yield such impurities, and even these may be removed without affecting the mist.

Again, may not the mist be produced either by the action of the oxygen unaffected by the electricity, or by the action of the ozone which thus results? Oxygen itself, under no known conditions, exhibits this property; and as to ozone, the above experiments show that it may be completely removed by various absorbing solutions without interfering with the phenomenon. Indeed, the mist is the stronger, the more perfect the removal of the ozone from the electrized oxygen. So that, contrary to the opinion expressed in the first paper, not a trace of mist appears until the ozone has been, at least partially, removed. That this mist is not due to a compound of ozone with water, is established by the fact that contact of the electrized oxygen with water before de-ozonization is prejudicial to its subsequent production. Further, such a compound must hold the ozone with such force as to prevent such easily oxydized bodies as potassium iodid, pyrogallic acid, or alkaline sulphids, from withdrawing it, and yet be at the same time one of the most unstable of substances. And moreover, were there such a compound of ozone and water, the de-ozonized oxygen, after careful drying, would yield water when subjected to a high temperature; Meissner however, has failed entirely to detect any moisture under these circumstances. Finally, the only known compound of water and oxygen, hydrogen peroxyd, forms no such mist. If therefore, neither the ozone itself, nor any compound of it, is concerned in the phenomenon, there must be contained in the pure dry electrized oxygen, besides the unaltered oxygen and the ozone, some third body, a third condition or modification of oxygen, to which the result is to be ascribed.

Assuming now the existence of antozone, Meissner proceeds to study more minutely the action of absorbing agents, with special reference to this substance. He divides them into two classes, one of which removes ozone alone from the solution, the other absorbs both modifications. The first substance examined is pyrogallic acid itself, by which the ozone is completely removed, the solution becoming beautiful hyacinth-red in color; but, unlike the action of the alkaline pyrogallates, the free acid removes also the antozone, so that not a trace of mist appears when the gas is subsequently passed through water. On adding a few drops of sodium hydrate, the liquid becomes dark brown, and the mist at once appears. Meissner accounts for this result, by supposing that in the oxydation of the free acid both ozone and antozone are taken into combination in equal proportions, while in that of its alkaline salts, the ozone combines to a larger extent than the antozone. Another substance examined is sodium thio-sulphite (hypo-sulphite). When the electrized oxygen passes through a concentrated solution of this salt, both ozone and antozone disappear, not a trace of mist being formed. This result is especially noticeable, inasmuch as sodium sulphite removes only the ozone, and inasmuch as the product is, in both cases, sulphuric acid. Since to oxydize sulphurous acid, two equivalents of oxygen are required for every two of sulphur; and to oxydize thio-sulphurous acid four equivalents of oxygen are needed to two of sulphur; Meissner believes that the two of oxygen in the former case are ozone; while the two times two in the latter are not of equal value, one pair being ozone, the other antozone, atoms. So also by the use of arsenous acid as an absorbent, both modifications are removed from the electrized stream of oxygen, both when the acid is free as well as when its sodium salt is employed. By using a very dilute solution, or the ordinary saturated solution in small amount, the ozone may be imperfectly removed; and by passing the gas afterward through potassium iodid solution to remove the ozone entirely, the antozone which remains gives a feeble mist with water; thus showing here also, that ozone and antozone are absorbed by the arsenous acid in equivalent proportions. Here again Meissner believes that of the two atoms needed to make arsenic acid from arsenous, one is ozone, the other antozone. Mercurous nitrate, in concentrated solution, absorbs, under the conditions of the experiment, both ozone and antozone only partially, though equally; thus acting like arsenous acid. The partial absorption in these cases is to be distinguished from that effected by the metals or alkaline ferrous salts; in the former case both the oxygen modifications are equally absorbed, while in the latter the ozone is almost entirely removed, but the antozone is unaffected.

Of particular interest is the action of electrized oxygen upon potassium hydrate. When fragments of this substance slightly moist upon their surfaces are placed in a horizontal glass tube, and subjected to the current of electrized oxygen, they shortly become covered for a short distance from the end of the tube, with orange-yellow potassium peroxyd. But however long the experiment continues, the other pieces remain unchanged, and not a trace of either ozone or antozone issues from the tube. So soon as the electrical action ceases, and ordinary oxygen again enters the tube, the yellow crust disappears, to be again formed as often as the electrized oxygen passes over it. Since the peroxyd is at once decomposed by water, and since too, the maximum production is very soon reached, it would seem that the peroxyd is successively produced and decomposed in the experiment, causing in this way the disappearance of ozone and antozone and yielding common oxygen. Absolutely dry hydrate has no effect on the electrized oxygen. In aqueous solution, the formation of the yellow peroxyd takes place only in the narrow tube which delivers the gas; but as the antozone and the ozone are materially lessened, though never absolutely destroyed in any of Meissner's experiments—it is fair to infer an action similar to that in the tube. Sodium hydrate acts similarly though less energetically; the two modifications appear moreover to be unequally absorbed, more of the antozone disappearing than of the ozone. These results illustrate well the two classes of absorbing agents; in the one case complete absorption of the antozone takes place with that of the ozone, in the other a portion of the antozone remains. In the one case partial absorption of both is equal, in the other unequal. One of these classes requires for its oxydation more ozone than antozone, the other requires them in equal proportions; possibly there is a third class requiring more antozone than ozone.

An important practical application is made of these facts to the employment of potassium iodid as an absorbing solution. A neutral solution of this substance becomes alkaline when subjected to the action of ozone, and contains free potassium hydrate, which, acting as above, diminishes the antozone. By so arranging the apparatus that acid or alkali can be added at pleasure to the iodid solution, the mist is seen to be diminished on making it alkaline and increased when it is made acid. The neutral solution of potassium iodid is not therefore a suitable de-ozonizing agent, if it is desirable subsequently to produce the mist. It should be previously acidulated with hydrochloric or sulphuric acid, especially where the quantity of antozone is small. Though for very delicate investigations Meissner prefers an aqueous solution of iodine.

The same injurious action of free alkali appears with ferrous oxyd and pyrogallic acid solution. It is completely obviated by using ammonia in place of potassa or soda, ammonium pyrogallate and ammonio-ferrous tartrate being far preferable to the same compounds of the fixed alkalies. Even with the sulphids this is true; the strongest antozone-mist Meissner observed was obtained by using ammonium sulphid as de-ozonizer, this compound absorbing more ozone and less antozone than any other substance tried. When ammonium hydrate is submitted to a stream of electrized oxygen, the ozone is partially absorbed by it, and the antozone cloud rests on its surface. By passing the mist through dilute sulphuric acid, its ammonia may be removed; and then, on collecting it in an open vessel, the water which it deposits is found to contain nitric acid which comes *from the oxydation of the nitrogen of the air.* Solutions of ammonium carbonate act in the same way. The energy with which ammonium sulphids absorb ozone, is further shown by the fact that, like moist phosphorus, some of them can polarize common oxygen, absorbing the ozone and emitting a powerful antozone-mist. Under the name "Liquor fumans Boylii," such an ammonium sulphid has long been known. It fumes strongly, but only with oxygen; neither hydrogen nor nitrogen causing it. Every unsaturated ammonium sulphid solution, if it contain sufficient sulphur, is such a fuming liquor. By passing pure oxygen through it and then through water, a dense mist is obtained which can be washed, be dried, and then be reproduced; a true antozone mist.

A singular cause of error in his previous paper is here pointed out. When dry air is electrized, the mist is easily obtained on contact with water, even without the previous use of any de-ozonizing solution. Meissner hence stated that the mist could be formed without previous removal of the ozone. But in fact the nitrogen of the air, being oxydized to nitric acid by the ozone, acts as a de-ozonizing agent. He obtained however the same result with pure oxygen. But upon examining his apparatus, he found a cork previously used with a solution of potassium iodid, which had iodine upon it and thus acted as the de-ozonizer. In his later researches all these sources of error were avoided, and the conclusion established that to produce the antozone mist, the ozone must first be wholly or partially removed.

"As the result of many oft-repeated experiments," says Meissner, "it may be asserted that there is no single fluid through which, or no single solid over which, electrized oxygen can be passed without exerting some action upon it; even when the substance is itself not oxydable, or does not fix in chemical combination any constituent of the electrized oxygen, some action takes place by which the amount of ozone or antozone in the stream of oxygen is more or less diminished." Not only finely divided gold and platinum, but also entirely indifferent bodies, such as asbestus, cotton, calcium chlorid, charcoal-powder, or finely broken coal, destroy the ozone and antozone in electrized oxygen. Even after the removal of the ozone, if the mist be passed over finely granular calcium chlorid, the antozone will disappear. The same is true of liquids; a sulphuric acid valve included in an apparatus to prevent the entrance of moisture into the electrizing tube, lessens the amount of ozone and antozone obtained.

II.

Section II treats of the "Quantitative estimation of Ozone, and the contraction of volume on electrizing oxygen." The apparatus employed is similar to that above described, the electrizing tube being widened to enclose a thermometer, and having a delicate mercury-manometer attached to it. A bulb-apparatus of simple construction holds the de-ozonizing solution, and a second similar one contains sulphuric acid. The last bulb of the former apparatus contains fine asbestus, which is found to destroy almost perfectly the cloud-forming substance, and so to retain the iodic acid and iodine carried over with it. Both bulb-tubes have wires by which they may be attached to the balance. In making an observation, the apparatus is filled with pure oxygen, the manometer, barometer, and thermometer carefully noted, the oxygen electrized, time allowed for the temperature to be equalized, communication established with the manometer, the diminution of volume ascertained, and the quantity of active oxygen determined: 1st, by the increase in weight of the bulb apparatus, and 2d, by titration with sodium thio-sulphite or occasionally, sodium sulphite. The free iodine is first determined, and then the solution is acidulated with hydrochloric acid; the hydriodic acid thus set free reacts with the iodic acid to set free more iodine, which is then titrated by itself. Hence every iodine-equivalent represents one of oxygen, and by dividing the weight of iodine found by 15·875 (127÷8) the quantity of oxygen absorbed is ascertained; or in other words, the oxygen absorbed shown by the increase of weight, must be to the iodine set free as measured by titration, as 1 : 15·875. In the experimental results given in the table, this proportion is not reached, but varies from 1 : 14·9 to 1 : 13. This variation, observed by other experimenters, was investigated. It was found not to be due to the action of the unozonized oxygen, nor to any loss of iodine in the gas-current, nor to imperfections in the titrition itself; nor did it appear when the iodine was completely oxydized to iodic acid, both weighing and titrating then giving the same results. These last experiments, Meissner believes in passing, establish the fact that ozone is simply a modification of oxygen, not an oxydized water; that no hydrogen peroxyd is produced in it; and that the method of experimenting here employed is free from errors. He believes too, that the variations observed when the potassium iodid is incompletely oxydized, may be due to the production of some intermediate oxyd of iodine; and hence, regards the method by weight as less liable to error in estimating the amount of ozone, than the method of titration. If, however, the solution of potassium iodid be either very dilute or very concentrated, or particularly, if it be acidulated; or, what is the same thing, if a solution of hydriodic acid be used,—in which case no iodic acid is formed—then the method of titration becomes accurate, the results agreeing with those obtained by weight. The objections however to the acidulated potassium iodid solution are: 1st, there is a loss of iodine, carried off by the antozone; and 2d, the iodine separates in thick masses of crystals which stop entirely the delivery tube. When too, the ozone enters the absorbing solution very dense, as when a given portion of oxygen is electrized for a long time, and then passed at once into it, the differences between the results of weighing and titrating are very much increased; the results then varying from 1:8 to 1:4, instead of 1:15·8.

The experiments on the influence of electrical tension upon the production of ozone showed that with a velocity of 2·25 to 2·50 liters per hour, a spark of 1·5 centimeters gave ·006 grms. ozone, of 4 to 5 centimeters ·0274 grms, of 5 to 6, ·0322 grms., and of 6 to 7 centimeters ·0389 grams, as the maximum; no increase being obtained with that apparatus, on increasing the length of the spark.

On the question of the contraction of volume on electrizing oxygen, a series of twelve experiments, made with the apparatus already described, show that, as a mean, the weight of oxygen absorbed as ozone, bears to a weight of oxygen equal to the observed contraction, the ratio of 1·984 to 1; i. e., essentially of 2:1. That is to say, the portion of electrized oxygen absorbed by potassium iodid, weighs twice as much as the volume of oxygen which disappears during the electrization. This result was so extraordinary, that the experiment was carefully repeated, the potassium iodid being completely oxydized; the oxygen absorbed weighed 0·0182 grams, the contraction of volume 0·00938 grams, being virtually 2:1. Meissner is not prepared to claim this numerical relation, however; he simply maintains that the electrized oxygen absorbed by the iodid, weighs more than the volume corresponding to the contraction observed. He then goes on to discuss the methods by which Andrews and Tait, and von Babo and Claus, came to different results; criticising in the former case, the apparatus used, the formula for calculation, and the method of determining the ozone by titration; and in the latter, the use of a neutral solution of potassium iodid having a wrong concentration, and the velocity of the stream of oxygen, both of which, as shown above, affect the result. As to Soret's results, which were made with electrolytic oxygen, Meissner merely remarks that if made with the use of a neutral solution of iodid as absorbing agent, as seems to be the case, they may not be reliable; though not having prepared ozone in this way, he can pass no further judgment upon them. No conclusion however, upon the density of ozone can be drawn by Meissner from his own experiments, since the results obtained are due to both the modifications of oxygen produced by the electrization; while the ratio of the two present, is unknown.

Finally, Meissner takes up the question of the electrizing of confined oxygen. He finds that the limit of the production of ozone is nearly reached in the first 10 to 20 minutes; that on diminishing the electrical tension after a time, the amount of ozone is increased; that the maximum contraction is 8·61 per cent, corresponding to $\frac{1}{11\cdot6}$, instead of $\frac{1}{12}$ as obtained by Andrews and Tait; that more than eight times the quantity of ozone is produced when the oxygen flows in a current through the electrizing tubes, than when confined, with an equal tension; that the agency which causes the return of the ozone and antozone to common oxygen, increases in activity with the density, with the percentage of electrized oxygen, and, in a given apparatus, with the electric tension; so that when this electrized oxygen reaches a certain density, the further action of the electricity destroys as much ozone and antozone as is produced. He believes that the electrization of the oxygen is dependent on the tension, the destruction of the ozone and antozone on the character of the discharge, so that for every apparatus there is a limit to the production of ozone, beyond which, if the tension be increased and the glass be not broken, the ozone and antozone rapidly disappear. If the oxygen be dry, it retains its diminished volume for many days; but if moist, much less electrized oxygen is produced, and it disappears entirely in the course of two days. Mercury appears also to have a specific action in destroying electrized oxygen.

This second research of Dr. Meissner, though not as startling in its results as the first, is yet quite as valuable a contribution to science. He has fully established the main positions taken in 1863, and it cannot longer be doubted that electrized oxygen contains two modifications of this element. The bearing of this fact upon our theories of ozone is evident.

Art. XXII.—*Description of Sclerostoma pinguicola, a new species of Entozoa, from the Hog*;[*] by A. E. Verrill.

On two occasions I have received specimens of a rather large parasitic worm, which lives in the fat of hogs. In the first case, five specimens were obtained, at New Haven, by Dr. M. C. White, from the fatty portion of a spare-rib; in the

---

[*] Extracted from "The External and Internal Parasites of Man and Domestic Animals;" by A. E. Verrill, page 138, figure 89*a*, July, 1870.

second instance, at Middletown, Conn., Dr. N. Cressy found large numbers of the worms in the fat about the kidneys of a young Suffolk pig, brought from New Jersey. Unfortunately, none of these specimens are in so good a state of preservation as to enable me to determine with certainty all the points of their structure. Those which I owe to the kindness of Dr. White had been mounted in glycerine, as microscopic objects, and pressed out flat, before they came into my possession, and the tissues were thus injured and the organs deranged. Those from Dr. Cressy were both pressed flat and dried. Yet by careful maceration, and considerable labor, I believe that most of the important characters have been made out. The body is rather robust, especially in the female, and tapers to both ends; the color is yellowish white, and the integument is seen to be finely striated transversely, when considerably magnified. The head is smaller than the body, truncated at the end; the mouth is terminal, roundish, or somewhat angular, surrounded by the thickened rim of the chitinous capsule, or pharynx. The edge of this chitinous ring rises at intervals into four to six denticles, or very small angles, which correspond to thickened, longitudinal, chitinous bands, that strengthen the pharynx, and give a slightly angular form to the mouth. The pharynx itself is small, short, and rather squarish, when seen in profile, and has three or four small, conical teeth at the bottom. The œsophagus is thick, club-shaped, and very muscular.

The male is 1·12 of an inch long, and about ·05 of an inch in diameter; the tail ends in a small blunt lobe, united with two small, entire, membranous expansions, one on each side, forming a small bursa, which is strengthened by several short rays, the exact number of which could not be determined in my specimens; there are two long, slender spicules. The females are 1·25 to two inches in length, and, as flattened between glass, they are ·10 to ·13 of an inch in breadth. The posterior end suddenly and obliquely narrows to a small conical point, which is turned to one side. The anal opening is close to the end, and the genital orifice appears to be adjacent to it. The oviducts are long, voluminous, much convoluted, and unite in a large and capacious uterus, which fills most of the cavity of the body toward the posterior end. The uterus and oviducts are both filled, in the larger specimens, with immense numbers of small oval eggs.

It is probable that this parasite is by no means uncommon.

1.

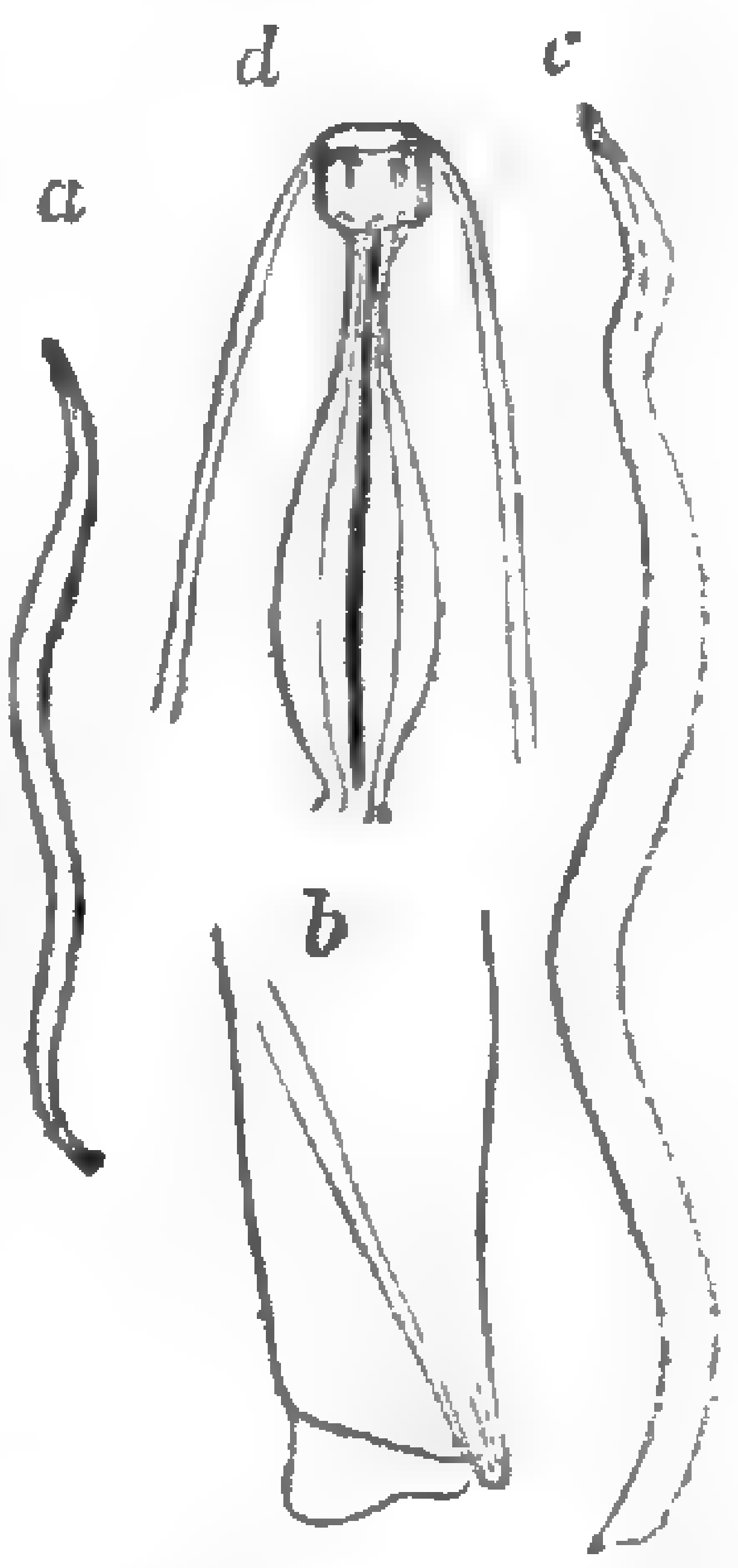


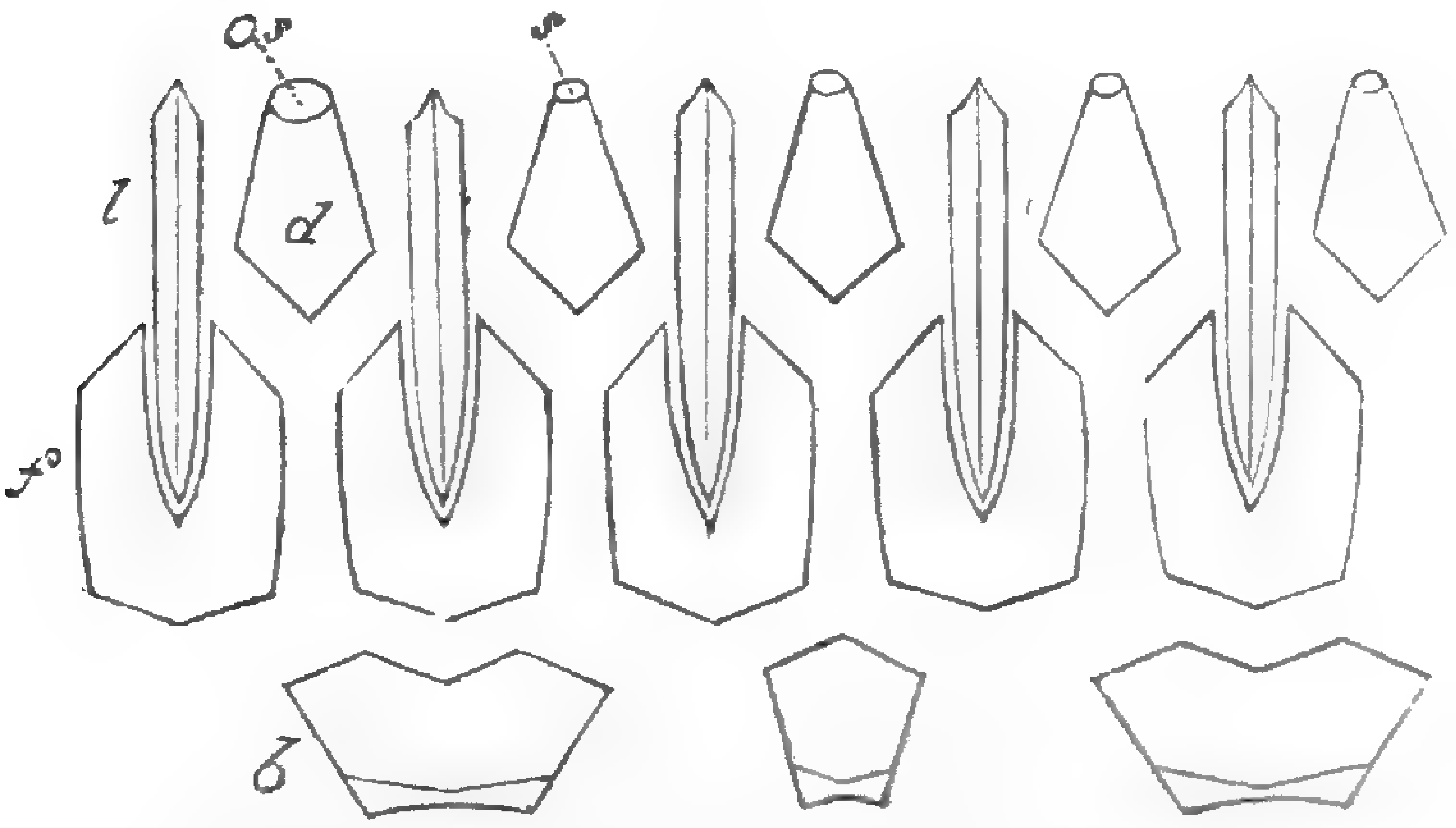
Figure 1.—*Sclerostoma pinguicola* Verrill; *a*, male, natural size; *b*, posterier end, enlarged, showing the bursa and spicules; *c*, female, natural size; *d*, anterior end of the same, enlarged, showing the chitinous capsule and œsophagus.

ART. XXIII.—*Notes on the structure of the Crinoidea, Cystidea and Blastoidea;* by E. BILLINGS, F.G.S., Paleontologist of the Geological Survey of Canada.

(Concluded from this Journal, II, vol. xlix, p. 58).

6. *On some points relating to the Structure of Pentremites.*

1.



2.

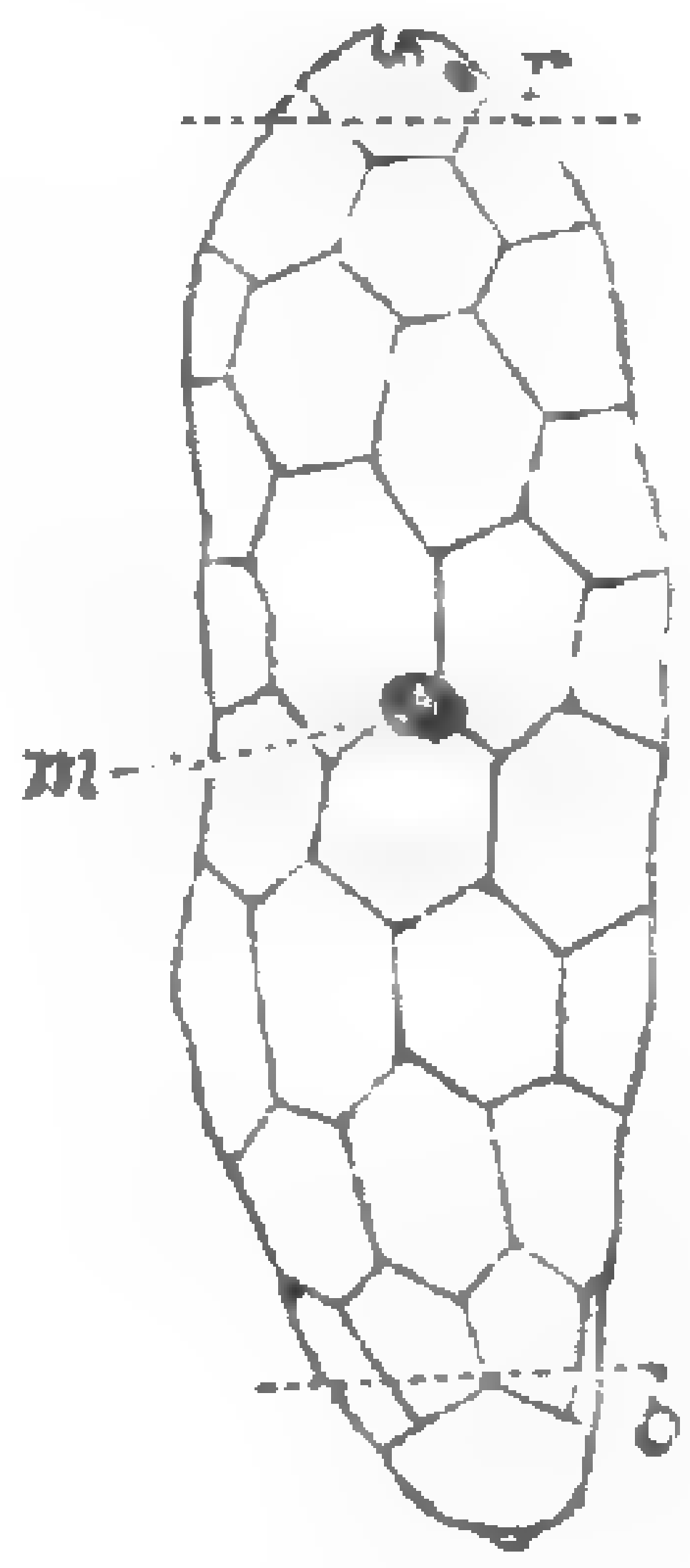


Fig. 1.—Calycine plates of *Pentremites*,—*b*, the basals; *f*, one of the five forked plates; *d*, deltoid plate; *l*, lancet plate; *os*, oral spiracle; *s*, spiracle.

Fig. 2.—*Caryocystites testudinarius*, Hisinger,—*b*, basal plates; *r*, radials; *m*, mouth.

Professor Wyville Thompson has proposed a division of the skeleton of the existing Crinoid, *Antedon rosaceus*, into two systems of plates, which he terms respectively the "*Radial*," and the "*Perisomatic*" systems.* These he considers to be thoroughly distinct from each other in their structure and mode of growth. The radial system consists of the joints of the stem, the centrodorsal plate, the radial plates, the joints of the arms, and also those of the pinnules. In the perisomatic system he includes the basal and oral plates, the anal plate, the interradial plates, and any other plates or spicula which may be developed in the perisom of the cup or disc. This I think a good arrangement, except in so far as it regards the stem, which appears to me to be, always, an appendage of the perisomatic, rather than of the radial system.

Throughout the whole range of the Crinoidea, the plates of the radial and perisomatic systems, are easily distinguished from each other. In general, the Cystidea have no radial plates in their calyces except, perhaps, in a small area around the ambulacral orifice. This accords well with an important observation

---

* On the Embryogeny of Antedon rosaceus Linck (*Comatula rosacea* of Lamarck). By Professor WYVILLE THOMPSON, LL.D., &c. Philosophical Transacions of the Royal Society, vol. clv, Part II, p. 540.

of Professor Thomson's on the structure of Antedon, while in the earlier periods of its growth. "The entire body of the Pentacrinoid is," he says, "at first, while yet included within the pseudembryo and during its earliest fixed stage, surrounded and enclosed by plates of the perisomatic system alone, and it is quite conceivable that plates belonging to this system may expand and multiply so as to form a tessellated external skeleton to the mature animal, the radial system being entirely absent, or represented only in the most rudimentary form." (Op. cit., p. 541). Such is the structure of all of the Cystidea. On referring to fig. 2, it will be seen that the whole of the body of *Caryocystites testudinarius*, is covered with polygonal plates, without any trace whatever of a radiated arrangement. The plates are disposed in nine transverse ranges, girding the body like so many rings. This species is, (and so are most of the elongated subcylindrical Cystideans), annulated rather than radiated, so far as regards the external integument. The lower range, below the line, *b*, consists of the basals, whilst the upper, above the line, *r*, may, possibly, be radiated. In all the globular or ovate Cystideans, with numerous plates, such as *Spœnonites*, *Malocystites*, *Comarocystites*, *Amygdalocystites*, and others, the shell is neither annulated nor radiated, but composed of an indefinite number of plates, increasing with the age of the individual, and arranged without any well defined or constant order. It seems clear, therefore, that the test of the Cystidea belongs mostly to the perisomatic system.

In *Pentremites* the three plates which are usually called the basals, consist each of two pieces, one placed above the other, and, in general, closely anchyclosed together. The lower pieces have each a re-entering angle, in their upper edges, for the reception of the upper pieces which stand upon them. This structure was first pointed out by Mr. Lyon (Geol. Ky., vol. iii, p. 468), and is not generally admitted, although I believe it certainly does exist. It is said that the lower pieces consist of the upper joint of the column, divided into three by vertical sutures. To me they appear to be calycine plates. It is true that they do not form the bottom of the visceral cavity, but this may be due to the growth inward of the lower edges of those of the upper series. Something like this occurs in *Antedon*, where, at first, the bottom of the cup is formed by the basals, but afterwards principally by the first radials.

The forked plates are usually called "*Radials*," but they certainly do not belong to the radial system. If they did, they would represent the first radials of the Crinoidea, and therefore they should support the bases of the ambulacra. A little consideration will, however, enable any one to perceive that in *Pentremites* the bases of the ambulacra, are situated in the apex of the fossil, and do not come in contact with the forked plates. The apex of *Pentremites* is identical with the actinal center of Sea-urchins and Star-fishes, in which the mouth is situated. It is here that the ambulacra originate and grow outward by the addition of new plates to their distal extremities. There can be little doubt that such was the mode of growth of the ambulacra of the *Pentremites.* The smaller extremity, therefore, of their ambulacra, which is received into the forked plate, is not the base, but corresponds with the apex of the ambulacrum of a Sea-urchin or of a Star-fish. It also represents the tip of the arm of a Crinoid. If the forked plate is radial, then the arrangement of the ambulacrum must be the same as that which would be exhibited in a Crinoid, with the upper end of the arm downward, and resting on the first radial, whilst the lower end would be upward, the tip being formed of the second radial. From this it follows that the forked plates do not belong to the radial, but to the perisomatic system.

The five deltoid plates alternate with the forked plates, and are also perisomatic.

It is not certain that the lancet plates represent any of those plates which in the Crinoidea are usually called "radials." They are so arranged that if they were loosened from the walls of the cup, and their smaller extremities turned upward, whilst their bases or larger ends retained their position, they would stand in a circle around the apex, as do the arms of an ordinary Crinoid. Their bases would alternate with the apices of the deltoid plates. They would form the outside of the arms, whilst the grooves and pinnulæ would be inside. Each would bear, on its outer or dorsal aspect, two elongated sacks, the two hydrospires that belong to the ambulacrum. I believe that the small groove in the ambulacrum of *Pentremites* was occupied by the ovarian tube only. If this be true, and if, also, the lancet plates represent the radial plates of the arms of the Crinoids, then the arm of *Pentremites* would have the respiratory portion of the ambulacral system on its dorsal, and the ovarian portion on its ventral aspect.

In the true Crinoids, both the respiratory and ovarian tubes are situated in the groove in the ventral side of the arm.* In

---

* Thomas Say, who was the first to recognize the Blastoidea as a group distinct from the Crinoidea, also supposed the function of the ambulacra to be respiratory. He says, "I think it highly probable that the branchial apparatus communicated with the surrounding fluid through the pores of the ambulacræ, by means of filamentous processes; these may also have performed the office of tentacula, in conveying food to the mouth, which was, perhaps, provided with an exsertile proboscis; or may we not rather suppose that the animal fed on the minute beings that abounded in the sea water, and that it obtained them in the manner of the Ascidia, by taking them in with the water. The residuum of digestion appears to have been rejected through the mouth." (Jour. Acad. N. S. Phil., vol. iv, p. 296, 1825).

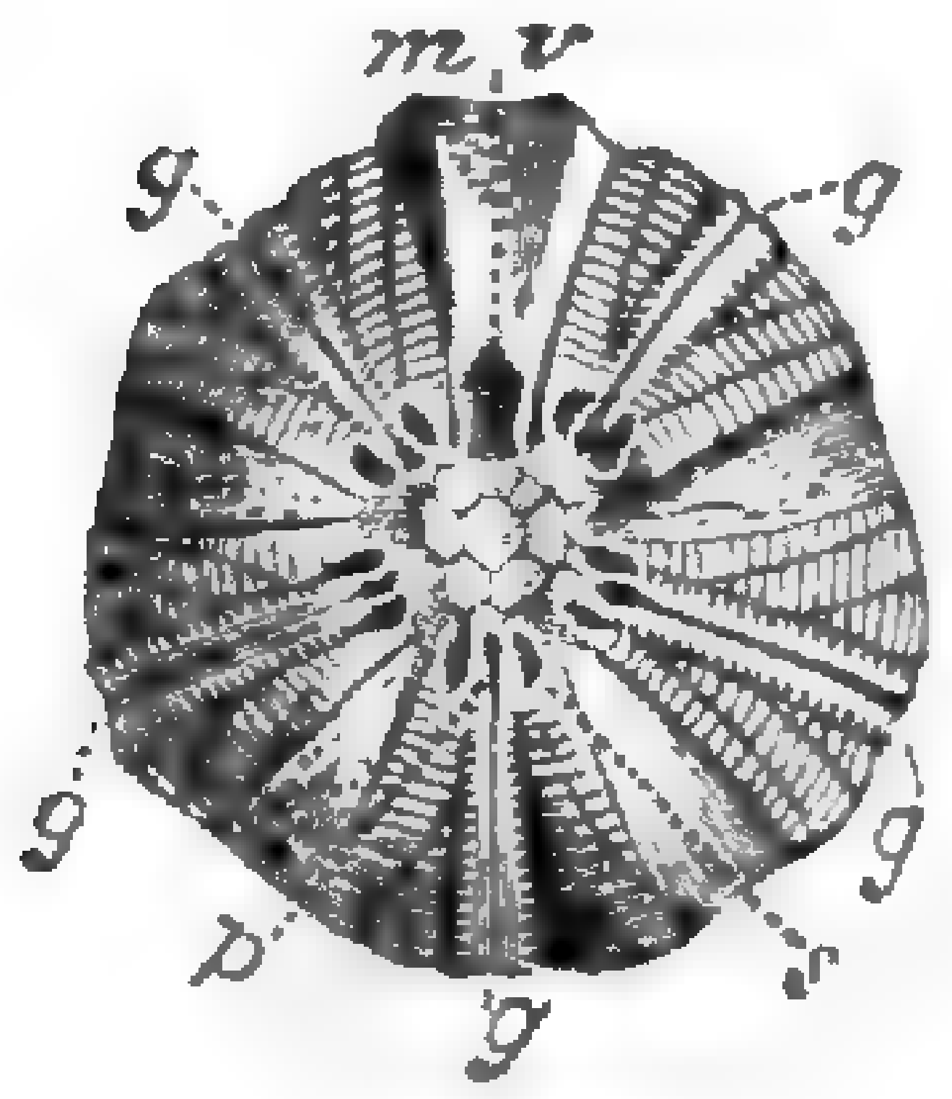
the Crinoids the pinnulæ are attached to the radial joints of the arm. In *Pentremites* they are not connected with the lancet plate, but with the pore plates. In *P. pyriformis* they appear to me to stand in sockets excavated in the suture between the pore plates proper, and the supplementary pore plates. Miller compared them to the series of azygos plates, which underlie that portion of the ambulacrum of *Pentacrinus* that runs from the mouth to the base of the arm. These resemble the lancet plates, in their being azygos and not connected with pinnulæ; but then, on the other hand, they differ from them in having, a portion at least, of the respiratory tubes on their ventral aspect. Mr. Rofe says that, "in many species of Pentremite, if not in all, this lancet plate is in reality a compound plate, formed of two contiguous plates, extending from the bottom of the sinus to the top, and, then turning right and left round the summit-openings, they pass down the adjoining sinus, to form half its lancet-plate, leaving at the apex of the body a pentagonal aperture, supposed to be the mouth. In some weathered specimens, the two parts of the lancet plate are separate; and in many they appear to meet only at the top and bottom of the cross section, leaving a lozonge-shaped opening between them." (Geol. Mag., vol. ii, p. 249.) In a large specimen of *P. obesus* (Lyon and Cassiday) which was given to me by Mr. Lyon, a polished section shows that one of the lancet plates is thus divided, but in general no trace of a suture can be seen in these plates.

There are several points in the structure of the ambulacra of *Pentremites* that are well worthy of the study of those who have plenty of well preserved specimens. Among these, I would direct special attention to the markings in the ambulacrum of *P. pyriformis.* The median groove, which I suppose to have been exclusively occupied by the ovarian tubes, sends off branches, right and left alternately, toward the sides of the ambulacrum. These branches do not run directly to the ambulacral pores. Each of them terminates at a point between the inner extremities of two of the pores. There is at this point a small pit which appears to be the socket of an appendage quite distinct from the pinnule. The groove does not reach the socket of the pinnule, which is situated further out, between two of the pores. On the other hand a small groove runs from each pore, inward, and terminates at another socket, about half-way between the pore and the main median groove of the ambulacrum. It would thus appear that besides the ordinary pinnules, there were two other rows of appendages on each side of the median groove.

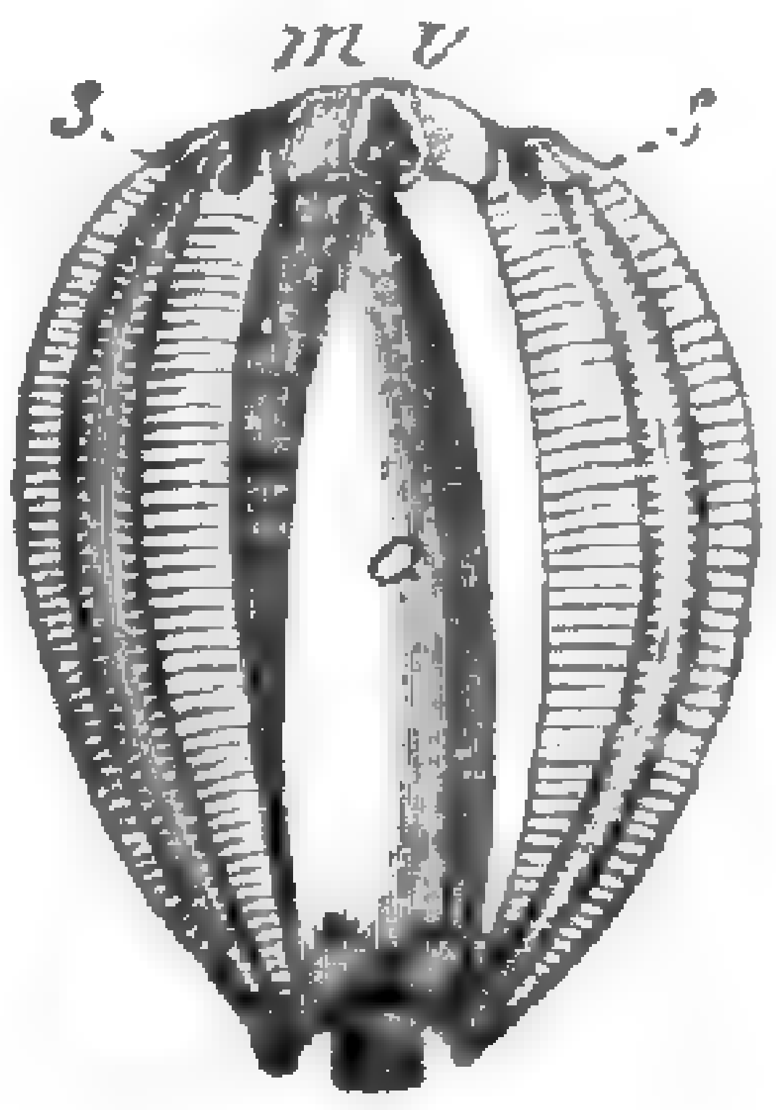
The general conclusions at which I have arrived from the above, are, that all the principal plates that compose the shell of *Pentremites*, belong to the perisomatic system of Professor Wyville Thompson; that it is doubtful whether or not the lancet plates are homologous with the radial plates of the Crinoids; and that the ambulacra are more complicated in their structure than is generally supposed.

7. *On the Structure of the genus Nucleocrinus.*

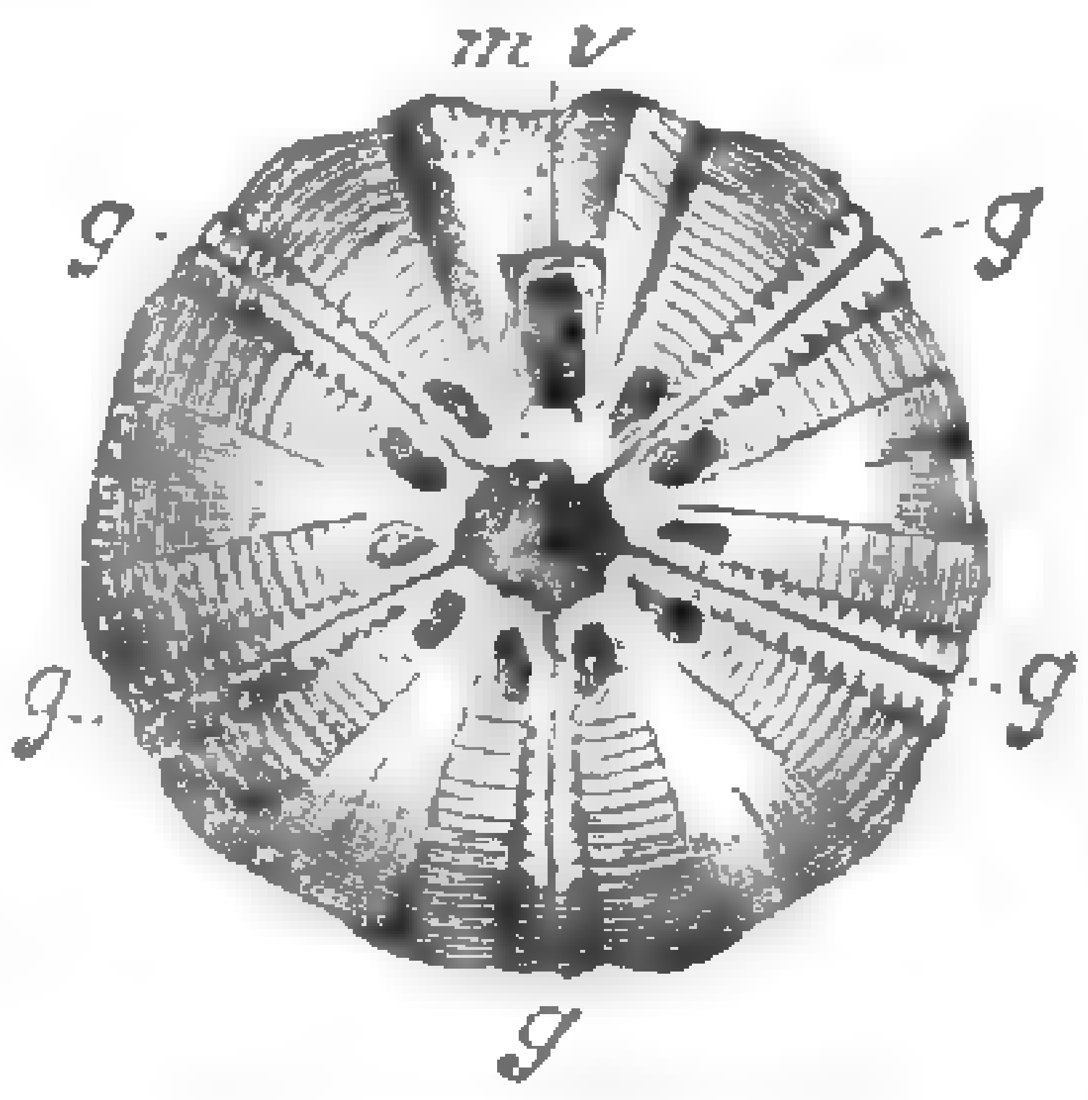
3



4



5.



6.

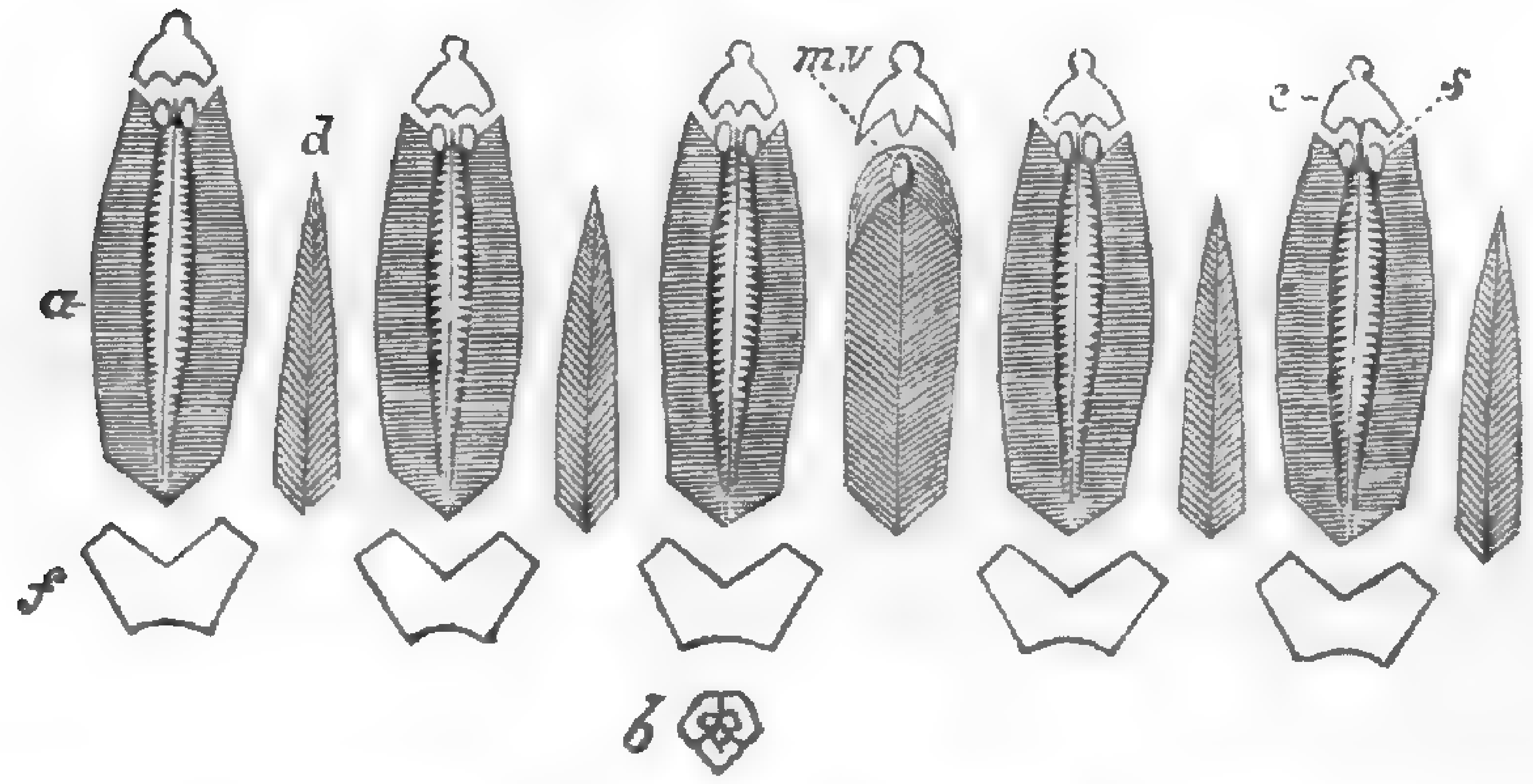


Fig. 3.—Apex of *Nucleocrinus Verneuilii* Troost. *g*, ambulacral groove; *p*, pore through which groove enters into the interior; *s*, one of the ten spiracles; *mv*, oro-anal aperture. 4. Anterior side of a specimen; *a*, the anterior interradial. 5. Apex of a specimen which has lost the integument that covered the center. 6. Diagram of the plates of the test; *a*, ambulacral plate; *b*, the basals; *c*, plates of the apex; *d*, one of the interradials; *f*, forked plate.

The body of this remakable genus is ovate, elliptical or oblong, and inclosed in a shell of strong perisomatic plates, which are, in general, so closely anchyclosed that the sutures between them cannot be distinguished. According to Mr. Lyon, who, through his long continued geological researches, has collected and studied a vast number of specimens, there are three minute lozenge-shaped, or quadrilateral basal plates, situated at the bottom of the columnal pit; always concealed when the column is present. These are surrounded by three other plates, the six altogether corresponding to the six pieces which constitute the compound basal plates of *Pentremites*. They are represented at fig. 6, *b*, as figured by Mr. Lyon (Geol. Ky., vol. iii, pl. v, fig 1, *b*.)

In the next series there are five plates which are undoubtedly the homologues of the five forked plates of *Pentremites.* They are very short and confined to the base of the body. They form a shallow basin with ten re-entering angles in its margin. Fig. 6, *f.*

Alternating above the forked plates, are five pieces corresponding to the deltoid or interradial plates of *Pentremites.* Some of these are lanceolate in form (fig. 6, *d*), their broader extremities fitting into the angles between the forked plates. They taper to a point upward, and their sides are bevelled so as to pass under the ambulacral plates, to which they are, in general, so closely united, that the line of junction is indicated only by the difference in the markings of the surface. Owing to this structure, these plates have not always been recognized by the authors who have described this genus. They were first pointed out by Mr. Lyon. The fifth deltoid or interradial plate is truncated at its apex for the reception of the ovo-anal orifice (*mv*, figs. 4, 6). The sutures on each side of this plate are generally distinctly visible, especially in the upper part of the body.

The ambulacra are narrow—one line wide in a specimen fifteen lines in length, with a fine median groove, about large enough to accommodate a tube of the size of a horse-hair. There are two rows of pores, those on one side of the groove alternating in position with those on the other side. These pores lead into the hydrospires. There appear to be only two rows of ambulacral ossicles. The pores are situated in the sutures between them. On each side of the ambulacrum there is a broad transversely grooved marginal plate. From each pore a small rounded ridge runs across this plate. The grooves between the ridges originate at the outer extremities of the ambulacral ossicles. In well-preserved specimens the surface of these marginal plates exhibits no other structure than the transverse grooves and ridges; but in one weathered specimen that I have examined, they seem to be composed of a number of narrow elongated pieces, arranged transversely, in such a manner that two of them abut against the outer extremity of each of the ambulacral ossicles, and extend outward toward the interradials. This seems to prove that the marginal plates belong to the ambulacra, as pointed out by Mr. Lyon, and not to the interradials, as represented by other authors. Although I have studied a large number of specimens, none of them were sufficiently perfect to enable me to make out the whole structure of this part of the test of *Nucleocrinus.* I have, however, seen enough to convince me that the ambulacra are much more complex than is usually supposed. The lancet plate, if it occur at all in this genus, must be very narrow. The ambulacral groove, as in *Pentremites*, sends off branches, right and left.

There is also evidence of the existence of minute marginal plates on each side of the groove.

7.

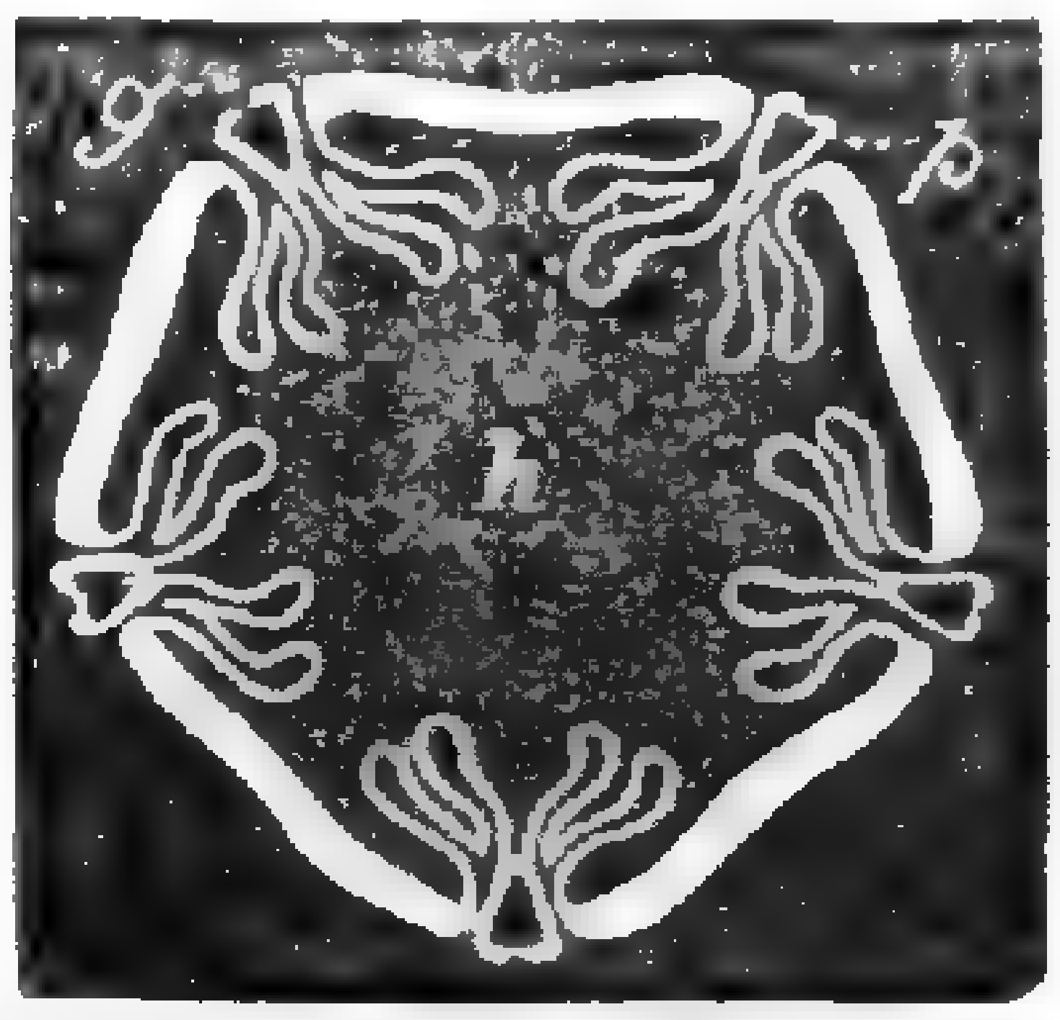


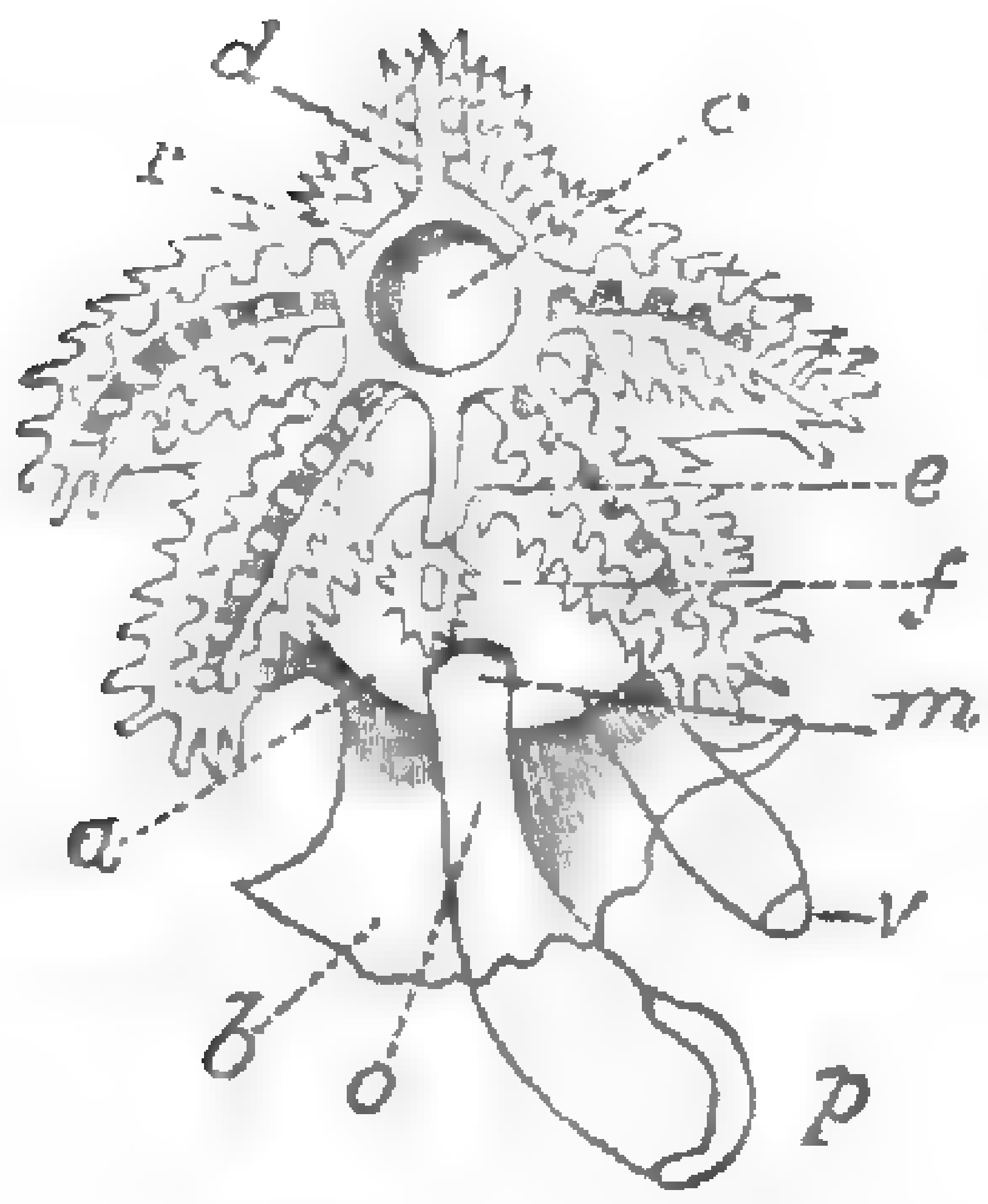
Fig. 7. Transverse section through a specimen which has all the hydrospires preserved. *h*, the two anterior hydrospires; *p*, pore leading into the hydrospire; *g*, one of the grooves.

The hydrospires are ten elongated sacks, each with two deep folds. They are perfectly homologous with those of *Pentremites*, only differing therefrom in not being united in pairs; consequently there are ten spiracles instead of five. The mouth, or oro-anal orifice, is larger in proportion to the size of the body than it is in *Pentremites.* Mr. Meek informs me that the mouth in some of the Blastoidea is protected by a single valve that covered it like the lid of a jug. From the structure of the orifice, I am inclined to think that in *Nucleocrinus* it possessed a similar protection.

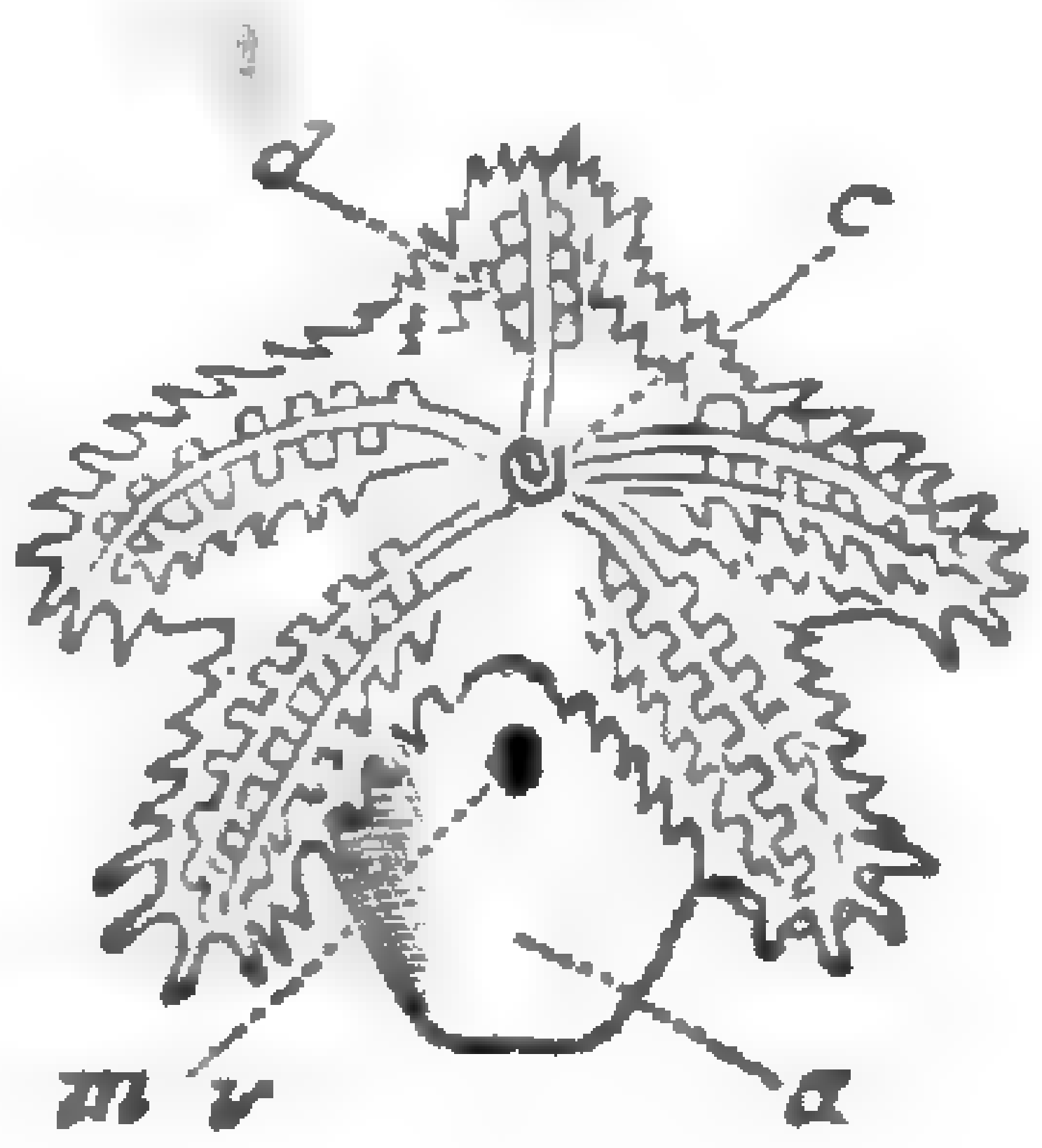
In the apex, nearly all the space within the circle of apertures is covered by a thin integument of small plates, fig. 3. When this is not preserved, a large sub-pentagonal aperture is seen, as shown in fig. 5. This aperture occupies the position of the mouth in the existing echinoderms. The integument, as will be shown further on, represents that which covers the mouth of an embryonic Star-fish. Mr. Conrad described this genus in 1842, as having only one aperture in the summit. "This genus differs from PENTREMITES, Say, in having only one perforation at top, which is central." (Jour. Acad. Nat. Sci. Phil., vol. viii, p. 280, pl. xv, fig. 17). His figure represents the fossil with the apex downward. Dr. Ferd. Roemer, showed that, when perfect, there is no central opening, and he made this one of the grounds for separating the genus from *Pentremites.* He described the apex as being provided with six apertures, five of which were divided by a partition within each. These he considered to be the ovarian orifices. The sixth he supposes to be both mouth and vent, which accords with my view. (Mon. der Blastoideen, p. 378). In 1868 I discovered the five small pores at the apical extremities of the ambulacral grooves. (This Jour., II, xcvii, p. 353, and Annals Nat. Hist., IV, vol. 4, p. 76). In general it is difficult to see these pores, but if a silicified specimen, which has been fossilized in a calcareous matrix, be placed in an acid for two or three minutes, the acid cleans them out and they then become distinctly visible. I believe these to be the pores through which the ovarian tubes passed outward along the grooves to the pinnulæ. There are thus, sixteen apertures in the apex of *Nucleocrinus,*—ten spiracles, five ovarian orifices, and one oro-anal aperture. There are no true radial plates. The whole of the test with the exception, perhaps, of the ambulacra belongs to the perisomatic system.

8. *On the occurrence of Embryonic forms among the Paleozoic Echinoderms.*

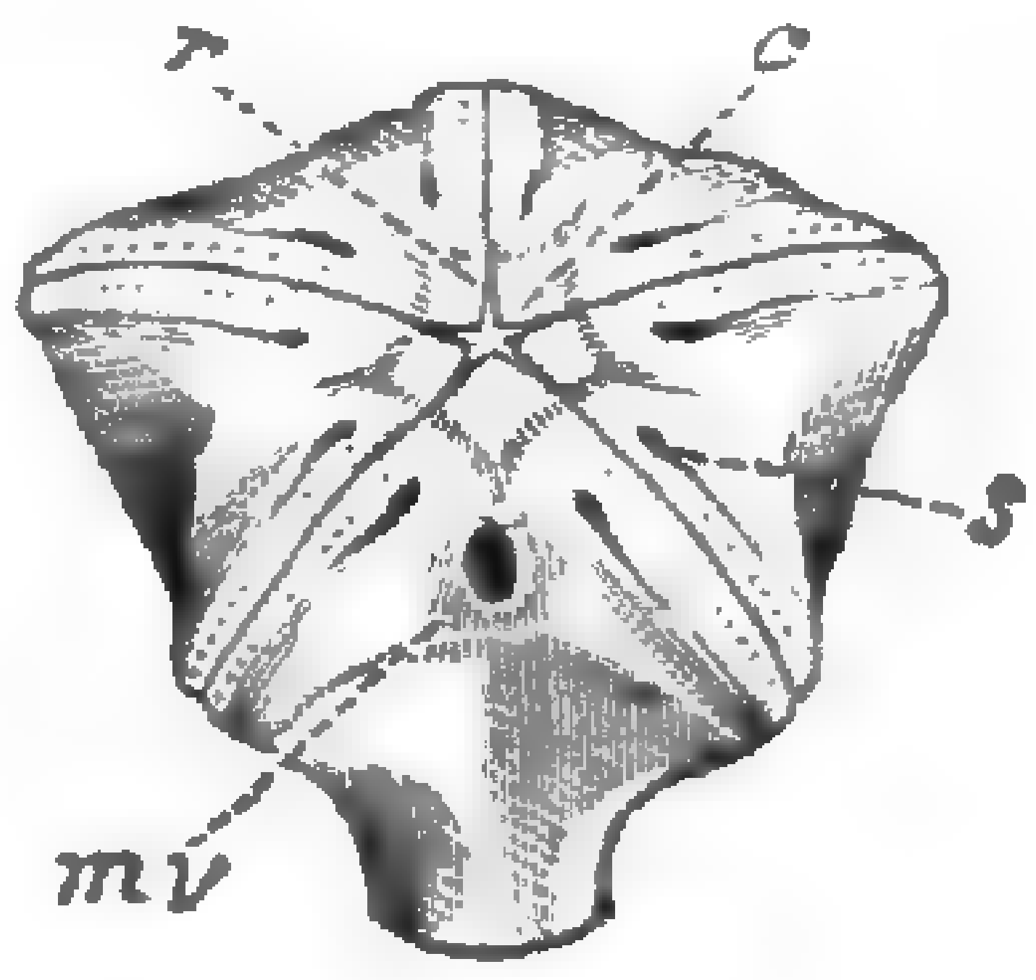
8.



9.



10.



11.

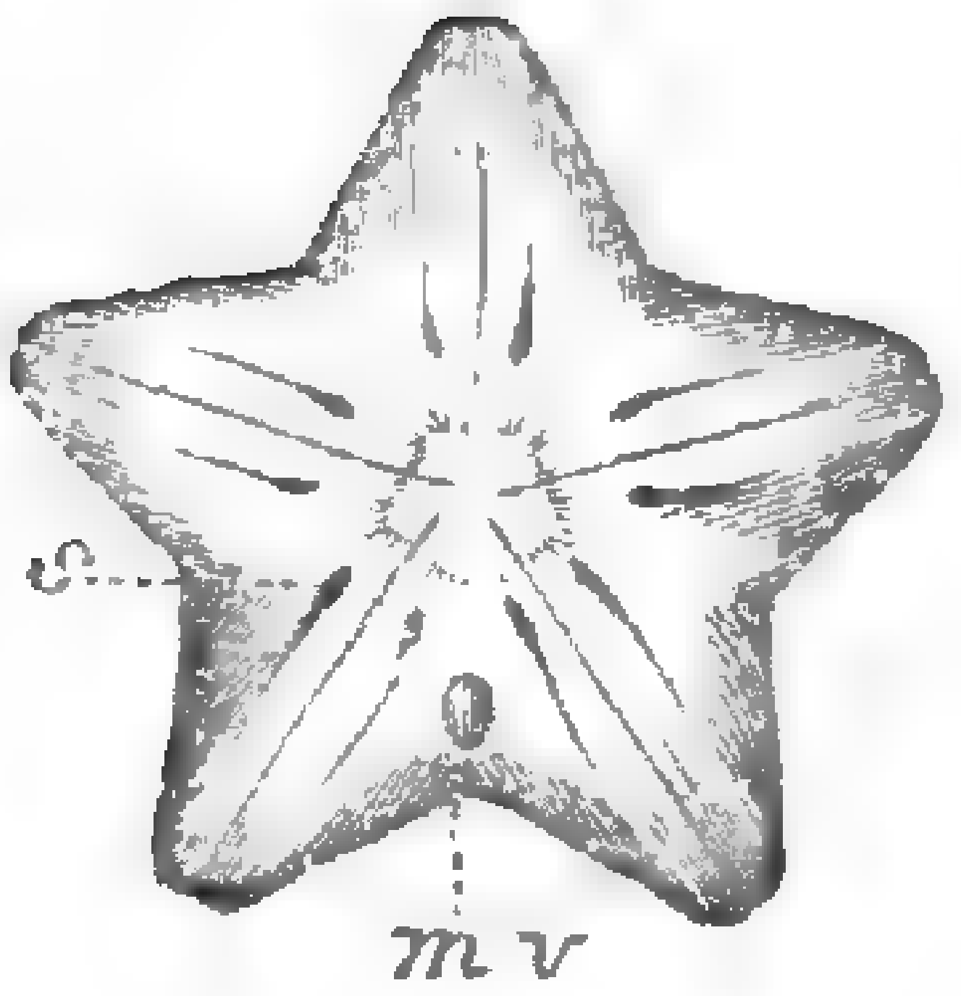


Fig. 8. *Bipinnaria asterigera* Sars, (copied from Müller). *a*, the stomach; *b*, part of the body of the larva; *c*, ambulacral centre, position of the permanent mouth, in this stage not open; *d*, one of the five ambulacral canals; *e*, sand canal, *f*, madreporic plate; *m*, entrance into the stomach; *o*. œsophagus; *p*, larval mouth or pseudostome; *r*, œsophageal ring; *v*, vent. 9. Ideal figure described below. 10. *Codonites stelliformis*, oblique view to show both body and summit. 11. Summit of fig. 10.

No proposition in Natural History has been more clearly demonstrated than this:—That, in general, the paleozoic animals resemble, both in external form and internal structure, the embryonic stages of those of the same class at present existing. Prof. Agassiz has long taught in his lectures and various publications, that this is especially observable in the Echinodermata. Judging from the figures and descriptions of Müller, Agassiz, Thomson, Carpenter and others, I should say, that in this class, the most striking resemblance is that which occurs between the adult stages of the Cystidea, Blastoidea, and Crinoidea, on the one hand, and the embryonic Star-fishes on the other. The structural character that has the most important bearing on the subjects discussed in these notes, is, that in all four of these groups, the mouth is situated in one of the interradial areas,—not in the ambulacral center, as it is in the adult forms of the existing Echinodermata.

In *Bipinnaria asterigera* Sars, according to Müller, the digestive cavity is a sub-globular sack without any extensions into the rays, as there are in the adult Star-fishes. The œsophagus, fig. 8, *o*, is a fleshy, consistent tube, with a large mouth or pseudostome, *p*. It passes through the wall of the stomach by an opening somewhat smaller than the mouth, and situated in one of the interradial spaces at *m*. The madreporic plate, *f*, and sand canal, *e*, the latter holding the convoluted plate (when it occurs), are situated above the orifice, *m*, and between it and the ambulacral center, *c*. The circular space at *c*, is undoubtedly the homologue of the central space in the apex of *Nucleocrinus*, figs. 3 and 5, and of *Codonites*, figs. 10 and 11. It is also the position of the mouth in the adult Star-fish; but in the larval stage it is completely closed by the soft external skin and sarcode of the body. In the fossils it is also closed, but by an integument of thin calcareous plates. The *Bipinnaria* is nourished by minute particles of matter diffused through the water, and drawn into the digestive sack through the mouth and œsophagus by the action of interradial cilia. I believe that all the fossil Crinoidea, Blastoidea and Cystidea, ingested their food in this way, and without any aid whatever from the arms or pinnulæ.

Perhaps there is no embryologist who will not admit, that it is possible for an animal like *Bipinnaria* to develope organs of reproduction and propagate its species, none of its other parts making any farther advance. Such an animal, with some slight modifications, would not be very widely different from a paleozoic Crinoid. If the sarcodic body wall were to be consolidated into a thin calcareous integument, with the mouth even with the surface, the swimming appendages aborted, and the vent closed up, it would resemble the cup of an *Actinocrinus*, fig. 9, *a*. The lateral orifice would then be both mouth and vent, as it is, at first (according to Prof. A. Agassiz, Seaside Studies, p. 125), in the embryo of *Asteracanthion Berylinus*. The ambulacral canals of *Bipinnaria* are the homologues, in a general way, of those which are found beneath the vault of *Actinocrinus*, and extend out into the grooves of the arms. If the ventral perisome of the Crinoid were to be removed (the internal organs remaining undisturbed) the arrangement disclosed would be that represented in fig. 9,—a convoluted plate in the center with the canals radiating from it. The most striking difference is the absence of the œsophageal ring. According to the organization of *Actinocrinus* there could be no œsphagus at that point, and consequently there is no ring. The convoluted plate represents the madreporic apparatus. The sucking feet of the Star-fish, most probably, represent the respiratory tentacles that border the grooves of the Crinoids, but modified into prehensile and locomotive organs. *Bipinnaria* and *Actinocrinus* agree in having the mouth in one of the interradial areas, and in the absence of an orifice through the perisome at the ambulacral center. These two characters are embryonic and transitory in the Star-fish, but they were permanent in most paleozoic Crinoids.

In *Codonites stelliformis* (*Pentremites stelliformis* Owen and Shumard), figs. 10, 11, the ambulacral center, *c*, is completely closed. Five minute grooves radiate out to the extremities of the five angles of the disc. These grooves are identical with those of *Pentremites* and *Nucleocrinus* and were occupied by the ovarian tubes. The ambulacral canals of the true Crinoids and of the Star-fishes are represented in a rudimentary condition, in this species, by the hydrospires which open out to the surface through the ten fissure-like spiracles, *s.* The oro-anal orifice is interradial. *C. stelliformis* in external form, the interradial position of the mouth, and the closed ambulacral center, resembles *Bipinnaria* and *Actinocrinus*, but differs importantly in having its respiratory organs arranged in ten separate tracts, all totally disconnected from each other. It is a lower form than *Actinocrinus*, which in its turn is lower than *Bipinnaria*, and yet all three are constructed on the same general plan.

C. stelliformis, although much resembling a *Pentremite*, is a true Cystidean. Its affinity to *Codaster* was first pointed out by Dr. C. A. White, who also suggested that it should be assigned to a distinct group. (Bost. Jour. N. H., vol. vii, pp. 486, 487). The main difference between the Cystidea and the Blastoidea is, that in the former the hydrospires do not communicate with the pinnulæ, whilst in the latter the cavities of the pinnulæ and hydrospires are directly connected by the ambulacral pores.

The developement of the recent Crinoid *Antedon rosaceus*, as described by Prof. Wyville Thomson (Phil. Trans., 1866), pursues a course that could not possibly result in the production of such an animal as *Actinocrinus.* The pseudembryo, as it is called by Prof. Thomson, is a small ovate organism, with four transverse ciliated bands, a large key-hole-shaped mouth (pseudostome), and a small circular vent (pseudoproct). These orifices are connected by a rudimentary intestine (pseudocele). In this stage there is no trace of radiation, and the mouth, therefore, cannot be said to be interradial in its position.

The nascent Crinoid originates within the pseudembryo, but developes a mouth, vent and stomach, of its own, all quite distinct from those of its nurse. This new, or permanent mouth, is for a short time both oral and anal in its function, but although in this respect it resembles that of *Actinocrinus*, its position in the center of the ambulacral system, shows it to represent the mouth of the adult Star-fish, while that of *Actinocrinus* rather homologates with the oral orifice of the *Bipinnaria.* At no time during its development does the ventral perisome exhibit the structure of that of the paleocrinoids, i. e., no orifice in the ambulacral center, and at the same time one in an interradial space. In the central position of its mouth, and in the possession of an œsophageal ring, *Antedon* stands above *Actinocrinus* in rank, and between it and the adult Starfish. In none of its stages does it resemble a *Bipinnaria* either in form or in structure.

9. *On some of the objections that have been advanced against the views advocated in the preceding notes.*

In all the known species of the existing Echinodermata, the mouth is situated in the center of the ambulacral system, and it is contended that this fact proves that such must have been its position also in the paleozoic forms.

This reasoning is not strictly logical. It is true that in the known existing species, the mouth is in the center, but it does not certainly follow that it is so in all the Echinodermata, living and extinct. Whether it be so or not in any particular fossil species whose structure may be under investigation, *is a question of fact which can only be positively determined by direct observation of specimens.* On appealing to these we find that, in a large proportion of the fossil forms, there is no aperture in the perisome at the ambulacral center. It also becomes evident by the comparison that, in general, the paleozoic species resemble the embryonic stages of some of the recent Echinoderms, and that in these, (*Bipinnaria* for instance), the mouth is interradial. Rules such as is relied on in this case, afford a certain amount of presumptive evidence, which, however, cannot prevail against material and visible facts. When we can see clearly that there is no aperture in that point, in the vault of a Crinoid, beneath which we know the ambulacral center is situated, it is perfectly useless to supply one by deduction.*

The second objection is, that many of the fossils have a *Platyceras* attached to them, in such a position as to cover the aperture which I call the mouth, and under such circumstances as to induce the belief that it lived parasitically on the Crinoid. The only answer I can make to this is that, admitting the facts, we must suppose that space was left for a stream of water to pass under the edge of the shell, into the mouth of the Crinoid. In general, where one animal lives parasitically upon another, it does not destroy its host. Some of the gasteropods of the Devonian and Carboniferous ages, were carnivorous, as is proved by the bored shells and Crinoids that are occasionally found. I have seen a number of such specimens, and several

---

* The position of the ambulacral center may thus be found. When the mouth is eccentric, the ambulacral tubes usually converge to the center of the vault. But when the mouth is central, we first find the azygos interradius, in general easily recognized by its possessing a greater number of plates than do any one of the other four interradii. On the opposite side of the fossil is the azygos arm. The ambulacral center is always situated between this arm and the mouth, never on the side of the mouth toward the azygos interradius.

years ago I read a paper on the subject (which was never published) before the Natural History Society of Montreal. There were several good Conchologists present, and the specimens exhibited were compared with bored shells of existing species. All pronounced the style of workmanship to be precisely the same. I have the proboscis of an *Actinocrinus* that is bored near the base, and among the fossils lent me by Mr. Wachsmuth, is a *Codonites stelliformis*, that is bored through one of the ambulacra. The view I took of the subject in my paper, was that the gasteropod ascended the stalk of the Crinoid, and thrust its proboscis into the mouth of the latter. The Crinoid then slowly drew its arm together, and held the shell fast until both died.

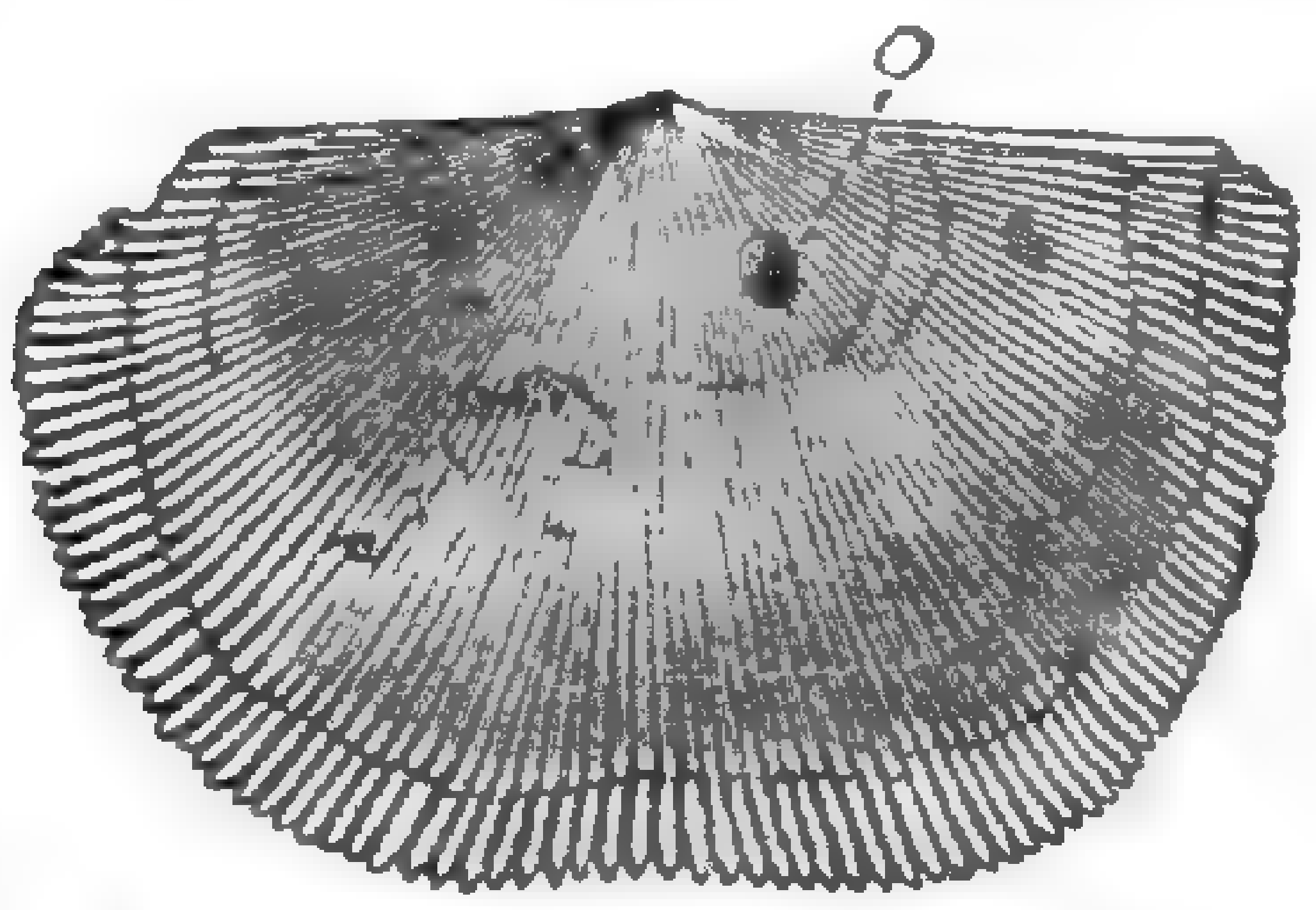


Fig. 12. *Streptorhynchus Pandora.* A specimen bored at *o* by a carnivorous gasteropod. From the Corniferous Limestone, Devonian, Canada.

A third objection is the small size of the aperture in some of the species. In general, where there is no proboscis, the orifice is from one-twentieth to one-tenth of an inch in diameter, quite sufficient for an animal that subsists on microscopic organisms. It is stated by Meek and Worthen that where there is a proboscis, the aperture is sometimes scarcely "more than *one-hundredth* of an inch in diameter." I believe that in many such instances the tube filled up by calcareous deposits on its inside, and that when entirely obstructed, either a new aperture opened out in the side of the proboscis, or that the animal died. In Mr. Wachsmuth's collection, I saw a specimen with a second aperture in process of formation. A ticket was attached to it by him, giving this explanation. I am also informed that in some of the existing species of *Antedon* "the mouth is an exceedingly minute aperture."

A fourth objection is that the aperture is so situated that the arms could not have conveyed food to it. It is, however, proved by Dr. W. B. Carpenter, that in the recent Crinoids the arms are not prehensile organs. The animal while feeding remains motionless, attached by its dorsal cirrhi to a stone, shell, or other object on the bottom. Its arms are either stretched out to their full length, or more or less coiled up, but quite immovable. As Dr. Carpenter's remarks have a very important bearing upon the subject, I shall take the liberty of quoting the following :—

"Whatever may be the purpose of the habitual expansion of the arms, I feel quite justified that it is *not* (as stated by several authors whom I have cited in my historical summary) the prehension of food. I have continually watched the results of the contact of small animals (as Annelids, or Entomostracans and other small Crustaceans) with the arms, and have never yet seen the smallest attempt on the part of the animal to seize them as prey. Moreover, the tubular tentacula with which the arms are so abundantly furnished, have not in the slightest degree that adhesive power which is possessed by the "feet" of the ECHINIDEA and ASTERIADA; so that they are quite incapable of assisting in the act of prehension, which must be accomplished, if at all, either by the coiling-up of a single arm, or by the folding-together of all the arms. Now I have never seen such coiling up of an arm as could bring an object that might be included in it into the near neighborhood of the mouth; nor have I seen the contact of small animals with a single arm produce any movement of other arms towards the spot, such as takes place in the prehensile apparatus of other animals. Moreover, any object that could be grasped either by the coiling of one arm, or by the consentaneous closure of all the arms together upon it, must be far too large to be received into the mouth, which is of small size and not distensible like that of the ASTEROIDA."*

Farther on Dr. Carpenter says:

"It was affirmed by M. Dujardin (l'Institut, No. 119, p. 268) that the arms are used for the acquisition of food in a manner altogether dissimilar to ordinary prehension; for recognizing the fact that the alimentary particles must be of small size, he supposed that any such, falling on the ambulacral (?) furrows of the arms or pinnæ, are transmitted downwards along those furrows to the mouth wherein they all terminate, by the mechanical action of the digitate papillæ which fringe their borders. This doctrine he appears to have abandoned; since in his last account of this type (Hist. Nat. des Echinoderms, p. 194) he affirms that the transmission of alimentary particles along the ambulacral (?) furrows is the result of the action of cilia with which their surface is clotted. Although I have not myself succeeded in distinguishing cilia on the surface which forms the floor of these furrows, yet I have distinctly seen such a rapid passage of minute particles along their groove as I could not account for in any other mode, and am therefore disposed to believe in their existence. *Such a powerful indraught, moreover, must be produced about the region of the mouth, by the action of the large cilia which* (*as I shall hereafter describe*) *fringe various parts of the internal wall of the alimentary canal, as would materially aid in the transmission of minute particles along those portions of the ambulacral* (*?*) *furrows which immediately lead toward it;* and it is, I feel satisfied, by the conjoint agency of these two moving powers that the alimentation of Antedon is ordinarily affected. In the very numerous specimens from Arran the contents of whose digestive cavity I have examined, I have never found any other than microscopic organisms;

---

* Researches on the Structure. Physiology, and Development of *Antedon* (*Comatula*, Lamk.) *rosaceus*.—Part I. By W. B. Carpenter, M.D., F.R.S. Philosophical Transactions of the Royal Society, vol. clvi, Part II. 1866

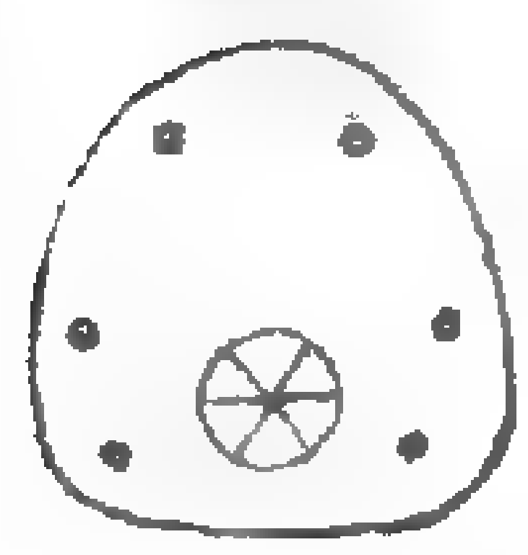
and the abundance of the horny rays *Peridinium tripos* (Ehr.) has made it evident that in this locality that Infusorium was one of the principal articles of its food. But in Antedons from other localities, I have found a more miscellaneous assemblage of alimentary particles; the most common recognizable forms being the horny casings of ENTOMOSTRACA or of the larvæ of higher CRUSTACEA." (Op. cit., p. 700).

The existence of large cilia within the intestinal canal, capable of producing a powerful indraught of water, renders any movement or concurrent action of the arms quite unnecessary in the ingestion of food. It does not matter, therefore, in what part of the body the mouth of a Crinoid may be situated, or how remote from the reach of the arms. Attached permanently to the bottom of the sea by their columns, the paleozoic Crinoidea, Cystidea and Blastoidea remained, while feeding, most probably motionless, drawing in streams of water through their mouths by the action of their intestinal cilia. The long tubular proboscis, with which many of the species are provided, would be, thus, analogous in function to the siphon of the acephalous mollusca. The indigestible particles would be, from time to time, thrown out through the mouth, just as a Star-fish or a Zoophyte frees itself of the refuse portions of its food, by casting it out of the same aperture through which it entered.

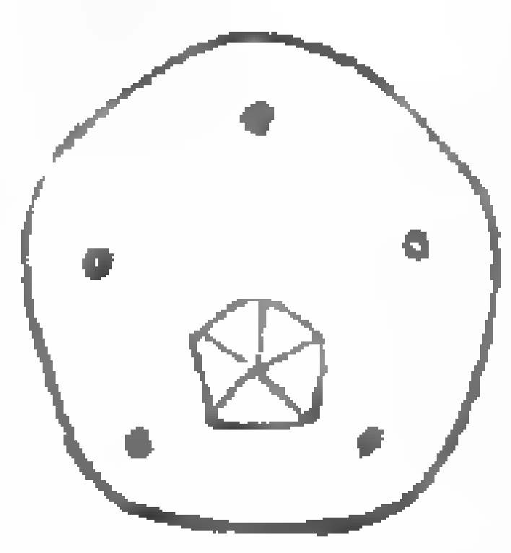
10. *On the Theory that the ambulacral and ovarian orifices are the oral apertures.*

Assuming that the four objections above noticed are sufficient to prove that the aperture which I call the mouth is not that organ, it is contended that the Cystidea, Blastoidea and Palæocrinidea ingested their food through their ambulacral and ovarian orifices. This appears to me in the highest degree improbable. In the recent Crinoids the grooves of the arms are occupied by four sets of tubes, which Dr. Carpenter calls the cœliac, the sub-tentacular, the ovarian and the tentacular canals. None of them communicate with the stomach. It is impossible that the most minute particle of food could gain access into the interior of the animal through any of them. The structure of the arms of the paleozoic Crinoids is such, that we must presume that their grooves were occupied by similar tubes, which passed through the ambulacral orifices into the perivisceral space. In the Cystidea and Blastoidea the respiratory organs were not situated in the grooves of the arms, and the ambulacral orifices were therefore only ovarian in their function. The improbability of their being also oral apertures is best shown by an illustration.

13.



14.



In fig. 13, is represented (natural size) the apertures of the smallest specimen of *Caryocrinus ornatus*, in our collection, selected for the present purpose because in the young of this species, the valvular orifice is larger in proportion to the size of the disc, than it is in the adult. It is in this specimen, about one-third of the whole width of the apical disc, while in a full grown *Caryocrinus* it is only one-ninth of the width. The same proportional size of the mouth according to age, occurs in *Antedon rosaceus.* The valvular mouth at first is as wide as the disc. But as the age of the animal increases the disc grows wider but the mouth does not. The ovarian pores in *Caryocrinus* are, however, as large in the small ones (once they make their appearance) as they are in those full grown. For recognizing these as ovarian pores we have the following reasons:—1. They are situated at the bases of the arms where the ovarian tubes must pass from the grooves into the perivisceral cavity. 2. When compared with the ovarian pores of a Sea-urchin they have the same size, form and aspect. Fig. 14, represents the ovarian pores of the Sea-urchin *Toxopneustes Drobachiensis* Ag. natural size and arrangement. It may not appear at first view that this latter comparison has any probative effect. But it has, in this way. If these apertures in *Caryocrinus* were large openings a line wide, as are some of the ambulacral orifices of the Crinoids, I would say that they were unlike true ovarian apertures.

According to the new theory, this Echinoderm *Caryocrinus ornatus* was a polystome animal, and drew in its food through its six ovarian apertures, the large valvular orifice being the anus. To me this appears to be utterly incredible.

In fig. 14 I have represented the mouth of *Leskia mirabilis* Gray. Both Dr. I. E. Gray and Prof. Lovén have pronounced this aperture to have the structure of the valvular orifice of the Cystidea. I have not the slightest doubt whatever but that the mouth of the Cystideans foreshadows that of the Sea-urchins. There is nothing whatever in its structure to show that it is not the mouth but on the contrary.

The new theory is not founded upon any peculiarities in the structure of the ambulacral orifices, which would show that they are oral apertures, but only upon the four objections above noticed. The first of these is not logical, while at the same time it is purely theoretical, and avails nothing against material and visible facts. The fourth is completely disposed of by Dr. Carpenter's observations, which prove that in the Crinoidea the arms have no share whatever in the ingestion of food. The second and third objections are the same in substance, i. e., according to the second the supply of water to the mouth, is diminished by the occurence of a *Platycerus* over it, while, according to the third, the same effect is produced by the small size of the aperture itself in some instances. It does not require much consideration to convince one, that if these two objections are fatal to my views, they are equally so to the opposite theory. In *C. stelliformis*, for instance, the pores through which we must suppose the ovarian tubes issued from the interior are only large enough to admit of the passage of a fine hair. They are scarcely visible to the naked eye. The tube, under any circumstances, must have filled them almost entirely. If any space at all were left for the passage of a stream of water through the pore by the side of the tube it must have been exceedingly minute.

When weighed as above, therefore, the evidence gives the following results :—The first and fourth objections avail nothing. The second and third militate against both theories. But when we take into account that in no instance, in the existing Echinodermata, where ovarian pores occur, are they at the same time oral orifices, the balance seems to be in favor of my view. This is all I desire to say upon the subject at present. Although I now firmly believe that the valvular orifice in the Cystidea, the larger lateral aperture of the Blastoidea, and the so-called proboscis of the paleozoic Crinoids are all oro-anal in function, yet I shall not maintain that view obstinately against good reason shown to the contrary.

ART. XXIV.—*Contributions to Chemistry from the Laboratory of the Lawrence Scientific School.* No. 12.

§ 1.

On the precipitation and determination of the metals of the magnesium group in the form of oxalates; by W. GOULD LEISON.

PROF. GIBBS has recently* called attention to the fact that a number of metallic oxyds may be completely precipitated from their neutral solutions by means of oxalic acid, provided that a large excess of alcohol be also added. As it is not easy to obtain precise quantitative results by igniting the oxalates so precipitated, in consequence of the extreme subdivision of the resulting oxyds, Prof. Gibbs suggested the employment of potassic hypermanganate for the combustion of the oxalic acid, a method which—as is well known—gives excellent results in the case of calcic oxalate precipitated in the ordinary manner. The following investigation was undertaken for the purpose of testing this method of analysis :—

* This Journal, xliv, p. 213.

Cadmium.—Cadmic sulphate was dissolved in the least possible quantity of water, oxalic acid added in excess, and then a large quantity of strong alcohol. The resulting oxalate was beautifully crystalline, and the precipitation was so complete that SH_2 gave in the filtrate a scarcely perceptible yellowish tinge. The oxalate was washed with alcohol by Bunsen's method and dried at 110° C., until every trace of alcohol was expelled. The filter was then pierced with a glass rod, and the cadmic oxalate washed into a flask with hot diluted sulphuric acid. A few cubic centimeters of strong sulphuric acid were then added, and the hot solution titrated with potassic hypermanganate. In this manner four experiments gave 44·19 pr. ct., 44·65 pr. ct., 44·88 pr. ct., and 44·27 pr. ct. of cadmium, as computed from the oxalic acid. These results are all much too high, and show that the acid had acted sensibly upon the filter. Two other experiments were then made. In the first a hot solution of ammonic sulphate was used as a solvent for the oxalate; in the second hot dilute chlorhydric acid was employed. Of the hypermanganic solution employed 100 c. c. contained 0·1103 gr. of available oxygen.

I. 0·4330 gr. cadmic sulphate required 24·5 c.c. hypermanganate = 43·68 pr. ct. Cd.

II. 0·3724 gr. cadmic sulphate required 21·1 c.c. hypermanganate = 43·74 pr. ct. Cd.

The received formula $3CdSO_4+8H_2O$ requires 43·75 pr. ct. In these two analyses the filters were not broken.

Barium.—Baric chlorid gave extremely variable results in my first experiment, notwithstanding the fact that the barium is completely precipitated by oxalic acid and alcohol. The resulting oxalate, after washing and drying, was not completely decomposed by sulphuric acid, which appeared to form a crust of baric sulphate upon the crystals of the oxalate. This difficulty was finally overcome by dissolving the baric oxalate in chlorhydric acid and diluting the solution largely. In this manner:

0·6505 gr. baric chlorid required 80 c.c. hypermanganate = 56·21 pr. ct. Ba (100 c.c. hypermanganate solution contained 0·053 gr. available oxygen). The formula $BaCl_2+2H_2O$, requires 56·15 pr. ct. Ba.

Strontium.—To avoid the use of paper filters so as to be able to employ sulphuric acid as a solvent, I resorted to sand filters. A light funnel was ground truly conical near the throat. A little pear of glass with a long stem was then dropped into the funnel, stem upward. In this manner a valve was formed impassible to the sand laid upon the ball of the glass, but allowing liquids to pass freely. By means of the stem the valve could be lifted from its seat, and the sand and precipitate washed together into a flask, after careful drying. With this arrangement:

0·4292 gr. strontic nitrate required 47·8 c.c. hypermanganate = 48·90 pr. ct. SrO.

0·3657 gr. strontic nitrate required 40·8 c.c. hypermanganate = 48·90 pr. ct. SrO. (100 c.c. hypermanganate solution contained 0·1099 gr. available oxygen.)

The formula $Sr(NO_3)_2$ requires 48·93 pr. ct. SrO. Sulphuric acid only was used to decompose the oxalate.

Calcium.—Iceland spar was dissolved in chlorhydric acid, and the solution treated with oxalic acid and alcohol. The filtrate contained calcium. When, however, the solution was evaporated to dryness before adding alcohol, and the oxalate was washed on a sand filter, no traces of calcium could be detected in the filtrate. In this manner:

0·5090 gr. $CaCO_3$ required 70·6 c.c. hypermanganate = 56·10 pr. ct. CaO. (100 c.c. hypermanganate corresponded to 0·11559 gr. oxygen.)

0·5590 gr. $CaCO_3$ required 77·5 c.c. hypermanganate = 56·08 pr. ct. CaO. (100 c.c. hypermanganate corresponded to 0·11495 gr. oxygen.)

The formula requires 56·00 pr. ct. CaO. Sulphuric acid only was employed.

Magnesium.—When magnesic sulphate is treated with oxalic acid, the mixture evaporated, but not to dryness, and alcohol added, the filtrate is perfectly free from magnesium. In this manner:

0·3243 gr. $MgSO_4 + 7H_2O$ required 39·6 c.c. hypermanganate = 16·18 pr. ct. MgO.

0·3949 gr. $MgSO_4 + 7H_2O$ required 48·4 c.c. hypermanganate = 16·25 pr. ct. MgO.

In these analyses the oxalate was collected on a paper filter and washed into the flask with water after piercing the filter, which was washed with cold dilute sulphuric acid. The formula requires 16·26 pr. ct.

Zinc.—Zinc is completely thrown down from its sulphate by the unmodified process. The oxalate forms an extremely fine powder. A sand filter and warm dilute sulphuric acid were employed.

0·9301 gr. sulphate required 47·1 c.c. hypermanganate = 28·14 pr. ct. ZnO.

1·0788 gr. sulphate required 54·6 c.c. hypermanganate = 28·15 pr. ct. ZnO.

The formula requires 28·22 pr. ct. ZnO.

Cobalt.—Perfectly pure anhydrous cobaltous chlorid was prepared by igniting chlorid of purpureo-cobalt. The chlorid was then precipitated by oxalic acid and alcohol, collected on a sand filter and digested with dilute sulphuric acid. The solution was intensely red. A solution of nickelous sulphate was then added, until the red color disappeared and a faint smoky hue took its place.* In this manner:

0·4292 gr. $CoCl_2$ required 47·8 c.c. hypermanganate = 45·30 pr. ct. Co.

0·3657 gr. $CoCl_2$ required 40·8 c.c. hypermanganate = 45·37 pr. ct. Co.

The formula requires 45·38 pr. ct. (Co = 59).

Nickel.—In the case of nickelous sulphate it was found necessary, after adding the oxalic acid, to concentrate the mixture on a water bath before adding alcohol, and then further digest for about half an hour, replacing the alcohol as fast as it evaporated. The oxalate was collected on a paper filter, and, after washing, dissolved in ammonia on the filter. The filtrate was then acidified with sulphuric acid, and the color finally discharged by a solution of cobaltous sulphate. In this manner:

0·9585 gr. nickelous sulphate required 42·2 c.c. hypermanganate = 28·57 pr. ct. NiO.

1·0287 gr. nickelous sulphate required 45·3 c.c. hypermanganate = 28·58 pr. ct. NiO.

The formula $NiSO_4 + 6H_2O$ requires 28·24 pr. ct. NiO (Ni = 58), but it is very difficult to obtain the sulphate in a perfectly definite state of hydration.

Manganese.—Although manganese is completely precipitated from its soluble salts by oxalic acid in the presence of alcohol, my results with the method have not been satisfactory, owing as I suppose to my not having a definite salt for analysis. The following analyses show at any rate that closely corresponding results can be obtained when the same substance is taken:

0·3760 gr. manganous oxalate required 30·50 c.c. hypermanganate = 38·38 pr. ct. MnO.

0·4013 gr. manganous oxalate required 32·55 c.c. hypermanganate = 38·38 pr. ct MnO.

The salt $2C_2MnO_4 + 5H_2O$ when dried in air requires 37·77 pr. ct. MnO, while the salt analyzed was dried at 100° C. In like manner two analyses of a sulphate, which had probably absorbed a little water, gave 45·28 pr. ct. and 45·29 pr. ct. of MnO. The crystallized sulphate $MnSO_4 + 7H_2O$ requires 46·67 pr. ct.

---

* Compare, as regards this method, W. Gibbs, in this Journal, vol. xiv, p. 204.

Iron.—Good results could not be obtained by the application of this method to the determination of iron, but I am not at present able to assign the reason of the failure in this case.

To complete my work it remains for me to point out the applicability of the process to the determination of the whole quantity of oxygen contained in a number of bases present together in solution—a problem which is sometimes of interest. Sulphates of Manganese, Magnesium, Nickel, Cobalt, Cadmium and Zinc in undetermined quantities were dissolved together in water, and the solution well shaken. Four portions, of 100 c.c. each, were then taken, and the acid determined in each by baric chlorid. In two other equal portions the bases were precipitated as oxalates and titrated as above. The oxygen in the acid was then calculated from the amonnt of baric sulphate. In this manner it was found that the oxygen in the acid was to that in the mixed bases as 3 to 1 very nearly, the precise ratio being in the one case as 0·054 is to 0·018 and in the other as 0·055 is to 0·018. Another experiment was made with a crystallized dolomite containing 0·45 pr. ct. of insoluble residue. The lime, magnesia and iron were precipitated together, and titrated as oxalates; the carbonic acid was determined by ignition. In this manner the oxygen of the acid was found to be to the oxygen in the bases as 34·48 is to 17·28. The bases after ignition amounted to 52·41 pr. ct.

In another experiment with a dolomite from a different locality containing 0·13 pr. ct. of insoluble residue, the oxygen ratio was found to be as 34·56 is to 17·28, the bases amounting to 52·33 pr. ct. If we calculate the relative quantities of calcic and magnesic carbonates from the sum of the two oxyds in the last analysis and the oxygen found by titrating the oxalate, we find:

$MgCO_3$	42·77
$CaCO_3$	57·07
Insol. residue	0·13
	99·97

the small quantity of ferrous oxyd present being neglected. This analysis will serve to show the applicability of the method in indirect analyses. My results appear to me to furnish a generalization of the use of potassic hypermanganate, which will be received with favor by those who employ this reagent frequently in volumetric analyses.

§ 2.

On new analytical processes; by J. H. TALBOTT.

1. *On the precipitation of zinc and manganese as sulphids.*—Zinc is thrown down from cold solution by an alkaline sulphid in the form of a slimy mass which settles slowly and is ex-

tremely difficult to wash. The precipitation is, however, more complete than when sodic carbonate is used, and may be rendered very easy and rapid by the following process: The solution of zinc, if acid, is to be neutralized as nearly as possible by sodic or ammonic carbonate. To the boiling solution sodic or ammonic sulphid is to be added, a large excess being very carefully avoided. The white precipitate, on continued boiling, soon becomes granular, and settles readily. The supernatant, clear liquid is then to be tested with a drop of the alkaline sulphid, to be sure of complete precipitation, and the sulphid then washed with hot water by Bunsen's method. The filtrate is perfectly clear, and absolutely free from zinc; the washing is easy and rapid. The sulphid of zinc is then to be partially dried with the filter, in the manner recommended by Bunsen, brought into a porcelain crucible and ignited, at first gently, and afterward strongly with free access of air. The expulsion of the last traces of sulphuric acid is much facilitated by occasionally dropping fragments of ammonic carbonate into the crucible. Pure ZnO finally remains, the ignition being continued until a constant weight is obtained. In this manner the following results were obtained:

0·3216 gr. pure ZnO gave 0·3216 gr. = 100·00 pr. ct.
0·3208 gr. " " " 0·3209 gr. = 100·03 pr. ct.
0·2412 gr. " " " 0·2410 gr. = 99·91 pr. ct.
0·1785 gr. " " " 0·1784 gr. = 99·94 pr. ct.

In zincic sulphate, which had probably lost a little water:

0·6485 gr. gave 0·1851 gr. ZnO = 28·54 pr. ct.
0·6510 gr. " 0·1858 gr. " = 28·54 pr. ct.
0·8198 gr. " 0·2338 gr. " = 28·52 pr. ct.

The formula requires 28·29 pr. ct. ZnO. The advantages of this process over the older methods of precipitating in the cold are, I think, very evident, even if only the saving of time be taken into consideration.

Manganese may be precipitated completely from its boiling solutions by precisely the same process. The flesh-colored sulphid is granular and sometimes even sandy, though not distinctly crystalline, and may be washed with the utmost facility. The precipitated sulphid, after washing upon a filter, is to be redissolved in chlorhydric acid, and precipitated as ammonio-phosphate in the manner proposed by Prof. Gibbs. To test the method with a perfectly definite salt of manganese, manganous pyrophosphate was selected, dissolved in dilute chlorhydric acid, and the solution nearly neutralized by sodic carbonate. To the boiling solution sodic sulphid was then added, and the manganese finally weighed—in one analysis as pyrophosphate, in another as anhydrous sulphid by ignition in a currrent of SH_2. In this manner:

0·3132 gr. $Mn_2P_2O_7$ gave 0·3126 gr. $Mn_2P_2O_7$ = 49·56 pr. ct. MnO.

0·3786 gr. $Mn_2P_2O_7$ gave 0·2310 gr. MnS = 49·65 pr. ct. MnO.

The formula requires 49·64 pr. ct. MnO. It is perhaps worthy of notice that ammonic sulphid does not completely decompose manganous pyrophosphate under the circumstances above described. The greater portion of the salt is precipitated at once as crystalline ammonio-phosphate of manganese.

2. *On the quantitative separation of tin and tungsten.*

The quantitative separation of tin from tungsten has always been regarded as a difficult problem not hitherto solved in a satisfactory manner. The following method will, I think, be found to leave nothing to be desired as respects both ease and accuracy. It depends upon the fact that stannic oxyd, SnO_2, is reduced by potassic cyanid with great facility, while tungstic acid, WO_3, undergoes no reduction, even when heated with the cyanid to a high temperature. The oxyds of tin and tungsten are to be heated in a porcelain crucible with 3 or 4 times their weight of commercial potassic cyanid, previously fused, pulverized, and thoroughly mixed with the two oxyds. The mass is kept fused for a short time, when the tin separates in the form of metallic globules, while the tungstic acid unites with the alkali of the potassic cyanate and carbonate present. After cooling, the mass is to be treated with hot water, which dissolves the alkaline tungstate and other salts, and leaves the tin as metal. This is to be filtered off, washed, dried and weighed as stannic oxyd after oxydation in the crucible with nitric acid. The tungstic acid is most conveniently estimated by the difference, but may of course be precipitated by mercurous nitrate, after boiling the solution with nitric acid to decompose the excess of potassic cyanid present, and then redissolving the precipitated tungstic acid by means of an alkali. To test the method, weighed portions of pure stannic and tungstic oxyds were mixed and treated as above:

0·6662 gr. SnO_2 and 0·5880 gr. WO_3 gave 0·6679 gr. SnO_2 = 53·24 pr. ct. The calculated percentage of stannic oxyd is here 53·11.

0·7098 gr. SnO_2 and 0·5460 gr. WO_3 gave 0·7096 gr. SnO_2 = 56·51 pr. ct., the calculated percentage being 56·52.

0·5378 gr. SnO_2 and 0·4373 gr. WO_3 gave 0·5405 gr. SnO_2 = 55·43 pr. ct., the calculated percentage being 55·15.

0·5073 gr. SnO_2 and 0·4334 gr. WO_3 gave 0·5081 gr. SnO_2 and 0·4349 gr. WO_3. This corresponds to

	Found.	Calculated.
Stannic oxyd,	54·01	53·92
Tungstic oxyd,	46·23	46·08
	100·24	100·00

As it might perhaps be objected to the examples given above that I employed only purely mechanical mixtures of stannic and tungstic acids, and that this is not the case which occurs in practice, I made the following additional analyses: Portions of the two metallic oxyds were fused in a silver crucible with pure sodic hydrate; the fused mass was then dissolved in water and the two oxyds precipitated together from the solution by nitric acid with the usual precautions. The ignited mixed oxyds were then fused with potassic cyanid, as above. In this manner:

0·7292 gr. of a mixture of SnO_2 and WO_3 gave 0·4211 gr. SnO_2 = 57·74 pr. ct.

0·9826 gr. of the same gave 0·5661 gr. SnO_2 = 57·61 pr. ct.

Tin cannot be separated from molybdenum by fusing the mixed oxyds with potassic cyanid, as the molybdic acid is always more or less completely reduced to a lower oxyd.

§ 3.

On the treatment of gelatinous precipitates; by THOMAS M. CHATARD.

The inconveniences and loss of time which attend the washing of gelatinous precipitates are familiar to all chemists. Even the methods of washing recently introduced by Bunsen are not always perfectly satisfactory in their operation when applied to this class of substances. The following method will be found, I think, to give results which leave nothing to be desired: The solution containing the substance to be determined is to be simply evaporated to perfect dryness with a small excess of the precipitant, and the gelatinous mass stirred with a rod until it becomes a perfectly dry powder. In this manner the precipitate diminishes extremely in bulk, and may then be washed with the greatest ease upon the filter. The evaporation is usually effected with sufficient rapidity on a water bath. The following analyses will be sufficient to show the degree of precision attainable by this process in the cases of some of the more familiar precipitates: Weighed portions of potassic dichromate were dissolved in very small portions of water, reduced with chlorhydric acid and alcohol, the excess of alcohol expelled, and ammonia added in excess. After evaporation, in the manner above described, the chromic sesquioxyd presented a greenish blue granular powder very easily washed.

0·7782 gr. $K_2Cr_2O_7$ gave 0·4023 gr. Cr_2O_3 = 68·02 pr. ct. chromic acid.
1·5646 gr. $K_2Cr_2O_7$ gave 0·8102 gr. Cr_2O_3 = 68·13 pr. ct. chromic acid.

The formula requires 68·04 pr. ct.

Alumina treated in the same manner is also very easy to wash.

2·4097 gr. potassic alum gave 0·2626 gr. Al_2O_3 = 10·89 pr. ct.
1·9571 gr. " " " 0·2130 gr. " = 10·88 pr. ct.

The formula requires 10·86 pr. ct.

The process applies with almost equal advantage to iron. Weighed portions of ammonio-ferrous sulphate were dissolved in water, and sodic chlorid added in large excess to furnish solid matter to be washed out. The iron was then oxydized with nitric acid, precipitated by ammonia, and evaporated as above.

1·5824 gr. gave 0·3229 gr. Fe_2O_3 = 14·28 pr. ct. Iron.
1·4840 gr. gave 0·3019 gr. " = 14·24 pr. ct. Iron.

The formula requires 14·29 pr. ct.

Nickelous carbonate also loses its gelatinous character when treated as above. 0·2201 gr. metallic nickel gave, after solution, precipitation as carbonate, and reduction by hydrogen, 0·2199 gr. metallic nickel = 99·91 pr. ct. of the quantity taken. Cobaltous carbonate may be treated in the same manner, but the alkali cannot be completely washed out, and the method is in this case not to be recommended.

It seems at least extremely probable that other gelatinous oxyds and hydrates will give equally good results when treated in the manner which I have described.

§ 4.

On the precipitation of antimonous sulphid from boiling solutions;

by STEPHEN P. SHARPLES.

In the precipitation of antimonous sulphid I have found it of very great advantage to employ the following process: Into the solution, containing as usual tartaric and free chlorhydric acid, a current of sulphydric acid gas is to be passed, the liquid being, during the passage of the gas, gradually heated to the boiling point. The boiling is then to be continued for 15 or 20 minutes, the current of gas passing uninterruptedly, until the voluminous sulphid has become a dense granular powder, occupying but a small portion of the original volume of the sulphid. The sulphid may then be washed with great facility, and dried upon a sand filter at 200°–300° C. All the determinations of antimony made in this Laboratory for some years have been executed in this manner, the results leaving nothing to be desired. Arsenous sulphid does not become granular and dense under the same circumstances. In this connection I may be permitted to mention that the sulphids of nickel and cobalt, when precipitated from boiling solutions in the manner recommended by Prof. Gibbs some years since, should be filtered off, and washed immediately after precipitation. In this manner there is no oxydation upon the filter, even during the drying of the precipitate. But if the sulphids are allowed to stand in the solution from which they have been precipitated, for even a few hours, they will usually oxydize upon the filter during the washing.

§ 5.

On the introduction of the principle of repetition into chemical analysis; by Bryant Godwin.

The method of repetition, so frequently and so advantageously employed in physical investigations, has not, so far as I am aware, been applied to chemical analysis. It seems at least desirable that it should be so applied, and I will here give a particular instance in which it may be employed with advantage. In the determination of iron by means of potassic hypermanganate, all the iron is, at the end of the operation, in the form of sesquioxyd, while there is also a very small excess of unreduced hypermanganate. When the solution is boiled for a short time with pure zinc-dust and then filtered through a ribbed filter, which is quickly washed with water previously boiled to expel air, the iron is wholly in the form of ferrous oxyd, and the process of titration may be repeated a second time. After a new reduction the iron may again be determined, and this process may be repeated until the volume of liquid becomes too large to be easily handled. The following analyses were made to test this process:

0·4725 gr. ammonio-ferrous sulphate required in 5 successive titrations 49·0, 47·2, 48·7, 48·0, 48·5 cubic centimeters of potassic hypermanganate, 1 c.c. corresponding to 0·0014 gr. iron. The mean of these 5 determinations gives 14·31 pr. ct. iron in the salt.

0·4888 gr. required in 7 successive titrations 49·5, 48·75, 50·5, 49·8, 49·7, 49·8, 49·5 c.c. of hypermanganate, the mean of which gives 14·23 pr. ct. iron.

The formula requires 14·27 pr. ct.

These analyses, which with more practice and experience on my part would doubtless have corresponded much more closely, will at least serve to show that the principle of repeated observations of the same quantity to be measured may sometimes be introduced into chemical analysis.

ART. XXVI.—*On the Corona seen in Total Eclipses of the Sun;* by Professor W. A. NORTON.

CERTAIN observations made on the total eclipse of the sun of August 7, 1869, have led some of the observers to the conclusion that the corona seen on that occasion, and in previous eclipses, is of the nature of a *Solar Aurora.* It is proper that it should be publicly stated that this theory is not a new one. It has been advocated for several years by the author of the present communication, both in publications and in public lectures. It is essentially involved in the explanation of the Zodiacal Light, propounded in his Treatise on Astronomy, 2nd edit. (1845); and in the theoretical views set forth in a note appended to the discussion of the topic of Terrestrial Magnetism, in a memoir on Molecular Physics, published in this Journal (1864–6).* It is distinctly presented in the last edition of the Treatise on Astronomy (1867), pp. 172, 174, 175, and 178. I propose, in a communication to the next No. of this Journal, to state the principal grounds upon which I have maintained the auroral origin of the corona in different publications, as well as give the results of my own observations on the eclipse of 1869, and of those of other observers of that and previous total eclipses, which lend a powerful support to the auroral theory of the corona.

This introductory notice is now published mainly with the view of calling, at an early day, the attention of astronomers who may observe the eclipse of December next, to the importance of noting the exact positions, with respect to the plane of the sun's equator, of the more prominent portions of the corona. From two to four points of special outstreaming have been observed in different eclipses. In the eclipse of last year the more conspicuous extensions of the corona were nearly in the plane of the sun's equator, and from the vicinity of the poles. The figure of the corona, accompanying the Report of P. Prof. Capelotti of observations on the eclipse of April 15, 1865, made at Chili, shows the same to have been the case in that eclipse. The delineations of the corona as seen in other eclipses, so far as I have been able to ascertain, fail to give any accurate indication of the positions of the more prominent parts from the absence of all lines of reference in the drawings. It is to be hoped that observers of subsequent eclipses will take the precaution to ascertain these positions, and note them in their Reports. If they really have any general uniformity, in different eclipses, the fact cannot fail to throw light on the origin and nature of the corona.

* See this Journal, vol. xli, No. 121, pp. 76, 77.

ART. XXVII.—*Observations on Prehistoric Archæology in Greece;* by GEORGE FINLAY, LL.D.*

A FEW objects belonging to the stone period were observed in Greece before it was known that they are relics of the people who inhabited the country in prehistoric times. The pieces of obsidian, generally called flint by travelers, that were picked up on the tumulus of Marathon, were termed Persian arrow-heads. This arose from a strange misapplication of the mention of stone arrow-heads having been employed by the Ethiopians in the army of Xerxes by Herodotus (Polymnia, vii, 69), who says they used short arrows of reeds pointed with a stone with which they engraved their signets. But why Datis, who can hardly have had any Ethiopians in his army, thought it expedient to bring to Marathon immense quantities of stone arrow-heads has not been explained. They do not appear to have been likely to prove efficient missiles against Athenian hoplites. Yet a sagacious traveler like Dodwell, in 1805, says that he found "a great many small arrow-heads of black flint, which probably belonged to the Persian army."† Even Colonel Leske, the ablest and most observant of modern travelers, was misled by this opinion. He says, "while I was employed on the summit of the *Soros*, as the tumulus of the Athenians is called, my servant amused himself in gathering at the foot of the barrow, a great number of pieces of black flint which happened to strike his observation. These flints are so numerous, and have been so evidently chipped by art into their present form like gun flints, that there is good reason for believing them to have been the heads of arrows discharged by the Persians who fought at Marathon, and to have been interred with the Athenians after having been gathered from every part of the plain after the battle. Herodotus shows that some of the barbarians were armed in this manner, though his remark is applied not to the army of Darius but to that of Xerxes. Flint of this kind, if produced in any of the adjacent parts of Greece is at least very rare." (*Travels in Northern Greece*, vol. ii, p. 431). The great quantity and small size of the fragments found in the tumulus of Marathon caused the writer of these pages to doubt the possibility of these fragments having anything to do with the Persians, for such feeble weapons as they could form must have been useless against the panoply of the Greek infantry of the period. Had they been employed the glory of Marathon would be a vain boast. Sir William

---

* From a letter to Mr. H. T. DeForest, dated Athens, Feb. 26, 1870, communicated by him for this Journal.

† A classical and topographical tour through Greece during the years 1801, 1805 and 1806, by Edward Dodwell, Esq., vol. ii, p. 159.

Gell, in his *Itinerary of Greece*, page 166, mentions that similar fragments of flint are found at the *σχίστη ὁδὸς*, where was the tomb of Laius, and he adds "perhaps a confirmation of the discomfiture of the barbarians in the *Odos schiste*." These fragments of obsidian, wherever they are found in Greece, are now admitted to be relics of prehistoric times, and a careful examination of the tumulus of Marathon convinced the writer of these pages, as early as the year 1835, that they were scattered about in the soil in their actual state when it was heaped up to form the tumulus over the bodies of the Athenians who were slain in the battle. The material is obsidian from the island of Melos. * * * *

No traditions of the existence of a stone period appear to have reached the inhabitants of Greece in historic times, though the mythical history of the remains of Tyrinths and Lykosura ascend almost to the prehistoric ages. As I have already mentioned, my attention was first called to the certainty that a numerous race of people in Greece used stone implements by the fragments of obsidian picked up on the tumulus at Marathon. I subsequently observed that similar fragments of obsidian are found in various parts of the neighborhood in the rear of the Greek position, and far out of reach of the arrows of the Persians. I also found myself similar chips of obsidian over all Attica, and in many parts of Greece, and several of the islands of the Archipelago, where no native obsidian can ever have existed, which I visited after my attention was directed to the subject. I have picked up these so-called Persian arrow-heads even in the now barren island of Hydra. My first notice on the subject was published in the year 1836. In that year, while examining the topography of Attica I discovered the extensive deposit of fossil bones at Pikermi, of which there is a valuable collection in the Museum of Natural History at Athens. A detailed description of these remarkable fossil bones was published by Professor Roth of Munich, in the Transactions of the Royal Academy of Bavaria, and they have since been described in the splendid work of Monsieur A. Gaudry, *Animaux fossiles et Geologie de l'Attique.* In a notice of the discovery which I read at a meeting of the Society of Natural History of Athens on 13th December, 1836, I observed that I had picked up fragments of obsidian, called Persian arrow-heads, not only on the tumulus of Marathon but also at Liosia near Aphidna, at Kakosialesi near Tanagra, and at Aghios Kosmas on the Attic coast. When my memoir on the battle of Marathon, which was read to the Royal Society of Literature in January, 1838, was printed, I added a note "concerning the pieces of flint called Persian arrow-heads found in the tumulus at Marathon." (Transactions of the Royal Society of Literature, first series, 4to, vol. iii, p. 392).

I have since collected several specimens of stone implements, particularly celts, that is, axes and chisels very similar in form to those found in the lake dwellings of Switzerland. I have obtained six specimens of jade, one of which I have presented to the Museum, with a similar piece which I procured at Robenhausen. I have also a fine small axe of nephrite, and several others of extremely hard stones. Unfortunately it has not yet been in my power to ascertain the precise mineralogical character of the most remarkable of my specimens.

I have obtained several fine specimens from Dobrena (the site of the ancient Thisbe) where a lake must have existed in prehistoric times, and where there is still a marsh. Two small axes found at Orchomenas, near the lake Copaïs, were given to me by Mr. Merlin, Her Majesty's Consul for Northern Greece.

Lake dwellings continued to exist in Macedonia down to the time of Herodotus. His description (Terpsichore, v, 16) proves that the dwellings of the Pæonians, in the Lake Prasias, were very similar to those constructed on the lakes of Switzerland. "They who dwell on the lake Prasias construct their dwellings in this manner. They fix strong piles in the lake, and on these piles they fasten planks, making a bridge with a narrow entrance from the land. The piles supporting the planks were in former times fixed by the inhabitants in common, but afterward the law established that every one who married a wife (and they take many wives), should bring down from Mount Orbelos three piles and fix them in the lake. The manner of their dwellings is in this fashion. Each man has his own hut on the piles, and a trap door through the flooring by which he can descend to the lake. The young children are tied by the foot lest they should fall into the water." The lake Prasias is the lake of the Strymon of Thucydides, v, 7, the Kerkinites of Arrian, Anab. 1, 11, 3, and is now called Tachynós, from a village on its western side. The fisheries are still valuable as they were in ancient times, and the fish caught for sale are principally carp, tench and eels. (Leake, Travels in Northern Greece, vol. iii, 198).

The description which Herodotus gives of these lake dwellings makes it an object of the greatest importance to the archælogists of Switzerland and Greece that the lake Prasias and other lakes in Macedonia, Thessaly and Greece, should be carefully examined in order to ascertain whether any traces can still be discovered of lake dwellings. Some traces of the piles on which dwellings were constructed are said to have been observed in the lake Prasias, in 1862, by Monsieur Deville of *l'Ecole française d'Athénes.* But a superficial examination might easily lead to considering stakes for nets or fishing huts as remains of ancient pile dwellings, and a searching examination of the locality ought to be undertaken by skillful and experienced observers.

The lakes in Greece that deserve particular attention are, the lake Copais, Hylica or Livadi, and Paralimni in Bœotia, the lake Trichonis with its connected lake Hyrie in Etalia, the lakes in Acarnania and the lagoons between the mouths of the Evenus and the Achelous. In the Peloponnesus there are, the lakes of Pheneus (which becomes in alternate periods of years a deep lake as at present, and a plain that dries in summers as it was in the year 1821), and Stymphalus, with their physical peculiarities and mythical associations running back toward a prehistoric period. The lake of Orchomenos, the marsh of Mantinea, the lakes near Tegea, the lagoons at the mouths of the Eurotas and the Alpheus, and the marsh at Pylos, (Palæo Avarino) all these places, and some others that might be pointed out, offer an extensive field for research. It may also be possible to identify sites of prehistoric habitations in the mountains, from the remains found in their vicinity. Such positions would have been selected because they were easily defensible by men having weapons of stone only. They must have commanded access to an abundant supply of water equally capable of defense. I have observed such a position overlooking the plain of Aphidna, where I have picked up a considerable quantity of fragments of obsidian and flint artificially worked.

Pliny contains several passages in his Natural History that refer to stone axes and chisels (celts), with particular reference to those found in Greece, for he quotes Greek authorities about them. He speaks of *Ceraunicæ* (thunderbolts) as being, according to the testimony of Sotacus (an ancient Greek writer on minerals), black and red, and resembling axe-heads in shape. I have specimens of red celts from Eubœa made of red ironstone, one $3\frac{3}{4}$ in. long and 2 in. broad; and several that are black, of the same size but narrower.

The stone period has been divided into a paleolithic and a neolithic period. In western Europe, particularly in France and England, numerous remains of stone implements of the paleolithic period have been found in strata with the bones of the mammoth and other extinct animals. But I am not aware that any stone implements that can be attributed with any certainty to this period, have yet been discovered in Greece, though bones of these animals have been found in several places in great quantities both in Attica, Eubœa and Arcadia. All the stone implements that have fallen under my notice consist of specimens that belong to the neolithic or polished-stone period, and many display considerable skill in their workmanship, being composed of the hardest stones.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the theory of the Bunsen flame.*—The non-luminosity of the flame of the Bunsen gas-burner is commonly ascribed to the more complete combustion of the gas by the air which is mixed with it before it is burned. But this is an assumption entirely unproved as yet; and moreover, the positive experiments of KNAPP show that this cannot be the only cause of the phenomenon. His burner was so constructed that by means of a lateral tube, other gases beside air could be mixed with the coal-gas; and the results obtained prove that nitrogen, hydrochloric acid, or carbon dioxyd gas causes the flame to burn fully as blue as is seen in the burner as ordinarily used. The experiment is most easily tried with carbon dioxyd gas; but with this gas it might be urged that dissociation into carbonous oxyd and oxygen takes place, and that the latter gas assists the combustion. Though this is hardly conceivable at the low temperature there present, the assumption may be entirely disproved by using in the experiment only pure nitrogen; which for this purpose may be conveniently obtained by boiling a solution containing equivalent quantities of potassium nitrite and ammonium chlorid. The explanation of this result is not easy; Knapp believes that the disappearance of the luminosity is due partly to the cooling of the flame, but principally to the dilution of the illuminating gas; and that the flame of the Bunsen burner is non-luminous from the same cause which lessens the light of a candle burned in vacuo, or at high altitudes.—*J. Pr. Ch.*, II, i, 428, June, 1870. G. F. B.

2. *Production of Ozone in Rapid Combustion.*—The observation of Loew upon the production of ozone in quick combustion, published in the May number of this Journal, seems to have been anticipated not only by Pincus (noticed in vol. xlvii, page 238,) but much earlier—in December, 1864—by THAN, and communicated to the Hungarian Academy at Pesth. Becoming interested in the application to vegetable physiology of the fact that ammonium nitrite is formed when hydrogen burns in an atmosphere containing nitrogen, Than sought for a method of exhibiting the result as a lecture-experiment; for this purpose he drew the air which closely surrounds the flame of a Bunsen burner through a Varrentrapp and Will's bulb-apparatus, containing an acidulated solution of potassium iodid and starch. The air which was drawn from about the lower $\frac{2}{3}$ of the flame, colored the solution blue very rapidly. As the odor of ozone was not detectable in this air after it had passed through the apparatus, Than substituted a bulb-tube filled with water; and then on drawing through this a rapid current of the air from this part of the flame, the odor of ozone was distinctly obtained. Were ammonium nitrite formed at the same time, it would be retained by the water; and this, on adding a

few drops of the acidulated potassium iodid solution, would become at once blue. But no blueing took place, even when the air was passed through the water for 15 or 20 minutes. The blueing in the former case must be due, therefore, entirely to ozone. Than next investigated the conditions of this ozone-production. He found that a small quantity of ozone is present in the air which surrounds the lower portion of every flame composed of burning hydrogen; that this ozone may be detected by withdrawing this air rapidly from the flame through a glass tube drawn out to a fine point, when it gives its characteristic odor and reactions; that the point of the tube should not exceed a millimeter in diameter, and that it should be placed in the lower half of the flame where the flame comes in contact with the cold air. In a candle-flame, or that of an alcohol-lamp, most ozone is found near the lower blue portion. The current of air must be rapid enough to deflect the flame, yet not so rapid as to draw in any unburned gas. By the ozone thus obtained, the solution of potassium iodid and starch was blued in a few seconds, a mixture of manganous sulphate solution with a few drops of potassium hydrate became dark brown from the formation of the peroxyd, and when inhaled, the mucous membrane was irritated and a transient catarrh produced. No ozone could be detected in the air surrounding ignited charcoal; and all attempts to determine it in the other cases quantitatively, failed. Than then goes on to say: "Wenn man durch das obere Dritttheil einer nicht leuchtenden Gasflamme, durch ein schief aufwärts gerichtete zwei Linien weite Glasröhre, einen sehr kräftigen Luftstrom mittelst eines Blasebalges durchtreibt, so kann man in der durchgeblasenen Luft nicht unbedeutende Mengen von Ozon theils durch den Geruch, theils durch Jodkalium-Stärkepapier nachweisen;" an experiment almost precisely that mentioned by Loew. In Than's view, the diatomic molecule of oxygen in the air, when it comes in contact with the hydrogen of the flame, gives up one atom, to form water, thus: $H_2+O_2=H_2O+O$; the other atom thus set free, unites with another molecule of oxygen to form ozone: $O_2+O=O_3$. No ozone is formed in the vicinity of the burning carbon, because each of its atoms unites directly with an entire molecule of oxygen, $C+O_2=CO_2$, producing carbon dioxyd.—*J. Pr. Ch.*, II, i, 415, June, 1870. G. F. B.

3. *On the Constitution of Narcotine and its decomposition-products.*—MATTHIESSEN thus sums up the results of his extended research upon Narcotine:—

(1.) The analysis of various specimens of narcotine, derived from various sources, has shown that it always has the same composition, $\text{Є}_{22}H_{23}N\Theta_7$.

(2.) As already noticed by previous observers, narcotine, by the action of oxydizing agents, splits up into opianic acid and cotarnin:

$$\text{Є}_{22}H_{23}N\Theta_7+\Theta=\text{Є}_{10}H_{10}\Theta_5+\text{Є}_{12}H_{13}N\Theta_3.$$

(3.) Heated alone, to a temperature somewhat above 200°, or for a somewhat longer time with water, narcotine splits up into meconin and cotarnin:

$$Ꞓ_{22}H_{23}NΘ_7=Ꞓ_{10}H_{10}Θ_4+Ꞓ_{12}H_{13}NΘ_3.$$

(4.) Heated for a short time (about two hours) with an excess of hydrochloric acid, methyl chlorid is formed, and one atom of hydrogen replaces the methyl in the narcotine; by a longer heating (for some days) two atoms of H may replace two of $ꞒH_3$; heated with fuming hydriodic acid, three atoms of the methyl are replaced by three of hydrogen; thus forming a series of homologous bases, whose decompositions are analogous to those of narcotine itself.

(5.) The formula of cotarnin has been shown to be $Ꞓ_{12}H_{13}NΘ_3$, and not $Ꞓ_{13}H_{13}NΘ_3$; it is capable of crystallizing with one-half and with a whole molecule of water.

(6.) Cotarnin, when heated with dilute nitric acid under certain, not yet clearly-defined conditions, is decomposed, yielding cotarnic acid and methylamine:

$$Ꞓ_{12}H_{13}NΘ_3+2H_2Θ=Ꞓ_{11}H_{12}Θ_5+ꞒH_5N.$$

By the use of concentrated nitric acid, as already noticed by former observers, apophyllic acid is formed. The action of other oxydizing agents gives results not yet studied.

(7.) Cotarnin heated with concentrated hydrochloric acid, yields methyl chlorid and the hydrochlorate of cotarnamic acid:

$$Ꞓ_{12}H_{13}NΘ_3+H_2Θ+2HCl=ꞒH_3Cl+Ꞓ_{11}H_{13}NΘ_4, HCl.$$

Hydriodic acid produces a similar result; only one atom of $ꞒH_3$ being eliminated from each molecule of cotarnin.

(8.) Opianic acid, by the action of nascent hydrogen (as by treatment with sodium-amalgam or by zinc and sulphuric acid) is reduced to meconin:

$$Ꞓ_{10}H_{10}Θ_5+H_2=H_2Θ+Ꞓ_{10}H_{10}Θ_4.$$

(9.) Opianic acid by heating with potassium dichromate and dilute sulphuric acid is oxydized to hemipinic acid:

$$Ꞓ_{10}H_{10}Θ_5+Θ=Ꞓ_{10}H_{10}Θ_6.$$

(10.) Opianic acid, on heating with caustic potash, splits up into meconin and hemipinic acid:

$$2Ꞓ_{10}H_{10}Θ_5=Ꞓ_{10}H_{10}Θ_4+Ꞓ_{10}H_{10}Θ_6.$$

(11.) Opianic acid, heated with an excess of hydrochloric acid, exchanges its methyl for hydrogen. Beside methyl chlorid, two substances are probably thus produced, noropianic acid and methyl-noropianic acid; the first by a single, the second by two replacements of methyl by hydrogen.

$$Ꞓ_{10}H_{10}Θ_5+2HCl=2ꞒH_3Cl+Ꞓ_8H_6Θ_5.$$

$$Ꞓ_{10}H_{10}Θ_5+HCl=ꞒH_3Cl+Ꞓ_9H_8Θ_5.$$

Only the latter has been obtained pure, the former spontaneously decomposing. Hydriodic acid acts similarly. Methyl-noropianic acid, like opianic acid, is monobasic.

(12.) All attempts to oxydize meconin to opianic or hemipinic acid, or to any other product, were unsuccessful.

(13.) Meconin, treated with an excess of hydrochloric or hydriodic acid, yields methyl chlorid or iodid, and a body derived from meconin by replacing $ЄH_3$ by H, methyl-normeconin:

$$Є_{10}H_{10}Θ_4+HCl=ЄH_3Cl+Є_9H_8Θ_4.$$

Experiments to produce the hypothetical normeconin by substituting H_2 for $(ЄH_3)_2$, gave nothing capable of being isolated in a pure state.

(14.) Hemipinic acid, by treatment with various reducing agents, was in no case reduced to opianic acid or meconin. So, experiments to obtain opianic acid from the union of hemipinic acid and meconin, have yielded no result. Moreover, hemipinic acid could not be oxydized to any other product.

(15.) Heated with an excess of hydrochloric acid, hemipinic acid yields, beside methyl chlorid and carbon dioxyd, a new acid, methyl-hypogallic acid:

$$Є_{10}H_{10}Θ_6+HCl=ЄH_3Cl+ЄΘ_2+Є_8H_8Θ_4.$$

Heated with hydriodic acid, it affords methyl iodid, carbon dioxyd and hypo-gallic acid:

$$Є_{10}H_{10}Θ_6+2HI=2ЄH_3I+ЄΘ_2+Є_7H_6Θ_4.$$

(16.) Anderson's observations proving hemipinic acid to be dibasic are confirmed, the anhydrid being obtained by simple drying:

$$Є_{10}H_{10}Θ_6=H_2Θ+Є_{10}H_8Θ_6.$$

Methyl-hypogallic acid is also dibasic.

(17.) Hemipinic acid may crystallize with various quantities of crystal water; crystals having been obtained with $\frac{1}{2}$, with 1, and with 2 molecules of such water.

(18.) All the reactions of narcotine and of its decomposition products, are explained in a satisfactory manner by assuming for it the following rational formula:

$$\begin{array}{rl} \left.\begin{array}{r} ЄH_3 \\ (Є_{11}H_9Θ_2)'' \end{array}\right\} & N \\ (Є_8H_4Θ)^{iv} \Big\} & Θ_3 \\ (ЄH_3)_2H \Big\} & Θ_3 \end{array}$$

—*Ann. Ch. Pharm.*, Suppl. Band vii, 66, Nov., 1869. G. F. B.

4. *On the size of Molecules;* by Sir WM. THOMSON. (Proc. Lit. and Phil. Soc. Manchester, March 22; Nature, May 19, p. 56.)—My occupation on the Kinetic theory of gases has led me at last to come to definite terms as to the size of molecules. Ever since about the first year of my professorship I have taught my students that Cauchy's theory of Dispersion proves heterogeneousness, or molecular structure, to become sensible in contiguous portions of glass or water, of dimensions moderately small in comparison with the wave-lengths of ordinary light. I have spoken to you also, I think, of the argument deducible from the contact electricity of metals. This, I now find, proves a limit to the dimensions of the

molecules in metals quite corresponding to that established for transparent solids and liquids by the dynamics of dispersion. In experiments made about ten years ago, of which a slight sketch is published in the Proceedings of the Literary and Philosophical Society of Manchester, I found that a plate of zinc and a plate of copper kept in metallic connection with one another (by a fine wire or otherwise) act electrically upon electrified bodies in their neighborhood, and upon one another, as they would if they were of the same metal and kept at a difference of potentials equal to about three-quarters of that produced by a single cell of Daniell's. Hence, and from my measurement of the electrostatic effects of a Daniell's battery, published in the Proceedings of the Royal Society, for February and April, 1860, I find that plates of zinc and copper held parallel to one another at any distance, D, apart which is a small fraction of the linear dimensions of their opposed surfaces, and kept in metallic communication with one another, exercise a mutual attraction equal to

$$2 \times 10^{-10} \times \frac{A}{D^2} \text{ grams weight.}$$

Hence, if they were allowed to approach from any greater distance, D′, to the distance, D, the work done by their mutual attraction is

$$2 \times 10^{-10} \times \frac{A(D'-D)}{D'D} \text{ centimeter grams;}$$

which, if D is very small in comparison with D′, is very approximately equal to

$$2 \times 10^{-10} \times \frac{A}{D}.$$

Now suppose a pile to be made of a great number (N+1) of very thin plates alternately of zinc and copper, kept in metallic connection while they are brought toward one another. Let their positions in the pile be parallel, with narrow spaces intervening. For simplicity, let the thickness of each metal plate and intervening space be D. The whole work done will be

$$2 \times 10^{-10} \times N \frac{A}{D}.$$

The whole mass of the pile (if we neglect that of one of the end plates) is $NAD\varrho$, where ϱ denotes the mean of the densities of zinc and copper. Hence, if *h* be the height to which the whole mass must be raised against a constant force equal to its weight at the earth's surface, to do the same amount of work, we have

$$NAD\varrho h = 2 \times 10^{-10} \times N \frac{A}{D};$$

which gives

$$h = \frac{2 \times 10^{-10}}{\varrho D},$$

or, as $\varrho = 8$, nearly enough for the present rough estimate,

$$h = \frac{1}{(200000D)^2}.$$

Hence, if $$D=\frac{1}{200000} \text{ centimeter,}$$
$$h=1 \text{ centimeter.}$$

The amount of energy thus calculated is not so great as to afford any argument against the conclusion which general knowledge of divisibility, electric conductivity, and other properties of matter indicates as probable; that, down to thicknesses of $\frac{1}{200000}$ of a centimeter for the metal plates and intervening spaces, the contact electrification, and the attraction due to it, follow with but little if any sensible deviation the laws proved by experiment for plates of measurable thickness with measurable intervals between them. But let D be a two-hundred-millionth of a centimeter. If the preceding formulæ were applicable to plates and spaces of this degree of thinness we should have

$$h=1,000,000 \text{ centimeters or 10 kilometers.}$$

The thermal equivalent of the work thus represented is about 248 times the quantity of heat required to warm the whole mass (composed of equal masses of zinc and copper) by 1° Cent. This is probably much more than the whole heat of combination of equal masses of zinc and copper melted together. For it is not probable that the compound metal when dissolved in an acid would show anything approaching to so great a deficiency in the heat evolved below that evolved when the metallic constituents are separately dissolved, and their solutions mixed; but the experiment should be made. Without any such experiment, however, we may safely say that the fourfold amount of energy indicated by the preceding formula, for a value of D yet twice as small, is very much greater than any estimate which our present knowledge allows us to accept for the heat of combination of zinc and copper. For something much less than the thermal equivalent of that amount of energy would melt the zinc and copper; and, therefore, if in combining they generated by their mutual attraction any such amount of energy, a mixture of zinc and copper filings would rush into combination (as the ingredients of gunpowder do) on being heated enough in any small part of the whole mass to melt together there. Hence, we may infer that the electric attraction between metallically-connected plates of zinc and copper of only $\frac{1}{400000000}$ of a centimeter thickness, at a distance of only $\frac{1}{400000000}$ of a centimeter asunder, must be greatly less than that calculated from the magnitude of the force and the law of its variation observed for places of measurable thickness, at measurable distances asunder. In other words, plates of zinc and copper so thin as a four-hundred-millionth of a centimeter from one another, form a mixture closely approaching to a molecular combination, if, indeed, plates so thin could be made without splitting atoms. Wishing to avoid complication, I have avoided hitherto noticing one important question as to the energy concerned in the electric attraction of metallically-connected plates of zinc and copper. Is there not a change of temperature in molecularly thin strata of the two metals adjoining to the opposed surfaces, when they are allowed to approach one another, analogous to the heat produced by the condensation of a gas, the changes of temperature produced by the application of stresses to elastic solids which you have investigated experimentally, and the cooling effect I have proved to be produced by drawing out a liquid film which I shall have to notice particularly below? Easy enough experiments on the contact electricity of metals will answer this question. If the contact-difference diminishes as the temperature is raised, it will follow from the Second Law of Thermodynamics, by reasoning precisely corresponding with that which I applied to the liquid film in my letters to you of February 2d and February 3d, 1858,* that plates of the two metals kept in metallic communication and allowed to approach one another will experience an elevation of temperature. But if the contact-difference increases with temperature, the effect of mutual approach will be a lowering of temperature. On the former supposition, the diminution of intrinsic energy in quantities of zinc and copper, consequent on mutual approach with temperature kept constant, will be greater, and on the latter supposition less, than I have estimated above. Till the requisite experiments are made, further speculation on this subject is profitless; but whatever be the result, it cannot invalidate the conclusion that a stratum of $\frac{1}{200000000}$ of a centimeter thick cannot contain in its thickness many, if so much as one, molecular constituent of the mass. Besides the two reasons for limiting the smallness of atoms or molecules which I have now stated, two others are afforded by the theory of capillary attraction, and Clausius' and Maxwell's magnificent working out of the Kinetic Theory of gases. In my letters to you already referred to, I showed that the dynamic value of the heat required to prevent a bubble from cooling when stretched is rather more than half the work spent in stretching it. Hence, if we calculate the work required to stretch it to any stated extent, and multiply the result by $\frac{3}{2}$, we have an estimate, near enough for my present purpose, of the augmentation of energy experienced by a liquid film when stretched and kept at a constant temperature. Taking ·08 of a gram weight per centimeter of breadth as the capillary tension of a surface of water, and therefore ·16 as that of a water bubble, I calculate (as you may verify easily) that a quantity of water extended to a thinness of $\frac{1}{200000000}$ of a centimeter would, if its tension remained constant, have more energy than the same mass of water in ordinary condition by about 1,100 times as much as suffices to warm it by 1° Cent. This is more than enough (as Maxwell suggested to me) to drive the liquid into vapor. Hence, if a film of $\frac{1}{20000000}$ of a centimeter thick can exist as liquid at all, it is *perfectly certain* that there cannot be many molecules in its thickness. The argument from the Kinetic Theory of gases leads me to quite a similar conclusion.

5. *Comparison of Mechanical Equivalents;* by PLINY EARLE CHASE, (Proc. Am. Phil. Soc., xi, 313, 1870).—The comparison of different mechanical equivalents will open a new field for investigation, which may prove to be fertile in valuable results. For

---

* Proceedings of the Royal Society for April, 1858.

example, recent determinations, by the different methods of Thomsen and Farmer, fix the mechanical equivalent of light, in a wax candle burning 126½ grains per hour, at 13·1 foot-pounds per minute, the equivalent of 1 grain being 6·213 foot-pounds. According to Dulong, the heat evolved during the combustion of 1 grain of olive oil in oxygen, is sufficient to heat 9862 grains of water 1° C. According to Favre and Silbermann, 1 grain of oil of turpentine, burned in oxygen, would heat 10,852 grains of water 1° C.

It may therefore be presumed that the total heat given out by the combustion of 1 grain of wax, is about sufficient to raise 10,000 grains or water 1° C., or 18,000 gr. 1° F. This represents a mechanical equivalence of $(18,000 \times 772 \div 7000 =)$ 1985·143 foot-pounds, which is 319·5 times as great as the corresponding equivalent of the light given out during the combustion.

Tyndall, in his lecture on Radiation, states that the visible rays of the electric light contain about one-tenth of the total radiated heat. The relative luminous intensity of an electric lamp would therefore appear to be about 32 times as great as that of the wax candle. This ratio so nearly resembles that of solar to terrestrial superficial attraction, and the connection of electric and magnetic currents with solar radiation is so evident, that additional experiments, to furnish materials for a great variety of similar comparisons, seem desirable. While it is possible that the resemblance, in the present instance, may be accidental, the numerous harmonies between the manifestations of cosmical and molecular forces render it at least equally possible that it may have a weighty significance.

II. GEOLOGY AND MINERALOGY.

1. *On a Fossil Tooth from Table Mountain;* by Prof. WILLIAM P. BLAKE. (Communicated by the author for this Journal.)—The fossil tooth, found by Mr. D. T. Hughes, 1,700 feet under Table Mountain, and 300 feet below the surface, I have carefully examined and compared with specimens in the Smithsonian Institution. It proves to be a back lower molar of an equine animal of the genus *Hipparion*, or a closely allied genus. This genus is one of the connecting links between the *Palæotherium* and the horse.

The specimen closely resembles a fossil in the Smithsonian museum, from the Pliocene formations of the Niobrara river in Nebraska,* not only in size but in the foldings of the enamel, and particularly in the posterior part of the tooth, but it differs enough, in several particulars, to justify the belief that it is a distinct species. Dr. Leidy does not attempt to determine, specifically, the specimen from Nebraska, but considers it closely related to, if not identical specifically with, *Hipparion gratum*, possibly *Protohippus placidus*.

---

* Described by Prof. Leidy in his work upon the Extinct Mammalian Fauna of that region, p. 319, pl. xix, fig. 7,

The size of the Table Mountain specimen, which is considerably worn by attrition in the gravel, is: length, 11 lines; breadth upon the crown, 9 lines; breadth at the base, 10 lines; thickness, anteriorly, 4 lines, posteriorly, 2 lines.

This fossil is the first of the kind discovered west of the Rocky Mountains. It adds to the list of the fauna of the period antedating Table Mountain—a list which includes the mammoth (*Elephas*, from Knight's Ferry), the rhinoceros, and an animal allied to the elk.' I have believed that remains of man were also found under the lava; but upon this point, after diligent inquiry, I am satisfied that the evidence is insufficient. But we now add this fossil allied to *Hipparion*, and I regard it as another indication that the Table Mountain beds are Pliocene, and homotaxial with those of the Bad Lands of Nebraska.

2. *Cause of the Descent of Glaciers.*—Rev. HENRY MOSELEY, before the Royal Society in January, 1869, in the Philosophical Magazine for the May following, and at the meeting of the Royal Institution of Great Britain, May 13, 1870, opposes the view that glaciers owe their movement mainly to gravity, and gives as the principal cause contraction and expansion due to change of temperature through the mass. He compares the movement to that of a sheet of lead on a sloping surface, in its expansion the lower edge working downward, and in its contraction the upper edge or part.

In the Phil. Mag. for July, 1870, Mr. John Ball, after alluding to the criticisms on the above theory by Mr. Wm. Mathews in the Alpine Journal for February last, shows that the supposed contraction and expansion to which Canon Moseley appeals, does not take place, and that the "crawling theory" of glacier motion is therefore unsatisfactory. He argues that the glacier is not a continuous solid mass like a sheet of metal; that the temperature of the interior, as observers have proved, is very nearly constant; that the movement is half as fast in winter as in summer; that the rate of motion is not proportioned at all to the length of the glacier as it should be by the theory; that the supposed expansion and contraction, if a fact, would exceed twenty or more times the rate of actual motion but for modifying causes,—and this is an amount of modifying intervention for the sake of the theory, sufficient to prove the theory of no value. He concludes as follows:

"If I might presume to estimate the net results of this renewed discussion of the causes of glacier-motion, I should say that they are not considerable, but yet are far from worthless. Canon Moseley's experiments have added something to our knowledge, and especially those on the tenacity of ice, which have some bearing on the origin of crevasses. Of far greater importance are the observations on ice-planks made by Mr. William Mathews. The first of these, published in the 'Alpine Journal,' gave prominence to a fact which had long been familiar to myself, and probably to many others. I have often found that long icicles placed in an inclined position, and supported only at the upper end, will gradually resume the vertical direction, and I had, perhaps too lightly, assumed that this was a particular instance of the process by which ice changes its form through fracture and regelation. In Mr. Mathew's first experiment, conducted during a thaw, a thick plank of ice supported at each end was deflected at the middle through a space of 7 inches in as many hours. Although none but very minute fissures were observed, the facts did not seem to me altogether inconsistent with that explanation. In the second series of observations, made during the severe frost of February last, Mr. Mathews found that at temperatures notably below the freezing-point a plank of ice, supported as before, subsides slowly between the points of support under the sole influence of its own weight. The deflection under these circumstances was about 1½ inch in twenty-four hours. Taking this observation in connection with a multitude of facts recently brought to light, and especially the researches of M. Tresca, we are led to admit that ice, in common with very many apparently rigid bodies, does possess a certain degree of plasticity which is exhibited by changes of form effected very slowly under the action of forces of moderate amount, rather than by the rapid action of more powerful agencies.*

"The admission of this conclusion may slightly modify, but will not materially alter, the views now generally held as to the causes of glacier-motion, which are mainly derived from the remarkable researches of Professor Tyndall. Whatever may be the final judgment of men of science, I feel quite sure that it will not confirm the opinion expressed by Canon Moseley in his latest publication: that "the phenomena of glacier-motion belong rather to mechanical philosophy than to physics." Every real advance that has been made toward the explanation of those phenomena has been due to the application of increased knowledge of the physical properties of glacier-ice; and if any thing be wanting to complete the explanation now generally accepted, it must be derived from such additional acquaintance with those properties as may be derived from continued observation and experiment."

3. *The North American Lakes considered as Chronometers of Post-glacial time;* by Dr. EDMUND ANDREWS. 24 pp. roy. 8vo. (Trans. Acad. Sci. Chicago, vol. II).—Dr. Andrews discusses in this paper the nature of the post-glacial deposits on the shores of Lake Michigan, especially in the vicinity of Chicago, their extent, the areas of the several beaches, the erosion these beaches have undergone, the width of the subaqueous plateau formed along the border of the lake out of the material removed in the erosion, and the amount of sand moved in the process; and from the elements thus obtained, arrives at the following conclusions:

(1.) The upper beach began to form immediately after the Boulder Drift period, and continued to accrete for about 900 years. No animal fossils have yet been found in it.

(2.) The waters then fell suddenly to about their present level, where they remained till a thin bed of peat accreted on the marshy slope vacated by the waves. I have not been able to collect data for a calculation of this first low-water period, but from the posi-

---

* In Dana's Manual of Geology, p. 673, this plasticity is recognized among the means of motion on the ground of observations, similar to the above, made by Kane in his "Arctic Explorations."

tion of the soil-bed in the eastern dunes, I incline to think it lasted 500 or 1000 years.

(3.) The water rose again, submerging for a short time the upper beach, but soon fell to the line of the middle one, where it remained about 1,600 or 2000 years. This period appears to be cotemporary with the Loess.

(4.) The water, which had already slowly fallen some feet, now retired more rapidly to near its present level, which it has maintained with only moderate fluctuations ever since.

(5.) The total time of all these deposits appears to be somewhere between 5,300 and 7,500 years.

The discussion is an interesting and important one. Some uncertainties in the calculations occur to us; but without a special examination of the region we are not at present prepared to mention any but the following. The author writes as if he supposed that sand in the course of transportation always remained sand. He observes that "the sand movement in the lake is confined to the shore line," as proved by the fact that "there is no sand in deep water," not recognizing the well-known geological fact that sands on coasts are always undergoing wear through the attrition of grain upon grain under the action of waves and currents, and that while the finer material made by this attrition is floated off to deeper waters, the coarser is left behind in such cases near or on the shores.

3. *Fossils in the Mineral veins of the Carboniferous Limestone of Great Britain.*—A paper on this subject by Mr. CHARLES MOORE, (Rep. Brit. Assoc. for 1869, p. 360), contains notices of numerous fossils in the Lead mines of the Carboniferous limestone. In the walls of the Charterhouse Lead-mine, in the Mendip range, 270 feet from the surface, over 80 species of Liassic fossils were obtained by him, and more than 30 of Carboniferous. The Liassic included a Chara, wood in the form of jet, Rhizopods, Pentacrinites, a Cidaris and other Echinoderms, Serpula, claws of Crustacea, many Mollusks, remains of about 10 species of fishes of the genera *Acrodus*, *Hybodus*, *Lepidotus*, &c., and a tooth of an Ichthyosaur; and among the Carboniferous, there were species of Helix, Hydrobia, Planorbis, Proserpina, Valvata, Vertigo, all either land or freshwater Mollusks, also 11 species of Ostracoids, besides Mollusks, Serpulæ, Encrinites, Corals and Conodonts. A similar range of facts was observed in connection with other lead mines. We cite the following general remarks.

Whilst the various mines and mineral deposits I have examined have certain species in common, it may be said that they have each special paleontological features of their own.

In the Keld-Head Mines organic remains are very abundant at about 450 feet from the surface, amongst which are many Foraminifera, chiefly of the genus *Involutina*, of which there are six species, and univalves of about twelve genera, the freshwater species *Valvata anomala* Moore, and *Planorbis Mendipensis*

Moore, being present, and also Entomostraca of several new species.

The Fallowfield mines, although not yielding a very long list of species, have their special interest in the presence of the land and freshwater genera *Stoastoma* ?, *Hydrobia*, and *Pisidium* ; *Involutina*, as in the Keld-Head mines, though rarely ; and a single seed of the *Flemingites gracilis* Carr. The richest samples from this mine are at 90 and 450 feet from the surface.

The Grassington mines are not only very rich in individual specimens, but have yielded the greatest number of species, among which are again freshwater remains of *Hydrobia*, *Planorbis*, *Valvata*, and *Lithoglyphus*. Entomostraca of at least ten species, Conodonts of several varieties, and fish-remains of the genera *Petalodus*, *Orodus*, &c.

The Alston mines have yielded about twelve species of univalves, though they are not in good condition. Foraminifera are present, but are rare, and fish-remains of the genera *Petalodus*.

The Weardale mines, and those of Allenheads, are comparatively not rich, the vein stuff in them being much mineralized. Conodonts occur in the former, Entomostraca rather abundantly in the latter, and also, though rarely, the genus *Hydrobia*. In these veins, and also at Alston, I have detected, for the first time, large cells of a foraminiferous shell, for which Mr. Brady suggests the generic name *Carteria*.

In the White and Silver Band mines remains are somewhat rarely distributed, the richest deposit being a friable ochreous sandstone, on the "sun" side of the Silver Band Old Mine, which yielded many specimens of *Hydrobia*, and one or two of *Valvata anomala*, several genera of Foraminifera, including *Involutina* and *Dentalina*, with Conodonts, and portions of teeth of *Psammodus*.

The Mount-Pleasant mines of Mold contain Foraminifera, and also the freshwater *Hydrobia*, though rarely, and Conodonts rather abundantly; but they are especially remarkable for the great variety of fish-remains they yield, which appear to represent at least ten different genera. Mixed with the "dowks" of the mine are occasionally small pieces of laminated stone the surfaces of which exhibit numerous traces of fish-scales.

The researches I have been making have involved very considerable labor and minute investigation; but as they will to some extent have opened up a new field of inquiry, I hope they will not be without some results. Before concluding, I desire to refer to several of the more interesting paleontological facts which have been obtained. * * * * * *

Not the least important fact in my mine explorations has been the discovery of a land and freshwater fauna. Until I obtained the three genera of *Helix*, *Vertigo*, and *Proserpina*, with the freshwater genera *Planorbis* and *Valvata*, in the Charterhouse Mine, the only known terrestrial shell below the secondary beds was the *Pupa vestusta* Daws., found by Sir Charles Lyell and Dr.

Dawson in the Coal measures of Nova Scotia. To the above genera I have now to add those of *Hydrobia*, *Stoastoma?*, *Lithoglyphus*, and *Pisidium*, from the mines of the north of England, some of which I have little doubt are older than the *Pupa vetusta* of the coal-beds. There is thus the fact of the presence of nine genera of land and freshwater shells in the lead-veins of this country.

In addition to the list of organic remains which follows, numbering about 112 species from the north of England and North-Wales mines, eight, which are not in common, have been obtained from Weston, and to these again are to be added 89 in the list previously given from Charterhouse, so that in true and workable mineral veins I have found 209 species. In the Carboniferous Limestone of the Frome district precisely similar phenomena occur, though the fissures are not worked. These Rhætic and Liassic veins have yielded me about 70 species, so that, including the districts I have enumerated, I have obtained from vein-fissures, with their deposits of different ages, about 279 species of organic remains.

Under these peculiar circumstances, I have discovered the oldest known Mammalia, the oldest land and freshwater Mollusca, about 52 species of fish, and about 8 of Reptilia, besides the other groups to which reference has been made.

With regard to the origin of the veins, Mr. Moore observes as follows:

The chief material of all the mineral veins I find to be of marine origin; all the organic contents are fossil, and their precise geological age can be arrived at without much difficulty. Wherever they contain land shells, as on the Mendips, or freshwater shells, which occur in the veins of Alston, and are wide-spread elsewhere, they are also fossil and of contemporaneous age with the other remains. It is certain from this that the veins received their infilling when within the influence of the ocean, and before their present elevation, since which time, as I have before stated, I doubt if there could be any material alteration in their contents. * * *

It has now been established, without doubt, by the highest chemical authorities, that many of our most important minerals are present in minute quantities in the waters of the ocean. This is admitted by those who believe in segregation, the difference being that they think it was first deposited and afterward extracted from the parent rock, and redeposited in the veins, rather than originally collected in the veins themselves.

As regards the connection subsisting between the ocean and the vein-fissures, I believe it will be recognized to be the case that, in the great majority of instances, the different veins come directly to the surface, and wherever a later rock has been deposited, which is only in exceptional cases, covering up the mouth of the vein, there will still be found a break in the sequence of the strata, which might give almost unlimited time for the precipitation of the minerals therein. Wherever systems of veins occur, it is probable there will be found connected therewith an abnormal condition and considerable breaks in the deposition of the rocks. I have shown this to be the case in connection with the Carboniferous Limestone of the Mendip range, and its continuation through South Wales, in which districts it can be seen that those rocks were, through enormous periods, exposed to the influence of the ocean, possibly forming reef-like barriers around the edges of the Carboniferous basin, the fissured veins and floor of this sea-bottom receiving at some periods materials of Rhætic or of Lower or Middle Lias age, whilst an occasional capping of the beds of Inferior Oolite, left in some Carboniferous limestone trough, now and then cover up the mouths of the veins that had received all their contents prior to its deposition. Some most instructive examples are present in this district, in which it may be seen that whilst there are on the walls of the vein the usual vertical conditions of vein-stuff, such as calc-spar, sulphate of barytes, &c., with occasional hematite iron-ore, calamine, and galena, the central portion of the vein is unmistakably of Liassic or Rhætic age. In all such instances there are combined the elements of open fissures communicating with the ocean, and greater or less time in the reception of their contents.

4. *Additional note on Elasmosaurus;* by E. D. Cope.—To my preceding note on Elasmosaurus, I append the following, in consequence of the reading of another criticism by Prof. Leidy, in the Proceedings Acad. Nat. Sci. Philad., January to April, 1870, (issued in June). [See p. 139 of this volume.—Eds.].

In this Dr. Leidy agrees with my identification of Cimoliasaurus and Discosaurus made in 1868, regarding them as the same. But he employs the name Discosaurus instead of Cimoliasaurus, to which we object for three reasons: 1, in works written subsequent to his determination, Cimoliasaurus had been exclusively used, and has therefore obtained considerable currency; 2, Discosaurus was founded upon a miscellaneous collection of species, and not defined; 3, the name refers perhaps to an individual peculiarity of one of the species, as suggested by Leidy, and conveys an erroneous impression of there being a vertebral disc characteristic of the genus, whereas the peculiarity consists of a groove. The name Cimoliasaurus is open to none of these objections.

He however unites with the above genus my Elasmosaurus, although a few pages previously he considers them distinct, on the same grounds that convinced me of the propriety of separating them, viz: the enormous neck with compressed vertebræ in the one, and the short transverse cervicals of the other. No such difference is displayed by the species of Plesiosaurus, though there is considerable variation in the genus in this respect. It cannot however be predicted, that no species combining the characters of the two will ever be found. All genera in paleontology stand open to this risk.

The *Cimoliasaurus grandis* (*Brimosaurus* Leidy) presents the shortened cervicals of *C. magnus* and therefore is not an Elasmosaurus. The *E. orientalis* is as yet but little known, and there may be doubts as to which genus it represents. In a restored figure of it which was given in an article in the American Naturalist (1868, p. 84), it is represented with a neck of the shorter type of Cimoliasaurus. Whether the shorter or longer type of cervicals belong to it will remain uncertain until more remains are found. If it be a true Elasmosaurus, the figure will represent better a Cimoliasaurus.

In his second notice Leidy mentions his having reversed the extremities of the vertebral series in the three Cimoliasauri described by him.

5. *Lias and Oolite of Australia.*—Mr. CHARLES MOORE has published an important paper on Australian Mesozoic Geology, in the Quart. J. Geol. Society for 1870, describing beds of the age of the Middle and Upper Lias and Lower Oolite from Western Australia, and Oolitic or Lower Cretaceous from Queensland (at Wollumbilla). Out of 50 species of Mollusks in Western Australia 20 are found to be identical with British species, viz:

Ammonites Aalensis, var. *Moorei* Lycett, Upper Lias; *A. radians* Rein, Upper Lias; *A. Walcottii* Sow., Upper Lias; *A. macrocephalus* Schloth, Oolite; *A. Brocchii* Sow., Oolite; *Nautilus semistriatus* D'Orb., Upper Lias; *Belemnites canaliculatus* Mill, Oolite; *Gresslya donaciformis* Goldf., Upper Lias; *Maycites liassianus* Quenst., Middle Lias; *Cucullæa oblonga* Sow., Oolite; *Pholadomya ovulum* Ag., Oolite; *Avicula Münsteri* Goldf., Oolite; *A. echinata* Sow., Oolite; *Pecten cinctus* Sow., Oolite; *P. calvus* Münst., Oolite; *Lima proboscidea* Sow., Oolite; *L. punctata* Sow., Oolite; *Ostrea Marshii* Sow., Oolite; *Rhynchonella variabilis* Schloth, Oolite; *Cristellaria cultrata* Montfort, Oolite. *Lima proboscidea* and *Ostrea Marshii* appear to have been as abundant as in the hills around Bath; and *Pecten cinctus* from Australia attains the same large proportions as in this country. The Corn brash is apparently represented in Australia by *Ammonites macrocephalus* and *Avicula echinata*, and the Middle Lias by the *Mya cites liassianus* before mentioned.

The paper describes many new species.

6. *Plants of the Coal formation of Langeac, Haute-Loire;* by Dr. H. B. GEINITZ.—The plants mentioned in this paper as occurring at Langeac and remarked upon are *Calamites cannæformis* Schl., *C. Cisti* Bgt., *C. Suckowi* Bgt., *Annularia longifolia* Bgt., *Cyatheites arborescens* Sch., *C. dentatus* Bgt., *C. Miltoni* Artis, *Alethopteris pteroides* Bgt., *Cardiocarpus emarginatus* Göpp. & B., *C. Gutbieri*, *Noeggerathia palmæformis* Göpp., *Rhabdocarpus ovalis* Göpp. & F., *Cordaites principalis* Germ., *Trigonocarpus Nœggerathi* Sternb., *T. ventricosus* Göpp. & F. The several kinds of fruit here described give special interest to the memoir. The accompanying plate contains figures of several forms under six species, two species of Cardiocarpus, one of Rhabdocarpus, one of Cordaites, and two of Trigonocarpus.

7. *Sivatherium in Colorado.*—Dr. Leidy refers a fragment of a fossil from Colorado (received through Dr. Hayden from Dr. Gehrung), with a query to a genus near Sivatherium, which he calls *Megacerops.* He regards the fossil as corresponding to that portion of the face which comprises "the upper part of the nose, together with the forehead and anterior horn cores." The animal to which it belonged was nearly as large as the Sivatherium. The distance between the center of the two horn cores is $10\frac{1}{2}$ inches; the length of the horn cores above the intervening space 5 inches; the breadth of the face where narrow below the horn cores $7\frac{1}{4}$ in. He names the species *Megacerops Coloradensis.—Proc. Acad. Nat. Sci. Philad.*, 1870, p. 1.

8. *Megalonyx Jeffersoni in Illinois.*—A metacarpal bone of this Megalonyx is announced by Dr. Leidy (Proc. Acad. Nat. Sci., 1870, p. 13) as having been found in a crevice in the lead-bearing rocks near Galena, 130 feet below the surface, along with a last lower grinder of the extinct Ox, *Bison antiquus.* The Museum of the Academy contains other bones from the same locality, being remains of a large extinct Peccary, *Platygonus compressus*, an extinct Raccoon, *Procyon priscus*, and a large insectivore, *Anemodon Snyderi*, all of which were probably cotemporaries of the Megalonyx.

9. *Mineralogical Contributions of G. vom Rath* (Pogg. Ann. cxxxviii, 449).—VOM RATH describes and figures here crystals of the following Vesuvian minerals: twins of Anorthite, crystals of Oligoclase, Wollastonite, Orthite and Humite: besides giving an account of a new mineral from Laach which he names *Amblystegite*, and a description of certain twins of orthoclase.

Amblystegite occurs in small crystals of a brown to reddish-brown color, a gray streak slightly greenish; adamantine luster. The crystals are orthorhombic; $i\text{-}\bar{i}:I=135° 50'$, $i\text{-}\bar{i}:1\text{-}\tilde{2}=119° 26'$, $\frac{1}{4}\text{-}\breve{i}:\frac{1}{4}\text{-}\breve{i}=163° 47'$, $i\text{-}\breve{i}:\frac{1}{4}\text{-}\breve{i}=98° 6\frac{1}{2}'$. B.B. very fusible and thus differs from humite and chrysolite. Composition,

$$\ddot{Si}\ 49{\cdot}8\quad \overset{\cdots}{\bar{Al}}\ 5{\cdot}05\quad \dot{Fe}\ 25{\cdot}6\quad \dot{Mg}\ 17{\cdot}7\quad \dot{Ca}\ 0{\cdot}15=98{\cdot}30,$$

corresponding to the oxygen ratio for $\dot{R}$, $\overset{\cdots}{\bar{R}}$, $\ddot{Si}$, 12·81 : 2·36 : 26·56. Only half a gram was here used. The mineral is so mixed with magnetic iron as to be separated with difficulty for analysis.

10. *Laxmannite* of Nordenskiöld (J. f. pr. Ch., cvii, 491) according to Hermann (ib. II, i, 447) is probably Vauquelinite; Nordenskiöld made the crystals monoclinic with the inclination of the vertical axis 69° 46′; G. = 5·77, H. = 3, color olive-green; and obtained for the composition (mean of two analyses)—

$$\overset{\cdots\cdot\cdot}{\bar{P}}\ 8{\cdot}31\quad \overset{\cdots}{\bar{Cr}}\ 15{\cdot}91\quad \dot{Pb}\ 61{\cdot}16\quad \dot{Cu}\ 11{\cdot}64\quad \dot{Fe}\ 1{\cdot}06\quad \dot{H}\ 1{\cdot}10=99{\cdot}18$$

Hermann observes that Berzelius probably analysed the same mineral in his examination of the species he named Vauquelinite, and that if the pure oxyd of chrome of Berzelius be taken as a mixture of phosphoric and chromic acids, the two analyses closely agree, as follows:

	$\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{P}}$	$\dddot{\mathrm{Cr}}$	$\dot{\mathrm{Pb}}$	$\dot{\mathrm{Cu}}$	$\dot{\mathrm{Fe}}$	$\dot{\mathrm{H}}$
Berzelius, ...	8·31	15·85	60·87	10·80	1·10	1·10=98·03
Nordenskiöld, ...	8·31	15·91	61·16	11·64	1·06	1·10=99·18

both leading to the formula $2\dot{\mathrm{Cu}}^3\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{P}}+5\dot{\mathrm{Pb}}^2\dddot{\mathrm{Cr}}+2\dot{\mathrm{H}}$.

11. *Phosphorchromite.*—Hermann (J. f. pr. Ch., II., i, 450) thus names a chromophosphate of lead and copper from Beresofsk. It occurs concretionary, crystalline or massive within, with a blackish-green color, pistachio-green streak, H. =3, G. =5·80. An analysis afforded:

$\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{P}}$ 9·94 $\dddot{\mathrm{Cr}}$ 10·13 $\dot{\mathrm{Pb}}$ 68·33 $\dot{\mathrm{Cu}}$ 7·36 $\dot{\mathrm{Fe}}$ 2·80 $\dot{\mathrm{H}}$ 1·16=99·72,

leading to the formula $3\dot{\mathrm{Cu}}^2\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{P}}+5\dot{\mathrm{Pb}}^3\dddot{\mathrm{Cr}}+3\dot{\mathrm{H}}$.

Vanadiolite of Hermann (J. f. pr. Chem., ib. 445) occurs in small crystals, partly in druses, of a dark or blackish-green color and gray-green streak; dark emerald-green color and transparent, in small grains; of strong vitreous luster, with G. = 3·36. B.B. melts on the edges to a black blebby slag. His analysis afforded

$\dddot{\mathrm{Si}}$ 15·61 $\dddot{\mathrm{V}}$ 44·85 $\dddot{\mathrm{Al}}$ 1·10 $\dot{\mathrm{Fe}}$ 1·40 $\dot{\mathrm{Mg}}$ 2·61 $\dot{\mathrm{Ca}}$ 34·43=100

and he regards it as a subvanadate combined with augite like the Lavroffite of the same locality, Slüdänka, near Lake Baikal. He writes the formula $3\dot{\mathrm{R}}\dddot{\mathrm{Si}}+\dot{\mathrm{Ca}}^6(\mathrm{VO}^4+2\mathrm{VO}^5)$.

12. *Wolframite.*—Descloizeaux suggested in 1850 (Ann. Ch. Phys., III, xxviii) that Wolfram was oblique rhombic in crystallization, after some measurements of crystals. He has recently confirmed this conclusion (ib., IV, xix, 168, Feb. 1870) by both optical and crystallographic observations. He finds the obliquity 90° 38′, and the prismatic angle 100° 37′. His crystals were from Bayewka, near Ekatherinenbourg, in the Urals, a variety which afforded Mr. Koulibine,

$\dddot{\mathrm{W}}$ 74·32 $\dot{\mathrm{Fe}}$ 2·11 $\dot{\mathrm{Mn}}$ 20·90 $\dot{\mathrm{Ca}}$ 1·30 $\dddot{\mathrm{Si}}$ 0·28=98·91,

agreeing nearly with Hubnerite, of Nevada. G.=7·357.

13. *Namaqualite, a new ore of Copper;* by Prof. CHURCH (J. Ch. Soc., II, viii, 1).—Namaqualite is named after the region in which it occurs, Namaqualand, S. Africa. It occurs in thin layers of silky fibers, which alternate with chrysocolla and are sparingly mixed with small crystals of "magnesia mica." Color pale blue; H.=2·5; G.=2·49; isolated fibers transparent under the microscope. In the closed tube yields much water and blackens; at 100° C., or over sulphuric acid in vacuo, no loss of weight. Mean of analyses:

$\dddot{\mathrm{Si}}$	$\dddot{\mathrm{Al}}$	$\dot{\mathrm{Cu}}$	$\dot{\mathrm{Mg}}$	$\dot{\mathrm{Ca}}$	$\dot{\mathrm{H}}$
2·25	15·29	44·74	3 42	2·01	32·38=100·09.

The oxygen ratio for the protoxyds, alumina and water is 4 : 3 : 11. Prof. Church remarks that its crystallized condition, and its definite and constant composition show that it is a true species; and the presence of a sesquioxyd that it is related in composition to hydrotalcite and pyroaurite.

14. *Contributions to the Mineralogy of Victoria;* by G. H. S. ULRICH, F.G.S. 32 pp. 8vo. Melbourne, 1870. (Reprinted from

the Report on the Mineral Statistics of Victoria for the year 1869, presented to Parliament.)—These "contributions" contain detailed descriptions of many species, with figures of crystals, and some analyses, the latter by Mr. C. Newbery. The following are among the new or more interesting species noted by Mr. Ulrich.

Maldonite, or Bismuthic Gold, from the Nuggety reef, of the Alliance Company, Maldon. It occurs in particles in the granite veins affording gold. It has a pinkish-white color, but tarnishes easily on exposure to a dull copper color, and ultimately to black. Cleavage apparently cubic. H.=1·5—2. Malleable and very sectile. Formerly in larger pieces in the upper workings of the Alliance Company's mine.

Native Bismuth, Bismuth Glance and Bismuthite occur in a quartz vein at Linton, Ballarat district.

Selwynite of Ulrich (Dana's Min., p. 509*), a chrome-bearing mineral, has been further examined. Mr. Newbery obtained in new analyses—

$\ddot{\mathrm{Si}}$	$\dddot{\bar{\mathrm{Al}}}$	$\dddot{\bar{\mathrm{Cr}}}$	$\dot{\mathrm{Mg}}$	$\dot{\mathrm{Na}}$	$\dot{\bar{\mathrm{H}}}$
47·25	35·28	7·82	2·42	—	5·67
48·42	34·72	6·94	2·11	2·03	4·83
48·23	38·16	6·14	1·21	3·12	2·90

the mean of which affords very nearly the oxygen ratio for $\dot{\mathrm{R}}$, $\dddot{\bar{\mathrm{R}}}$, $\ddot{\mathrm{Si}}$, $\dot{\bar{\mathrm{H}}}$, 1 : 16 : 22 : 3⅓. Mr. Ulrich observes that the mineral is probably an altered feldspathic mineral, and related to the Pinite group, or especially to gieseckite and dysyntribite. When polished it has some resemblance to nephrite.

Talcosite is a new mineral occurring in thin seams in the selwynite—the seams lamellar-columnar. Resembles talc in feel. H.=1; but transverse to lamination 1·5–2·0. G.=2·46–2·5. Color silver-white, faint greenish or yellowish. Luster pearly. Scales flexible, not elastic. B. B. exfoliates, whitens, gives off water, and fuses at 4 to a blebby enamel; with cobalt solution a fine blue. A mean of two analyses by Mr. C. Newbery—

$\ddot{\mathrm{Si}}$ 49·04 $\dddot{\bar{\mathrm{Al}}}$ 45·98 $\dddot{\bar{\mathrm{Cr}}}$, $\dot{\mathrm{Fe}}$, $\dot{\mathrm{Mg}}$, $\dot{\mathrm{Na}}$, *tr.* $\dot{\bar{\mathrm{H}}}$ 4·30=99·32,

giving the oxygen ratio of $\dddot{\bar{\mathrm{R}}}$, $\ddot{\mathrm{Si}}$, $\dot{\bar{\mathrm{H}}}$=5 : 6 : 1.

Herschelite is found at a quarry of basalt near the river Yarra at Richmond. Several interesting figures of its crystals are given. It occurs with phillipsite, analcite and calcite. It is also found in basalt at the shaft of the Ballarat and Clunes Gold-Mining Co., Clunes.

Struvite in crystals occurs in guano, in the Skipton Caves near Ballarat. The depth of the guano is about 20 feet, and it has been derived from the *excrements of bats.*

Tetrahedrite, Stibnite, Molybdenite, Brookite, Cassiterite, Scheelite, Sapphire, Topaz are among the other minerals of which mention is made in the memoir.

15. *Lavroffite* (Lawrowit), has been analyzed by Hermann (J. f. pr. Ch., 1870, II, i, 444) and shown to be a vanadiferous diopside.

---

* In the analysis cited in the Mineralogy, $\dot{\mathrm{Mg}}$ 4·36, should be $\dot{\mathrm{Mg}}$ 4·56.

He obtained $\ddot{\mathrm{Si}}$ 53·65 $\dddot{\bar{\mathrm{Al}}}$ 2·25 $\dot{\mathrm{Fe}}$ 2·48 $\dot{\mathrm{Mg}}$ 16·00 $\dot{\mathrm{Ca}}$ 23·05 = 100, with 2·57 p. c. of hypovanadic acid.

16. *On Sellaite, a new native fluorid*, by Dr. STRUVER, Atti della R. Accad. di Torino, iv, 1868, 35.—This mineral was detected on a specimen of anhydrite from Geibroulà, in the State of Maggiore, in Piedmont. On the anhydrite there were also crystalline sulphur, dolomite, and rare twins of albite. It is tetragonal in crystallization, with I on 1 (∞P on P)=123° 30′, and *i-i* on *i*-3 (or ∞P∞ on ∞P3) =161° 34′. Cleavage parallel to I and *i-i* perfect. H.=5. G.=2·972 at 24° C. Fracture conchoidal. Luster vitreous. Colorless. Transparent. Powder white.

It is insoluble in water; also in acids, except concentrated sulphuric, which causes an evolution of fluohydric acid. Small fragments melt in the flame of a candle with intumescence. From the similarity of its reactions to those of fluorite, the author concludes as not improbable that the mineral is a monofluorid of magnesium, which contains 38·71 of magnesia and 61·29 of fluorine. In treating a small fragment of it with concentrated sulphuric acid, he obtained for the proportion of magnesium 39·64 p. c. The small amount of the mineral in hand prevented his making a complete analysis. The species is named after the distinguished crystallographer, QUINTINO SELLA.

17. *Ambrosite.*—C. U. SHEPARD describes under this name, in The Rural Carolinian, Feb., 1870, p. 311, a resin resembling amber, from the phosphatic formation near Charleston, S. C. It is yellowish-brown externally, and clove-brown within; feebly translucent; sinks slowly in water; and melts into a clear liquid at 460° F., after softening at a much lower temperature. It gives off much succinic acid long before it melts, and a dense yellow oil is volatilized on its fusion. It is very combustible, burns with a bright yellowish-white light and pleasant odor, and leaves no ash behind. The name is made from the two words *amber* and *rosin*.

18. *On the Guanape Island Guano and its minerals ;* by Prof. C. U. SHEPARD, (ib. May, p. 469).—Guanape island is two miles northeast of the Chincha Islands. A mineral occurring in balls and veins in the Guanape Guano, having H. =1·5, G. =2·3, and resembling a little the red Cheshire rock-salt, but rhombic instead of cubic in cleavage, is called *Guanapite* by the author. He found it to consist of sulphate of potash 67·75, sulphate of ammonia 27·88, oxalate of ammonia 3·75=99·38. It loses ammonia slightly on exposure to the air.

Another substance from the Guano is named *Guanoxalite.* It contains sulphate of potash 40·20, oxalate of ammonia 29·57, water 30·46 =100·23. It occurs as pseudomorphs of a bird's egg, having the size and shape of that of a domestic duck. Color white exteriorly, but from the presence of the altered shell. Within, the material is foliated with a rhombic cleavage; a cream-white color; somewhat pearly luster; feeble translucence; hardness below 2, and G. =1·58. When heated it swells up, blackens, partially fuses, gives off copious fumes of ammonia, and leaves a white residue of sulphate of potash.

In the cavities of the spinal column and the stomach of the remains of birds from the Guano, yellow crystals of the mineral *Taylorite* occur, a species first noticed by W. J. Taylor in the guano of the Chincha Islands. Along with the Taylorite there are numerous minute scaly crystals of *Aphthitalite;* also oxalate of ammonia, which Prof. Shepard names *Oxammite. Oxalate of lime* is occasionally observed in crystals; also phosphate of ammonia (*phosphammite* of Shepard) in crystals and lumps; and, as an efflorescence from this mineral, biphosphate of ammonia (*Biphosphammite* S.).

III. BOTANY AND ZOOLOGY.

1. *Miscellaneous Botanical Notices and Observations.*

The Code of Botanical Nomenclature, as digested by M. Alph. DeCandolle and adopted by the International Congress at Paris in 1867, while on the whole approved by botanists, has been variously criticised in certain particulars, some writers naturally objecting to one article, some to another. To the more important of these criticisms DeCandolle made reply at a meeting of the Botanical Society of France on the 26th of February, 1869. The number of the *Bulletin* which contains his communication was issued, we believe, at the beginning of the present year. M. DeCandolle intimates that he passes by the still controverted question as to the proper mode of citing the authority for species where they have been transferred to another than the original genus, as being one upon which further discussion could hardly be more than iteration. He merely remarks that the more experience we have of the working of the system of double citation of authors, whether with or without parenthesis, the more grave do the inconveniences appear. Since we cannot readily make a brief abstract of the several points which he does discuss, we will now notice only those upon which this Journal ventured to differ from M. DeCandolle and the Congress. The chief serious objection was raised upon article 50, which prescribed the mode in which unpublished names, taken up by an author, should be subsequently cited. We remarked that: "For instance, there may be no necessity for taking up one of Commerson's names affixed by him to his plants in herbaria; but if taken up, simple verity would seem to require this botanist's name to be cited. We should feel bound to write '*Flacourtia* Commerson,' although published by L'Heritier or Jussieu, who probably supplied the character. The rule as proposed would apply to names communicated with manuscript characters by one botanist to another, as well as to named specimens. Now, no botanist is bound to do the work of publication for another; but if he chooses to do so, the maxim *qui facit per alias*, etc., must [conversely] fairly apply, and succeeding writers should not be required to take the godfather for the father. If we rightly understand the editor, he proposes that we should write *Eulophus*, *Leptocaulis*, and *Trepocarpus* DC., although the elder DeCandolle, accepting these names with the specimens from Nuttall, scrupulously attributes them to 'Nutt. in litt.' To us, all such names, which the elder DeCandolle has, at his own discretion, published for Nuttall, are of *Nutt. in DC. Prodr. &c.*"

To which M. DeCandolle now rejoins: . . . . "But see the inconvenience which results. All the catalogues or dictionaries, and all indexes have *Leptocaulis* Nutt. So we search the works of Nuttall, and perhaps even the small papers scattered in the journals, and lose much time and pains; for Nuttall never published this name. It may be that he would not have desired to publish it if he had examined the question subsequently. The date of the genus is that of the publication; and the publication is really the one essential thing; for what are the most important discoveries if not published? In writing *Leptocaulis DC. ex Nutt. litt.* the primitive author is equally indicated; but then one will see in the books *Leptocaulis DC.*, and any one will readily find in the works of DeCandolle the origin of the genus and the date of publication."

We are disposed to add that the information sought would be as readily found when written "*Leptocaulis Nutt. in DC.*," and that this more strictly as well as more tersely represents the fact than "*Leptocaulis DC. ex Nutt. litt.*" The full reference is "*Leptocaulis* Nutt. in DC. Prodr. 4, p. 107;" and the question is, whether in mere enumeration this is to be abbreviated into "*Leptocaulis Nutt*," or "*Leptocaulis DC.*" Either way, the work, as well as volume and page, has to be looked up, and the trouble of finding the origin of the name in the pages of the Prodromus may probably be far less than if Nuttall had published it directly in one of his various scattered papers. But let us in our turn test the new rule by the results which would ensue from its consistent application. To the first volume of the Flora of North America Nuttall contributed a large number of new species and genera, which were published for him by the authors of that work; but for which, if cited according to the new rule, they would seem to have incurred a responsibility much beyond what was counted on. They might well insist that the new mode of citation was misleading. Moreover, if "*Centrostegia* A. Gray, mss.," so published in Bentham's *Eriogoneæ* contributed to DeCandolle's Prodromus, is briefly to be cited "*Centrostegia Thurberi* Benth. in DC. Prodr," must not *Eriogonum Douglasii* and a score of other such species on the same principle be cited as *E. Douglasii* Alph. DC., &c.? for we should search the works of Bentham in vain for these names, losing much time and pains. It will of course be said that the "auctor Bentham" at the top of the page explicitly indicates the actual author; but so equally does the "A. Gray mss." at the head of that article. To take another instance: upon this plan we may be required to write "*Eliottia* Elliot" (Sketch, 1, p. 448). Elliott himself wrote "*Elliottia* Muhlenberg," adding, "I have inserted it, as requested by Dr. Muhlenberg, under this name." He adds to the specific name "Muhl. Cat." indeed. But the context shows that the second edition of Muhlenberg's Catalogue was not then published; it is doubtful if it was so when that part of Elliott's first volume was issued; and if it were, the enumeration in the catalogue, according to the Code, is not tantamount to publication. There are other well-known cases of a similar sort, in which an author would become liable to the imputation of dedicating a genus to himself.

When we consider how punctilious the elder DeCandolle was in this respect and others of a similar sort, and how generally he has been followed, we must regard the change proposed in the code and exemplified in some (but not all) of the later volumes of the Prodromus as an innovation rather than the declaration of an existent common law, or a natural development of it. The laudable end sought, however, in this and in article 42, is to obviate or diminish grave and growing inconveniences which arise from imperfect publication and merely recorded names in collections and herbaria. And here we do not complain that the rights of publication through the distribution of specimens, conceded in art. 42, are seemingly very much restricted by the new commentary. The duty, or perhaps comity, of respecting imperfectly published or unpublished names is only one of imperfect obligation, as the moralists would say, and therefore not to be fixed by law but governed by discretion. Legal rights begin only with publication, no matter by whose agency. We would only urge that subsequent citation, purporting to indicate the origin, should not in effect misquote the record.

Baillon's Histoire des Plantes, briefly noticed in our January number, goes on well. We have received two more monographs published since the year began; one of the *Proteaceæ*, the other and much larger one of the Papilionaceous *Leguminosæ*. The paper and print are most excellent and attractive: so also are the wood-cuts, as is usual in French publications of this kind. We notice that an English translation is announced. Considering the high price and generally limited sale of botanical books in England we should not expect it would pay. But, if it could ever be finished upon the plan adopted, it would be a proper companion and complement to LeMaout and Decaisne's *Traité General de Botanique*, of which an English edition is now in preparation under the best auspices. The latter copiously illustrates the families, and barely enumerates some of the principal genera. The former illustrates the tribes, &c., by a general history of the structure of some leading representatives, cast in a popular and readable form, and adds the characters of the genera, in the ordinary technical form and in the Latin language.

An attempted Improvement in the Arrangement of Ferns and in the Nomenclature of their Subdivisions, is the title of a pamphlet by the Rev. Prof. Hincks of the University of Toronto and President of the Canadian Institute; a popular account of the structure, fructification, and fertilization of this group of plants, which he regards as an alliance (*Filicales*), containing the three orders, *Osmundaceæ*, *Cyatheaceæ*, and *Polypodiaceæ*, founded on the sporangia, the *Ophioglorsaceæ* being excluded by their "straight æstivation" [vernation] and referred, as is natural, to the *Lycopodiaceæ.* The tribes of the first two filical orders three each, of the *Polypodiaceæ* four times three, or three for each division or subdivision, founded on the sori. For genera he is disposed to make more of the venation than is the present mode, and less of stipes and articulation, and so discards *Phegopteris.* Finally, he takes up the questions relating to *Aspidium,* both as to generic comprehension and nomenclature, and proposes to keep *Lastrea* and *Polystichum* as genera.

The Genus Hydrolea is reviewed by A. W. Bennett in the 53d number of the Journal of the Linnean Society (May, 1870), and the species are brought up to thirteen, of which three are here established. That *H. affinis*, described in the last edition of Gray's Manual, should have been overlooked is not to be wondered at; but specimens of it doubtless occur in the Kew herbaria, probably confounded with *H. quadrivalvis.* Mr. Bennett sides with Choisy in keeping up the order *Hydroleaceæ,* as distinct from *Hydrophyllaceæ,* "most clearly," on account of "the bilocular many-seeded ovary, axile placentation, and leaves invariably simple." These characters come down to two, the indefinitely numerous ovules and the two-celled ovary. But the new Californian genus *Draperia* Torr. stands in the gap, having the two-celled ovary as well as the habit of *Nama,* with the geminate ovules of proper *Phacelia.*

Tampico Jalap has within the last few years come into the markets as a distinct sort, and "although less rich in resin and less purgative than true jalap, yet on account of its lower price it has found a ready sale." Mr. Daniel Hanbury has traced the article to its source, in the root of a new *Ipomœa, I. simulans* Hanbury, described and figured in the 53d number of the Journal of the Linnean Society, which inhabits the Sierra Gorda, etc., in Mexico.

A Revision of the Flora of Iceland is the title of an extensive paper in the same Journal, by Professor Babington. The catalogue of Phænogamous and Acrogenous plants reaches 467 species. Although Iceland touches the arctic circle and is largely occupied by mountains, "many of which rise to the height of 6000 feet and are covered through fully their upper half with perpetual ice and snow, from whence extensive glaciers descend almost to the level of the sea," yet the climate is rendered so mild comparatively by the influence of the Gulf Stream which washes the coast, and the rain-fall and prevailing cloudiness in summer are so great, that, while on the one hand there is no proper forest, on the other "only 62 species are found which do not form part of the British Flora," and not more than three are decidedly arctic, viz., *Gentiana detonsa, Pleurogyne rotata,* and *Epilobium latifolium.*" All three of these come down to comparatively low latitudes and low levels in North America. A plant of much interest, and which should be thoroughly compared with that so named in North

America, is *Platanthera hyperborea*, of which the present writer craves Icelandic specimens.

The Fertilization of various Flowers by Insects is the title of an original paper, contributed by Dr. Wm. Ogle to the April number of the Popular Science Review. Of the various mechanical contrivances described, as also of the views suggested, some are not novel, the writer, as he states, not being familiar with the botanical literature of the subject (and this is most evident): but the whole article is well worthy of much attention, and would have been most interesting to the general reader—the narrative being remarkably clear—except that it is so disfigured by typographical errors as to be almost unintelligible to ordinary readers; indeed, the proofs would seem not to have been revised at all. The most interesting and original portion is that which relates to the fertilization in the Heath family, and to the use of the awns or appendages on the back of the anthers in Heaths, Uva-Ursi, Vaccinium, &c., by which the bee in sucking the flower topples or moves the anther, so that the pollen falls out of the terminal openings upon the insect's head. Bean-flowers and their fertilization, as also in related papilionaceous blossoms, are well described; and it is noticed that, while some bees have learned to get at the nectar feloniously by making a hole in the side of the calyx-tube, and others enter the regular way, and so do their proper work, an individual bee, visiting a succession of bean-flowers, keeps persistently to the one or the other plan. "It would thus appear that the habit is not an instinct, belonging by inheritance to the whole species, but is in each case the result of individual experience. As with the same experience some bees have acquired the habit and others have not, we must admit not only that these insects are intelligent, but that they differ from each other in their degrees of intelligence, some being slow in acquiring knowledge, others quicker." Perhaps the knowing ones have inherited the knack, in which case it is instinct in them, after all. Instinct, briefly defined, is congenital habit.

Eléments de Botanique et de Physiologie Végétale, suivis l'une petite Flore simple et facile pour aider à découvrir les noms des Plantes les plus communs du Canada: par l'Abbé Ovide Brunet, Prof. Bot. à l'Université Laval. Quebec, 1870.—A very neat little book, prepared by Prof. Brunet, as a sort of first book in Botany for Lower Canada, where French is still the language of instruction. It is illustrated by wood-cuts mostly from very good drawings by the author's own hands; and the structural and physiological part, and also the little flora, seem to be as clear as they well can be in the diminutive space they occupy; excepting that *Abies Canadensis* is put with Balsam-Fir into a genus in which the scales are said to fall from the axis, which is an obvious oversight. It is curious to see what the plants are which are taken as the commonest in Lower Canada, and what the popular names are. For instance, *Sarracenia* is known as "Petit-cochons;" Wood-sorrel as "Alléluia;" *Impatiens fulva* as "Chou-sauvage;" *Hama-*

melis as "Café-du-Diable;" Cranberry as "Atoca," but *Vacinium Vitis-Idœa* as "Pommes-de-terre."

The Herbarium of the late Von Martius has been purchased for 30,000 francs by the Belgian Government, to form the nucleus of a national collection at Brussels.

The Michaux Grove Oaks.—This name is to be applied to a collection of Oaks to be planted in Fairmount Park, Philadelphia, "in which, if practicable, shall grow two oaks of every kind that will endure the climate," in commemoration of the younger (F. A.) Michaux, author of the Sylva Americana, and his father, author of *Histoire des Chênes de l'Amerique.* The grove is to be maintained by the income of a legacy of $6000 bequeathed by the younger Michaux to the American Philosophical Society, in trust for arboricultural purposes, which legacy is now devoted by the Society to this use.

Otto Bœckeler is publishing in the current volume of the *Linnæa*, a full account of the Cyperaceæ of the Royal Hebarium of Berlin, which now, in the fourth fasciculus reaching the *Scirpeæ*, is becoming of consequence to United States botanists. He describes several supposed new species of *Cyperus* and a few of *Eleocharis*, &c., and identifies several of ours with older species, making some changes of nomenclature. By what may be a good hit he refers the inner squamula of *Hemicarpha* to an abortive stamen, and so refers the species to Scirpus, ours becoming *S. micranthus* Vahl.

Generis Astragali Species Gentogææ pars altera.—The first part of this monograph by Prof. Bunge of the Old-World species (almost a thousand in number) of the great genus *Astragalus*, was mentioned in our January number. We have now received the completion, containing the systematic enumeration of the species, with characters and descriptions of most of them, and the synonymy. The work is published by the Imperial Academy of St. Petersburgh, this part being the initial number of the 15th volume of its *Mémoires*, pp. 254.—The succeeding number, of over 300 pages, consists of part first of

Flora Caucasi, by Rupprecht, with six plates. It carries the work from *Ranunculaceæ* down to *Vitaceæ.* The species are described or annotated at large, but not by definite technical characters, and vast observation and learning are exhibited, along with a liking for multiplication of genera. Dr. Rupprecht considers the name *Viola umbrosa* of Fries (1828) to have precedence of *Viola Selkirkii* Pursh ex Goldie, 1822, on the ground that the latter name was not published until 1833 by Hooker. But in Goldie's paper (which indeed was drawn up by Hooker) there is a good Latin diagnosis of the species followed by almost half a page of detailed description in English; in fact, hardly any species has been more effectually published.

Mr. Bentham's Presidential Address at the anniversary meeting this year is somewhat briefer than usual, but not less likely to attract attention. It is mainly devoted to two topics: 1, the results obtained from the recent explorations of the deep-sea faunas; and

2, those from the investigation of the tertiary deposits of the arctic regions, "tending both of them to elucidate in a remarkable degree one of the most important among the disputed questions in biological history, the continuity of life through successive geological periods." After briefly indicating where the principal data are recorded, Mr. Bentham continues:

"It would be useless for me here to retrace, after Dr. Carpenter and Prof. Verrill, the outlines of the revolution which these marine discoveries have caused in the previously conceived theories, both as to the geographical distribution of marine animals, and the relative influences upon it of temperature and depth, and as to the actual temperature of the deep seas, or to enter into any details of the enormous additions thus made to our knowledge of the diversities of organic life; and it would be still further from my province to consider the geological conclusions to be drawn from them. My object is more especially to point out how these respective dips into the early history of marine animals and of terrestrial forests have afforded the strongest evidence we have yet obtained, that apparently unlimited permanency and total change can go on side by side, without requiring for the latter any general catastrophe that should preclude the former.

"There was a time, as we learn, when our chalk-cliffs, now high and dry, were being formed at the bottom of the sea by the gradual growth and decay of Globigerinæ and the animals that fed on them —amongst others, for instance, *Rhizocrinus*, and *Terebratulina caput-serpentis;* and when, at a later period, the upheaval of the ground into an element where these animals could no longer live arrested their progress in that direction, they had already spread over an area sufficiently extensive for some part of their race to maintain itself undisturbed; and so, on from that time to the present day, by gradual dispersion or migration, in one direction or another, the same *Rhizocrinus* and *Terebratulina* have always been in possession of some genial locality, where they have continued from generation to generation, and still continue, with Globigerinæ and other animals, forming chalk at the bottom of the sea, unchanged in structural character, and rigidly conservative in habits and mode of life through the vast geological periods they have witnessed. So also there was a time when the hill-sides of Greenland and Spitzbergen, now enveloped in never-melting ice and snows, were, under a genial climate, clothed with forests, in which flourished *Taxodium distichum* (with *Sequoiæ*, *Magnoliæ*, and many others); and when at a later period these forests were destroyed by the general refrigeration, the Taxodium already occupied an area extensive enough to include some districts in which it could still live and propagate; and whatever vicissitudes it may have met with in some parts, or even in the whole, of its original area, it has, by gradual extension and migration, always found some spot where it has gone on and thriven, and continued its race from generation to generation down to the present day, unchanged in character, and unmodified in its requirements. In both cases, the permanent animals of the deep-sea bottom and the permanent trees of the terrestrial forests have witnessed a more or less partial or complete change in the races amongst which they were commingled. Some of these primitive associates, not endowed with the same means of dispersion, and confined to their original areas, were extinguished by the geological or climatological changes, and replaced by other races amongst which the permanent ones had penetrated, or by new immigrants from other areas; others, again, had spread like the permanent ones, but were less fitted for the new conditions in which they had become placed, and in the course of successive generations have been gradually modified by the Darwinian process of natural selection, the survival of the fittest only among their descendants. If, in after times, the upheaved sea-bottom becomes again submerged, the frozen land becomes again suited for vegetation, they are again respectively covered with marine animals or vegetable life, derived from more or less adjacent regions, and more or less different from that which they originally supported, in proportion to the lapse of time and extent of physical changes which had intervened. Thus it is that we can perfectly agree with Dr. Duncan, that 'this persistence (of type and species through ages, whilst their surroundings were changed over and over again) does not indicate that there have not been sufficient physical and biological changes during its lasting to alter the face of all things enough to give geologists the right of asserting the succession of several periods;' but we can, at the same time, feel that Dr. Carpenter is in one sense justified in the proposition, that we may be said to be still living in the Cretaceous period. The chalk formation has been going on over some part of the North Atlantic sea-bed, from its first commencement to the present day, in unbroken continuity and unchanged in character."

A portion of this address will probably astonish the vegetable paleontologists, excepting perhaps, Mr. Lesquereux, whose cautious language, and his statement in this Journal that, properly speaking, no *species* can be established from leaves or mere fragments of leaves, are commended. For the President of the Linnæan Society avows himself wholly skeptical as to the "New Holland in Europe" of eocene times, denying the existence of a single specimen out of the nearly one hundred supposed tertiary species which a modern systematic botanist would admit to be Proteaceous, unless received from a country where *Proteaceæ* were otherwise known to exist. The grounds of this opinion are given in detail.

The Student's Flora of the British Islands, by J. D. Hooker, C.B., &c., just issued by Macmillan & Co., comes to take the place of Hooker & Arnott's British Flora, now out of print and antiquated, as that took the place of Sir William's British Flora which did such good service in its day and in its five editions. In these a goodly octavo, then a thick duodecimo, it now becomes a compact 16mo, a very "handy book," which may be carried in the pocket; but this compactness is not at the expense of either clearness or fullness. "The object of this work," so the preface opens, "is to supply students and field-botanists with a fuller account of the plants of the British Islands than the manuals hitherto in use aim at giving." The body of the volume, exclusive of Synopsis and Index, is contained in 474 pages; the species appear nearly to average three to the page. We are glad to see that orthodox specific characters are kept up, and that they are specific characters, diagnoses, not descriptions. Descriptive matter follows and, with habitat, range in elevation, and distribution in general, forms a secondary paragraph of much greater extent than the diagnosis; and then "*sub-species*" are numerous and fully characterized, as are occasionally varieties of second order under them. The species are naturally arranged, under sections when needful; but keys to the species are not given, the author "finding from experience that such keys promote very superficial habits amongst students,"—which they certainly do. As the book is wholly new, and the descriptions all at first hand, from living plants and dried specimens, a good many slips and oversights are inevitable. With the longest experience and great painstaking these cannot be avoided, but have to be weeded out at leisure. Here where a British Flora is only incidentally used, we have no call to point out such as we have noticed, most of which are sure to be rectified in the new edition, which must needs be promptly called for. But we will venture to find fault with one typographical blemish, as we think it, viz., the insertion of the accent mark between the letters in generic and specific names, so as to display an unsightly cleft in the middle. Surely the London printing-houses have, or can have, the vowels with accents cast upon the type; and there are ways of indicating whether the vowel be long or short, if that be worth the while. This whole business of accentuation is, no doubt, of secondary consequence, and plainly is not so much considered by English botanists of the present day as by those of a past generation. But ordinary students and amateurs may fairly ask for guidance; and those who have occasion to coin many botanical names are bound to consider in advance how they must needs be pronounced. Artificial keys to the natural orders are liable in some degree to the same objection as are keys to species; but they are almost indispensable to young students; only they must be made with extreme care, and every deviation or exception provided for. A synopsis of the natural orders, such as Dr. Hooker has prepared, guarded by mention of exceptions and qualifications, has its own advantages, and where the number of orders is not larger than in Britain may perfectly serve the purpose. A. G.

The American Entomologist, now published at St. Louis, has this year added the words "and Botanist" to its title, and Dr. George Vasey has taken charge of the Botanical Department. He has entered upon his duties with promising vigor, and is producing a series of popular botanical articles which are likely to be widely read throughout the western country and to be very useful.

In the combined July and August number the editor has a spirited article upon the Origin of Prairie Vegetation, which is mainly a criticism of the writings of Prof. Winchell upon this subject in his "Sketches of Creation." If it is seriously maintained in that work that seeds buried in diluvial deposits may have retained their vitality during the glacial period, and by their germination after it "reproduced the flora of the præ-glacial period," we can only wonder that this immense extension of the hypothesis of the indefinite vitality of buried seeds should have been made at a time when most biologists probably doubt whether any seed ever preserved the power of germination for a century or two. A. G.

2. *Carbolizing Birds;* by H. W. Parker. (Communicated by the Author.)—The following methods, carefully studied for two years, with results noted, are recommended for the saving of birds in warm weather until the operator finds time to skin them; for the permanent preparation of drawer specimens, where the student needs a large series of individuals to determine the variations and limits of species; and for mounting small birds, at least as temporary representatives, when neither the time nor the expense involved in the old methods can be afforded.

The viscera are removed, to effect which neatly the legs are pinned widely apart, and a paper several times folded is pinned over the tail in the direction whither the viscera are drawn out. With proper care, the sex is readily observed. A wad of cotton absorbs the fluids remaining in the cavity. The leg is then grasped close to the body, and a knife or wire is introduced into the cavity and run down into the flesh of the leg, working the instrument around, but not so as to break the skin. For a small bird, five to ten drops of the commercial fluid preparation of carbolic acid is made to anoint the whole interior, and to penetrate the leg by stretching and relaxing the same in proper position. The application is repeated after the first drops are absorbed; and a wad of cotton, wet with the acid, may be left close under the breastbone next to the neck. The cavity is then filled with cotton and the skin drawn back into place. The inside of the mouth is well anointed, and a saturated wad of cotton pushed down the whole length of the neck. The eyes are removed by a hooked wire inserted into the ball, the head being so held that the humors of the eye will drop without soiling the lids. The moist lids are left as open as possible, and the specimen placed in a cool cellar till the next day, when the lids are dry enough to take their open shape. Then a nail is inserted through the lids and pushed through the bone at the back part of the orbit into the brain, and so worked as to make a good opening. A tightly rolled bit of cotton, saturated with the acid, is pushed into the brain and worked around in it, care being taken not to wet the eyelids. If by chance the feathers are wet, the acid can be removed by powdered chalk, repeatedly applied.

Specimens so prepared in warm weather, can be skinned a week or two after, if kept boxed in a cellar. No smell of decomposition is observed; the acid gradually and completely penetrates the pectoral muscles; the skin is strong and the feathers not loosened.

For permanent preparation, the skin should be laid open from the abdomen to the neck, the pectoral muscles removed and replaced by cotton, and the incision sewed up. The throat, neck and orbits are also filled with cotton. The specimen should then be suitably arranged, encircled by a slip of paper, and placed on a bed of cotton. Before this, the flesh of the wings should be laid open and arsenic applied in the usual manner.

For mounting, it only needs to run one wire through the foot, tarsus, and so on through the neck to the forehead, and another wire through the other foot to any point in the back or breast where the end of the wire catches firmly. Papers or strings for keeping the feathers in place should remain long. Some shrinking about the head and neck will eventually follow in the case of many birds, particularly those of the smallest size or of scanty, or close, plumage; but in other instances no shrinking whatever can be noticed after more than a year of drying. The cabinet in which they have been set up is made insect-proof by means of pasted cloth and paper, putty and paint, fifteen inches passage way being left in front of the shelves and the only access being through a tight door at one end, fastened by a screw.

Travelers, who desire to collect a large number of birds for comparison, will find this method one of great advantage; and the specimens will be better for study than skins, inasmuch as the proportions will be better preserved. Small mammals can be kept some days for skinning by a similar process, and an opening into the brain may be made through the roof of the mouth, if preferred. A Fox Squirrel, so treated, was in good condition for skinning after four day's preservation, in very warm weather.

Iowa College, July, 1870.

This with all similar methods of preparing permanent specimens, without skinning, has been found to be of comparatively little use in the damper air of the Eastern States, especially near the coast, where all dried preparations are so liable to mould and decay.—Eds.

3. *A Synopsis of the Family Unionidæ;* by Isaac Lea, LL.D., Vice-Pres. Amer. Phil. Soc., &c. 4th ed., very greatly enlarged and improved. 184 pp. 4to. Philadelphia, 1870. (Henry C. Lea).—Dr. Lea has given further completeness to his labors on the Unionidæ by preparing and publishing this fourth edition of his Synopsis. During the eighteen years that have elapsed since the issue of the preceding edition, the number of known species has much increased, and various corrections of former determinations have been made. The subject of the arrangement of the species in genera is discussed in the earlier part of the volume, and then tables are given, with very full synonymy and numerous annotations. The table of geographical distribution, which next follows, is very much enlarged. The volume closes with an Index of all the names of species and a statement of the place of publication of each, and finally a long Bibliography.

4. *Commensalism among Animals.*—Van Beneden, at the meeting of the Royal Academy of Belgium, on the fifth of March last, continued his observations on commensalism, or the living together of different species of animals. He first cited the following facts which he had received from Mr. Al. Agassiz. A species of *Lepidonotus*, of California, is always found attached on an *Asteracanthion* (*A. ochraceus* Brandt) near its mouth, on different parts of the ambulacral rays, sometimes five of them on a single individual. A small fish of the genus *Clupea* is often found lodged in the folds of the fringes of a species of Pelagia (*Dactylometra quinquecirra* Al. Ag.) A species of *Hirudinea* lives within the cavity of a Beroë (*Mnemiopsis Leydii*) of Buzzard's Bay, the Beroë never being seen without four or five of these worms, and often harboring seven or eight. Between the buccal fringes of the large Medusa *Cyanea arctica* (as stated in the Seaside Studies), a species sometimes 7½ feet in diameter, there lives an Actinia (*Biccidium* of L. Ag.); three to five of these Actiniæ reside commonly on each Cyanea. In an *Aurelia* of our coasts, a large number of Crustacea of the family Hyperina are often harbored. Another interesting fact is the commensalism of the young Comatulæ on the adult. The young of the species of the coast of South Carolina attaches itself to the basal cirri, and they live like a small colony of young Pentacrini. A species of Planaria, the *P. angulata* Müller, lives always in free "commensal" on the under surface of the American Limulus, near the base of the tail.

Van Beneden next speaks of the relation of the siliceous *Hyalonema* to the associated polyps (a species of *Polythoa*), and makes it as a case of commensalism of polyps on a sponge. He adds the fact that Mr. Oscar Schmidt has found in the Adriatic a *Polythoa* living on a species of sponge of the genus *Axinella*. He closes with repeating an opinion he had before expressed (in the *Zool. Médicale*, published long since by him in connection with *P. Gervais*) that sponges are only polyps of extreme simplicity, in which the active part is reduced to a membranous tube without tentacles about the orifice.—*L'Institut*, July 20.

IV. ASTRONOMY.

1. *Discovery of a new Asteroid, the* 111*th;* by Dr. C. H. F. Peters. From a communication by the author to one of the editors, dated, Litchfield Observatory of Hamilton College, Clinton, N. Y., Aug. 16, 1870.—I take pleasure in forwarding the first observation made upon an asteroid, the 111th of the group, discovered here night before last, viz:

	h m s		h m s	° ′ ″	
1870. Aug. 14.	12 38 31 H. C. m. t.	A.R. =	21 25 20·21	Decl. = −13 10 5·4	10 Comp.
" 15.	9 5 40 " "		21 24 32·88	−13 12 29·6	5 "

The planet was of about 11½ magnitude.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Nineteenth Meeting of the American Association for the Advancement of Science, held in Troy, New York, August* 17–25, 1870.—The meeting of the American Association which has just closed at Troy, was, in point of interest and value, one of the best in its history. Owing to the continued illness of President William Chauvenet of St. Louis, the duties of President were performed by the Vice President, Dr. T. Sterry Hunt, of Montreal. And in the absence of Professor C. F. Hartt of Ithaca, the General Secretary, now in Brazil, Professor F. W. Putnam of Salem was elected to that office for the session. The attendance at the meeting was good, the names enrolled upon the Treasurer's book at the close of the session being about two hundred. The titles of papers entered with the Permanent Secretary numbered one hundred and forty-three. And, though there was no one subject of great general interest under discussion, as at Chicago and Salem, yet the absolute value of the papers read at Troy was quite equal to that of those presented at either of the meetings mentioned.

Of the papers read in General session, that by Lieut. C. E. Dutton, U. S. A., upon the Chemistry of the Bessemer Process, deserves particular mention. It was an admirable analysis of the process itself, and also of the successive stages which are noticed in the conversion. An abstract of the paper, by Professor Barker, is deferred to our next number.

The address of Col. J. W. Foster, the retiring President, was delivered on Thursday evening in the First Presbyterian church. It was upon "The Latest Investigations in Geology and Archæology, with special reference to the condition of Pre-historic man."

Among the various objects of interest in and about Troy, none attracted more attention than the various Iron-works. The proprietors of the Burden, the Albany, and the Rensselaer Iron-works, and of the Bessemer Steel-works provided special facilities for their inspection. Mr. A. L. Holley, to whose mechanical skill the wonderful machinery of the latter establishment is due, was particularly attentive and courteous. The U. S. Arsenal at Watervliet, the Rensselaer Institute at Troy, and many other places of interest received also their share of attention.

Two excursions were made from Troy; one to Albany by invitation of the Albany Institute, the other to Saratoga by invitation of the citizens of Troy. The time in Albany was divided between the Dudley Observatory and the Geological Hall. A sumptuous entertainment at the State Library closed the day's festivities. The Saratoga excursion occupied an entire day, dinner being served at Congress Hall at 4 P. M. The Association was the recipient also of various private hospitalities.

The Association voted to hold its next meeting in Indianapolis, Indiana,—in accordance with an invitation from that city—on the 17th of August, 1871. A committee was also appointed to arrange for the following meeting in San Francisco in 1872, upon invitation of the California Academy of Natural Sciences. The following officers were elected for the next meeting:

President, Prof. ASA GRAY, of Cambridge; Vice-President, Prof. George F. Barker, of New Haven; General Secretary, Prof. F. W. Putnam of Salem; Treasurer, Mr. Wm. S. Vaux, of Philadelphia.

The following is a list of the papers presented:

1. IN GENERAL SESSION.

1. On the Chemistry of the Bessemer Process; CLARENCE E. DUTTON.
2. Mrs. Willard's Theory of Circulation by Respiration; Mrs. A. L. PHELPS. (Read by J. G. Morris.)
3. Last Winter's occupation of Moosilauke Mountain in New Hampshire; J. H. HUNTINGTON.

2. IN SECTION A.

Mathematics, Physics, and Chemistry.

1. Some new applications of the Graphical Method; EDWARD C. PICKERING.
2. On Dispersion and the possibility of attaining perfect Achromatism; EDWARD C. PICKERING.
3. An Examination of the Doctrine of Atomicities; F. W. CLARKE.
4. The Isothermals of the Lake Region; A. WINCHELL.
5. A Description of a new apparatus for illustrating the Precession of the Equinoxes; JAS. BUSHEE.
6. The Magnetic Wells of Michigan; A. WINCHELL.
7. To whom is due the credit of the most important application of Steam as a Motive Power; CLINTON ROOSEVELT.
8. On Methods of illuminating optical Meteorology, particularly the formation of Halos and Coronæ, according to the theory of Bravais; JOSEPH LOVERING.
9. Researches in Electro-Magnetism; ALFRED M. MAYER.
10. Abstract of a research on a simple method of measuring Electrical Conductivities by means of two equal and opposed Magneto-electric Currents or Waves; ALFRED M. MAYER.
11. Unpublished experiments of Prof. W. B. Rogers on the Influence exerted by the presence of Carbonic Acid in gas, on its Illuminating Power; FREDERICK E. STIMPSON.
12. A Graphical Discussion of the various formulæ proposed for the relation between the quantity of light produced by the combustion of Illuminating Gas and the volume of gas consumed; FREDERICK E. STIMPSON.
13. Aurora Borealis; L. BRADLEY.
14. Cosmogony; L. BRADLEY.
15. The Northers of Texas; SOLOMON SIAS.
16. The Connection between Solar Spots, Terrestrial Magnetism, and the Aurora Borealis; ELIAS LOOMIS.
17. A Theory of the Constitution of the Corona of the Sun; SIMON NEWCOMB.
18. On the Assumption that Matter is Impenetrable; H. F. WALLING.
19. Elasticity, a mode of Motion: H. F. WALLING.
20. The conditions of Stable Equilibrium in Atomic Orbits: H. F. WALLING.
21. Spectroscopic examination of the Bessemer Flame; J. M. SILLIMAN.
22. Description of a new Meteorograph, for the automatic registration of Meteorological phenomena; G. W. HOUGH.
23. Remarks on the total fluctuation of the Barometric Column; G. W. HOUGH.
24. Relations existing between temperature, pressure, wind, and rain-fall, as indicated by automatic registering instruments; G. W. HOUGH.
25. On the rate of the Dudley Observatory Sidereal Clock for two years; G. W. HOUGH.
26. On a new form of mercurial horizon, by which vibrations are extinguished; J. H. LANE. (Presented by J. E. Hilgard).
27. Description of Batchelder's Arctic Tide-Gauge: J. E. HILGARD.
28. On proposed improvements for Common Roads; S. D. TILLMAN.
29. On a new Musical Notation; S. D. TILLMAN.

30. On improvements in Inland Navigation; S. D. TILLMAN.

31. On improved facilities for transmitting heat from one fluid to another; S. D. TILLMAN.

32. Gaseous and Liquid Rings; E. N. HORSFORD.

33 On the possibility of a Limit of Visible Magnitude; F. A. P. BARNARD.

34. On the brightness appearing on the limb of the Moon's Image, in Photographs of Solar Eclipses; F. A. P. BARNARD.

35. On the testimony of ancient Eclipses in regard to the Uniformity of the Earth's Rotation; J. N. STOCKWELL.

36. The Discovery of the force which originally imparted all their motions to all the stars; JACOB ENNIS.

37. On the Survey of the Northern Heavens instituted by the German Astronomical Society; T. H. SAFFORD.

38. On a New Method of determining the Level-error of the axis of a Transit instrument; C. A. YOUNG.

39. On Solar Prominences and Spots observed with the Spectroscope during the past year; C. A. YOUNG.

40. Some account of progress in the investigation of the laws of Winds; J. H. COFFIN.

41. Abstract of a paper on Temperature for twenty-five years; O. W. MORRIS.

42. The solvent power of anhydrous liquid ammonia; CHARLES A. SEELY. (Read by Prof. Walling).

43. The inadequacy of the prevailing Baconian system by Induction, and the fallacy of the too exclusive use of the *a priori* method; F. L. CAPEN.

44. On an improved form of Solar Eyepiece; S. P. LANGLEY.

45. The universal method of approximation; THOMAS HILL.

46. Note on the involute of a circle, and the analytical value of the hyperbolic base; THOMAS HILL.

47. Molecular classification; GEO. F. BARKER.

48. On the latest discoveries in regard to the manufacture of Ice by mechanical power; P. H. VANDER WEYDE.

49. Further improvements in the method of transmitting, audibly, musical melodies by the electric telegraph wire; P. H. VANDER WEYDE.

The following papers were read only by title:—

50. On Elasticity as a Feature in Physics; S. J. WALLACE.

51. On the Advancement of the Sciences; CLINTON ROOSEVELT.

52. On the present aspects of Organic Physics; HENRY HARTSHORNE.

53. Suggestions for systematizing chemical nomenclature; A. M. EDWARDS.

54. The relation between the bands of the Spectroscope and the musical scale; P. H. VANDER WEYDE.

55. The most important result obtained from the researches during some years of travel, consists in the establishment of the following theory:

The law of gravity is not the motive power of cosmic bodies; A. HABEL.

56. Gravity is not the principal motive power on terrestrial bodies, but acting very secondary; A. HABEL.

57. Past and Future Astronomy; JOSEPH TREAT.

58. Demonstration of the Perturbation of Uranus, which discovered Neptune; JOSEPH TREAT.

59. Universal Mathematical Demonstration of the impossibility of Gravity, or the Attraction of Matter as Matter; JOSEPH TREAT.

60. Corollaries of the Milky Way; JOSEPH TREAT.

61. An account of an Experiment upon the Physiological Action of Nitrous Oxyd; F. W. CLARKE.

62. Acid reaction of Tribasic Phosphate of Lime; E. N. HORSFORD.

3. IN SECTION B.

Geology and Natural History.

1. On the Early Stages of Discina; E. S. MORSE.

2. On the Organization of the Brachiopoda, (Discina and Lingula); E. S. MORSE.

3. The Brachiopoda as a Subdivision of the Annulata; E. S. MORSE.

4. Observations on Seedling Compass-plants (Silphium laciniatum); THOS. HILL.

5. The Development of Limulus Polyphemus; A. S. PACKARD, Jr.

6. The Terrace Epoch in Michigan; A. WINCHELL.

7. On the Relation of Organic Life of the several continents to the Physical Character of those land areas; N. S. SHALER.

8. On the character of the observations necessary to interpret the record of the last Glacial Period; N. S. SHALER.

9. On a method of collecting certain Geological facts adopted by the Social Science Association; N. S. SHALER.

10. Notes on the Condors and Humming birds of the Equatorial Andes; JAMES ORTON.

11. On the evidence of a Glacial Epoch at the Equator; JAMES ORTON.

12. On the Homologies of the Cranial Bones of the Primary Types of the Reptiles; EDWARD D. COPE.

13. On the Reptiles of the Triassic formations of the United States; EDWARD D. COPE.

14. On the existence of two classes of male flowers in the common sweet chestnut, and the influence of nutrition on the sex; THOMAS MEEHAN.

15. On objections to Darwin's theory of Fertilization through Insect agency; THOMAS MEEHAN.

16. On the law of fasciated branches, and its relation to the law of sex in plants; THOMAS MEEHAN.

17. The supposed Elevation and Depression of the Continent during the Glacial period; J. B. PERRY.

18. The Boulder-Trains of Berkshire county, Mass.; J. B. PERRY.

19. The Development and Old Age of the Tetrabranchiate Cephalopods; ALPHEUS HYATT.

20. The Genetic Relations of the Arietes; ALPHEUS HYATT.

21. The porphyries of Marblehead; ALPHEUS HYATT.

22. Geology and Topography of the White Mountains, N. H.; C. H. HITCHCOCK.

23. Description of a new Trilobite from New Jersey; C. H. HITCHCOCK

24. The Distribution of Maritime Plants a proof of Oceanic Submergence in the Champlain Period; C. H. HITCHCOCK.

25. On the young of Orthagoriscus; F. W. PUTNAM.

26. On the salt deposits of Western Ontario; T. STERRY HUNT.

27. On Iron Sand Ores; T. STERRY HUNT.

28. Notes on Granitic Rocks; T. STERRY HUNT.

29. On the oil-bearing limestone of Chicago; T. STERRY HUNT.

30. On the Lignites of West America, their Distribution and Economic Value; J. S. NEWBERRY.

31. On the Sequence and Chronology of the Drift Phenomena in the Mississippi Valley; J. S. NEWBERRY.

32. On some new relics and traces of the Mound Builders; J. S. NEWBERRY.

33. On the relation of the Onedonta Sandstone and Montrose Sandstone of Vanuxem to the Hamilton and Chemung Groups; JAMES HALL.

34. Notice of the Fossil Plants of the Hamilton and Chemung Groups, with reference to the source of the sediments of these Formations; JAMES HALL.

35. Note upon the Rocks of the Huronian System on the Peninsula of Michigan; JAMES HALL.

36. Remarks on the occurrence of the genus *Dithyrocaris* in the Hamilton and Chemung Rocks of New York; JAMES HALL.

37. On the Geology of the Delta, and the Mudlumps of the Passes of the Mississippi. I. Geological Structure of the upper Delta plain. II. The lower Delta, and the Mudlumps of the Passes; E. W. HILGARD. (Read by J. R. Walker.)

38. Apatite Deposits of Lanark Co., Ontario, Canada; GORDON BROOME.

39. On some new generic forms of Brachiopoda, with remarks on some points of their structure; W. H. DALL.

40. On the order Docoglossa of Troschel; W. H. DALL.

41. On the nature of the foliage of Pines, etc.; a criticism; A. GRAY.

42. On the local Glaciers of the White Mountains; L. AGASSIZ. (Read by J. B. Perry.)

43. Notes on the relative age of the Niagara and so-called Lower Helderberg series; A. H. WORTHEN.

44. Probable Origin of the South Carolina Phosphates; W. C. KERR.

45. On some points in the Stratigraphy and surface Geology of North Carolina; W. C. KERR.

46. A point in Dynamical Geology; W. C. KERR.

47. Lava-ducts in Washington Territory; R. W. RAYMOND.

48. The Salt Marsh at Silver Peak, Southern Nevada; R. W. RAYMOND.

49. On Abnormal Vertebræ of the Elephant; SANBORN TENNEY.

50. On a new locality of Kyanite; SANBORN TENNEY.

51. On some points in the Geology of Eastern Mass.; SANBORN TENNEY.

52. Brief Notices on Hoosac Mountain and Tunnel; JAMES HYATT.

53. On the Relations of the orders of Mammals; THEODORE GILL.

54. On the subdivisions of the Branch Mollusca; THEODORE GILL.

The following papers were read by title only:—

55. Sketch of the researches made during seven years' travel through the five states of Central America, part of New Granada, the Republic of Ecuador and Peru; then on the Island of Chincha, and the Galapagos Archipelago; M. A. HABEL.

56. On the occurrence of native iron, not meteoric; H. B. NASON.

57. On the composition of the American Opium; H. B. NASON.

58. On parallel striæ in quartz crystals; L. FEUCHTWANGER.

59. Geology of the Cottonwoods Mining District of the Utah Territory; P. A CHADBOURNE.

60. The Geysers of Iceland and California; P. A. CHADBOURNE.

61. Guano, the origin of the Apatite of Rideau, Canada; E. N. HORSFORD.

62. Fresh Water Pond Overlying a Salt Water Pond in Middlesex county, Mass.; E. N. HORSFORD.

63. Evidence of glacial action in the placer and gulch gold of California; E. N. HORSFORD.

64. Lakes and Lake Regions; S. J. WALLACE.

4. IN SUB-SECTION C.

Microscopy.

1. Microscopic Circuits of Generation: *a.* Of Zymotic Fungus; *b.* Of the (nominal) Genera of Fresh Water Algæ, as development-phases of Bryaccæ, etc.; *c.* Of Vorticello-Planarians; T. C. HILGARD. (Read by J. E. Hilgard.)

2. On a new form of Binocular Microscope; F. A. P. BARNARD.

3. On the Structure of the scale of Podura plumbea; F. A. P. BARNARD.

4. On the Illumination of Binocular Microscopes, with proposal for a new Diaphragm-stop; R. H. WARD.

5. Remarks on Stereoscopic vision as applied to the Microscope; R. H. WARD.

6 Some remarks on Nobert's lines, with particular reference to Dr. Woodward's photographs; R. H. WARD.

7. Some remarks on a Pocket Microscope and Telescope combined; JOSIAH CURTIS.

8. Some remarks on two deposits of Diatomaceous earths recently thrown up by the Sea; EDWIN BICKNELL.

9. Remarks on a method of producing very low power with the microscope, with demonstration; EDWIN BICKNELL.

5. IN SUB-SECTION E.

Archæology and Ethnology.

1. Observations of the stone used by the Indians within the limits of Massachusetts in the manufacture of their implements, with some remarks on the process of manufacture; JAMES J. H. GREGORY.

2. The Substitution of General Laws for Special Legislation; GEO. A. LEAKIN.

3. On some peculiarities of the Grammar of the Eskimo Dialects; W. H. DALL.

4. Sign-Language as illustrative of the Laws of Written and Vocal Language; G. W. Samson.

5. Remarks on customs of the Waribiara and Blanco Indians in the vicinity of Chiriqui Lagoon, Central America; J. A. McNiel.

2. *On "Pure Scarlet;"* in a letter from Wm. H. Dall (letter to Prof. Silliman dated Smithsonian Inst., Washington, D. C., May 3, 1870.)—In 1863, being engaged in making anatomical drawings from living microscopic animals, I purchased among other colors, a cake made by Windsor and Newton of England stamped with the name "Pure Scarlet." It was of the finest and most vivid scarlet, far superior in brilliancy to any of the various shades of vermillion, &c., which are commonly sold in the shops.

I had, however, no occasion to use the color until 1865, when I colored a lithographic plate (on ordinary paper used for such purposes) of *Pentstemon Cerrosensis* Kellogg, putting in the darker shades with carmine.

In the summer of 1865, at Sitka, Alaska Terr., I made some drawings of Æolis and other nudibranchiate mollusks, which had streaks of bright scarlet upon various parts of their bodies and tentaculæ. These were sent by mail to the Smithsonian Institution where they remained until my return from the north in 1868.

In 1866 being in San Francisco I chanced to examine the plate which I had colored the winter before, and noticed a fading of the colors, but not considering the matter particularly, I recolored the plate with the same pigment and left it in perfect condition.

Having returned to Washington, during the spring of 1869, I used the cake of "pure scarlet" again, for coloring a map (upon stout, thin, map-paper) to show the distribution of the Indian tribes. The color was laid on thickly and uniformly over the entire surface of two or three of the divisions of the map. As this lay on my table for convenient reference I noticed that it began to fade rapidly, and in the course of a few months the paper was left perfectly white and clean, not a trace of the heavy, opaque pigment remaining. This recalled the fading of the colored plate to my mind, and upon examination I found that the latter was in the same condition, not a particle of scarlet remaining, and the only color upon the plate being the streaks of carmine which had been put on by way of shading and which were unchanged.

I had believed the drawing of the nudibranchs to have been lost, as I could find it nowhere, but now a more careful search revealed the fact that the scarlet having entirely faded away, I had failed to recognize it. It was made on the best rough drawing paper of English make.

An examination of the substance showed it to be soluble in iodid of ammonium and hyposulph. soda, and with nitrate of silver in solution it gave iodid of silver, hence I suppose it to consist wholly or in part of iodid of mercury. I enclose a fragment for examination. The supposition that it was acted upon by hyposulph. soda in the paper is doubtful, in view of the fact that three kinds of paper, one of them made expressly for water color drawings, gave the same results.

As the pigment is an exceedingly attractive one, liable to be used by naturalists in valuable drawings, it may not be out of place to call their attention to its ephemeral properties.*

3. *Professor Marsh's Rocky Mountain Expedition. Discovery of the Mauvaises Terres formation in Colorado.*—Prof. Marsh and party left New Haven on the 30th of June for Omaha and beyond. By a letter from him dated Cheyenne, August 4th, we learn that he had already made a successful expedition to the Loup Fork, and obtained large collections of specimens, but they were prevented by the Indians from going on to the Niobrara region. The following is the copy of a later letter to J. D. Dana, dated Pine Bluffs, Wyoming Territory, Aug. 12th:

The Scientific Expedition from Yale College, while recently examining the geology of Northern Colorado, discovered an extensive outcrop of the true Mauvaises Terres or White River formation, at a point nearly 200 miles south of the region where it had been previously identified. The locality first detected, which contained all of the characteristic fossils of this deposit, was on one of the branches of the Little Crow Creek, about five miles south of the Wyoming State line. The strata there observed consisted of at least 150 feet of light-colored clays, overlaid by sandstones and conglomerates about 200 feet in thickness. The lower portions of the clays are the true Titanotherium beds, containing many remains of *Titanotherium Proutii.* Above these are similar clay deposits corresponding closely in age with those on the White River, and marked by abundant remains of *Oreodon Culbertsoni*, *Testudo Nebrascensis*, *Helix Leidyana*, and many other fossils, which characterize that horizon. Associated with these were found several new species of mammals and birds.

This interesting series of fresh-water Tertiary strata lies almost horizontal, dipping apparently, but very slightly, toward the northeast. It probably forms the southwest border of the great Miocene lake-basin, east of the Rocky Mountains, which is so remarkable for its extinct animal remains. Our party traced the same formation, with its more common fossils, about thirty miles northeast into Wyoming, along the hills known as Chalk Bluffs, and still farther north in the Pine Bluff range. We hope soon to examine it at other points.

4. *Glaciers of Scotland.*—Prof. Croll, in a memoir on the "Boulder-clay of Caithness," in the Geological Magazine for June, 1870, takes the ground that the boulder drift of Scotland is due mainly to glaciers, and not to icebergs, and points out the direction of the movement of the great glacier, not only over Scotland, but over the seas north and east, illustrating the subject by a map. He finds an argument for its glacier origin in the fact that the drift is *unstratified*, remarking that depositions from icebergs would necessarily be more or less stratified by the waters in which they fall. The argument is one that cannot be set aside,

---

* The color sent by Mr. Dall is, as he supposes, *mercuric iodid*, well known to chemists as very volatile, changing at a gentle heat from scarlet to nearly white, and the latter by friction returning again to scarlet.—S.

and holds as well for New England and the rest of North America as for Scotland. Prof. Dana, in his recent paper on New Haven Geology, draws the same distinction between the drift that was deposited by glaciers over the land, and by glaciers over waters.

Dr. Robert Brown also presents the glacier theory for Scotland in an article read before the Geological Society of London on June 22, entitled—On the Physics of Arctic Ice as explanatory of the Glacial remains in Scotland.

5. *Meteoric Irons.*—The Museum of Vienna has recently received an iron meteorite from Atacama having a maximum diameter of a third of a meter, and weighing 51 kilograms. The iron contains about 6 per cent of nickel.

Tschermak has studied the meteoric iron which fell at Lodran, near Moultan in India, on the 1st of October, 1868, a fragment of which was sent to the Vienna museum by Mr. Oldham. Niccoliferous iron constitutes about 32 per cent of it; and but for the iron, the meteorite is identical with terrestrial chrysolite rocks. It contains crystals of bronzite and bluish-gray chrysolite, and of magnetic pyrites and chromic iron. The chrysolite contains 12 per cent of protoxyd of iron.—*L'Institut*, July 20.

6. *Fall of a Meteorite in Stewart Co., Georgia.*—Prof. J. LAWRENCE SMITH states in a letter to Prof. Brush, dated Louisville, Ky., July 18th, 1870, that a meteoric stone fell in Stewart county, Georgia, in October, 1869. He adds that a description of its fall and phenomena by Prof. Willet, and an analysis of it by himself, will be sent for the next number of this Journal.

7. Prof. HELMHOLTZ takes the chair of Physics at Berlin made vacant by the death of Magnus. He will enter upon his new duties in April next.—*Nature*, July 28.

Prof. J. WATSON has been awarded, by the Paris Academy of Sciences, the Astronomical Prize, Lalande foundation, for the discovery of eight new asteroids in one year.

8. *On the occurrence of a Peat bed beneath deposits of Drift in Southwestern Ohio.*—The author of the article on this subject, at p. 54, is Professor EDWARD ORTON.

9. *Woodward on the Magnesium and Electric Lights for Photo-micrography.*—The illustrations of this article, on pages 298 and 301, of the last volume of this Journal, should be transposed.

OBITUARY.

JACOB P. GIRAUD, Jr., the ornithologist, died at Poughkeepsie on Tuesday, July 19th, in the fifty-ninth year of his age. A particulr friend of Audubon, Bachman and Wilson, and a member of the New York Lyceum of Natural History since 1840, he made his favorite science of ornithology his special study. In 1841 he published "A Description of Sixteen New Species of Texas Birds," and in 1844, a more valuable work on "The Birds of Long Island." His collection of North American Birds, one of the most complete in the country and containing many of Audubon's types, he presented to Vassar College. In addition he bequeathed to Vassar

College the sum of thirty thousand dollars to endow the professorship of Natural History, and two thousand dollars to complete his cabinet of North American Birds.

Mr. A. H. HALIDAY, of Antrim, Ireland, an entomologist of distinction died on the 13th of July, near Lucca, Italy. He graduated at Trinity College, Dublin, in 1822, at the age of 15, and five years afterwards as a gold medalist.—*Nature*, July 21.

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *The Andes and the Amazon, or across the Continent of South America;* by JAMES ORTON, Prof. Nat. Hist. in Vassar Coll., Poughkeepsie. 356 pp. 12mo. New York, 1870. (Harper & Brothers).—Prof. Orton made his journey through South America in the years 1867–68, under the auspices of the Smithsonian Institution, with Messrs. P. V. Myers and A. Bushnell of Williams College, and others. His course from Paita, Peru, was by Guayaquil to Quito, and thence over the mountains to Napo; thence along the Napo river to the Amazon and Para at its mouth. The book of travels which he has given to the public is pleasantly written, and is full of facts and animated descriptions relating to the interesting region passed through—its general features and people, its climate and resources, its mountains and volcanoes, its landscapes and tropical productions. It contains a map of the part of South America from the Pacific to the Atlantic in the latitudes of the Amazon and its tributaries, which appears to have been prepared with care, and also several excellent woodcuts of scenery, etc. The party had not the time or instruments for extended physical investigations. Some of the observations of Prof. Orton have appeared in this Journal (vol. xlvii). Specimens were collected in zoology and botany, and some of fossils, and these have been put into the hands of different naturalists for description. Among the geological facts we observe the discovery of tertiary fossils at Pebas, on the Amazon, in long. 72° W., at a height of 345 feet above the sea; the fossils have been described by Mr. Gabb in the American Journal of Conchology.

2. *The Chemical History of the Six Days of Creation;* by JOHN PHIN, C.E. 96 pp. 12mo. New York, 1870.—This work is not, what its title seems to imply, a discussion by a chemist of the chemical history of the earth's genesis. It treats briefly of the bearing of some of the general principles of science, chemical, physical, geological and astronomical, on the interpretation of the first chapter of Genesis. The aim of the work is good. The views with regard to the "works" of the successive days accord in the main with those that have been presented by Prof. Guyot, and may be read with profit. There are also ideas peculiar to the author which we cannot commend. For example, he says that the "evening" of the "second day" resulted "from the cooling of the ashes of the great combustion;" that the "evening" of the "fifth day" resulted from the separation of Venus from the sun;

and the "morning" from "the restoration of the brilliancy of the sun;" the "evening" of the sixth day "from the separation of Mercury from the sun," etc. The author by bringing more exact science to bear on the subject and a wider philosophy will be enabled to prune his work of some manifest defects.

3. *The American Chemist: a Monthly Journal of Theoretical, Analytical and Technical Chemistry.* Edited by CHAS. F. CHANDLER, PH.D., and W. H. CHANDLER. New York, 1870. (William Baldwin & Co., 434 Broome St.)—The American reprint of the Chemical News having been given up, the subscription-list and stock of that reprint have been purchased by Mr. Baldwin, who starts the American Chemist to take its place, putting the editorial charge into the hands of Prof. Chandler and his brother. While we regret that a journal of the recognized ability of the Chemical News cannot be continued in this country, we believe that the well-known scientific reputation of the editors of the American Chemist is a guarantee that their new enterprise will not be second to it in value. The first two numbers have been received; i. e., those for July and August. They contain 32 articles, of which 17 are taken from the Chemical News, and eight are original. For the present, the admirable "Chemical Notices from Foreign Sources" printed in the Chemical News, will be retained. It is announced that after the close of the current year, the American Chemist will be conducted as an entirely independent journal. The subscription price is $5.00.

4. *On the Gulf Stream, and the Thermometric knowledge of the Atlantic Ocean and adjoining lands in the year* 1870; by Dr. A. PETERMANN. 64 pp. 4to, with three large colored temperature charts. Gotha, June, 1869. (Justus Perthes. From Petermann's Geogr. Mittheilungen, 1870, Heft 6–7.)—Dr. Petermann has here given an admirable exposition of the former and present state of knowledge of the temperature of the Atlantic and the Arctic seas and the lands adjoining, including the later results of the most recent expeditions. Deep sea temperatures, as well as the saltness of the ocean, and much detail with regard to special localities, come within the wide range of subjects under review. The first two charts contain the isotherms for every 2° Reaumur for the region between the meridians of 75° W. and 60° E., north of the parallel of 35°; the first, the isotherm for July and the second for January; and the third includes 5 charts of the arctic regions, giving the isotherms severally for January, July, the winter, the summer, and the isotherms of absolute minima for every 10° R. Eight pages are devoted to the instructions given to the second German North-polar Expedition, 1869–1870.

5. *Roasting of Gold and Silver Ores, and extraction of their respective metals without Quicksilver;* by G. KUSTEL. 145 pp. 12mo. San Francisco, 1870. (Dewey & Co.)—This little treatise contains an introductory chapter on the classification of silver ores, which is followed by a chapter on *roasting*, divided into sections, treating, respectively, of the chloridizing and the oxyd-

izing roasting of silver ores. The author then proceeds to give the different methods for the extraction of silver by lixiviation and subsequent precipitation; and, finally, a brief chapter on the extraction of gold by the chlorination process. The book contains a large amount of valuable facts expressed in a very compact manner, besides embracing much original matter, the results of the experience of the author and his friends in the Pacific States and in Mexico. It forms an excellent supplement to Mr. Kustel's previous work on the "Nevada and California processes of Silver and Gold extraction."

Bulletin of the National Association of Wool Manufacturers, founded Nov. 30, 1864. No. 1 of this Quarterly Bulletin appeared on January of 1869, and the first number of vol. ii in Jan. 1870. The numbers contain about 100 pages.

Report on the Coals and Iron Ores of Pictou County, Nova Scotia; by Edward Hartley, F.G.S., Mining Engineer to the Geological Survey; being an Appendix to Reports on the Pictou Coal-Field. From the Report of the Geological Survey of the Dominion of Canada, for 1867–69. 78 pp. 8vo. Montreal, 1870.

Index to the Literature of Uranium, by H. Carrington Bolton. Ann. Lyc., New York, Feb. 1870., pp. 362-377.

Uebersicht der im Königreiche Sachsen zur Chausseeunterhaltung verwendeten Steinarten; zusammengestellt von Dr. H. B. GEINITZ, K. Professor, and C. TH. SORGE. K. Oberbaurath. 116 pp. 4to. Dresden, 1870.

Paris Universal Exposition.—The following additions have been made to the list of Publications of Reports connected with the Paris Universal Exposition, given on page 258 of the last volume of this Journal:

Extracts from the Report of the International Committee on Weights, Measures and Coins, with a notice of the use of the Metric System in the United States, and its relations to other systems of Weights and Measures.

Report on Clothing and Woven Fabrics, being Classes Twenty-seven to Thirty-nine of Group Four; by Paran Stevens, U. S. Commissioner. 86 pp. 8vo.

Report on Education; by J. W. Hoyt, U. S. Commissioner. 398 pp. 8vo.

Bibliography of the Paris Universal Exposition of 1867; by Wm. P. Blake. 40 pp. 8vo.

Introduction, with selections from the Correspondence of Commissioner-General Beckwith and others, showing the organization and administration of the United States Section.

PROCEEDINGS OF THE ACAD. NAT. SCI. PHILADELPHIA, No. 1, 1870.—Page 1, Supposed Sivatherium from Colorado; *J. Leidy.*—p. 3, 4, 5, Reptile remains from Colorado; the Alabama Cretaceous; Fort Bridger Tertiary (a turtle named Baptemys Wyomingensis); *J. Leidy.*—p. 5 and 11, Fossil Birds of the U. S. Cretaceous and Tertiary; *O. C. Marsh.*—p. 6, Analysis of the nickel ore of the Gap mine, Lancaster Co., Pa.; *Wharton.*—p. 8, Mylodon from Central America; *J. Leidy.*—p. 9, Note on the Triassic Dromatherium sylvestre of Emmons; *id.*—p. 9, Note on Elasmosaurus; *id.*—p. 11, Dicotyles antiquus, from N. J. Miocene; *O. C. Marsh.*—p. 12, Ichthyodorulites; *J. Leidy.*—p. 13, Megalonyx Jeffersoni and Bison antiquus in Illinois; *id.*—p. 13, Growth of wood in Yucca; *J. Meehan.*—p. 14 Cross fertilization and the law of sex in Euphorbia; *id.*—p. 15 Relations of Synocladia of King to Septopora of Prout; *Meek* and *Worthen.*—p. 18, On Discosaurus and its allies; *J. Leidy.*—p. 22, New fossils from Illinois; *Meek* and *Worthen.*—p. 56, New fossils collected by the U. S. Coast Survey under Clarence King; *F. B. Meek.*

PROCEEDINGS OF THE CALIFORNIA ACAD. SCI., Vol. IV, Part ii, 1870.—Page 48, Discovery of a nearly entire skeleton of a Mastodon near Petaluna.—p. 50, Shells of Antioch, Cal.; *H. P. Carlton.*—p. 57, Shells of Truckee River and vicinity; *id.*—p. 61, Fauna of California and its geographical distribution; *J. G. Cooper.*—p. 82, 83, Indian mounds.—p. 86, Shell mounds; *L. Ransom.*—p. 88, Section of rocks in Hamilton, Nevada; *J. G. Clayton.*—p. 89. Rise of water in Mono Lake.—p. 90, Explorations in the Rocky Mountains; *J. D. Whitney.*—p. 92, the West-coast Fresh-water Univalves, No. I; *J. G. Cooper.*

SOLAR SPOTS, MAGNETIC DECLINATION AND AURORAL DISPLAYS COMPARED.

Punderson & Crisand, New Haven, Ct.

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. XXVIII.—*On the Examination of the Bessemer Flame with Colored Glasses and with the Spectroscope;* by J. M. SILLIMAN, M.E., Adj. Prof. of Metallurgy, Lafayette College, Easton, Pa.

[Read at the Troy Meeting of the Am. Association for the Advancement of Science.]

I. *Examination with Colored Glasses.*

IN the Bessemer process, the progress of the decarbonization is determined chiefly by the appearance of the smoke, flame and sparks which are emitted from the apparatus. Owing to the rapidity with which the reactions take place. it is highly important to catch the exact moment when the blast should be turned off. This is indicated by the color and brightness of the stream of gas issuing from the converter, and by this the moment of total decarbonization can generally be accurately determined by the naked eye. When, however, pig-iron of certain qualities is used (manganiferous iron, for example) this determination is very difficult; even those who have had much experience make frequent mistakes and find it impossible to produce the same quality of steel at every blow.

In order to intensify these flame-indications, use has been made of the spectroscope, and also of various combinations of colored glasses. The former was first attempted by Dr. Roscoe, and the latter by Mr. Rowan at the Atlas Works.

Mr. Rowan experimented with a great variety of colored glasses and obtained the best results by using three glasses, two of ultramarine blue and one of dark yellow. This little instrument, or chromopyrometer, as he terms it, is now in daily use at the Atlas Works, its indications being so marked and unmistakable as to render its use safe in the most inexperienced hands.

The following experiments were made at the Bessemer Steel Works of John A. Griswold & Co., in Troy, while pursuing the chemical course in the Winslow Laboratory of the Rensselaer Polytechnic Institute. In my observations on the flame I made use of the spectroscope, and also of a combination of colored glasses. This combination consisted of two light-yellow glasses and a blue one, through which the sunlight appeared of a deep purplish-blue tint; and as it differed slightly from Rowan's, it gave somewhat different results.

In order to reproduce the appearance of the flame at the different stages of the process, I prepared a plate consisting of about a hundred varieties of colors and tints, all of which were numbered and thus referred to a table which indicated their composition. They were also arranged to be seen with either a light or dark background. The use of this plate was of necessity limited to daylight, but the illustration and description are given as occurring at night in order to show its illuminating power.

At the beginning of the process that which issues from the converter does not appear to be a true flame, but only an illumined stream of gas carrying with it innumerable red-hot pellets of iron. This gas has scarcely any illuminating power, extends but a short distance from the mouth of the converter, and is sometimes sheathed with a whitish smoke. Seen through the glasses the flame and sparks have a deep crimson color, the converter is invisible, and at the base of the flame is a crimson band which continues throughout the process.

As the reaction continues, this stream of gas grows brighter and more elongated, and after a few minutes a small pointed whitish flame appears, which suddenly increases in size. At this instant the blast-pressure falls from twenty to eighteen pounds.

When viewed through the glasses the upper part of the converter comes dimly into view, and the flame and pellets of iron appear of a lighter color, while the fragments of slag which begin to be thrown out are of a deep red. This difference in shade between the iron and slag thrown out is probably entirely owing to the lower temperature of the latter, for the reason that while the iron is discharged from the metallic bath the slag is washed up on the sides of the converter, and can be seen clinging around its mouth in a spongy mass until detached and thrown out by the blast. The greater porosity of the slag and its consequent more rapid cooling would also cause a difference of temperature.

In the second period the discharge of slag increases, and the flame is very bright and illuminating, with occasional dark streaks. Through the glasses at the beginning of this period the flame is of an ashy blue color with streaks and flashes of crimson; the edges being sometimes of a purplish hue. At this point surrounding objects are illuminated, and the converter becomes distinctly visible. A wreath of crimson is seen surrounding the flame where it strikes the chimney. By the middle of this period the crimson almost entirely disappears from the body of the flame, leaving only a slight cone at its base, and a border of greenish hue makes its appearance, and gradually grows more decided. Streaks of a dark blue color are also seen in the body of the flame.

The beginning of the third period is scarcely indicated to the naked eye, though the flame becomes somewhat weakened, and after a few minutes shows dark streaks running through it. Through the glasses at the commencement of this period the rose-colored cone begins to expand and deepen, the greenish sheath is more decided, while streaks of dark and green are visible. After a few minutes the change becomes very rapid, a few seconds only being required to reduce the flame from rose-color to the deep crimson non-illuminating gas, as at first, and again the converter is lost to view, by which time the blast should have been turned off.

The gradual fading of the crimson from the beginning of the blow and its deepening at the termination of the process, as well as the crimson band at the base of the flame and the wreath of crimson surrounding the flame at the chimney, tend to confirm Mr. Rowan's views, which are, that the different shades of crimson are due to changes of temperature. The stream of gas which comes from the mouth of the converter at the beginning of the process being illumined from within, derives its color from the metallic bath, the temperature of which, owing to the combustion of silicon, increases more rapidly during this period than at any other.

The crimson band at the base of the flame and the wreath of crimson at the chimney might also be accounted for by this theory. The flame rushing from the mouth of the converter has a tendency to create a vacuum at its base around the converter's edge, and thus to cause a wreath of flame to pass over this surface and by consequent cooling produce the crimson band. The wreath of crimson at the chimney may be also due to the cooling of the flame consequent upon deflection.

It is true we have a seeming contradiction to this theory in the rose-colored cone extending from the base at the center, which we would naturally consider the hottest part of the flame; but, as in the flame of the Bunsen burner, the hottest part is in its outer sheath, the conditions of combustion in both being similar, it is probable that that part of the flame occupied by the cone is at a lower temperature than that surrounding it.

The green streaks in the flame are most intense when the manganese spectrum is brightest; and as the color of the flame when the spiegeleisen is added is also green, we are led to suppose them due to the presence of manganese.

On two occasions simultaneous observations were made with the spectroscope and the colored glasses; but with the exception of that just mentioned, and the changes at the commencement and termination of the blow, no striking coincidence was noticed.

II. *Examination with the Spectroscope.*

The science of spectrum analysis is yet in its infancy, and there has been no scientific investigation, perhaps, which has been more contradictory in its results than that of the Bessemer flame. The first application of the spectroscope to the analysis of the Bessemer flame was made in 1862 by Dr. Roscoe at the works of Messrs. John Brown & Co., in Sheffield. Soon after this it was in constant use in Brown's works for controlling the process. It was next introduced at Crewe, and from there said to have been taken to Seraing, in Belgium, in 1865.

Roscoe's account of the general appearance of the spectrum has not altogether been verified by subsequent observers. His not having seen any line beyond 80° indicates an imperfection in his instrument. He, also, is the only one who claims to have seen the sodium line as an absorption band, or who professes to have detected the lines of nitrogen and hydrogen in the Bessemer spectrum. His spectroscope was so arranged that the spectrum of the Bessemer flame was seen in the upper half of the field of view, while the spectrum with which it was to be compared was seen immediately below. The spectrum of the flame was thus compared with the following spectra:—

1. Spectrum of electric discharge in carbonic oxyd vacuum.
2. " " strong spark between silver poles in air.
3. " " " " " iron " " "
4. " " " " " " " " hydrogen.
5. Solar spectrum.
6. Carbon spectrum—oxyhydrogen blowpipe supplied with olefiant gas and oxygen.

The coincidences observed were very few, and totally failed to explain the value of the Bessemer spectrum. The lines of the well-known carbon spectrum did not occur at all, either as bright lines or absorption bands, nor was any coincidence observed between the lines of the Bessemer spectrum and those of the carbonic oxyd vacuum tube. The lines of lithium, sodium and potassium were strongly marked and identified with certainty. He found that three fine, bright lines between E and *b*, shown on the plate at 66½°, 67°, 67½°, coincided with those of iron; and in place of the red hydrogen line C, he discovered a black band which he considered an absorption-band, and states that it is better defined in wet than in dry weather.

In Austria, Prof. Lielegg followed up this subject with great perseverance, and gave more extended accounts of the varying character of the Bessemer spectrum during the different stages of the process. His experiments were made at Gratz, where the spectroscope was afterward used with great success in controlling the Bessemer process; but at Königshütte, where dark gray manganiferous iron was used, it was found that the indications which in other works so plainly determined the moment of decarbonization were unreliable. In this case, the lines whose disappearance is to indicate the exact point of time for ending the process, disappear too soon. During the period in which the spectrum is brightest, among the glowing vapors and gases that stream from the converter, carbonic oxyd next to nitrogen is most abundant, and it is for this reason that the first investigator, Roscoe, expressed himself as confident that the numerous lines of the spectrum were caused by this gas, although he could obtain no coincidence.

Brunner* states that "no part of the Bessemer spectrum is ever visible in the flame when the converter is heated for the first time after being re-lined, but that when the lining is not new, Lielegg's group of green lines (COγ) appears in the spectrum, which then contains also the lines of potassium, sodium, and lithium." From which he concludes that this spectrum is not to be identified with carbonic oxyd, but must be produced by other constituents of pig-iron. Others state that the Bessemer spectrum is sometimes visible while the converter is being heated after a blow. I made an observation of the flame from the converter while it was being heated the first time after being re-lined, and obtained with great distinctness the potassium, lithium, and sodium lines, but have not under any circumstances detected any other lines while the converter was being reheated.

Lichtenfels, by a series of simultaneous comparisons of the manganese with the Bessemer spectrum found the lines in the blue and green fields to completely harmonize in the two spectra. The violet manganese line which had been seen by some he could not detect in either of the spectra. I have never observed it, but Dr. Wedding, who has summed up the observations of others, states that he has repeatedly seen it. Its position is at 135½°.

The instrument used in my investigations was constructed by Alvan Clark of Cambridge, and consists of an equiangular flint-glass prism, in a metallic box, into the sides of which at the requisite angles are screwed an inverting telescope with a

---

* Van Nostrand's Eclectic Eng. Mag., vol. i, page 508.

magnifying power of six, and a tube containing the adjustable slit and lens for rendering the rays parallel; also a tube with a scale, which is placed at such an angle that it is reflected from the surface of the prism through the telescope to the eye; it can be so adjusted as to appear along the upper edge of the spectrum. I was provided with Bunsen's plates of spectra on a large scale, and in order to adapt them to the scale in my instrument, I took the spectrum of the sun and obtained Fraunhofer's lines with great distinctness. Two characteristic lines in the solar spectrum were then noted, one of which appeared at 37° and the other at 117°, and a space measured equal to their distance apart as given on Bunsen's scale. This was divided into eighty equal parts, and the division extended in both directions. By the application of this scale to Bunsen's, I found that the remainder of Fraunhofer's lines in my instrument exactly coincided with their position on his plates. The correctness of the new scale was also proved by other coincidences. By moving the prism, Fraunhofer's lines will vary slightly in their relative distances apart, but in no possible position in which I could place the prism could I obtain the sun-spectrum as given by Wedding in connection with the Bessemer spectrum; if the spectrum given by him was obtained by the use of bisulphid of carbon in his prism, that substance causes a greater variation than I had supposed.

I have recorded the results of twenty-five observations on the Bessemer flame, most of which were taken at a distance of about thirty feet from the flame, though I have stationed myself at intermediate points between that and the flame; at one time sitting so close as to be almost scorched. Nearly all my observations were made at night and the lines obtained much better defined than when seen in diffused sunlight.

The record of my observations was kept as follows:—Five columns were ruled, headed—

Degree.	Color.	Brightness.	Time.	Remarks.

Note was made of the dark bands as well as the bright ones, both of which were classed according to their distinctness, as very bright, bright, faint, and very faint. In the time-column was noted the number of minutes after the commencement of the blow at which the lines appear.

At the first two or three observations I attempted to make a thorough note of the changes as they occurred throughout the whole spectrum, but afterward abandoned it as utterly impossible, as at the beginning of the second period, the lines come in so fast and the changes are so rapid that they can not be accurately noted at the exact moment of their occurrence. I therefore confined myself to a few degrees at each observation, and by this method was enabled to note accurately, and at the exact moment of their occurrence slight changes which otherwise might have escaped notice. Note was also taken of the changes in the general appearance of the whole spectrum during the successive stages of the process. After having made half a dozen observations, while viewing the spectrum of the flame from the converter while it was being heated for another charge, it was discovered that a movement of the eye before the eye-glass occasioned a similar movement of the lines of the spectrum along the scale, on which their position could thus be made to differ more than half a degree. I have seen no notice of this in the statements of others, and it may account for some of the apparent discrepancies. Thereafter, when taking the readings of any of the lines, the position of the eye was so adjusted as to bring the sodium line exactly at 50°. Owing to the extreme brilliancy of the flame the aperture may be made exceedingly narrow, and thus the many lines of the spectrum which with a duller light and broader gauge would be blended together, may be separated.

At the beginning of the blow, the spectrum is continuous and very faint, and generally extends from 35° to 120°, covering about three-fourths of the length attained in the second period. This increases slightly in extent and brightness until the appearance of the sodium line. This line appears at the end of the first period at the beginning of a more decided flame. It comes flashing through from one extremity to the other for an instant, and then disappears only to return the next instant in brighter flashes which are continued for about a minute, by which time the line becomes permanently established. On one occasion the sodium line, instead of flashing and disappearing as usual, continued visible after a few seconds, and expanded and contracted in width almost isochronously until it became permanently established. The appearance of this line indicates the termination of the first period. This period I have found to vary in extent from three to seventeen minutes in blows lasting from thirteen to twenty-seven minutes. None of the other lines make their appearance in vivid flashes as does the sodium. The lithium line becomes visible three or four minutes after the first flash of the sodium. It is very faint at first but soon becomes quite distinct and lasts through the blow. The vivid flashing of the sodium line may be accounted for by the exceedingly small amount of sodium required to produce its spectrum—an amount not exceeding $\frac{1}{180000000}$ of a grain. The slightest momentary combustion taking place in the stream of gas from the converter, would at that instant render glowing a sufficient amount of the vaporized sodium to produce its spectrum, and thus occasion the flashes so characteristic in the first appearance of that line. Lithium exists in a much smaller quantity and requires $\frac{1}{6000000}$

of a grain, or thirty times that given for sodium. By the time the lithium line is established the red potassium line at 23½° and occasionally the violet line at 135° appear, and the blue and green fields become divided into bands which are so rapidly resolved into bright and dark lines, that it is difficult to note the exact time of the appearance of each. The spectrum increases to a dazzling brightness, and extends itself in both directions until it reaches from 23½° to 140°.

During the third period the spectrum becomes more brilliant, and the lines more distinct. Several new lines make their appearance in different parts of the spectrum, of which the ones at 51½°, 57°, and 67° are well defined, while others are faint and not always visible; some of them appearing only toward the close of the last period. In viewing the lines in the most refracted part of the spectrum, it has been repeatedly observed both by myself and others, that these lines were more strongly marked when entering the eye at an angle than when viewed directly. That this was not imagination is proved by repeated identification of lines at the same point on the scale.

At the termination of the blow the lines are rapidly swept away, sometimes in the inverse order of their appearance, but more generally they disappear within the space of two or three seconds, leaving a continuous spectrum as at first, though somewhat brighter. Sometimes the sodium and lithium lines are swept away with the others, and at other times they remain visible. In either case the change is very decided, and does not generally occupy more than three seconds. In the course of my observations, thirty-three lines have been detected, as given in the table below.

Some of the lines given by Lielegg I have failed to find, but have detected others not given by him.

1st Period, 23½, 35, 50, 135.
2d Period, 23½, 35, 43, 44, 44½, 45½, 46, 47½, 48½, 50, 52, 53, 56, 56½, 61½, 62, 62½, 63, 65, 66½, 67½, 70, 72, 120, 135.
3d Period, 23½, 35, 43, 44, 44½, 45½, 46, 47½, 48½, 50, 51½, 52, 53, 56, 56½, 57, 61½, 62, 62½, 63, 65, 66½, 67, 67½, 70, 72, 100, 102, 103, 105, 108, 135.

Among the dark bands detected, the most intense occurred at 44–46, 51–55, 56–58, 62–64½; others were found at 33–34½, 36½, 37½, 38½, 40, 68–72.

Many of the dark bands were crossed by bright lines.

I have repeatedly observed the dark band considered by Roscoe to be a hydrogen absorption line, but have not noticed that its intensity varied with the dampness of the weather. Whether it is an absorption band or not can be determined by a series of observations continued through wet and dry weather. If this proves to be a hydrogen line, the Bessemer spectrum will be found more complicated than is generally supposed. It has been thought by some that the dark bands in the spectrum are absorption lines due to the cooling of the outer sheath of flame, but it is more probable, that although the pellets of iron and slag tend to produce a faint continuous spectrum; yet in contrast with the very brilliant lines it appears discontinuous, the dark bands being merely intervals between the bright ones. The iron spectrum has not been satisfactorily identified. It has been suggested that the brightness and size of the lines of the Bessemer spectrum do not allow the iron lines to appear. In comparing the Bessemer spectrum with Bunsen's spectra of nickel, cobalt and calcium, no coincidences were observed except two or three in the latter spectrum. The brightest calcium line, however, was not visible in the Bessemer spectrum. The Bessemer spectrum contains yet many mysteries to be solved, among which is the cause of the non-appearance of the lines of the spectrum at the beginning and termination of the blow.

This was readily solved when the numerous lines of the spectrum were attributed to carbon, but in proving them to be caused principally by manganese, their disappearance is not so readily accounted for.

One theory to account for it is that the luminous power of the flame is too small at the beginning and end of the process to produce a spectrum. In regard to this it may readily be shown that the brilliancy of the spectra of incandescent metallic vapors does not depend upon the illuminating power of a flame but upon the heat of the flame into which they are introduced. For instance, the spectra are more distinct in the non-luminous flame of a Bunsen lamp than in the ordinary luminous gas-flame. If we take the theory as referring to the feebleness of light given off by those substances in the flame which produce the spectrum, it will resolve itself into the one of change of temperature, notwithstanding the fact that the illuminating power of flames of the same temperature varies with the composition of the gas, because there is evidently enough sodium in the flame to give its characteristic line; hence, whatever might be the illuminating power of the flame, if the heat is sufficiently intense the sodium line *will* show itself.

Dr. Wedding adopts the theory that the absence of the spectrum at the beginning and termination of the blow is because the absolute quantity of the bodies volatilized producing the spectrum is at these times too small. His reasons for holding this view are as follows:—"A trace of sodium will give its characteristic line, but, according to Simmler, a much larger quantity of manganese is needed to obtain a recognizable reaction than that which can be detected by the well known blow-pipe reaction with carbonate of soda. Consequently, spectrum analysis does not depend alone upon the *presence* of a body but also upon the presence of a certain quantity. And although manganese is always left in the iron, it may not be left in sufficient quantity at the termination of the blow to produce the spectrum, and for this reason the lines disappear."

To this theory there are same some strong objections. 1*st.* If we take manganese in sufficient quantity and hold it in a flame the spectrum will increase in brightness until a uniform temperature is attained; but when the amount of manganese vaporized begins to diminish, its spectrum will gradually decrease in brightness until it disappears. Now, if the disappearance of the manganese lines in the Bessemer spectrum is owing to the diminution of the quantity of manganese, we should infer that these lines would gradually grow more indistinct and then fade away; but on the contrary, the manganese spectrum increases in brilliancy from its first appearance, and is more intense just before being swept away than at any other time. The analysis of the smoke, which appears when the flame ceases, proves that a considerable quantity is still volatilized, and it is notable that in manganiferous iron this quantity increases towards the close of the blow. 2*nd.* It would be more difficult to account by this theory for the non-appearance of the sodium line at the beginning of the blow, as sodium then in all probability exists in the issuing gas in sufficient quantity to produce its spectrum at a high temperature, as it is only by special precaution that we can keep it out from any flame. 3*rd.* A still greater difficulty would arise in applying this theory to the spectra of sodium and lithium at the close of the blow. As has before been stated, these lines sometimes disappear at the moment of complete decarbonization, and sometimes remain. In the former case, to say that our friend sodium had given out would be doing great injustice to that element, as it has never given us reason for bringing so grave a charge against it. Dr. Wedding in attempting to demonstrate that the non-appearance of the manganese lines is owing to the lack of sufficient quantity volatilized to produce its spectrum, makes the following statements:—

From analyses made by Brunner we find that the manganese contained in the iron falls from 3·460 per cent in the raw material, to 1·645, 0·429, and finally to 0·113 per cent in the decarbonized product; and that the protoxyd of manganese in the slag first increases from 37·00 per cent to 37·90 per cent, and then sinks to 32·23 per cent, and furthermore, that a certain quantity of manganese is to be found in the smoke. How much manganese is really lost by volatilization cannot be determined, since data are wanting as to the absolute quantity of slag and iron, consequently we cannot determine how much manganese has been lost by means of the eruptions.

But since the manganese contained in the pig-iron decreases constantly, and that contained in the slag after the termination of the boiling period also decreases, a considerable volatilization of this body is probable just at the time when the spectrum is best developed. Comparing with this the experiments that can be made in the laboratory we arrive at the hypothesis, that the oxydized manganese which has entered into the slag is not volatilized but is retained by the slag; it can, therefore, get into the flame only in the shape of solid or fluid combinations.

In the above statements the results of the analysis prove that some of the manganese in the slag is volatilized. We cannot consider the manganese spectrum during the entire process as due wholly to the volatilization of the manganese directly from the iron, for while the amount eliminated from the iron grows continually less, the manganese spectrum grows brighter. Owing to the intimate mixture by the blast of the iron and slag, the manganese oxyd contained in the latter, is brought in contact with the melted iron and vaporized. This mixing of the slag and iron would cease at the termination of the process, and this would account for the sudden diminution of smoke.

If there was a sufficient carbonic oxyd flame to render the escaping gases glowing it is evident they would not issue from the converter as dark smoke, but as incandescent vapor having its characteristic spectrum. The lack of sufficient flame may, therefore, account for the disappearance of the manganese spectrum. The Bessemer flame presents other problems, and opens an intensely interesting field for scientific investigation; and by the use of more delicate instruments than have yet been employed for this purpose, discoveries may be made which will throw new light upon the subject of spectrum analysis.

ART. XXIX.—*On a simple method of measuring Electrical Conductivities by means of two equal and opposed magneto-electric currents or waves*; by ALFRED M. MAYER, Ph.D.

[Read before the Troy meeting of the American Association for the Advancement of Science.]

1. *General description of the Method.*

A MAGNET is firmly supported in a horizontal position with a portion of its length projecting beyond a fixed stop (see fig. 2); over this free end of the magnet, and resting against the stop, are placed two similar flat spirals, formed of the same quality of copper wire, and having the turns of one spiral in a direction the reverse of those of the other. The spirals are clamped together and their four terminal wires are carried vertically downward into four separate cavities containing mercury; these mercury-cups are so connected with a reflecting-galvanometer that, when the spirals are together slid off the magnet, the two equal electric currents, thus generated, simultaneously tend to traverse the galvanometer in opposite directions, and therefore its needle remains stationary. If we now introduce into the circuit of one of the spirals a resistance equal to that introduced into the circuit of the other, the needle will still remain at rest when the spirals are slipped off the magnet; but, if the resistance placed in one circuit is greater or less than that placed in the other, there will be a deflection of the galvanometer needles when the spirals are removed. Thus, by introducing wires of different metals into the circuits we can readily determine their relative conductivities, by making them of such length that their resistances are equal; which condition is attained when, on sliding off the spirals, the needle remains absolutely at rest. If, in the latter case, the wires have equal diameters then their conductivities are directly and their resistances are inversely as their lengths.

A modification of the above method is discussed in the conclusion of this paper; in which the magnet is replaced by the terrestrial magnetic force and the spirals and the wires by two similar coils, from two to three feet in diameter, formed of the two wires whose conductivities are to be compared. These coils contain equal lengths of the same sized wires and the same number of turns; the direction of the turns being opposed in the two coils. The coils having been bound together are placed in a plane at right angles to the line of "the dip," and the four terminal wires are so connected with the reflecting-galvanometer that the two induced currents tend to traverse it in opposite directions. The coils are now quickly rotated through 180°, around an axis at right angles to the line of the dip, and if the wires present equal resistances the needle remains at rest; if it is deflected, the direction and the amount of the deflection shows which coil has the lesser resistance and affords a means of estimating the same.

After this general description of the method I will present, in order, a description of the apparatus used, and of the actions which take place in it; the degree of precision of the method; examples of the determinations of electrical-conductivities, and experiments on the modification of the method.

2. *Description of the Apparatus.*

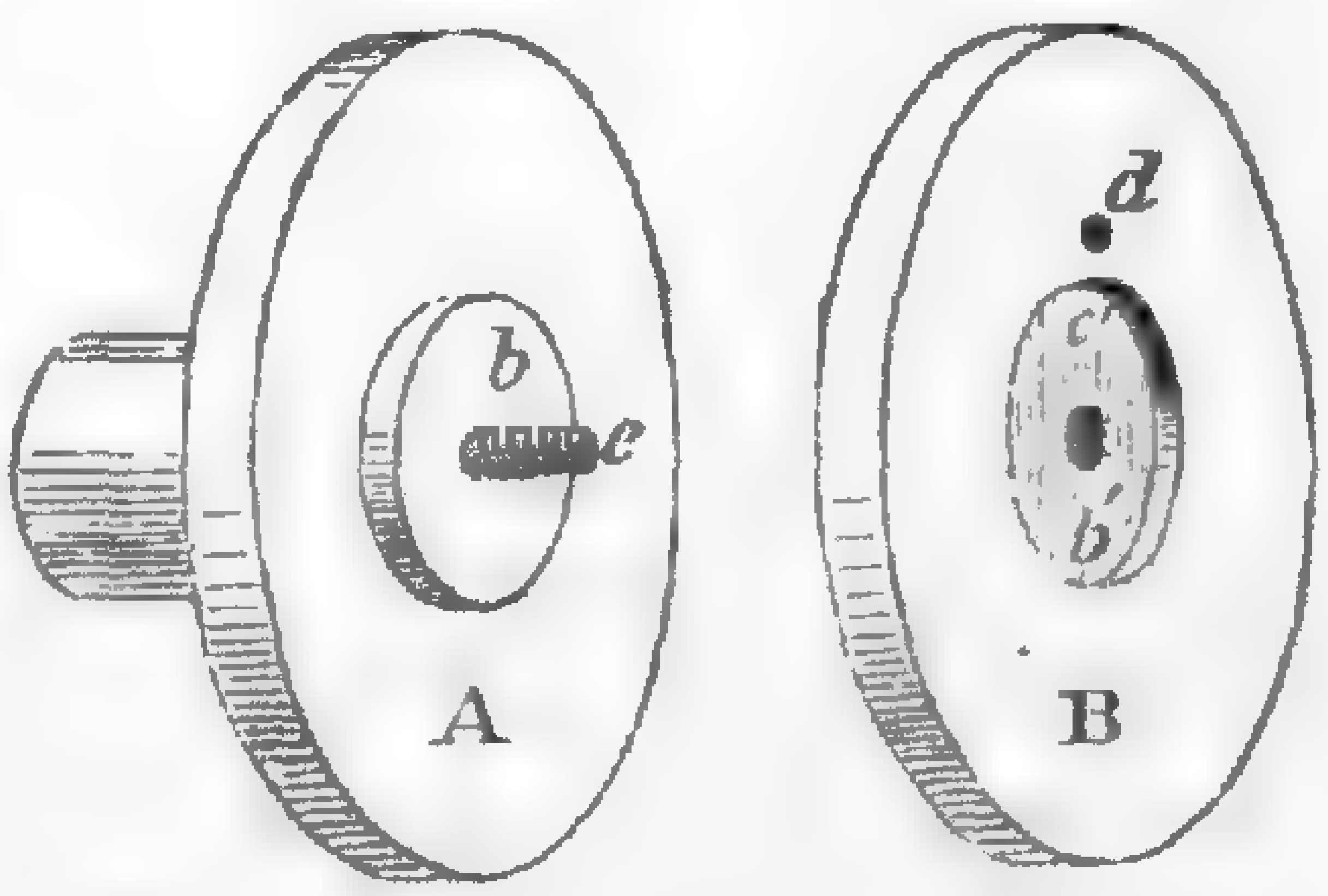
The magnet was formed of a combination of three steel bars, separated from each other by slips of wood ·2 in. thick. The middle bar was 10·4 in. long and its ends projected ·25 in. beyond the two side magnets. Each bar was ·27 in. thick and ·9 in. wide. About three months before this investigation was undertaken they had been magnetized to saturation by the following process. The axis of a helix, 8·7 ins. long and containing 558 feet of $\frac{1}{10}$ in. copper wire, was placed in the line of "the dip" and a current so sent through it from ten Bunsen cells that its N. pole was toward the earth. The separate bars were then drawn through the helix until they ceased to acquire an increase of magnetism. This method gives a uniform and powerful magnetization, and probably may be improved by causing the bars to vibrate as they pass through the helix; which can be accomplished by means of a tuning fork furnished with a long brass stem. After the magnets were combined, as described above, a weight of 1·5 lb. was sustained at the end of the middle bar.

The magnet was supported in an E. and W. line, 15·5 in. above the surface of the mercury in the cavities of the wooden "connecting-block" placed below it; and 2·5 in. of its S. end projected beyond the wooden clamp which held it.

The Spirals were formed of $\frac{1}{20}$ inch "double-covered" Lake Superior wire. Each spiral contained 176·06 in. of wire coiled in 20 turns, and the terminals were 15·5 in. long, thus making 207·06 in. of wire in each spiral. The greatest diameter of the spirals was 3·9 in. and each had a central opening of 1·7 in. Their thickness after they were covered with paraffined paper and varnished was ·06 in. The covering of the two terminal wires of each spiral was saturated with melted paraffin; they were then firmly tied together with silken cord to about ·4 in. of their ends where they separated and formed forked terminations.

The spirals were formed in this manner. An iron plate A, which screws on to the mandrel of a lathe, has cemented on to its face a disc of hard wood *b*, 1·7 in. in diameter and ·1 in. thick. From the center of the plate A projects a screw *e* which enters the wooden disc B at *e'*. When the plate B is screwed "home" the disc *b* fits into the cavity *b'* and the plates A and B are separated to a distance a little greater than the diameter of the covered wire, while the disc *b* forms a cylinder between them on which to wrap the spiral.

1.



The end of the wire to be coiled is passed through a hole *d* in the plate B, which is then screwed home on to A. The lathe is then turned so that the wire is coiled over the center disc from A to *b*. After the space between the disc is filled with coils, the free end of the wire is secured and the plate B unscrewed, while the wire slides through *d* and the coil is not unwrapped; which would have taken place if it had been coiled in the direction from *b* to A. The spiral is now saturated with very fluid paraffin and has cemented on to it, with a hot chisel, a paper disc previously saturated with paraffin. The spiral is now removed, the covered side placed against the disc and its other surface treated in the same manner. The spiral is then taken off the chuck, and on holding it up to the light the copper wire is distinctly seen through the translucent covering of the wire and the paraffined paper cover of the spiral. The insulation thus obtained is very perfect and the coils are firmly cemented together. The terminals are now led radially from the spirals, and are tightly bound together as described above. To still further strengthen the spirals, both they and their terminals are covered with a firm layer of shellac varnish.

I have thus minutely described the process of making these spirals for they are of inestimable value in many electrical researches; having been used in my recent investigations in electro-magnetism, and will be again used and referred to in a subsequent communication.

The galvanometer I specially constructed for this research, but experience has shown that a coil of shorter and thicker wire, (say, $\frac{1}{16}$ in. wire in 6 turns) offering less resistance, would have been better than the one employed. The wire of the galvanometer was $\frac{1}{20}$ in. thick, and was wrapped around the lower needle in two layers of 22 turns each; the opening of the coil being ·15 in. in width. The needles are 1·65 in. long and ·03 in. diam. The upper needle is ·6 in. above the lower with a thick copper plate intervening; it was not much affected by the current in the coil, and was under the influence of a feeble magnet, 13 in. long and ·18 in. diam., which was placed with its similar poles over the upper needle and 8·2 in. above it. Under these conditions the simple oscillations of the system were exactly 4½ per minute, and by lowering or raising the magnet I could render it more or less astatic. The needles were hung by a frame of fine copper wire to a plane mirror 1·2 in. square, formed of thin glass silvered by Foucault's process, and the whole was suspended by a few fibers of unspun silk. The instrument was enclosed in a cover, the front of which was made of a carefully selected piece of plate glass.

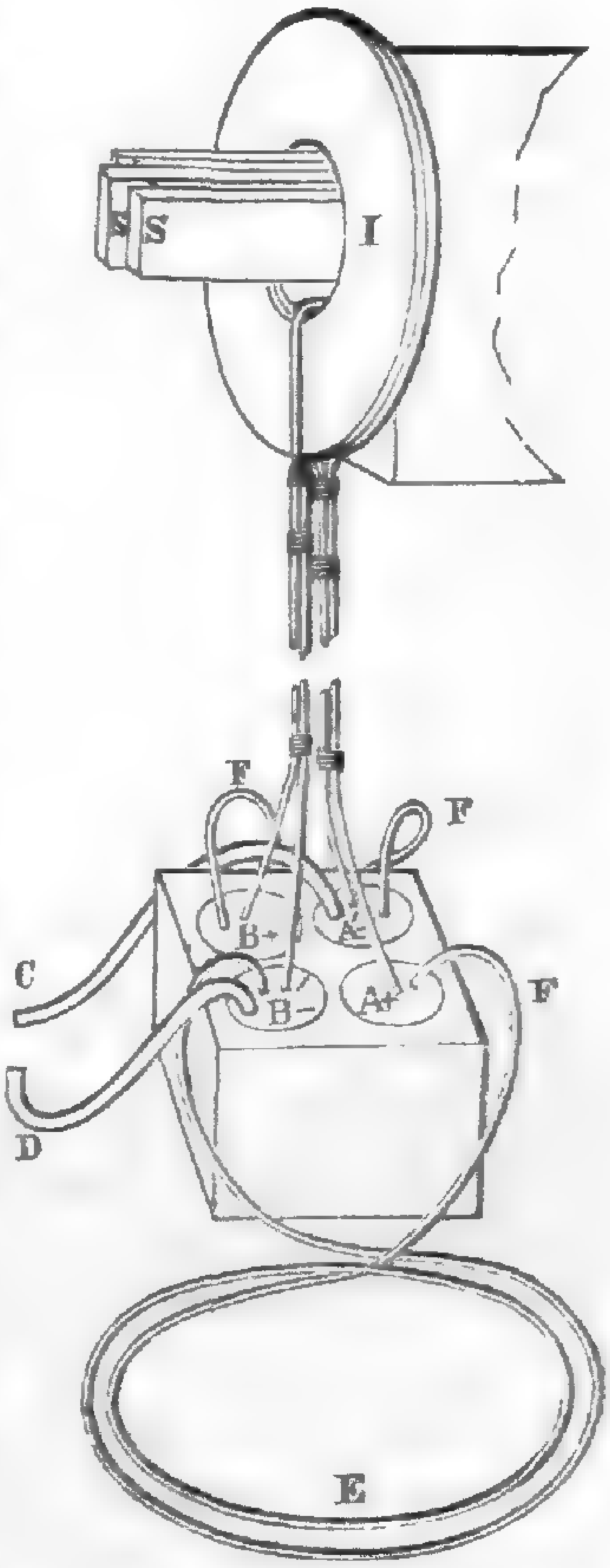
The S. end of the magnet used in inducing the electric currents in the spirals was 14 in. to the left of the magnetic meridian line drawn through the point of suspension of the galvanometer needles and 6 ft. 11 in. distant from the same. In this position the magnet caused a deflection of 52′·5 in the needles of the galvanometer and they were brought back into the meridian by means of the damping-magnet.

The deflections of the needles were read off by the beautiful method invented by the illustrious Gauss, using a telescope and scale placed as described below.

The telescope was of ·7 in. aperture and 12 in. focus; just under its object glass was placed, at right angles to its axis, a rod of wood 1 in. square and 1 meter long, covered with drawing paper. This rod was divided off into centimeters by lines 1^{mm} thick; thus, the division lines were $\frac{1}{10}$ of the distance between two similar sides of the centimeter divisions. A thick spider thread was selected which just covered a division line, and therefore was also, apparently, 1^{mm} thick. By this simple device,—using one and the same side of the spider line as point of reference,—we can accurately estimate deflections of the needles corresponding to $\frac{1}{10}$ of a division of the scale.

The scale was 2·285 meters distant from the center of the mirror, and therefore a motion of 1 division of the scale over the spider thread corresponded to an angular deflection of 7′ 30″, and as we have seen that $\frac{1}{10}$ of a division can be accurately read, it follows that we can determine a deflection of 45″. In this paper I will give the deflections in divisions of the scale, which can be converted into minutes of arc by multiplying them by 7·5.

2.



Connecting-block is the name I give to the block of wood, placed under the projecting end of the magnet; it has four cavities containing mercury, by means of which we make the various electrical connections required in the experiments. Fig. 2 gives a view of this block and shows the manner of making the connections when the object is the measurement of relative electrical resistances. Four holes, 1 in. in diam. and 1 in. deep, separated by walls ·1 in. thick are bored out of a block of wood, and then coated with thick shellac varnish. A, A′ are the terminal wires of one spiral, B, B′ those of the other. The wires to be compared are at E and F. If E represent the standard wire of a fixed length, then the wire F has to allow of its length being altered so that its resistance may be made equal to that of E. This is arranged by sliding one end of this wire through a heavy copper clamp (not shown in the fig.) which is fixed in the mercury-cup B, while the other end, previously well amalgamated, dips into cup A′.

The two spirals I are firmly clamped together, so that when slid quickly off the magnet S, they both have the same direction of motion. By referring to fig. 2, the directions in which flow the currents, thus produced, can be readily followed. Taking spiral A A′, the current flows down the terminal A+ through the wire E into B′, thence by the wire D through the galvanometer and back by the wire C to A′−, the other leg of the spiral, thus completing the circuit. In the case of the spiral B B′, the current flows down the terminal B+ through the wire F, thence by the wire C through the galvanometer; returning by D to B′, thus forming the circuit.

It is evident that if the spirals by themselves generate equal currents, and the resistances E and F are equal, no deflection of the needles will ensue, for equal and contrary currents will tend simultaneously to traverse the galvanometer.

The manner in which contacts are made in these experiments is of great importance. The double silk covering of the wires is unwrapped to ·2 in. from their ends; the unwrapped silk is then firmly wound over the end of the silk covering and saturated with thick shellac varnish. The uncovered end of the wire is now scraped, rubbed with nitrate of mercury, and well amalgamated, up to the silk covering. The iron wires were amalgamated by dipping their uncovered ends into sodium-amalgam. Thus, even if the end of the wire should dip deeper than ·2 in. into the mercury, the point of contact will yet remain at that distance from the end, as the shellac prevents contact above the amalgamated portion of the wire. The terminals of the spirals and of the galvanometer coil were formed in the same manner. One end of the wire whose resistance was to be compared to the standard copper wire, was uncovered and well cleaned for some portion of its length, so that it could be drawn through the heavy copper clamp until its length equalled in resistance the standard wire. The wires were then removed and their lengths accurately measured.

3. *Investigation into the actions which take place in the apparatus.*

In the general introductory description given of the method, I have, for simplicity of illustration, assumed that when the two spirals,—similar as to form, length of wire and resistance,—are slid off the magnet, no current would be sent through the galvanometer. But this cannot be, for the hinder spiral is further on the magnet than the other by ·06 in. and therefore cuts more "lines of magnetic force," and also, the two spirals traverse simultaneously portions of the field differing in magnetic intensity.

The following experiments will exhibit the above action. I will call the back and front spirals respectively A and B.

They were reversed on each other, clamped together and had their terminals dipping in A and A′ so that the two induced currents would tend to traverse the galvanometer coil in opposite directions.

The mean of three experiments shows that when the spirals are slid off there is a deflection of 2·43 div. in favor of spiral A. But, on making spiral B the back spiral, the needle moved 3·9 div. in its favor: thus showing that spiral B offers less resistance than A, though both lengths were contiguous pieces taken from the same sample of wire. On again placing A against the stop, I found that a resistance of 2·9 in. of $\frac{1}{20}$ in. wire, when attached to one leg of this spiral, reduced its action to equal the forward spiral and the needle remained absolutely unaffected when the spirals were quickly removed from the magnet.

The following experiments show the effects of separating the spirals. The balanced spirals remaining, in other respects, as in the last experiment, I separated them ·05 in. by intervening card-board; the needle was now deflected 1·05 div. in favor of the back spiral, and on increasing the separation to ·125 in. the action of the back spiral equalled 3·6 div. of the scale.

I have said above that the two opposed currents *tend* to traverse the galvanometer coil, because theoretic considerations induce me to hold the opinion that two currents cannot simultaneously traverse a wire in opposite directions, and that only the excess of the intensity of one current over the other is really propagated through the wire.

The next point to be considered is the mutual inductive action of the spirals. The directions of the turns in the spirals being opposed, and as the current in each, on sliding them off the magnet, rises rapidly to a maximum intensity and as quickly comes to 0, it follows that they must exert a mutual inductive action. But although the current in one spiral during the rise to its maximum causes an induced current in the other spiral in the same direction as that induced in it by the magnet, yet, as the current *decreases* as quickly to 0 after it has reached its maximum, it follows that a current in the opposite direction to that induced by the magnet in the other spiral will now quickly follow it, and as these currents, +, and − are equal, there will be no increased outside effect arising from their interaction; and many experiments showed that whether a copper disc was placed between the spirals or an equally thick disc of paper, the action at the galvanometer was the same.

The following experiments on this subject appear to confirm the above view. The two spirals were placed on the magnet, but only the front one was connected with the galvanometer, while the terminals of the back spiral were separated so that no current went through it when the spirals were together slid off the magnet. The action of one spiral alone was sufficient to deflect the galvanometer needle about 60°. This deflection was reduced to 1° 18′ by placing in its circuit a helix of 735 ft. of no. 18 copper wire; the mean of six experiments (the *range* of which was only $\frac{1}{10}$ div.) giving 10·4 div. The ends of the back spiral were now so connected that an equal current flowed through it in a direction the reverse of the other. The mean of six deflections, produced by sliding together the spirals off the magnet, equalled 10·4 div., the same as in the previous experiment; thus showing that the mutual inductive action of the spirals had no effect on the intensity of the induced magneto-electric currents.

It was also found that on passing the induced current from a spiral through another spiral on which rested a third spiral whose ends were connected with the galvanometer, that no deflection ensued when the magneto-electric current was passed through the inducing spiral.

However, the magneto-electric currents were of such low intensity that probably they were not able to produce an induced current in the second spiral capable of deflecting the needle, and that therefore the experiments here narrated are of little value; nevertheless, I think the reasoning given above will be supported by experiments made with more powerful magnets and with larger spirals.

4. *The degree of Precision of the method.*

The degree of precision of this special apparatus was determined in the following manner. A copper wire 123 ins. long had opposed to it a resistance which was about equal to 120 ins. of its length and the mean deflection of the galvanometer-needles was carefully determined. The copper wire was now shortened 1 in. and the deflection again determined; this was repeated,—determining the amount of deflection produced after each shortening of 1 in.,—until 6 in. had been cut off. These experiments showed that a diminution or increase of resistance of $\frac{1}{120}$ part in one of the wires caused a deflection of ·4 div. of the scale, or of 3′ of arc, in the galvanometer-needles. But we have seen that ·1 div. can be read on the scale, therefore, we can, with this special apparatus, detect and measure an increased or diminished resistance of $\frac{1}{480}$ part. But as the galvanometer can be removed to even twice the distance at which we read its deflections, I think I am safe in saying that with this method, as applied with the above apparatus, we can measure a difference of resistance in two conductors of $\frac{1}{500}$ part; which is far within the variations observed in different samples of wires of the same lengths and diameters.

If a galvanometer formed of 6 or 8 turns of ·1 in. wire were used in connection with a powerful magnetic battery and larger spirals of thicker wire, while the galvanometer is placed at a greater distance, I have no doubt that a variation of $\frac{1}{1000}$ part can thus be detected and measured.

5. *Examples of the determinations of electrical conductivities by this method.*

The object of these determinations was not to furnish science with new and accurate data,—for that would have required a careful personal supervision of the operations of preparing chemically pure metals,—but it was to give examples setting forth the practice of the method.

I had prepared "hard-drawn" wires, of No. 18 B. W. G. (=·049 in. diam.), of copper, silver, iron, and German silver. These wires were found to have the same diameter. They were all covered with a double wrapping of silk.

Silver.—The spirals were balanced, by the introduction of an increased resistance in the back-spiral, so that no deflection took place on sliding off the spirals. A length of 120 in. of the silver wire having been placed in the circuit of one spiral, it was found that 127 in. of copper wire were required in the other circuit, in order to equal it in resistance. Taking the copper wire as the standard of comparison, at 100, we have

$$127 : 120 :: 100 :: 94{\cdot}48.$$

Matthiessen (Phil. Trans., 1858, 1862) makes the ratio of the conductivity of silver to copper, both hard-drawn, as 100: 99·95 or about equality; but in my determination the silver is 5·5 per cent below the copper. I therefore suspected impurities in the silver, and an examination of the wire kindly made by my colleague, Dr. Wetherill, showed that it contained about ·01 per cent of gold and a trace of iron. This accounts for the low number found, and affords a good illustration of Pouillet's remark, that the purity of a metal is most readily determined by a measure of its electrical conductivity. The electrical test of purity, however, exceeds in delicacy the chemical examination; for a very minute percentage of alloy causes a great increase of resistance, and if we could be sure that the wires we compared were in the same physical condition as to annealing or hardness, we could probably use this method as a means of determining the percentage of a *known* metal which formed the alloy.

Pouillet shows (Traité de Physique, 1856, vol. i, p 606) that silver whose conductivity is 100 when pure, is only 51 when it contains ·037 of alloy, and is 47, 42 and 39 when it contains respectively ·100, ·143, and ·253 of alloy. Pure gold gave 39, but ·049 of alloy reduced its conductivity to 13; and Jenkin has found that an alloy of 1 part of silver and 2 of gold presents almost as much resistance as German silver.

Iron.—The three following determinations were made of the conductivity of the best quality of iron wire relatively to the standard copper wire.

(1) The resistance of 240 in. of copper wire = 36·7 in. of iron wire.
(2) " " " 111·6 " " " " = 16·16 " " " "
(3) " " " 60 " " " " = 8·67 " " " "

Giving for the relative conductivity of iron,

(1) 240 : 36·7 = 100 : 15·29
(2) 111·6 : 16·16 = 100 : 14·48
(3) 60 : 8·67 = 100 : 14·45

14·74 = Mean.

E. Becquerel (Ann. de Ch. et Phys., III, xvii, 266) gives 13·6 for the conductivity of iron, copper being 100; and both wires hard-drawn; while Matthiessen (Phil. Trans. 1858, 1862) determines 16·81 as the conductivity of iron, copper being 100, and both hard-drawn.

The mean of Becquerel and Matthiessen = 15·20
My determination = 14·74
Difference = ·46

The copper and iron wires in (3) were cut off from the lengths used in (2); but the wires used in (1) were taken from parts of the coils removed from the lengths (2) and (3). This accounts for the close agreement of (2) and (3) and the higher number obtained in (1).

My determination therefore appears to compare favorably with those made with different methods by these experimentalists. I say "appears," because although the copper was of excellent quality and the iron the best procurable, yet they were not chemically examined as to their purity.

Another series of determinations was obtained by comparing the lengths of copper and of iron wires which would equal in resistance one and the same length of German silver wire, used as a term of comparison. The result agreed with the above determinations.

6. *On a modification of the method.*

As long ago as 1832 Faraday (Exp. Res. 170–180) first obtained an electric current, directly induced by the earth's magnetism, by rotating a closed wire circuit around an axis at right angles "to the line of the dip;" an experiment whose theoretic beauty has ever been the admiration of natural philosophers.

A length of 38 ft. of $\frac{1}{10}$ in. insulated copper wire was wound into a coil of 3 ft. in diameter, containing 4 turns. The terminals of this coil were connected by binding screws with the wires leading to the galvanometer and the coil placed in a plane at right angles to the line of the dipping needle. On quickly rotating the coil through 180° the needles were deflected 25°, and by making the rotations correspond in direction and time with the oscillations of the needle, I found that six rotations brought the deflection to over 45°. Faraday (Exp. Res. 202–213 and 3145 *et seq.*) has shown that the intensities of the magneto-electric currents induced in wires of different metals are as their electrical conductivities, therefore a coil of iron wire similar in all respects to the above copper coil will give a deflection of about 4° for the first rotation; but by increasing the number of turns of the coil to 10 or more and by using a galvanometer with a shorter and thicker wire coil, the angle of deflection can no doubt be doubled.

The above facts show that we can substitute for the steel magnets, previously used, the magnetism of the earth, and can replace the spirals by two similar coils made of the two specimens of wire to be compared. The coils are placed on each other so that their convolutions are in opposite directions; and having been firmly tied together their plane is made to coincide with a direction at right angles to the dipping needle, while their terminals are so connected with the galvanometer that the currents induced in the two coils tend to traverse it in opposite directions.

Things being arranged as above, it is evident,—as the wire coils are similar in all other respects,—that if the conductivities of the wires are the same, there will follow no deviation of the galvanometer needle when the coils are quickly rotated through 180°; but if the wire of one coil offers a greater or less resistance than that of the other the needle will be deflected. By ascertaining what differential actions correspond to known differences of conductivity of coils of a certain diameter, number of turns and thickness of wire, we can, by always using similar coils in these relative measures, ascertain what difference in relative conductivity corresponds to a certain angle of deflection; the chords of these angles, or, the sines of half of the angles, being to each other as the intensities of the currents.

Minute differences of resistance in the two coils may be made to cause a deflection in the galvanometer needle by knowing the time of its oscillation, and by reversing the motion of rotation of the coils so as to correspond to the swing of the needle; thus after several reversals a motion is given to the needle which could not have been observed after a single rotation.

In point of ready application,—and especially in reference to the determination of the resistances of lengthy conductors,—I doubt whether this method will be generally adopted; but after the conception of the idea it appeared worth investigating; this

I have done, and have thus developed at least any value it may possess. It certainly presented an interesting problem and the pleasure afforded in its solution has repaid me for the considerable labor which it required.

S. Bethlehem, Pa., July 15, 1870.

ART. XXX.—*On the supposed absence of the Northern Drift from the Pacific Slope of the Rocky Mountains;* by Dr. ROBERT BROWN, M.A., F.R.G.S., etc., Edinburgh, Scotland.

IN some interesting remarks addressed to the California Academy of Sciences on the 4th of June, 1868, and published in their 'Proceedings' for that year (vol. iii, pp. 271, 272), Professor J. D. Whitney denies that there is any true Northern Drift within the State of California. "Our detrital materials," the learned Professor remarks, "which often form deposits of great extent and thickness, are invariably found to have been dependent for their origin and present position on causes similar to those now in action, and to have been deposited on the flanks and at the bases of the nearest mountain ranges by currents of water rushing down their slopes. While we have abundant evidence of the former existence of extensive glaciers in the Sierra Nevada, there is no reason to suppose that this ice was to any extent an effective agent in the transportation of the superficial detritus now resting on the flanks of the mountains. The glaciers were confined to the most elevated portions of the mountains, and although the moraines which they have left as evidences of their former extension are often large and conspicuous, they are insignificant in comparison with the detrital masses formed by aqueous erosion. *There is nothing anywhere in California which indicates a general glacial epoch during which ice covered the whole country, and moved bodies of detritus over the surface* independently of its present configuration, as is seen through the Northeastern States."

Mr. Whitney goes on to observe that the same condition of things prevails in Nevada and Oregon, the detritus seeming always to be accumulated at the base of the mountains. Further, from the observations of Messrs. Ashburner and Dall, he remarks that "it would appear that no evidences of a Northern Drift have yet been detected on this (Pacific) coast, *even as far north as British Columbia and Russian America* (Alaska). Neither of these gentlemen has observed any indication of a transportation of drift materials from the north toward the south, or any condition of things similar to that which must have existed in the Eastern States during the diluvial epoch." Mr.

W. H. Dall, the gentleman referred to in the foregoing extract, (and well known to the readers of this Journal as one of the most active and observant of the staff of Naturalists attached to the Collins Overland Telegraph Expedition), follows suit to these observations of Professor Whitney by declaring, in a paper published in this Journal for January, 1868, that though he had carefully examined the country over which he had passed, in Alaska, for glacial indications, he had not found any effects attributable to such agencies. His own opinion, indeed, from what he had seen of the west coast, *though yet unproved*, was that the glacier-field never extended in these regions to the westward of the Rocky Mountains, though single glaciers have existed and still exist between spurs of the mountains which approach the coast. No boulders, according to Mr. Dall, such as are common in New England, no scratches or other marks of ice action had been observed by any of his party, though carefully looked for.

It is this general theory of the absence of the Northern Drift in Northwestern America that I propose combating in the remarks which follow, and I do so with extreme diffidence, knowing well from old experience the care and caution with which Prof. Whitney has proceeded in his remarkable geological survey of California, as well as in his earlier work on the shores of Lake Superior. For this reason I will speak only of what I know from personal knowledge of the districts visited by myself, calling in, however, the observations of others as corroboration of my statements.

As far as Alaska and California, and even Oregon and Washington Territory, are concerned, I must leave the question of glacial remains within their boundaries, to observers more intimately acquainted with their country than I am, though I have a strong inclination to believe that what I say about other portions of the Pacific coast will hold equally good regarding them also. I have certainly visited and traveled through California, and have been in some portions both of Oregon and Washington Territory, and on the borders of Alaska, yet my knowledge of these countries does not entitle me to dispute statements so explicitly made by such excellent observers as those cited. But with the coast of British Columbia and the whole of Vancouver Island I am very intimately acquainted—perhaps more intimately than any other single individual—and can speak positively regarding the marked presence of true Northern Drift there, so that with every respect to the opinion of so distinguished a geologist as Prof. Whitney, I am compelled to dissent from his theory regarding the entire absence of glacial remains proper, from the Pacific slope of the Rocky Mountains.

Between 1863 and 1866—nearly four years—I traveled on foot and in canoes, through the forests, over the mountains, on the rivers, the lakes and the prairies of the whole of the region indicated, as Commander and Government Agent of the First Vancouver Exploring Expedition, and as Botanist of the British Columbia Expedition. Again I have three times visited the Arctic Regions, passing a whole summer in Greenland, studying these and other similar phenomena, and have for many years been very familiar with the remains of the Northern Drift in Scotland, the north of England and portions of the north of Europe. These personal particulars are mentioned to show that I am in a position to know glacial remains when found, and to distinguish them from the ordinary terrestrial debris accumulated by causes now in action in the temperate countries where formed. The result of these extended observations has, therefore, been to confirm me in an opinion entirely contrary to that expressed by Messrs. Whitney, Ashburner and Dall, viz: that so far from the Northern Drift being absent from Vancouver Island and British Columbia, it is present in as marked a manner as ever I saw it in countries celebrated for the presence of such remains. This opinion I casually expressed in 1869 in a memoir entitled *Das Innere der Vancouver Insel*, published in the volumes of Petermann's *Geographische Mittheilungen* for that year, and more recently and more explicitly, in another, *On the Physical Characteristics and Geographical Distribution of the Coal Fields of Northwest America* (Transactions of the Geological Society of Edinburgh, 1868–69). As that statement has been inclined to be called in question—scientific sceptics not unreasonably considering that a doctrine promulgated by so eminent a geologist as Prof. Whitney is entitled to further consideration, than a mere curt denial of its truth, I have considered it proper to present in a concise manner in this place the facts on which I base my disbelief in its truth. As early as 1860, Mr. Henry Bauermann, geologist of the British Northwest Boundary Commission, made many observations on this subject; and subsequently, in 1862, in a *Prize Essay on Vancouver Island, its resources and capabilities as a Colony* (Victoria, 1862), Dr. Charles Forbes, R.N., published similar facts, which my own researches have only tended to confirm and enlarge, over a greater area. Dr. Forbes showed, what is familiar to every one visiting that section, that in the whole southern portion of the Island, though from the open prairie-like character of some portions of the southeastern section it is there earlier observed than in the wooded districts, the scooping, grooving, and scratching of the rocks by ice action is very marked. The chief rock *in situ* there is a dense, hard, feldspathic trap, and this is ploughed in many places into furrows six to eight inches deep, and from six to eighteen inches wide. The ice action is also well shown in the sharp peaks of the erupted, intruded rocks, having been broken off and the surface smoothed and polished, as well as grooved and furrowed, by the ice action on a sinking land, giving to the numerous promontories and outlying islands which here stud the coast, the appearance of rounded bosses between which the soil is found to be composed of sedimentary alluvial deposits, containing the debris of tertiary and recent shelly beaches, which have, after a period of depression, been again elevated to form dry land, and to give the present aspect to the physical geography of Vancouver Island.

The whole surface of the country is strewn with erratic boulders. Great masses of 60 to 100 tons in weight,—chiefly of various igneous and crystalline, as well as sedimentary rocks, sufficiently hard to bear transportation,—are found scattered everywhere over the island from north to south, and through the region lying on the western slope of the Cascade Mountains. Some of these syenitic or granitic boulders are of a fine grain and accordingly some of the chief buildings in Victoria are built from them. I am not aware that any rock of a similar description is found *in situ* anywhere in Vancouver Island; it appears to have drifted in icebergs from the north. I am cordially of opinion with Dr. Forbes, that though the last upheaval of the land, which might have taken place at a geologically recent period, failed to connect Vancouver Island with the mainland of North America; it was at all events sufficient to effect to a great extent, the junction of numerous insular ridges, and thus to form a connected whole of what *was*, and might have continued to be, only an archipelago of scattered islets. The upheaving force elevated and connected these and brought to the surface, the great clay, gravel and sand deposits of the northern Drift which had swept over, and been deposited on, the submerged land. These sands, gravels and clays, were now to form the soil of land, prepared for the habitation of man. These constituents of the drift remain, in many parts, thinly covered by a coating of vegetable mould; but much has been washed away. The clay remains most generally and widely spread out, as a retentive sub-soil, having resting upon it a thick coating of vegetable mould. The most valuable soil is found sweeping down the sides of gentle slopes, filling up hollows and swampy bottoms, and, mixed with the rich alluvial deposits of such districts as Saanich, Cowitchan, Delta of Nanaimo, and Comax, forms an inexhaustible source of agricultural wealth. The true glacial or boulder clay is found in various portions of Vancouver Island. Bauermann has described it as seen near Victoria, and I am glad to be able to vouch for the correctness of his description: it is extensively developed not only there but on the opposite coasts of Washington Territory and British Columbia. In the neighborhood of Esquimault and Victoria, the rocks are deeply scored and grooved along the shore, and large boulders are scattered irregularly over the surface of the country, as already described. The other rocks observed as erratics were black cherty conglomerate, similar to that underlying the secondaries,* dark laminated mica schist with well defined garnet-crystals, hornblende rock and largely crystalline greenstone, and rarely and in small masses vesicular obsidian and pitchstone.

The following section is given to show the general character of the drift, at Esquimault Harbor.

Black sandy and peaty ground with broken shells	2 to 6 feet
Yellowish sandy clay with casts of shells (Cardium and Mya) and a few pebbles and boulders	6 to 8 "
Gravel of scratched pebbles resting on rock	2 to 3 "

The rocks are grooved and scratched at the junction; the direction of the glacial markings is between N.–S. and N.N.W–S.S.E. In a well sinking at Esquimault Barracks (for the boundary Commission) the lower gravel was reached at 42 feet, after going through a sandy blue clay without shells or boulders. The section in the cliff between Albert Head and Esquimault is as follows:—

Blue drift clay with boulders; junction with rock not seen,	70 feet
Fine sand and gravel, passing upward into coarse quartzose gravel,	100–120 "

—*Quart. Journ. of Lond. Geol. Soc.*, 1860, p. 202.

Mr. Bauermann is, or at least was, at the period his observations were made, a member of the Geological Survey of the British Islands, and therefore might be supposed to know what he was speaking about. I say so because though I have been able to confirm all his descriptions, yet it is satisfactory in a subject of controversy involving so many important matters to have the support of an additional qualified witness.

As already remarked, I cannot speak so confidently of Washington Territory, Oregon, and the interior of British Columbia to the east of the Cascade Mountains, being less familiar with that section of the Pacific slope. However, through western Oregon wherever I visited the country, down at least to the Umpqua river, and in Washington Territory to the very base of the Cascades, whatever further, I observed glacial remains not less marked than in the neighboring region of Vancouver Island. Some of the erratic blocks are scattered over the prairies of that region, standing on the stoneless grassy plains in marked contrast to their surroundings. These boul-

---

* "Tertiaries," Mr. Bauermann says, but if he refers to the Northern coal fields of the island, then there can be but little doubt that these beds are as I have given them, here and elsewhere, of secondary age.

ders and erratic blocks* have even attracted the notice of the Indians—otherwise so stolid in regard to the natural objects among which they live. One huge angular block, on the Snoqualami Prairie, has a tradition attached to it, to the effect that at one time it was suspended from the sky but was cast adrift to earth on account of the wrath of the Supreme Being, being roused at the licentiousness of a minor god and his myrmidons who for the time being were disporting themselves on it! Not far from the corner of the Peninsula of Saanich off the coast of Vancouver's Island, there are several large boulders—(apparently rounded by the waves and not by ice action?) which aboriginal tradition assert to have been some old witches turned into stone. My canoe-men in passing them used not unfrequently to stop there, and throw water on them, shouting: "Give us a wind, you old jade!" and as occasionally an afternoon breeze does not spring up in that region after the midday summer calms, the superstition obtained with them a semblance of belief, and so got handed on to posterity clothed in all the hoary sanctity of antiquity.

Groovings and other unequivocal marks of *general* ice action are not wanting in Washington Territory either. Even with the superficial glance we were enabled to give the subject in hurried journeys over that region, for other purposes, we observed not a few of such deep unmistakable ice planings. And in a note received recently from my friend and former traveling companion, Mr. Edmund T. Coleman, (well known as the author of the folio "Scenes from the Snow Fields of Mont Blanc," and who may therefore be supposed to know ice markings) he states, though with no view to combating the theory in hand, which indeed he knew nothing about:—

"I saw at Seahome (near Bellingham Bay) in the cuttings made for a tramway, the finest instances of fluting and grooving, evidences of glacial action, that I have ever seen on this coast. They were 90 feet in length, running N. and S. according to the theory of Professor Agassiz."

I have not been in Alaska proper, but in 1866 in a visit to the Queen Charlotte Islands lying some thirty or forty miles off the northern coast of British Columbia, close to the southern boundary of the former territory,† marks of the northern Drift quite as marked as in Vancouver's Island were found there.

Indeed in crossing the "spit" at the entrance of Skidegate

---

* By "boulders" I mean to designate rounded worn blocks of stone carried along in the *moraine profonde* of the ice sheet; by "erratic blocks," angular fragments of rock. apparently conveyed to their present resting place in ice (field ice or bergs) without having been subject to erosion. The necessity of the division is apparent. The first is very rare on the Pacific coasts and I suspect part of local moraines; the other is universal.

† I have described their geography in the Proceedings of the Royal Geographical Society of London, for 1869. Some remarks on their geology will likewise be found in the same place.

Sound, we had a dangerous reminder of the fact, having lost the keel of our schooner, on one of the great boulders which cover that locality. Masset Spit and other shoal localities are equally dangerous on that account. Here they present themselves disagreeably to the seaman's senses, but on *land*, though less visible on account of the dense vegetation concealing them, yet to one accustomed to search for such things, traveled blocks and ice groovings are sufficiently abundant. Boulder clay is also not wanting to complete the tale of the glacial period in Northwest America.

All throughout this paper I have sedulously avoided touching upon the modern local glaciers which are found scattered all throughout the northern portion of the Cascade and Coast Ranges of Mountains, in some places (as in some of the northern inlets on the coast of British Columbia) approaching to within a short distance of the sea; and in the southern part of the latter range they are found in most of the high mountains, such as Mt. Baker, Diamond Peak, etc. In another place, "On the formation of Fjords, etc."* I have shown that in all likelihood these British Columbian inlets were at one time the site of glaciers, and though the marks of local glaciers are evident here and there where none are now found, yet the appearances described are due to a totally different set of causes from these, or any now in existence on the American continent, unless indeed Greenland be included under that geographical division. These local glaciers in the limits assigned to a paper of this nature do not therefore require to be further touched upon.

Am I therefore in error, when I think that the case I have submitted, makes good the thesis with which I commenced these remarks, viz:—that whatever may be said of California and Alaska (and Messrs. Whitney and Dall are quite capable of holding their own in reference to their assertions about these regions), the Northern drift is certainly not absent from British Columbia, Vancouver's Island, Washington Territory and the Queen Charlotte Islands? With every respect to the observations of the gentlemen named, my more extended opportunities of investigation have, I think, enabled me to answer, with some degree of certainty, this question in the negative. Perhaps I would not have been so particular in discussing this question at length, had not Prof. Whitney's and Mr. Dall's idea been taken up in this country, and in America by geologists of no mean eminence,† and a disposition been shown by others less capable to build thereon theories, where no theories ought to be built.

4 Gladstone Terrace, Hope Park, Edinburgh, June 23d, 1870.

---

* Journal of the Royal Geographical Society, vol xxxix.

† Foster in "Mississippi Valley," p. 338, and A. Geikie in "Nature," vol. i, p. 436.

Art. XXXI.—*Extracts from the Address of* George Bentham, Esq., President of the Linnean Society, on the 20th of May, 1870.

It had been my intention on the present occasion to carry on the sketches of the general progress of biological science which I had attempted in 1862, 1864, 1866, and 1868; but I have, from various causes, been unable to devote so much time as usual to the preparation of my Address, and feel obliged to confine myself to a few points connected with subjects of special interest to myself, which, within the last two or three years, have made considerable advances.

The most striking are, without doubt, the results obtained from the recent explorations of the deep-sea faunas, and from the investigation of the tertiary deposits of the arctic regions, which, although affecting two very different branches of natural science, I here couple together, as tending, both of them, to elucidate in a remarkable degree one of the most important among the disputed questions in biological history, the continuity of life through successive geological periods.

An excellent general sketch of the first discovery and progressive investigation of animal life at the bottom of the sea at great depths, up to the close of the season of 1868, is given by Dr. Carpenter in the Proceedings of the Royal Society, vol. xvii, No. 107, for Dec. 17, 1868. The results of the still more important expedition of the past year have as yet been only generally stated by Mr. Gwyn Jeffreys, in the numbers of 'Nature' for Dec. 2 and 9, 1869, and by Dr. Carpenter, in a lecture to the Royal Institution, published in the numbers of 'Scientific Opinion' for March 23 and 30 and April 6 and 13 of the present year; and further details, as to the Madreporaria, are given by Mr. Duncan in the Proceedings of the Royal Society, vol. xviii, No. 118, for March 24 of the present year; whilst, in North America, the chief conclusions to be drawn from these researches into the deep-sea fauna are clearly and concisely enumerated by Prof. Verrill, in the American Journal of Science for January last; and some of the more detailed reports of the American explorations, by Louis and Alexander Agassiz and others, have been published in the Bulletin of the Museum of Comparative Zoology at Harvard College, Nos. 6, 7, and 9 to 13. For the knowledge of the data furnished by the tertiary deposits of the arctic regions we are indebted almost exclusively to the acute observations and able elucidations of Prof. O. Heer, in his 'Flora Fossilis Arctica,' in his paper on the fossil plants collected by Mr. Whymper in North Greenland, published in the last part of the Philosophical Transactions for

1869, and in the as yet only short general sketch of the results of the Swedish Spitzbergen Expeditions, contained in the Genevese 'Bibliothèque Universelle, Archives Scientifiques,' for Dec. 1869.

It would be useless for me here to retrace, after Dr. Carpenter and Prof. Verrill, the outlines of the revolution which these marine discoveries have caused in the previoualy conceived theories, both as to the geographical distribution of marine animals, and the relative influences upon it of temperature and depth, and as to the actual temperature of the deep seas, or to enter into any details of the enormous additions thus made to our knowledge of the diversities of organic life; and it would be still further from my province to consider the geological conclusions to be drawn from them. My object is more especially to point out how these respective dips into the early history of marine animals and of terrestrial forests have afforded the strongest evidence we have yet obtained, that apparently unlimited permanency and total change can go on side by side, without requiring for the latter any general catastrophe that should preclude the former.

There was a time, as we learn, when our chalk-cliffs, now high and dry, were being formed at the bottom of the sea by the gradual growth and decay of Globigerinæ and the animals that fed on them—amongst others, for instance, *Rhizocrinus* and *Terebratulina caput-serpentis;* and when, at a later period, the upheaval of the ground into an element where these animals could no longer live arrested their progress in that direction, they had already spread over an area sufficiently extensive for some part of their race to maintain itself undisturbed; and so, on from that time to the present day, by gradual dispersion or migration, in one direction or another, the same *Rhizocrinus* and *Terebratulina* have always been in possession of some genial locality, where they have continued from generation to generation, and still continue, with Globigerinæ and other animals, forming chalk at the bottom of the sea, unchanged in structural character, and rigidly conservative in habits and mode of life through the vast geological period they have witnessed. So also there was a time when the hill-sides of Greenland and Spitzbergen, now enveloped in never-melting ice and snows, were, under a genial climate, clothed with forests, in which flourished *Taxodium distichum* (with *Sequoiæ*, *Magnoliæ*, and when at a later period these forests were destroyed by the general refrigeration, the Taxodium already occupied an area extensive enough to include some districts in which it could still live and propagate; and whatever vicissitudes it may have met with in some parts, or even in the whole, of its original area, it has, by gradual extension and migration, always found some spot where it has gone on and thriven, and continued its race from generation to generation down to the present day, unchanged in character and unmodified in its requirements. In both cases, the permanent animals of the deep-sea bottom and the permanent trees of the terrestrial forests have witnessed a more or less partial or complete change in the races amongst which they were commingled. Some of these primitive associates, not endowed with the same means of dispersion, and confined to their original areas, were extinguished by the geological or climatological changes, and replaced by other races amongst which the permanent ones had penetrated, or by new immigrants from other areas; others, again, had spread like the permanent ones, but were less fitted for the new conditions in which they had been placed, and in the course of successive generations had been gradually modified by the Darwinian process of natural selection, the survival of the fittest only among their descendants. If, in after times, the upheaved sea-bottom becomes again submerged, the frozen land becomes again suited for vegetation, they are again respectively covered with marine animals or vegetable life, derived from more or less adjacent regions, and more or less different from that which they originally supported, in proportion to the lapse of time and extent of physical changes which had intervened. Thus it is that we can perfectly agree with Mr. Duncan, that "this persistence (of type and species through ages, whilst their surroundings were changed over and over again) does not indicate that there have not been sufficient physical and biological changes during its lasting to alter the face of all things enough to give geologists the right of asserting the succession of several periods:" but we can, at the same time, feel that Dr. Carpenter is in one sense justified in the proposition, that we may be said to be still living in the Cretaceous period. The chalk formation has been going on over some part of the North Atlantic sea-bed, from its first commencement to the present day, in unbroken continuity and unchanged in character.

If once we admit as demonstrated the coëxistence of indefinite permanency and of gradual or rapid change in different races in the same area, and under the same physical conditions, according to their constitutional idiosyncracies, and also that one and the same race may be permanent or more or less changing, according to local, climatological, or other physical conditions in which it may be placed, we have removed one of the great obstacles to the investigation of the history of races, the apparent want of uniformity in the laws which regulate the succession of forms. We may not only trace, with more confidence, such modifications of race through successive geological periods as Prof. Huxley has recently exhibited to us in respect of the Horse, but we can understand more readily the absolute identity of certain species of plants inhabiting widely dissevered areas, of which the great majority of species are more or less different. One of the arguments brought forward against the community of origin of representative species in distant regions, such as temperate Europe and the Australian Alps, the Arctic Circle and Antarctic America, the Eastern United States and Japan respectively—an argument which has long appeared to me to have considerable weight—was this:—if disseverance and subsequent isolation result necessarily in a gradual modification by natural selection, how is it that when all are subjected to the same influences, the descendants of some races have become almost generically distinct in the two regions, whilst others are universally acknowledged as congeners, but specifically distinct, and others again are only slight varieties or have remained absolutely identical? To this we can now reply, with some confidence, that there is no more absolute uniformity in the results of natural selection than in any other of the phenomena of life. External influences act differently upon different constitutions. Were we to remove the whole flora and fauna of a country to a distant region, or, what comes to the same thing, change the external conditions of that flora and fauna, as to climate, physical influences, natural enemies, or other causes of destruction, means of protection, &c., we should now be taught to expect that some of the individual races would at once perish; others, more or less affected, might continue through several generations, but with decreasing vigor, and, in the course of years or ages, gradually die out, to be replaced by more vigorous neighbors or invaders. Others, again, might see amongst their numerous and ever varying offspring some few slightly modified, so as to be better suited for the new order of things; and experience has repeatedly shown that the change once begun may go on increasing through successive generations and a permanent representative species may be formed. And some few races might find themselves quite as happy and vigorous under their new circumstances as under the old, and might go on as before, unchanged and unchanging.

Taking into consideration the new lights that have been thrown upon these subjects by the above investigations and by the numerous observations called forth by the development of the great Darwinian theories, amongst which I may include a few points adverted to in a paper on Cassia which I laid before you last year, but which a press of matter has prevented our yet sending to press, it appears to me that, in plants at least, we may almost watch, as it were, the process of specific change actually going on; or at least we may observe different races now living in different stages of progress, from the slight local variation to the distinct species and genus. As the first step we may take, for instance, those races which are regarded by the majority of botanists as variable species, such as *Rubus fruticosus*, *Rosa canina*, *Zornia diphylla*, *Cassia mimosoides*, &c. We shall find in each some one form, which we call typical, generally prevalent over the greater part of the area of the race, whilst others, more or less aberrant, are more or less restricted to particular localities, the same varieties not occurring in disconnected stations with precisely the same combinations of character and in the same proportions; local and representative varieties and subspecies are being formed, but have not yet obtained sufficient advantages to prevent their being kept in check by their intercommunication (and, probably, cross-breeding) with their more robust type. The British Batologist or Rhodologist transported to the south of France or to Hungary will still find one, or perhaps two or three forms of Bramble and Dog-rose with which he is familiar; but if he wishes to discriminate the thirty or forty varieties or subspecies upon which he had spent so much labor and acuteness at home, he must recommence with a series of forms and combinations of characters quite new to him. The species is still the same; the varieties are changed. As examples of what we may call a second stage in the formation of species, we may adduce such plants as *Pelargonium australe* or *grossularioides* and *Nicotiana suaveolens* or *angustifolia*, to which I alluded in the above-mentioned paper on *Cassia*. Here we have one race, of no higher than specific grade in the ordinary acceptance of the term, inhabiting two countries which have long been widely dissevered (in the one case South Africa and Australia, in the other Chili and Australia), which, if originally introduced by accident from one country to the other, have been so at a time so remote as thoroughly to have acquired an indigenous character in both; in both they are widely spread and highly diversified: but among all their varieties one form only is identical in the two countries (*Pelargonium australe*, var. *erodioides*, and *P. grossularioides*, var. *anceps*; *Nicotiana suaveolens*, var. *angustifolia*, and *N. angustifolia*, var. *acuminata*), and that so comparatively a rare one that it may be regarded as being in the course of extinction; whilst all other varieties, some of them very numerous in individuals over extended areas, and all connected by nice gradations, diverge nevertheless in the two countries in different directions and with different combinations of characters, no two of them growing in the two countries being at all connected but through the medium of that one which is still common to both. When that shall have expired, the distinct species may be considered established. A still further advance in specific change is exemplified in *Cassia* itself, in which I have shown that no less than eight or nine different modifications of type, sectional and subsectional, are common to South America, tropical Africa, and Australia, but without any specific or, at least, subspecific identity, except perhaps in a few cases where a more modern interchange may be presumed. The original common specific types are extinct, the species have risen into sections. Common types of a still higher order have disappeared in the case of Proteaceæ, an order so perfectly natural and so clearly defined that we cannot refrain from speculating on the community of origin of the African and of the Australian races, both exceedingly numerous and reducible to definite groups—large and small well-marked genera in both countries, and yet not a single genus common to the two; not only the species, but the genera themselves have become geographical. As in the case of the varieties of *Pelargonium* and *Nicotiana*, so in that of the species of *Cassia* and of the genera of Proteaceæ, it is not to be denied that precisely similar modifications of character are observed in the two countries; but these modifications are differently combined, the changes in the organs are differently correlated. In Asiatico-African *Chamœcristæ* a tendency to a particular change in the venation of the leaflet is accompanied by a certain change in the petiolar gland; in America the same change in the gland is correlated with a different alteration in the venation. In Australian Proteaceæ the glands of the torus are constantly deficient with a certain inflorescence (cones with imbricate scales) which is always accompanied by them in Africa.

In selecting the above instance for illustration of what we may, without much strain upon the imagination, suppose to be cases of progressive change in races, it is not that they are isolated cases or exceptionally appropriate; for innumerable similar ones might be adduced. In the course of the detailed examination I have had successively to make of the floras of Europe, N. W. America, Tropical America, Tropical Africa, China, and Australia, I have everywhere observed that community of general type, in regions now dissevered, is, when once varied, accompanied by more or less of divergence in more special characters in different directions in the different countries.

With regard to the succession of races which have undergone a complete specific change through successive geological periods, we have not in plants, as far as I am aware, any such cases of "true linear types or forms which are intermediate between others because they stand in a direct genetic relation to them," as Professor Huxley appears to have made out in favor of the pedigree of the Horse in his last Anniversary Address to the Geological Society. And I may, in regard to plants, repeat with still greater emphasis his dictum, that "it is no easy matter to find clear and unmistakable evidence of filiation among fossil animals; for in order that such evidence should be quite satisfactory, it is necessary that we should be acquainted with all the most important features of the organization of the animals which are supppsed to be thus related, and not merely with the fragments upon which the genera and species of the paleontologist are so often based." The difficulty is much greater in the case of fossil plants; for instead of bones, teeth, or shells, portions of internal or external skeletons, the parts preserved to us from the tertiary period are generally those least indicative of structural organization. Mr. Carruthers has recently (Geological Magazine, April and July 1869, and Journal of the Geological Society, August, 1869) adduced satisfactory evidence of the close affinity of *Sigillaria* and the allied genera of the coal-period with the living Lycopodiaceæ, formerly suggested by Dr. Hooker; but, as he informs me, no connecting links, no specimens, indeed, of the whole Order, have as yet been found in any of the intermediate cretaceous or tertiary deposits. Among the latter, the presence of numerous vegetable types, to which we may plausibly refer as to the ancestors of living races, is established upon unimpeachable data; but I have been unable to find that a single case of authentic pedigree, as successively altered from the cretaceous through the abundant deposits of the eocene and miocene period to the living races, has been as yet as satisfactorily made out as that of the absolute identity of *Taxodium* and others above mentioned, although I feel very little doubt that such a one will yet be traced when our pâleontologists shall have ceased to confound and reason alike upon the best proved facts and the wildest guesses. Our late distinguished foreign Member, Professor Unger, whose loss we have so recently to deplore, had indeed, shortly before his death, published, under the name of 'Geologie der europäischen Waldbäume, part I, Laubhölzer,' no less than twelve tabular pedigrees of European forest races; but it seems to me that in this, as in another of the same eminent paleontologist's papers to which I shall presently have to refer, his speculations have been deduced much more freely from conjectures than from facts. There is no doubt that the presence of closely allied representatives of our Beeches, Birches, Alders, Oaks, Limes, &c., in the tertiary deposits of Central and Southern Europe is fully proved by inflorescences and fruits as well as leaves; but how can we establish the successive changes of character in a race when we have only the inflorescence of one period, the fruit of another, and the leaf of a third? I do not find a single case in which all three have been found in more than one stage; and by far the great majority of these fossil species are established on the authority of detached leaves or fragments of leaves alone.

Now let us consider for a moment what place a leaf really holds in systematic botany. Would any experienced systematic botanist, however acute, on the sole examination of an unknown leaf, presume to determine, not only its natural order and genus, but its precise characters as an unpublished species? It is true that monographists have sometimes published new species founded on specimens without flower or fruit, which from collateral circumstances of habitat, collector's notes, general resemblance, &c., they had good reason to believe really belonged to the genus they were occupied with; but then they had the advantage of ascertaining the general *facies* derived from insertion, relative position, presence or absence of stipular appendages, &c., besides the data supplied by the branch itself. And with all these aids, even the elder De Candolle, than whom no botanist was more sagacious in judging of a genus from general aspect, proved to have been in several instances far wrong in the genus, and even Order, to which he had attributed species described from leaf-specimens only. Paleontologists, on the other hand, have, in the majority of these tertiary deposits, had nothing to work upon but detached leaves or fragments of leaves, exhibiting only outward form, venation, and, to a certain degree, epidermal structure, all of which characters may be referred to that class which Professor Flower, in his introductory lecture at the Royal College of Surgeons in February last, has so aptly designated as *adaptive*, in contradistinction to essential and fundamental characters. They may, when taken in conjunction with relative individual abundance, assist in forming a general idea of the aspect of vegetation, and thus give some clue to certain physical conditions of the country; but they alone can afford no indication of genetic affinity, or consequently of origin or successive geographical distribution.

Lesquereux, in speaking of Cretaceous "species, or rather forms of leaves," observes, in a note to his paper on Fossil Plants from Nebraska (this Journal, vol. xlvi, July, 1868, p. 103), that "it is well understood that when the word *species* is used in an examination of fossil plants, it is not taken in its precise sense; for indeed no *species* can be established from leaves or mere fragments of leaves. . But as paleontologists have to recognize these forms described and figured, to compare them and use them for reference, it is necessary to affix to them specific names, and therefore to consider them as species." But the investigators of the tertiary floras of Central and Southern Europe have acquired the habit, not only of neglecting this distinction and naming and treating these forms of leaves as species equivalent to those established on living plants, but of founding upon them theories which must fall to the ground if such specific determination proves inaccurate. Nothing can be more satisfactory than such determinations as that of *Podogonium*, for instance, which Professor Heer has succeeded in proving, by numerous specimens of leaves, fruits, and even flowers, some of them still attached to the branches, which I had myself the pleasure of inspecting last summer under the friendly guidance of the distinguished Professor himself. This genus of Cæsalpinieæ, from its evident affinity with *Peltogyne*, *Tamarindus*, and others now scattered over the warmer regions of America and Africa, and more sparingly in Asia, tells a tale of much significance as to the physico-geographical relations of the Swiss tertiary vegetation, confirmed as it is by some other, equally or almost equally convincing examples. But the case appears to me to be far different with the theory so vividly expounded by Professor Unger in 1861 in his Address entitled "Neu Holland in Europa." This theory, now generally admitted, seems to me to be established on some such reasoning as this:—There are in the tertiary deposits in Europe, and especially in the earlier ones, a number of leaves that look like those of Proteaceæ are a distinguished feature in Australian vegetation; *ergo*, European vegetation had in those times much of an Australian type derived from a direct land communication with that distant region.

This conviction, that Proteaceæ belonging to Australian genera were numerous in Europe in Eocene times, is indeed regarded by paleontologists as one of the best-proved of their facts. They enumerate nearly one hundred tertiary species, and most of them with such absolute confidence that it would seem the height of presumption for so inexperienced a paleontologist as myself to express any doubt on the subject. And yet, although the remains of the tertiary vegetation are far too scanty to assert that Protaeceæ did not form part of it, I have no hesitation in stating that I do not believe that a single specimen has been found that a modern systematic botanist would admit to be Proteaceous unless it had been received from a country where Proteaceæ were otherwise known to exist. And, on other grounds, I should be most unwilling to believe that any of the great Australian branches of the Order ever reached Europe. As this is a statement requiring much more than mere assertion on my part, I shall beg to enter into some detail, commencing with a short summary of my grounds of disbelief in European tertiary Proteaceæ, and then examining into the supposed evidences of their existence.

* * * * * * *

From the above considerations, I cannot resist the opinion that all presumptive evidence is against European Proteaceæ, and that all direct evidence adduced in their favor has broken down upon cross-examination. And however much these Eocene leaves may assume a general character, which may be more frequent in Australia (in Proteaceæ and other Orders) than elsewhere, all that this would prove would be, not any genetic affinity with Australian races, but some similarity of causes producing similarity of adaptive characters.

Another series of conclusions drawn by paleontologists from their recent discoveries, which appears to me to have been carried too far, relates to the region where a given species originated. The theory that every race (whether species or group of species derived from a single one) originated in a single individual, and consequently in one spot, from which it has gradually spread, is a necessary consequence of the adoption of Darwinian views; and when Mr. R. Brown ("On the Geographical Distribution of Conifers," Trans. Bot. Soc. Edin., x, p. 195) sneers at my having qualified it as a perfect delusion, he must have totally misunderstood, or rather misread the passage he refers to in my last year's Address. The expression is there specially applied to the idea of general centers of creation whence the whole flora of a region has gradually spread, in contradistinction to the presumed origin of individual races in a single spot, which is there as distinctly admitted. The determination of where that spot is for any individual race, is a far more complicated question than either geographical botanists or paleontologists seem to suppose. "Every vegetable species," as well observed by Professor Heer, "has its separate history," and requires a very careful comparison of all the conclusions deducible as well from present distribution as from ancient remains. The very important fact that *Taxodium distichum*, *Sequoiæ*, *Magnoliæ*, *Salisburia*, &c., existed in Spitsbergen in Miocene times, so satisfactorily proved by Heer, shows that the vegetation of that country then comprised species and genera now characteristic of North America; but it appears to me that the only conclusion to be drawn (independently of climate and geology) is, that the area of these species and genera had extended continuously from the one country to the other, either at some one time, or during successive periods. The proposition that "Spitsbergen appears to have been the focus of distribution of *Taxodium distichum*," because an accidental preservation of its remains shows that it existed there in the lower Miocene period, would require at least to be in some measure confirmed by a knowledge of the flora of the same and preceding periods over the remainder of its present area, the greater part of which flora, however, is totally annihilated and forever concealed from us. The fact that *Pinus Abies* existed in Spitsbergen in Miocene times, and that no trace of it has been found in the abundant Tertiary remains of Central Europe is very instructive. It might show that that tree was of more recent introduction into the latter than the former country; but it cannot prove that it was not still earlier in some other region, whence it may have spread successively into both territories, still less that its course of dissemination was directly from Spitsbergen over Northern and Central Europe. Moreover the determination of *Pinus Abies* is not so convincing as that of the *Taxodium*, resting as it does, if I correctly understand Prof. Heer's expression, on detached seeds and leaves, with a few scales of one cone, and may require further confirmation.

In the above observations it is very far from my wish to detract from the great value of Professor Heer's researches. Interested as I have been in the investigation of the history of races of plants, I have deeply felt my general ignorance of paleontology, and consequent want of means of checking any conclusions I may have drawn from present vegetation by any knowledge of that which preceded it, and the impossibility at my time of life of entering into any detailed course of study of fossils. Like many other recent botanists, I am obliged to avail myself of the general results of the labors of paleontologists; and if I have here ventured on a few criticisms, it is only as a justification of the hope that they may in some measure distinguish proved facts from vague guesses, in order that we may know how far reliance is to be placed on their conclusions.

* * * * * *

ART. XXXII.—*Account of the fall of a Meteoric Stone in Stewart County, Georgia;* by Professor JOSEPH E. WILLET.

IN October, 1869, I learned that a metoric explosionh ad occurred in Stewart county, Georgia. I immediately requested Hon. John T. Clarke, a resident of the county adjoining Stewart, to enquire whether any stone or stones had fallen, and to endeavor to procure them for Mercer University. Judge Clarke, after considerable labor, was entirely successful in his search; and, through him, Mr. Barlow, in whose yard the meteorite descended, generously presented it to our Museum. To Judge Clarke and to Mr. Latimer, I am indebted for the following history of the phenomena attending the descent of the meteorite.

Mr. J. B. Latimer of Bladen's creek, Stewart county, has kindly furnished the following particulars of the *flight* of the body through the air, and of the several *explosions*, which occurred nearly vertically above him.

"The morning of the 6th October last (1869) was quite clear, scarcely any cloud being visible, quite calm; about 10 A. M. the atmosphere grew somewhat hazy, no clouds; at about 15 or 20 minutes before 12 M. a roaring, rushing sound was heard in a northwesterly direction, about 80 degrees above the horizon. In a moment or two, it was almost directly over head, at which point a loud explosion occurred, followed in rapid succession by six other reports, but less in volume than the first—making seven in all. The explosions appeared about as loud as a 12-pound cannon, at a distance of 10 or 12 miles. These explosions did not occur all at the same point in the heavens, but seemed to emanate from some body moving rapidly to the southeast. After the explosions, a peculiar whirring sound was heard, apparently produced by some large irregular body, moving very rapidly. This also went in a southeasterly direction. This sound was heard several seconds; many have compared it, and aptly too, to an imperfect steam-whistle. I have no precise idea of the time consumed in all this demonstration; some persons say several minutes—but I think 10 or 15 seconds would about cover the time.

"As the larger body was going out of our hearing, (some moments after the explosions) a smaller one passed to the southwest, with just such a noise as is always produced by a flying fragment of a shell after its explosion, or of any angular body cast violently through the air. This piece descended to the earth, distinctly traced in its passage by many persons, and struck in the yard of Capt. E. Barlow—which point of contact is, on an air-line, about 2½ miles from a perpendicular beneath where the explosions occurred. This is the only one known to have fallen in this section.

"The explosions, together with the rushing sound afterward, were heard over a region about 30 miles N.E. and S.W. and 50 or 60 miles N.W. and S.E. No shock was felt—at least no tremor of the earth.

"Two men say, that they were looking in the exact direction of the explosions at the time they occurred, and saw a quantity of vapor, much like the volume of steam escaping from the pipe of an engine, at each successive stroke; which vapor or mist was violently agitated, and increased in bulk, with each successive report, but disappeared soon after the cessation of the reports. This corroborates the testimony of some of my own laborers, who say, that immediately after the explosions something like a thin cloud cast its shadow over the field they were in."

Hon. John T. Clarke, of Cuthbert, Ga., who has interested himself in collecting the history of the meteorite, and through whose influence it has come into the possession of Mercer University, writes me the following particulars of its *fall*.

"It fell about 11½ A. M. on the 6th of October last, (1869), in Stewart county, Ga., on the premises of Elbridge Barlow, Esq., about 12 miles south of west from Lumpkin. Capt. Barlow picked it up a few moments after it fell. His account of it is this. While standing in the open yard, the sky being bright and clear, he heard first a succession of about three explosions, resembling sudden bursts of thunder, or discharges of artillery, followed by a deep roaring for several seconds, and then by a rushing or whizzing sound of something rushing with great speed through the air near by. The sound ceased suddenly. The noise, from first to last, was some half a minute. Two negroes were washing near the well, in the same yard, about sixty yards from where Barlow stood. They heard the noise, and supposed it to be the falling in of the plank well-curbing, banging from side to side in its descent, and so spoke of it to one another before it fell. While they were speaking thus, it struck the ground about twenty steps from them, in full sight, knocking up the dirt. They called Capt. B. and showed him the spot. It was upon very hard trodden ground in the clean open yard. The earth was freshly loosened up very fine in a circle of about one and a half feet in diameter; and, upon scraping the loose dirt away with the hands, the stone was found about ten inches below the surface. From the direction in which the ground was crushed in, it must have come from the northwest, and at an angle of about 30 degrees with the horizon. The stone when picked up was covered all over with the black shell which it bears now, except a triangular spot on one corner, about one inch each way, where the corner appeared freshly knocked off, and about four other spots near a quarter of an inch in diameter, where the shell was slightly knocked off. The other bruises, which you will find upon it, have been made since by persons who have handled it. To enable you to distinguish the original breaks upon it, I have marked each of them with a red cross. The stone still has a strong odor, which I will not undertake to describe. Capt. B. says it smelled stronger when he first picked it up. He does not remember that it had any noticeable heat. It was not cold, as a stone found so deep in the ground should be.

"The stone weighs now 12¼ ounces, about ½ ounce has been pecked off from it. Its color within is strikingly like very light granite; and, with the exceptions above noted, it is entirely covered with a smooth, almost black shell, a trifle thicker than common letter paper, so that externally it looks very much like a lump of iron ore. It is an irregular, seven-sided figure, its longest side being about 2¾ inches long. If put into a spherical form, it would make a ball about 1¾ inches in diameter. So far as I have been able to ascertain, no other parts have been found.

"The noise attending this phenomenon is variously described by different persons, and from different places. Two intelligent ladies residing four miles south of Lumpkin, nearly east of where the stone fell, and about ten or twelve miles off, describe it thus. While sitting in the house they heard, as it were, the sound of a great fire suddenly bursting forth from some confinement into the open air. They rushed out of doors, and heard the roaring sound continue for several seconds. They located the source of the noise in the direction of Barlow's.

"In Cuthbert, about 18 miles from Barlow's, nearly southeast, a gentleman, engaged in a workshop, heard a lumbering noise, which he took to be several heavy pieces of machinery in an adjoining room, falling down one after another. On going in, he found no one, and that he had mistaken the cause of the noise. Many persons here heard sounds like repeated thunder followed by roaring. Some say that they first heard several rapid, cracking explosions, like that of volleys of small arms, followed immediately by the louder burst of artillery. Most persons here thought the noise came from the southeast, passed over the place in a northwesterly direction, and died away in the distant northwest.

"The foregoing statements have been selected from many in circulation, showing how differently the senses were affected at different points. The facts are purposely presented in their nakedness. If you can find them available in aid of a scientific investigation of the origin of this phenomenon, I shall have accomplished more than I expect."

The above accounts agree as to the main facts. They were furnished by Mr. Latimer and Judge Clarke, without being compared by them. It is possible that a comparison of notes by them might have thrown some light on the point of greatest discrepancy, viz: the direction of flight. It is probable that the meteorite came from some point in the *north* quarter; the statement of Mr. Latimer over whom it exploded, and that of Mr. Barlow as to the direction in which the earth was penetrated, concur in this regard. Persons in Cuthbert, who represent it as coming from the south, may have been misled by an echo, mistaking this for the original sound.

Prof. J. Lawrence Smith, who is giving special attention to the subject of meteorites, has requested the privilege of analyzing the stone above described.

Mercer University, Penfield, Georgia.

ART. XXXIII.—*Description and Analysis of a Meteoric Stone that fell in Stewart county, Ga.* (*Stewart county Meteorite*), *on the 6th of October*, 1869; by J. LAWRENCE SMITH.

IN October, 1869, I learned through the public press that certain meteoric phenomena had occurred in Stewart county, Georgia, and that one or more stones had fallen. Enquiries were immediately instituted by me, and, through Prof. Willet, I obtained for examination the only stone found, one that was seen to strike the ground, and from him received an account of the phenomena observed at the time by Messrs. Latimer, Clarke and others. [See preceding Article.]

The stone, as it reached me, was nearly intact, and weighed 12¼ ounces; it must originally have weighed 12½ ounces. It is of an irregular conical shape, having a flattened base, and is covered with a dull heavy black coating. The specific gravity is 3·65. The fractured surface has a grayish aspect, and when examined closely, especially by the aid of a glass, exhibits numerous greenish globules with a whitish granular material between; through the mass are dark particles consisting principally of nickeliferous iron, with some pyrites, and a few specks of chrome iron. The nodules are sometimes three or more millimeters in diameter, and of an obscure fibrous crystalline structure, the crystals radiating usually from one side of the nodule; they have a dirty bottle-green color, a greasy aspect when broken, and are more or less opake.

Some of these little nodules were separated in a tolerable state of purity, amounting to 121 milligrams; on analysis they afforded:

		Oxygen.	Ratio.
Silica	48·62	25·90	2
Alumina	8·05	3·79	
Protoxyd of iron	11·21	2·51	1
Magnesia	30·18	11·80	
	98·06		

The hardness of the mineral is about 6, and it is quite tough. The formula would be $\dot{R}\bar{S}i$, with a part of the silica replaced by alumina, a not unfrequent case in minerals such as hornblende, hypersthene, &c. As it is impossible to derive any light from its crystalline structure, the above analysis warrants me in concluding that it is either *bronzite*, or *hornblende*, but I am more inclined to the former supposition as it appears to take the place of the enstatite in many meteorites.

Nickeliferous iron constitutes about 7 per cent of the mass, and a portion separated in as pure a state as possible, afforded on analysis—

Iron	86·92
Nickel	12·01
Cobalt	0·75
	99·68

These are the proportions after allowing iron for a small amount of sulphur, present in a minute quantity in the nickeliferous iron, which could not be separated mechanically. I did not test for copper or phosphorus; the quantity of iron separated from the stone did not warrant my making special analyses for substances, the quantity of which present could only be exceedingly minute.

The stony matter freed from the iron was treated with nitro-muriatic acid and water, and heated for some time over a water bath, renewing the water and acid once or twice; the solution was filtered, and the residue washed; the residue was then treated with a warm solution of caustic potash, filtered and again washed. The filtrate was neutralized by hydrochloric acid, and added to the first filtrate, and the whole evaporated to dryness over a water bath, warmed gently over the lamp, and treated with water and a little hydrochloric acid, thrown on a filter, the silica collected and estimated; the last filtrate was treated with a solution of hydrochlorate of baryta to ascertain the quantity of sulphuric acid present, (due to the pyrites in the original mass); it was found to indicate 6·10 per cent of magnetic iron pyrites. The solution freed from the excess of baryta was now analyzed in the ordinary way.

The insoluble portion of the meteorite was fused with carbonate of soda and a small fragment of caustic potash, and its ingredients ascertained.

A separate portion of the stony part of the meteorite was examined for alkalies.

The various analyses referred to above gave—omitting the nickeliferous iron:

The part soluble in acid	58·05
" " insoluble "	41·95

	Soluble part.	Insoluble part.
Silica	41·08	56·03
Alumina	0·32	5·89
Protoxyd of iron	18·45	15·21
Magnesia	41·06	21·00
Lime	----	0·10
Soda, with a little $\dot{K}$ and Li	----	2·97
	100·83	101·20

The soluble part consists principally of olivine. The insoluble is doubtless the bronzite already refer to, with a little albite or oligoclase.

Chrome iron was detected by fusing some of the stony part of the meteorite with carbonate of soda and a little niter, and separating in the usual way. The quantity was quite minute.

The composition of the stone as made out would be

Nickeliferous iron	7·00
Magnetic pyrites	6·10
Bronzite, or hornblende	86·90
Olivine	
Albite, or oligoclase	
Chrome iron	
	100·00

Art. XXXIV.—*Some practical remarks on the use of Flame Heat in the Chemical Laboratory, especially that from burning gas without the aid of a blast;* by J. Lawrence Smith, Louisville, Ky.

There is probably no more important era in the operations of the chemical laboratory than that of the introduction of the lamp as a source of heat for a large number of chemical operations, and that without the aid of a blast. Berzelius was doubtless the first to accomplish much in this direction, which he did by the agency of the lamp that so commonly bears his name, and which, more or less modified, is still in use where the ordinary illuminating gas is not to be had.

Although illuminating gas has been in use for about seventy years, it is only within a comparatively recent date that it has been pressed into service, and used as a heating agent in the laboratory. The reason of this arose from the fact that when burnt in the ordinary manner it deposited soot on the vessels heated by it. This difficulty has been overcome by burning the gas from small orifices made in a tube bent in the form of a circle, the holes being from 1 to 2 centimeters apart, and, sometimes, combining two or more rings in concentric circles. This method, however, has not been generally adopted.

We must date the successful introduction of gas for heating purposes to the use of a mixture of gas and air passed through wire gauze and ignited above the gauze, giving a flame without light and with great heat; the invention of this method is claimed by several, and doubtless was discovered by different individuals at about the same time, without a previous knowledge of each other's results; this method is still more or less employed for certain purposes.

The next step in this direction, and doubtless the most important up to the present time, is to burn the mixture of gas and air without the agency of wire gauze; it was first made known to the public in the burner commonly called the Bunsen burner, doubtless from its being either invented or brought into extensive practical use by the distinguished chemist of Heidelberg. Its form is too well known to require more than a mere mention here, and it is now made of all sizes from those capable of burning 4 cubic feet of gas and under, to those which can burn 15 or 20 cubic feet from a single burner, or from a combination of several smaller ones. To this burner, some material additions have been made by different individuals. J. J. Griffin, (the chemical instrument dealer in London), was, I believe, the first to introduce the use of the rosette and the register for the supply of air. The most remarkable results accomplished by this method of burning gas and air are those obtained by G. Gore of Birmingham, (all of whose results I have verified), where gold, copper, cast iron, &c., were fused in crucibles without the agency of any artificial blast. Mr. Gore evidently realized fully the true principle of burning this mixture, so as to obtain a maximum effect; the burner, however, with its furnace arrangements, is unavoidably of a form and on a scale limiting its application.

The usual form of the Bunsen burner, with the rosette and register (when required), bids fair to hold its own against any other form for general purposes, and whatever modifications may be made on it should be of such a character as not to entrench on its simplicity. One or two of these modifications are now in daily use in my laboratory, for which there is no claim to any special originality, nor are they intended to supplant the ordinary form.

As simple an instrument as the Bunsen burner appears to be, its principles and effects are well worthy of being carefully studied.

As the gas passes from the small orifices* in the lower part of the burner, and mixes with the air drawn in at the lower opening, and passes out at the open end of the tube, it usually contains not quite enough oxygen for its complete combustion, and requires free access of air to the outer portion of the flame

---

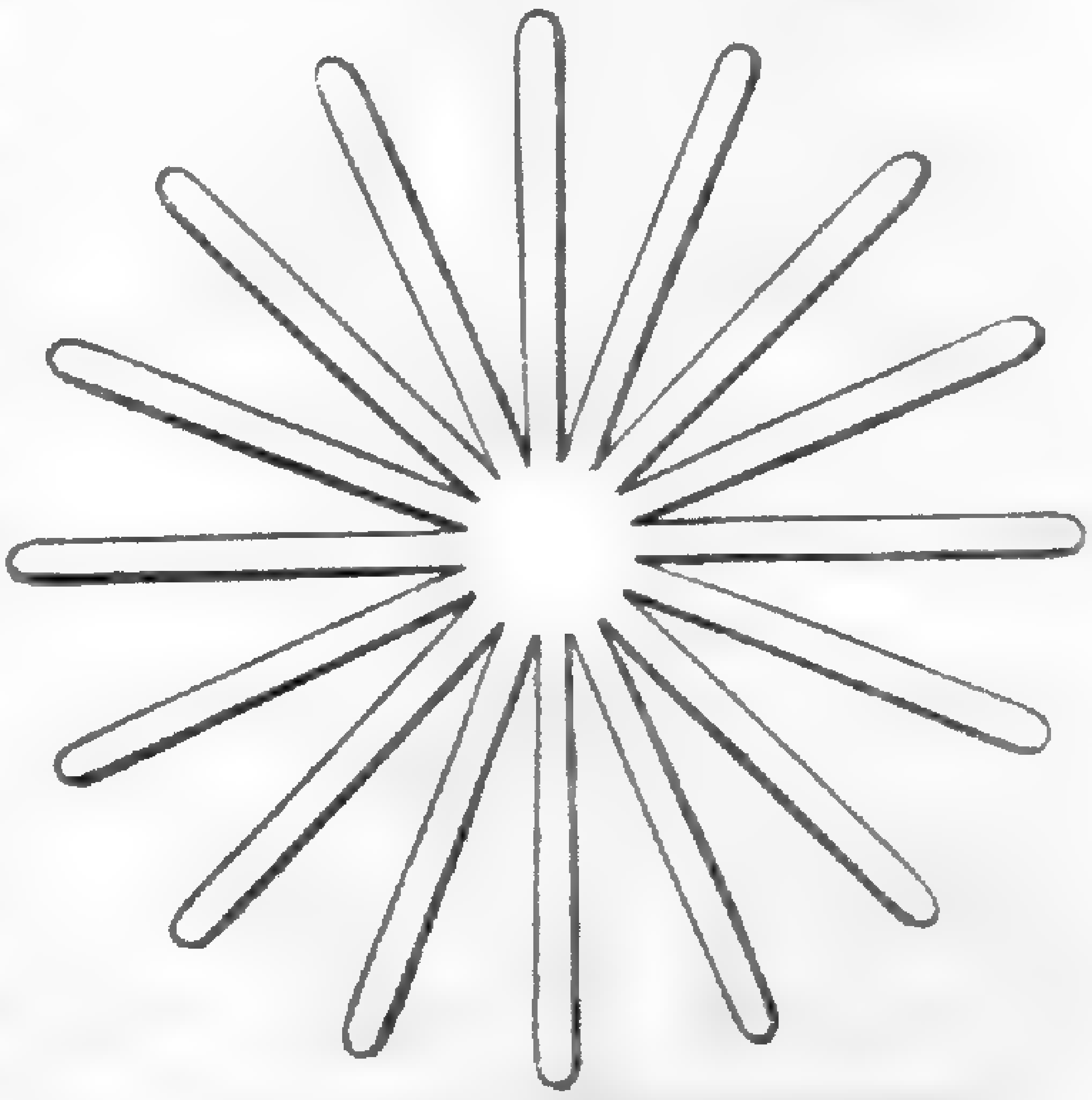
* The outlet for gas may be in the form of crossed slits or two small holes of (1-32 inch diameter each) for the small size burner, the length of tube being about 4 to 4½ inches long; the next larger has four openings (about 1-25 inch diameter each) and the tube about 5 inches long.

to complete the combustion; yet even with this, the flame is hollow in its lower portion, having a cool center, its most intense heat being at about three or four inches above the end of the tube in the smaller Bunsen-burners, and eight or ten inches in the largest size. If a proper access of air is not allowed to the flame, as sometimes happens in some of the furnace connections occasionally used with Bunsen's burner, acetylene is formed from the imperfect combustion, which is recognized by its disagreeable odor, or by collecting some of the gas formed during the combustion; the presence of acetylene may be rendered evident by a small amount of a solution of ammoniacal cuprous chlorid.

The best heating effects of the gas used in the ordinary round Bunsen burner, when employed in the heating of crucibles and other vessels, are not obtained; yet in the great majority of cases the small loss of gas is not worth considering, especially as to obtain better results in most cases, would only complicate this beautifully simple instrument.

To get the best effects of heat, we must imitate the principle applied in the Argand burner, namely to flatten down the exit of the mixed gases. It was by following out this principle that Mr. Gore was enabled to make a burner having a number of radial flat orifices as represented in the figure (1), the air from without having free access to the flame along the entire length of the slit openings, the number of slits used are more numerous than those represented in the figure. With the flame from this burner introduced into a certain form of refractory cylinders, cast iron can be melted in a crucible, without the aid of a blast, as has already been stated; the little chimney to the furnace being two inches in diameter, and four feet long. This burner and its furnace is of but limited application, and the amount of gas consumed considerable.

1.



The openings at the exit of Gore's burner.

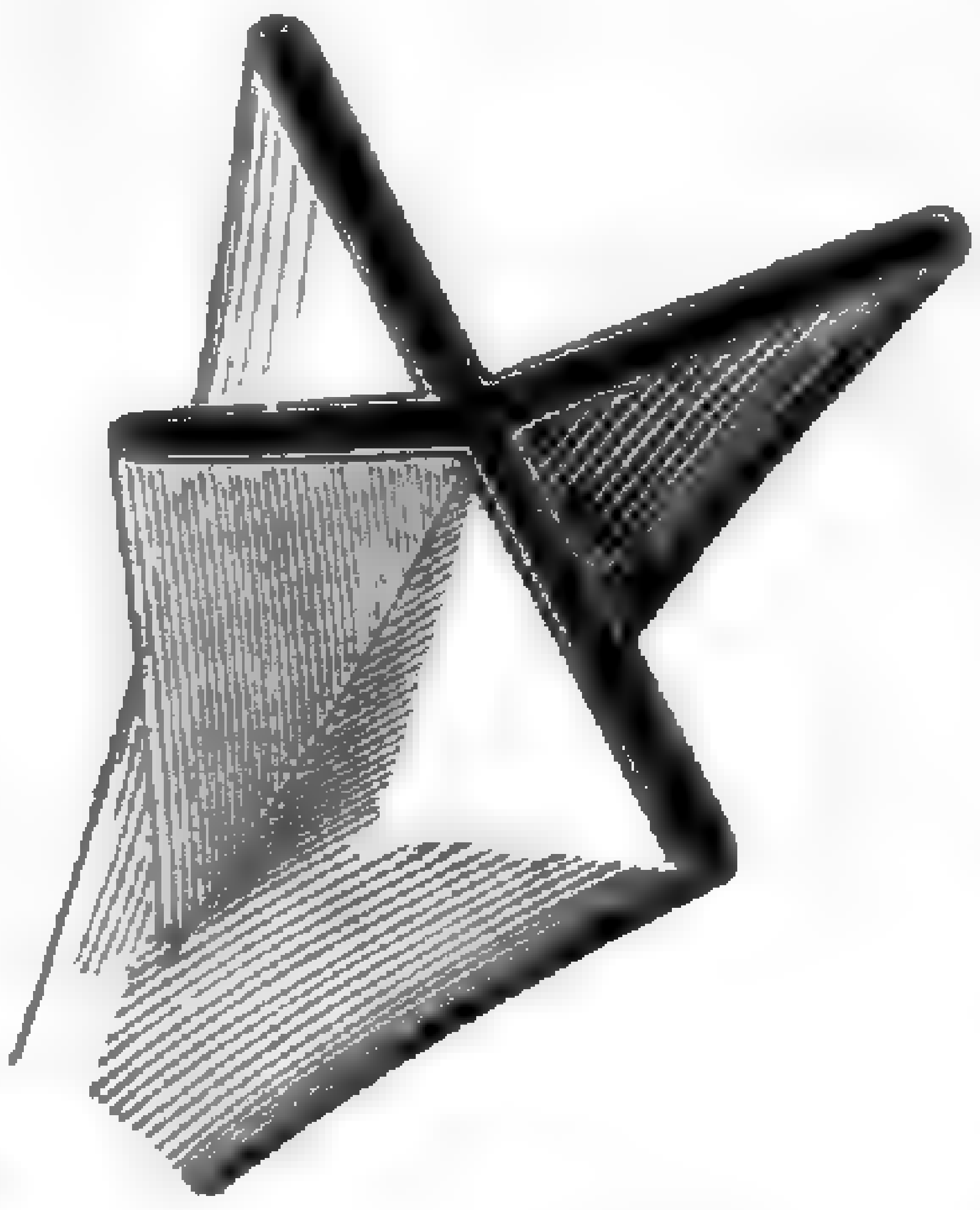
The principle, however, of the above burner is introduced in constructing a more simple form, and the flattened orifice is now used in the construction of what I conceive to be the best form of furnace for heating glass tubes for organic analyses and other purposes; such furnaces are made by Weisnig of Paris, and Desaga of Heidelberg.

The use of the flattened burner is not fully appreciated; its advantages are, that there is no cold point in the flame, and the burner can be brought much nearer to the object to be heated, within 20 to 25 millimeters for the small sized burners. In this burner as usually made, the opening is too broad, experience having convinced me that a slit 2 millimeters across and about 40 millimeters in length is the most effective one for a small size burner, consuming about 5½ cubic feet per hour; this burner is represented in fig. 1, which can be used with the ordinary tube, by detaching the tube with the flattened orifice.

By taking a burner of this description and putting two pieces on each side of the center, as represented in fig. 2, a very efficient burner is made for heating platinum crucibles in silica fusions, &c., and with such a burner, consuming 5½ to 6 cubic feet of gas per hour, I conduct most effectually all silica fusions in one hour or less, taking care to protect the crucible from the current of the air by a properly constructed short conical chimney, which chimney can be made of soap stone, sheet iron, or any other convenient material.

2.

3.



As was stated in the commencement of this article, it was not intended to describe the more complicated methods of burning gas in furnaces and by means of a blast, but to confine the remarks to the simpler forms in every day use, which can be made to accomplish all the requirements of the usual laboratory operations, and when a higher heat is required, the furnace must be our recourse, whether burning gas, charcoal or coke. The burner represented in figure 2 is the one I now employ in heating the crucible in my method of alkali determination with carbonate of lime and sal ammoniac, which method, with its more recent modifications, will be published in a very short time. The description of it, with all the minute details of manipulation, being ready for the press.

ART. XXXV.—*On the connection between Terrestrial Temperature and Solar Spots*; by CLEVELAND ABBE, Director of the Cincinnati Observatory.

WE are indebted to Sir Wm. Herschel for the suggestion that probably the presence of numerous spots on the surface of the sun is indicative of increased chemical activity, and is accompanied by increased radiation of heat. The investigations and theories of the past ten years however would lead us to an opposite conclusion from that of Herschel.

Immediately on the receipt of the Astronomische Nachrichten containing Wolf's tabular view of the relative frequency of the solar spots for the past three centuries, I made an extended comparison of the numbers therein given with such meteorological tables as were then accessible to me. After much labor I was forced to conclude that the variations of solar heat are so slight that they are masked in the local climatic peculiarites.

On further reflection, however, it seemed certain that the heat radiated from a dark spot should be of low intensity, and would therefore be largely absorbed by the aqueous vapor of our own atmosphere as well as by that of the sun. I have therefore been lately led to make a special study of the series of observations made on the Hohenpeissenberg, and published in the supplementary volume I. of the Annals of the Munich Observatory.* This series specially deserves attention because of the remarkable uniformity of the circumstances under which the observations were made; it extends from 1792 to 1850, omitting the years 1793, 1799, 1811, 1812 and 1817.

Assuming that the number of visible solar spots or groups are an index of the existing solar radiation of heat, we have but to compare the number (s) expressing the relative spot frequency as given by Wolf with the mean annual temperature, (t_1) as given by Lamont. The solution of the equation $t_1 = t_0 + s\tau$ gives us t_0 and τ, which latter is the coefficient of solar spot influence on the radiation of heat.

The accompanying table exhibits for each year the value of s and t_1,—the latter expressed in degrees of Reaumur.

The arithmetical mean of the annual temperatures gives

$$M_1 = +5^\circ{\cdot}178 \pm 0^\circ{\cdot}061$$

$$\text{prob. error of one annual mean} = \pm 0{\cdot}449$$

The residuals are given in the column $t_1 - m_1$.

Introducing the term $s\tau$ we find by the method of least squares

$$(t_1) = +5^\circ{\cdot}450(\pm 0^\circ{\cdot}086) - s \times 0^\circ{\cdot}00789(\pm 0^\circ{\cdot}00204)$$

$$\text{p. e. of one annual mean} = \pm 0^\circ{\cdot}430.$$

---

* Volume VII, containing the continuation of this series has not yet been received.

The residuals are given in the column $t_1-(t_1)$.

Year.	s.	t_1	t_1-m_1	$t_1-(t_1)$	t_2	t_2-m_2	$t_2-(t_2)$
		°	°	°	°	°	°
1792*	53	5·38	+0·20	+0·38	+7·11	+0·28	+0·43
94	24	6·16	+0·98	+0·90	7·77	+0·94	+0·85
95	16	5·66	+0·48	+0·34	7·32	+0·49	+0·34
96	9	5·36	+0·18	−0·02	6·90	+0·07	−0·14
97	6	6·06	+0·88	+0·66	7·77	+0·94	+0·71
98*	3	5·30	+0·12	−0·13	7·61	+0·78	+0·53
1800	10	6·24	+1·06	+0·87	8·68	+1·85	+1·65
01	31	5·56	+0·38	+0·35	7·23	+0·40	+0·37
02	38	4·64	−0·54	−0·51	7·50	+0·67	+0·70
03	50	4·84	−0·34	−0·22	6·56	−0·27	−0·15
04	70	5·31	+0·13	+0·41	6·87	+0·04	+0·32
05	50	4·09	−1·09	−0·97	5·67	−1·16	−1·04
06	30	6·24	+1·06	+1·03	7·54	+0·71	+0·67
07	10	5 87	+0·69	+0·50	7·44	+0·61	+0·41
08	2	4·90	−0·28	−0·53	6·47	−0·36	−0·62
09	1	6·09	+0·91	+0·65	6·80	−0·03	−0·30
10*	0	6·56	+1·38	+1·11	8·20	+1·37	+1·09
13	14	4 39	−0·79	−0·95	6·07	−0·76	−0·93
14	20	4·34	−0·84	−0·95	6·16	−0·67	−0·79
15	35	4 05	−1·13	−1·12	6·15	−0·68	−0·68
16*	46	3·66	−1·52	−1·43	5·31	−1·52	−1·43
18	34	5·57	+0·39	+0·39	7·30	+0·47	+0·46
19	22	5·58	+0·40	+0·30	7·30	+0·47	+0·27
20	9	4·38	−0·80	−1·00	6·21	−0·62	−0·83
21	4	5·42	+0·24	0·00	7·07	+0·24	−0·01
22	3	6·37	−1·19	+0·94	8·09	+1·26	+1·01
23	1	4·82	−0·36	−0·62	6·67	−0·16	−0·43
24	7	5·33	+0·15	−0·06	6·85	+0·02	−0·20
25	17	5·27	+0·09	−0·05	7·00	+0·17	+0·03
26	29	5·15	−0·03	−0·07	6·80	−0·03	−0·08
27	40	5·06	−0·12	−0·07	6·15	−0·68	−0·63
28	52	5·46	+0·28	+0·42	7·04	+0·21	+0·35
29	54	3·99	−1·19	−1·03	5·06	−1·77	−1·62
30	59	5·00	−0·18	+0·02	6·30	−0·53	−0·34
31	39	5·39	+0·21	+0·25	6·87	+0·04	+0·07
32	22	4·96	−0·22	−0·32	6·70	−0·13	−0·23
33	8	5·17	−0·01	−0·22	6·07	−0·66	−0·97
34	11	5·99	+0·81	+0·63	7·88	+1·05	+0·86
35	46	4·69	−0·49	−0·40	6·26	−0·57	−0·48
36	97	4·98	−0·20	+0·30	6·51	−0·32	−0·18
37	111	4·23	−0·95	−0·34	5·74	−1·09	−0·48
38	83	4·20	−0·98	−0·60	5·76	−1·07	−0·68
39	68	5·10	−0·08	+0·20	6·82	− ·01	+0·26
40	52	4·38	−0·80	−0·66	5·96	− ·87	−0·73
41	30	5·60	+0·42	+0 39	7·37	+ ·54	+0·50
42	20	5·09	−0·09	−0·20	6·80	− ·3	−0·15
43	9	5 24	+0·06	−0·14	6·79	− ·4	−0·25
44	13	4·67	−0·51	−0·68	6·32	− ·51	−0·68
45	33	4·87	−0·31	−0 32	6·58	− ·25	−0·26
46	47	6·24	+1·06	+1·16	7·87	+1·04	+1·14
47	79	5·05	−0·13	+0·22	6·76	− ·7	+0·28
48	100	5·62	+0·44	+0·96	7·36	+ ·53	+1·05
49	96	5·27	+0·09	+0·58	7·03	+ ·20	+0·69
1850	64	4·76	−0·42	−0·19	6·45	− ·38	−0·15

That the probable errors are on the whole very little diminished, is owing to the presence of a few large discordances; on the other hand, the small probable error of the coefficient of *s* would indicate that it has a real existence.

As the daily 2 P. M. observation may be supposed to show with special clearness the direct heating power of the sun, I have sought for a confirmation of the preceding results by applying the same formula to the annual means of the temperatures observed at this hour.

The annual mean temperature at 2 P. M. is given for each year in the column t_2 of the accompanying table.

The arithmetical mean gives

$$M_2 = +6^\circ{\cdot}830 \pm 0^\circ{\cdot}067$$

$$\text{p. e. of one annual mean} = \pm 0^\circ{\cdot}489$$

The solution for the co-efficient of *s* gives

$$(t_2) = +7^\circ{\cdot}108(\pm 0^\circ{\cdot}100) - s \times 0^\circ{\cdot}00801(\pm 0^\circ{\cdot}00221)$$

$$\text{p. e. of one annual mean} = \pm 0{\cdot}465.$$

This result therefore corroborates the former in indicating a decrease in the amount of heat received from the sun during the prevalence of spots—a result clearly in harmony with the recent investigations into the nature of the solar photosphere.

The reality of the existence of the above coefficient of *s* will be rendered more striking to the eye if the mean of several years' observations is taken at the period of maximum and minimum spot frequency.

It would be interesting to seek in the above residuals for evidence of other temperature periods than that dependent on the eleven year spot period. There are indeed plain indications of such a period of about fifty or fifty-five years duration—probably identical with Wolf's fifty-six year period—but our series of observations is not extended enough to justify any exact conclusion.

If we acknowledge the probability of a connection between planetary configurations and solar spots, then we are at once led to make a direct connection between the former and the temperature variations. Such an investigation I have begun and the indications are that positive results will be attained, and such as will demonstrate that the solar spots are but an imperfect index to the periodic changes in the solar radiation; these periodic changes being apparently more intimately and directly connected with the tides in the cool atmosphere surrounding the solar photosphere. The results of this investigation will be made known so soon as the recent observations on the Hohenpeissenberg can be incorporated into the work.

Cincinnati, July 20, 1870.

ART. XXXVI.—*On a new method of determining the Level-error of the axis of a meridian instrument;* by C. A. YOUNG, Ph.D., Professor of Natural Philosophy and Astronomy in Dartmouth College.

[Read at the Troy meeting of the Am. Association for the Advancement of Science]

THE inclination of the axis of a meridian instrument to a horizontal plane has hitherto been measured by three different methods: by the use of the spirit level; by examining with a collimating eye-piece the image of the wires as in nadir-point observations, the collimation having been previously determined either by reversal of the instrument or by collimators; and lastly by observing the transits of stars by reflection from an artificial horizon.

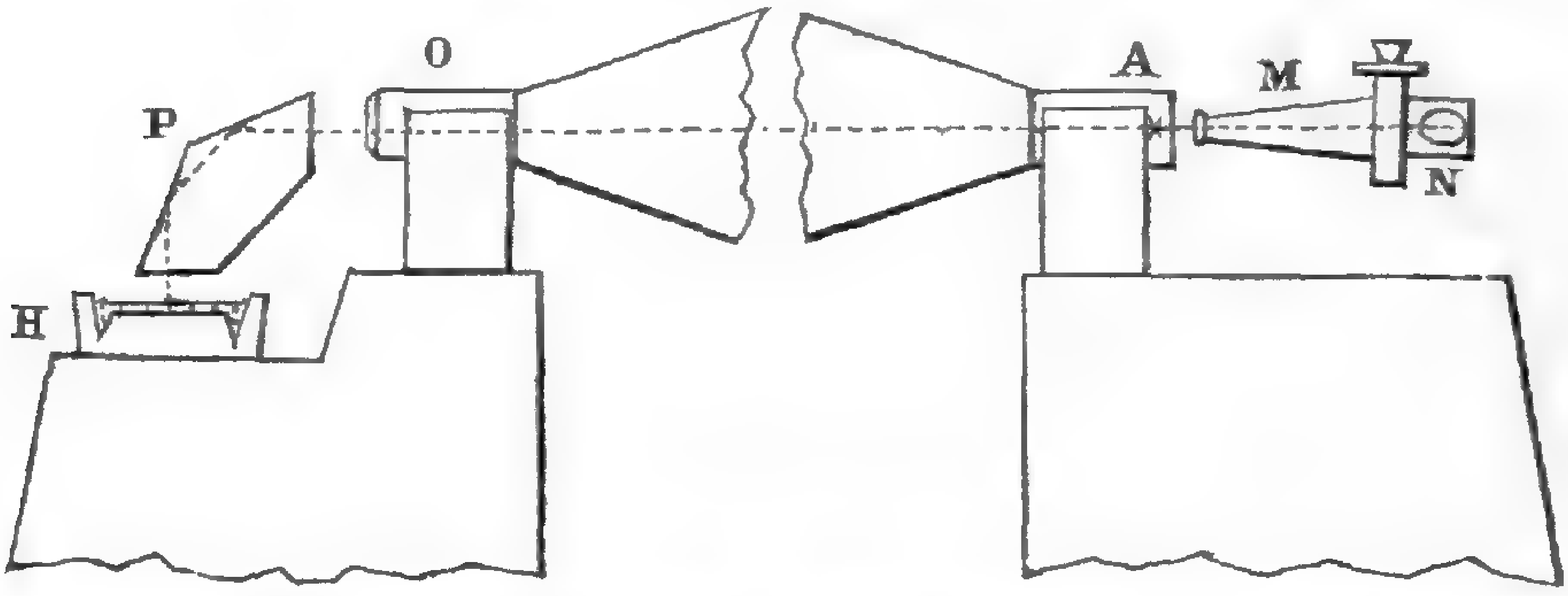
The first of these methods is by far the most used, and with portable instruments is sufficiently convenient. Still it requires a good deal of time, and, in the case of a large instrument, of hard work; and if there are sensible irregularities upon the pivots of the instrument it is a very troublesome operation to ascertain and apply the necessary corrections.

The second and third methods are still more laborious: the second gives the level error corresponding to but one single position of the telescope, i. e., with the telescope pointing downward, and is therefore liable to a constant error depending upon any malformation of the pivots which affects the instrument in this particular position: the third method can be used only when the air is perfectly still.

The method I have to propose, allows the determination of this error without any further labor than two readings of a microscope, in any position of the telescope, and without that uncomfortable climbing which is involved in the use of the striding level or nadir observations.

The annexed diagram illustrates the arrangement of the apparatus, in which, however, no regard is paid to the relative proportion of parts; the prism and mercurial horizon being grossly exaggerated in size for the sake of distinctness.

The axis of the instrument is to be fitted up as a collimator in the same manner already practised by Challis, Airy and others. In place of cross-wires, however, the extremity A should be provided with a plate of thin glass having a minute dot or circle engraved upon it, the plate being adjustable so that this dot can be brought into the geometrical axis of revolution and into the focus of the small object-glass which is situated in the other pivot, O. A reading microscope, M, is attached to the pier and provided with an ordinary collimating eye-piece N, by which light can be thrown upon the dot through the tube of the microscope. This enables us to measure the



vertical distance between the dot and its image formed by reflection in the manner to be described.

Opposite O is fixed a prism shaped like that of a camera lucida, which by *two* total reflections bends the light through a right angle. Immediately below it is placed a mercurial horizon. If an ordinary right angled prism were employed, producing the bend by a *single* reflection, then any disturbance of the prism would disturb twice as much the relation between the ray passing from A to O and that returning; but with a prism of the form proposed this relation is independent of any small changes in the position of the prism. Distortion of the prism, which is hardly to be feared, could alone do any harm.

A prism of this form and mercurial horizon, thus combined, form in effect a vertical plane mirror, whose verticality is independent of any small instability of the pier upon which it is mounted.

It is then easy to see that if the dot be accurately centered and if the axis is level the image of the dot will exactly coincide with the dot itself, provided that the reflecting angle of the prism be exactly 135°. If, however, the angle vary slightly from this the image of the dot will fall above or below the dot itself by a small amount, which will be constant, and can be determined once for all by any one of several different methods. Any deviation of the axis from horizontality will immediately be indicated by a change in this distance twice as large as the deviation itself, and may be accurately measured by the microscope. Any inaccuracy in the centering of the dot is immediately eliminated by taking two measurements in opposite positions of the telescope.

The mercurial horizon employed should of course be so devised as to be free from tremors as far as possible. The form recently described by J. H. Lane, of the U. S. Coast Survey, appears to leave little to be desired in this respect.

There is no difficulty in arranging the apparatus so that the ordinary illumination of the wires at the eye-piece of the telescope shall be effected by the light transmitted through the body of the microscope. So arranged, the apparatus remains in constant readiness for use, and, as before remarked, requires only the labor of taking two microscope readings for each determination of level error, without involving any disturbance of the setting of the instrument.

I may add in closing, that this virtual mirror of constant inclination to the horizon may easily find other applications,—as for instance in determining the horizontal points of a vertical circle where the object glass of the instrument is not so large as to require a too unwieldy and expensive prism.

Hanover, N. H. Aug. 1, 1870.

Art. XXXVII.—*Influence of Temperature on the modulus of Elasticity of certain Metals;* by F. Kohlrausch, Ph.D., and Francis E. Loomis, Ph.D.

[Communicated in extracts to the Academy of Sciences, Göttingen, May 7, 1870.]

The results obtained by Wertheim,* in his investigations respecting the influence of temperature on elasticity, have thus far generally served as a basis for calculation. It will be observed in comparing these data, that for certain substances, especially for iron, the singular phenomenon manifests itself that for different temperatures the variations of the modulus undergo a change of sign. From 0° C. to 100° the modulus increases, and then from 100° to 200° decreases. This increase is certainly altogether contrary to what would naturally have been expected. Such a maximum, however, is still more remarkable. In view, therefore, of the uncertainty of the results of investigations thus far made known, and the importance of an accurate knowledge of the variations of the modulus of elasticity for different temperatures in many of the finer measurements, the present investigations were undertaken with a view if possible of obtaining more reliable results for some of the metals of greatest practical utility. For practical purposes it would suffice to determine the variations of elasticity within the limits of the more ordinarily occurring temperatures. This was partially accomplished by Kupffer, whose investigations we shall refer to hereafter. Nevertheless in consequence of the results obtained by Wertheim, it appeared of especial interest to determine again the variations in regard to their uniformity, as well as in regard to a change of sign. The observations were extended therefore to high temperatures, in general from 90° to 20° C., and in one case very nearly to 0°. Within these limits

* Pogg. Ann. Erg.. Band 2, S. 61; Ann. de Chimie, 3^{me} S., T. 12, p. 443.

the variations in the elasticity can be determined from the observations with great accuracy. Since they show a remarkable uniformity, there can be no hesitation in assuming the resulting empirical formula as very nearly accurate, considerably beyond these limits.

It is scarcely necessary to observe that the investigations of Wertheim have afforded the most valuable material for a knowledge of the coefficient of elasticity. His method of observation, however, is little adapted to afford a solution of the question here under consideration. For Wertheim determined the absolute moduli of elasticity by means of the dilatation of bars and wires at different temperatures. The difficulties besetting an absolute determination of the variations of quantities amounting in all to only about $1\frac{1}{2}^{mm}$ are obvious. They were especially serious for the higher temperatures, since the imperfect heating apparatus of Wertheim could not be kept at a constant temperature for any considerable period of time, and, in consequence, the variations in length due to the fluctuations of temperature, inevitably exercised a serious influence on those arising from a change in elasticity. Wertheim himself designated these investigations as not rigorously accurate.* It would unquestionably have been more rational to confine the absolute determinations to ordinary temperatures, and to determine the influence of the temperature by means of other methods which here as in other cases, where it is a question merely of variations, are capable of a much greater sensibility. The influence of the fluctuations of the temperature on the length of the object will always occasion serious inconvenience in investigations on dilatation.

All these difficulties disappear, however, and at the same time the most accurate method of observation is obtained, by employing for investigation the torsion elasticity, whose choice is further to be recommended from the fact that torsion is so generally employed in measurements. If a wire is loaded with a weight, and set in vibration about its vertical axis, the reciprocal value of the square of the time of vibration affords a direct measure for the coefficient of the torsion of the wire. Since observations of the period of vibration are among the most accurate known in physics, the variations of elasticity may be thus determined with all the rigor desirable.

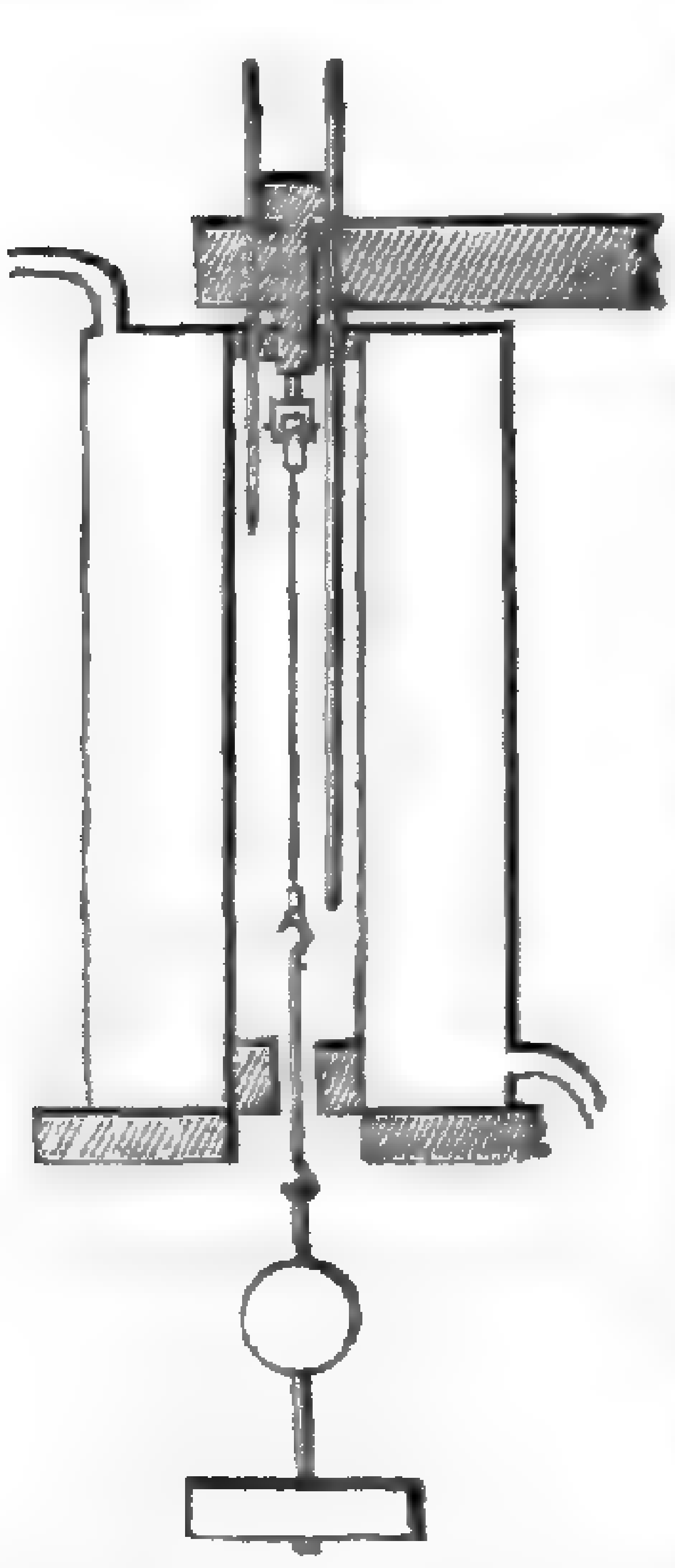
1. *Apparatus and mode of observation.*—The following dispositions were adopted for the practical application of this method. (See fig. in about $\frac{1}{12}$ natural size). The vibrating body consisted of a cylindrical leaden weight with vertical axis. Above it, attached to a brass rod, is a mirror for the purpose of observation with telescope and scale. The wire is clamped at each end

---

* Ann. de Chimie, 3me S., T. 12, p. 400 and 401.

between brass plates, of which the lower pair could be united to the weight by means of a upsilon rest, and the upper pair in like manner to a brass rod screwed into a wooden pivot. The latter was so secured in a solid support fixed to the wall as to be capable of rotation. The torsion vibrations were communicated to the weight by revolving the pivot by means of a lever. (Omitted in the figure).

2.



The space within which are enclosed the wire and a very considerable length of the connecting pieces is the hollow core of a double cylinder of tin, so constructed that the space inclosed between the two cylinders could be heated. For the determination of the temperature, three thermometers were prepared, such that the quicksilver reservoirs are at different distances from the zero point. When the graduated tubes project from the apparatus from the division 0°, the quicksilver reservoir of one of the thermometers is at a height corresponding to the middle of the wire and close beside it. The other two quicksilver reservoirs are at each end of the wire, and arranged symmetrically around it. The two latter thermometers alone are represented in the figure. The readings were made by means of a telescope.

The tin cylinder was tightly inclosed on all sides with a covering of felt of about a centimeter in thickness. It stood upon a support attached to the wall. The weight and mirror were contained in a box closed in front with a glass slide. These details are omitted in the figure.

It was originally designed to fill the vessel with water at different temperatures, but a simpler method, at first only employed to test what results might be expected, proved better adapted to the end in view. The felt envelop renders the loss of heat so gradual, that the empty apparatus, previously heated with steam, cools with sufficient slowness to permit the period of vibration at different temperatures to be observed with accuracy during the process. Accordingly the apparatus was heated for a series of observations by the admission of steam, until the temperature became tolerably stationary. The steam connection was then interrupted, and the temperature and vibrations alternately observed. Thus the variations of the elasticity of a wire for different temperatures are obtained in the course of a few hours. This simple mode of procedure affords at the same time the advantage of leaving the apparatus untouched from the beginning to the end of a series of observations, excepting only a slight turning of the wooden pivot, when the amplitude of the vibrations becomes too small. Several days were required for hot water to assume the temperature of the room, and it is not advisable to compare together observations made at long intervals, since the longer the interval the greater also becomes the danger that the conditions of the observation change in a manner to exercise on the period of vibration an influence feeble indeed, yet perhaps sensible, in view of the slight extent of the variations under consideration. For the same reason there are objections to employing water at different temperatures, since its renewal is scarcely possible without jarring the apparatus.

2. *Reduction of the observations.*—In the calculation of the data obtained from observations made with gradually diminishing temperatures, certain corrections are necessary, which render the reductions somewhat complicated. The manner in which the calculation was conducted, can be best explained by means of an example in detail. It is relative to a copper wire.

Each period of vibration was obtained according to the well known method of Gauss from two times of elongation, of which each depended in this case on six observations of the passage of the spider line through the position of rest. These times of elongation are contained in the first column of Table I. (See below). In the second column is contained the number of vibrations transpired between these times; in the third the period of vibration obtained by the division of the intervals in column one, by the numbers in column two.

TABLE 1.

Times of elongation.			Number of vibrations.	Period of vibration.	Temperature		
					Observed.	Corrections.	Corrected.
h	m	s		sec.	°	°	°
10	54	29·93					
			10	20·9970	79·4	—3·06	76·34
	57	59·90					
			9	20·9667	75·2	—2·95	72·25
11	1	8·60					
			9	20·9422	71·8	—2·83	68·97
	4	17·08					
			24	20·9000	66·4	—2·61	63·79
	12	38·68					
			29	20·8558	58·8	—2·28	56·52
	22	43·50					
			25	20·8248	52·7	—2·04	50·66
	31	24·12					
			39	20·7849	46·7	—1·78	44·92
	44	54·73					
			36	20·7614	41·3	—1·56	39·74
	57	22·14					
			58	20·7352	36·4	—1·36	35·04
12	17	24·78					
			92	20·7065	31·2	—1·15	30·05
	49	9·78					
			96	20·6861	27·0	—1·00	26·00
1	22	15·65					
			15	20·6767	25·2	—0·92	24·28
	27	25·80					

The temperatures, column 4, corresponding to the several periods of vibration, were obtained by a graphical representation of all the observed temperatures. The arithmetical mean was first taken of each three simultaneous readings of the thermometers, and then traced on coördinate paper, with the time of its observation as abscissa. The temperature for any moment can then be obtained from the curve, with an accuracy of about 0°·2 for the higher and about 0°·1 for the lower temperatures. The numbers in the 4th column are these mean temperatures for the several periods of vibration, i. e. they give the reading of the thermometers for the mean moment between the beginning and end elongation. When the interval of time is so considerable that the corresponding curve cannot be regarded as a straight line, the temperature τ would be thus obtained somewhat too low. To determine the true value, the arithmetical mean τ_0 was taken from the temperatures which correspond to the beginning and end time of elongation. The correct temperature is then $\tau+\frac{\tau_0-\tau}{3}$.

Correction of the readings of the thermometers.—The numbers in the 4th column give the true temperature of the wire on condition, 1st, that the thermometers are accurate; 2d, that their temperature for each instant is the same as that of the space in which they hang. The heavier connecting pieces attached to each extremity of the wire extend so far within the heated space that the difference of temperature which might result in the wire from conduction may be neglected. It may be assumed, therefore, that the very fine wire follows sensibly the temperature of the space within which it is suspended. Since finally the readings of the upper and lower thermometers differed from the mean of all three in the highest temperatures only some 2°·5, the mean can be regarded as the temperature of the wire, without sensible error.

The above two conditions, however, must be fulfilled, or since they are not fulfilled, the readings must be corrected.

1. The three thermometers were therefore first calibred, and their fixed points determined. Thence tables were constructed containing the corrections with regard to a quicksilver thermometer theoretically accurate. The three tables united in one, permit then to apply the correction directly to the mean value of the three readings.

2. Secondly, in consequence of the considerable mass of quicksilver, the thermometers do not follow the temperature of the space so completely as can be assumed of the fine wire. Now according to well known laws, the amount of heat lost by a body, and consequently therefore also the variation of its temperature, is proportional to the difference of its temperature and that of the surrounding space. Since in the present case, the velocity of the variation of temperature of the thermometers is known for each moment, it is only necessary to determine once for all the relation of the true temperature to that indicated by the thermometers, to be able to calculate the difference, i. e. the correction of the readings. For this determination a very delicate thermometer was employed, whose cylindrical quicksilver reservoir possessed a diameter of only some 3^{mm}, and contained in all less than 1·5 gr. of quicksilver. It

was assumed that this thermometer follows the gradually diminishing temperature of the air to within a negligable fraction, and the three thermometers were compared with it, under conditions similar to those existing in the observations on elasticity. All three thermometers were disposed at an equal height with the thermometer of comparison, in the gradually diminishing temperature of the hollow space of the tin cylinder, and simultaneous readings were made. They were also compared together in the ordinary manner in water of different temperatures. From these observations, which were conducted by Mr. Grotrian, it resulted that the mean of the readings of the three thermometers remained 2°·02 behind the temperature of the thermometer of comparison, when the velocity of the diminishing temperature amounted to 1° in 1 min. The correction of the readings due to the mass of quicksilver is therefore

$$2{\cdot}02\,\frac{d\tau}{dt},$$

where τ denotes the temperature of the thermometers, and t the time expressed in minutes. We obtain thus the temperatures which would have been observed, if thermometers had been employed containing the same amount of quicksilver as the thermometer here employed for comparison.

3. Finally a *third correction* is necessitated by the fact that the column of quicksilver of the thermometers, beginning with the zero point of graduation, possessed a lower temperature in consequence of projecting outside of the apparatus. There exists as yet no simple method of determining the mean temperature of the projecting column. It was assumed to have possessed the temperature of the air to the extent of three parts, and that of the interior of the apparatus one part. If τ_0 denotes the temperature of the room, the correction of the reading τ is therefore $-\frac{3}{4}\tau(\tau-\tau_0)\,0{,}00016.$

This value cannot be far removed from the truth. It coincides with the difference which the determination of the boiling point afforded, according as the thermometers were placed in the steam only to the zero point of graduation, or to the boiling point. The uncertainty of this assumption is at all events of small importance, since the whole correction for the highest temperature (90°) amounts only to 0°·8.

Thus, three corrections are to be applied to the readings of the thermometers. The first, which relates to the ordinary errors, is a function of the temperature; the second, in consequence of the mass of the quicksilver, is a function of the velocity of the diminution of the temperature; the third, finally, in consequence of the column of quicksilver projecting from the apparatus, depends on the difference of the outer and inner temperatures. Nearly the same conditions prevailed, however, during all the series of observations. The temperature of the room differed from the mean at most 5°, and consequently the velocity of the diminution of the temperature of the apparatus depends, within negligable differences, on the inner temperature alone, as is confirmed indeed by the observations themselves. Thus the true velocity of the diminution for different temperatures could be determined from a few series of observations, whence by multiplication with 2·02, (see p. 355) the correction itself is obtained as function of the temperature.

In like manner, for the calculation of the third correction in the formula (p. 355), it was permissible to take $\tau_0=20°$ as a mean for all the observations.

The corrections may now be very much simplified by uniting all three in tabular form, viz: for reading,

0° 10° 20° 30° 40° 50° 60° 70° 80° 90°

the correction is

0°·0* −0°·4 −0°·7 −1°·1 −1°·5 −1°·9 −2°·3 −2°·8 −3°·1 −3°·2

The interpolation for intermediate temperatures was accomplished by means of a graphical representation of this table. In one case, however, when the apparatus was filled with water, and the velocity of refrigeration could be neglected, the corrections 1 and 3 were applied separately.

Correction of the observed periods of vibration.—1. In the present investigations we are seeking the variations of the modulus of elasticity caused by a change of temperature, whereas we observe the directive force of the whole wire, which, in consequence of the heating, experiences a dilatation,as well as a change in its elasticity. Since this directive force of the wire depends on the length and section as well as on the modulus of elasticity, it is necessary to investigate what corrections are thereby necessitated.

It is an interesting fact that no correction at all is necessary. The dilatation in length and breadth completely neutralize each other.

If we denote by

l the length of the wire,
r the radius,
m the mass of the unity of length,
K the moment of inertia of the vibrating weight,
t the time of vibration,

---

* It is merely an accidental coincidence, that there is no correction to be applied to the reading 0°. The mean of the zero points of the three thermometers happens to be perfectly accurate.

D the directive force of the wire, i. e. the moment of revolution which it exercises for a torsion angle of $\frac{180^\circ}{\pi} = 57^\circ{\cdot}296$.

ε the modulus of torsion of the substance constituting the wire,

we have
$$D = \frac{\pi^2}{t^2} K$$

and
$$\varepsilon = \frac{Dl}{gmr^2} = \frac{\pi^2 K}{g} \frac{l}{mr^2t^2}, \tag{1}$$

where g denotes the acceleration due to gravity, and ε is the number, the quintuple of which (according to the theory of Poisson, i. e. when the ratio of the transversal contraction to the longitudinal dilatation is $=\frac{1}{4}$) multiplied by g gives the square of the velocity of sound in the substance.

When now in consequence of a change of temperature, the elasticity and dimensions of the wire vary, the time of vibration t changes also. If we denote the variations of ε, l, m, r and t, occurring for a small change of temperature, by $d\,\varepsilon$, $d\,l$, $d\,m$, $d\,r$ and $d\,t$, we obtain by differentiating (1) logarithmically,

$$\frac{d\varepsilon}{\varepsilon} = \frac{dl}{l} - \frac{dm}{m} - \frac{2dr}{r} - \frac{2dt}{t}.$$

But on the supposition that the wire experiences an equal dilatation in all directions on account of the temperature $\frac{dl}{l} = \frac{dr}{r}$. Also since m denotes the mass of the unity of length, obviously

$$-\frac{dm}{m} = \frac{dl}{l} = \frac{dr}{r}.^{*}$$

Therefore
$$\frac{d\varepsilon}{\varepsilon} = -2\frac{dt}{t}. \tag{2}$$

The modulus of torsion ε is therefore without correction reciprocally proportional to the square of the time of vibration.

The definition of the modulus of elasticity assumed here as a basis, denotes, with regard to the dilatation, the weight which must be suspended to a wire, whose unity of length is the same as the unity of its mass, to double the length. In practice it is customary, though frequently less convenient, to employ the section instead of the weight of the unity of length. In such case, the above expression (1) must be multiplied further by the density of the substance, and the coefficient of the cubical dilatation 3α must be subtracted from formula (2), so that

$$\frac{d\varepsilon}{\varepsilon} = -\frac{2dt}{t} - 3\alpha.$$

---

* $m.l = (m+dm)(l+dl)$

$$0 = l\,dm + m\,dl$$

$$\frac{dm}{m} = -\frac{dl}{l}.$$

2. A small correction is necessary when the moment of inertia of the vibrating weight changes in consequence of fluctuations in the temperature. In general it is inconsiderable in the same series of observations. The temperature in the box containing the vibrating weight was observed from time to time, and the period of vibration corrected when changes of temperature occurred. This was accomplished by multiplying the observed time of vibration by $1-0{,}00003.\Delta\tau$, where $0{,}00003$ denotes the coefficient of the linear dilatation of lead, and $\Delta\tau$ the variation of temperature.

3. Theoretically the time of torsional vibrations should be independent of the amplitude. In fact in the present observations on the brass wire, no influence was perceptible. The iron wire, hereafter to be discussed, was especially investigated with respect to such an influence, however, and showed a manifest, though very small, increase of the time of vibration for an increment of amplitude. The increase appeared to be proportional to the latter, and on this supposition amounted to $0{\cdot}000037^{\rm sec.}$ or $\frac{1}{500000}$ of the whole time for one division of the scale ($\frac{1}{7000}$ to one degree of arc). A series of observations on the copper wire were incidentally of a nature to afford the means of calculating the increase approximately. It amounted to $0{\cdot}000015^{\rm sec.}$ for one division of the scale. The times of vibration were reduced by means of these numbers to an infinitely small amplitude.

Although it would be of interest to examine this phenomenon more minutely, it hardly enters within the scope of the object in view in these observations. For in consequence of the inconsiderable amplitudes which we employed (at most 200 divisions of the scale, or some 3°) the correction in extreme cases amounted only to $0{\cdot}005^{\rm sec.}$, and it is almost completely compensated in its influence on the final result.

3. *Calculation of the coefficient of temperature.*—We have thus far obtained a series of temperatures with the corresponding times of vibration. From each two pairs of corresponding values may be readily derived the variation of the elasticity for 1°. We express it as a function of the total elasticity at 0°. If ε_0 designates the modulus of elasticity at 0°, and ε_1, ε_2, the moduli corresponding to the temperatures τ_1, τ_2, the desired decrease for 1° is found from formula (1) to be

$$\frac{1}{\varepsilon_0}\,\frac{\varepsilon_1-\varepsilon_2}{\tau_2-\tau_1}=t_0^2\,\frac{\dfrac{1}{t_1^2}-\dfrac{1}{t_2^2}}{\tau_2-\tau_1},$$

where t_1 and t_2 designate the times of vibration for the temperatures τ_1 and τ_2, and their squares are inversely proportional to the modulus of elasticity.

The following Table 2 contains in the second column the preceding diminutions of the modulus of elasticity for 1° taken from Table 1, by combining in it each two adjoining values. The time of vibration t_0 for 0° was obtained from the graphical representation of the observations by prolongation of the curve, a method which affords sufficient accuracy. Thus it was found that $t_0 = 20{\cdot}58$ sec. In the first column is contained the mean of the two temperatures at which the times of vibration were observed.

TABLE 2.

Temperature.	Decrease of the coefficient of elasticity for 1° as func. of the coeff. of elast. at 0°.		Observ.—Calc.
	Observed.	Calculated.	
74°	0,00068	0,00068	±0,00000
71	69	67	+ 2
66	75	65	+ 10
60	56	62	− 6
54	50	60	− 10
48	65	57	+ 8
42	43	55	− 12
37	53	53	± 0
33	55	51	+ 4
28	48	49	− 1
25	0,00052	0,00047	+0,00005

The numbers of the second column can be regarded as the values of $-\frac{1}{\varepsilon_0}\frac{d\varepsilon}{d\tau}$ for the temperature τ of the first column. It will be observed that this mode of observation affords the decrease of the modulus of elasticity for 1° with tolerable accuracy, even for the times of vibration for two temperatures differing only by a few degrees. (The greatest difference is 7°, the smallest 2°). This decrease for the brass wire is noticeably greater for the higher temperatures than for the lower.

The numerical law of the variation is to be derived from all the observations. We seek to represent it by the formula

$$\varepsilon = \varepsilon_0 \, (1 - a\,\tau - b\,\tau^2).$$

It would obviously be more in accordance with the laws of the calculus of errors to determine the values of the coefficients ε_0, a and b by means of least squares from all the observed values of ε or $\frac{1}{t^2}$ for all temperatures. This would prove, however, when applied to all the series of observations, excessively laborious. It would be hazardous to unite previously together all the observations made on the same wire, since the time of vibration for the same temperature appears to be subject to slight variations from one day to another, probably in consequence of the considerable heating to which the wire is subjected.

By differentiating the above equation we obtain

$$-\frac{1}{\varepsilon_0}\,\frac{d\varepsilon}{d\tau} = a + 2b\tau.$$

The left hand expression denotes the values contained in the second column of Table 2. If therefore we substitute these values in the above equation, the coefficients a and b can be readily calculated.

By means of least squares there results for the brass wire

$$a = 0{,}000368 \qquad 2b = 0{,}00000425$$

by means of which the figures in the third column were calculated. In view of the small differences of temperature the accordance must be regarded as satisfactory. It appears still more manifest when the time of vibration itself is calculated. Since a and b are known, the most probable value of the time of vibration for 0° may also be readily determined by means of least squares. The calculation gives $t_0 = 20{\cdot}5696^{\text{sec.}}$

The times of vibration calculated from this value of t_0, together with the times observed, are given in the following Table 3. The difference between the observed and calculated values of the times of vibration amounts at most to $\frac{1}{120}$ of a second, and indicates sufficiently the admissibility of the simplified method of reduction, although the regularity in the sign of the differences would seem to indicate that the rigorous method would afford a still greater accordance.

TABLE 3.

Times of vibration.

Temperature.	Observed.	Calculated.	Observ.—Calcul.
76°·34	20^{s}·9970	20^{s}·9986	—0^{s}·0016
72·25	20·9667	20·9680	—0·0013
68·97	20·9422	20·9443	—0·0021
63·79	20·9000	20·9078	—0·0078
56·52	20·8559	20·8592	—0·0033
50·66	20·8248	20·8218	+0·0030
44·92	20·7849	20·7869	—0·0020
39·74	20·7614	20·7569	+0·0045
35·04	20·7352	20·7309	+0·0043
30·05	20·7065	20·7041	+0·0024
26·00	20·6861	20·6837	+0·0024
24·28	20·6767	20·6751	+0·0016

4. *Temperature and modulus of elasticity of iron, copper and brass.*—The method of observation and reduction, already described and applied to an example, was employed for wires of iron, of copper and of brass. All three wires were hard drawn and of a diameter between 0·2 and 0·3$^{\text{mm}}$. The copper wire was composed of copper precipitated electrolytically. The iron

and brass wires are the ordinary wires of commerce wound on spools.

To abbreviate the calculation, the times of vibration and the temperatures of each series of observations are united first in groups by taking the mean, and especial care was taken to allow about the same weight to all the several coefficients to be derived. The observed data corrected according to the rules of chapter 2, are contained in the following Table 4, in which the Roman characters denote the chronological succession of the several series of observations.

TABLE 4.

	Iron.			Brass.			Copper.	
	Temp.	Time of Vibration.		Temp.	Time of Vibration.		Temp.	Time of Vibration.
	°	s.		°	s.		°	s.
I	76·34	17·6154	I	69·01	20.9412	I	74·06	12·4444
	56·87	17·5282		47·95	20.8067		47·96	12·3476
				28·85	20.7012		55·20	12·3696
II	76·55	17·5950					40 80	12·3221
	55·75	17·5087	II	72·14	20.9105		24·04	12·2667
	42·42	17·4553		48·78	20.7600			
	32·11	17·4150				II	62·60	12·3852
	21·91	17·3679	III	70·28	20·8694		22·05	12·2465
				49·11	20·7368			
III	78·73	17·5920		36·24	20·6669	III	77·27	12.4303
	59·72	17·5130		27·23	20·6193		56·16	12·3546
	20·97	17·3583					40·79	12·3028
			IV	50·05	20·7333		23·37	12·2413
IV	49·57	17·4715		42·81	20·6915			
	37·80	17·4279		34·59	20·6425			
	21·95	17·3627						
			V	19·43	20·5428			
				5·05	20·4737			

The following Table 5, in which the numbers are arranged according to the temperatures, is obtained by reducing the values contained in Table 4 in the same manner as those of Table 2 of the example.

The coefficients a and b are derived by means of least squares from the values contained in the third and fourth columns of Table 5, as in the example. Designating by ε_0 the modulus of elasticity at 0°, the modulus for the temperature τ is found to be,

for iron, $\varepsilon = \varepsilon_0(1 - 0{\cdot}000447\tau - 0{\cdot}00000012\tau^2)$
for copper, $\varepsilon = \varepsilon_0(1 - 0{\cdot}000520\tau - 0{\cdot}00000028\tau^2)$
for brass, $\varepsilon = \varepsilon_0(1 - 0{\cdot}000428\tau - 0{\cdot}00000136\tau^2)$

The numbers of the next to the last column of Table 5 were calculated with these values. The probable error of each value of the decrease of the coefficient of elasticity for 1° for a given temperature calculated from two groups of observations amounts accordingly to ± 0·000014.

TABLE 5.

		Temp.	Decrease of the coeff. of elastic. for an incr. of temp. of 1° as func. of the coeff of elast. at 0°. Observed.	Calcul.	Observ. — Cal.
Iron.	IV	29°·9	0·000467	0·000454	+ 0·000013
	II	32·2	482	455	+ 27
	III	40·3	450	457	— 07
	IV	43·7	416	458	— 42
	II	43·9	445	458	— 13
	II	66·1	459	463	— 04
	I	66·6	494	463	+ 31
	III	66·9	459	464	— 05
Copper.	III	32·1	0·000566	0·000538	+ 0·000028
	I	32·4	528	538	— 10
	II	42·3	542	544	— 02
	I	48·0	521	547	— 26
	III	48·5	531	547	— 16
	I	61·0	578	554	+ 24
	III	66·7	556	557	— 01
Brass.	V	12·2	0·000466	0·000461	+ 0·000005
	III	31·7	504	514	— 10
	I	38·4	523	532	— 09
	IV	38·7	570	533	+ 37
	III	42·7	514	544	— 30
	IV	46·4	556	554	+ 02
	I	58·5	593	587	+ 06
	III	59·7	584	590	— 06
	II	60·5	599	592	+ 07

If the other definition of the modulus of elasticity is employed (as was done by Wertheim) referring to the section, and not, as above, to the mass of the unity of the length of the wire, the coefficients of the temperature will be slightly altered in accordance with the observation on page 357. If the coefficients of dilatation for 1° are assumed to be for iron $=0{\cdot}000012$; for copper $=0{\cdot}0000175$; and for brass $=0{\cdot}000019$, the coefficients of τ become,

for iron,	0·000483,
for copper,	0·000572,
for brass,	0·000485.

The coefficients of the quadratic terms remain sensibly unchanged.

It is evident from the results of these observations that the mean variation of the elasticity for the three metals investigated differs but little from that of the temperature. For a change of temperature from 0° to 100° the diminution is for

Iron,	4·6	and	5·0	per cent.
Copper,	5·5	"	6·0	"
Brass,	5·6	"	6·2	"

where the numbers of the second column relate to the second definition of the modulus of elasticity.

If the variation in elasticity is compared with the influence exercised by the temperature on other properties of bodies it will be remarked that it is much greater than the cubical dilatation as well as the variations of refraction. It is of about the same order as the variation of the permanent magnetism caused by the temperature, as well as that of specific heat. The increase of the galvanic resistance on the other hand is much greater.

It results further from the sign of the quadratic term that the variation of elasticity for all three metals is the most rapid in the higher temperatures. While, however, the increase for iron is almost imperceptible, and is also very small for copper, it is quite considerable for brass. The decrease for 1° is for

	At 0°.	At 50°.	At 100°.	
Iron,	0·0447	0·0459	0·0472	per cent.
Copper,	0·0520	0·0548	0·0576	"
Brass,	0·0428	0·0564	0·0699	"

It will be observed that the differences in the variation of the coefficient of elasticity for the three metals investigated, are in the order of the height of their melting points.

The results of these investigations show no trace at all of the remarkable phenomenon of a maximum, alluded to at the beginning of this article, which would seem to be indicated for iron by the investigations of Wertheim. If, therefore, different varieties of iron do not manifest a totally different behavior, or if the modulus of the longitudinal elasticity does not undergo a very different change in consequence of temperature from that of the torsional elasticity, this anomaly must be accounted for by the imperfect method of observation employed by Wertheim. This supposition is confirmed further by the observations of Kupffer (see below), as well as by a very simple experiment. If, namely, two tuning forks are in vibration, and one of them is heated, the number of vibrations changes in the manner demanded by the assumption of a decrease of elasticity for increasing temperatures.

It is to be remarked here that Wertheim's calculation of the heat generated by condensation during vibration, loses thus all its value.*

Observations of Kupffer.†—These investigations appear to be much less generally known than they deserve, for they contain much varied and valuable material for practical use. Kupffer's observations are in general on bars vibrating trans-

---

* Pogg. Ann., Bd. 11, S. 32.

† Mem. de l'Acad. de St. Petersb. 1856, 6 ser. T. vi, p. 400.

versally at the temperatures of about $-10°$, $+15°$ and $+80°$. Unfortunately, however, very long intervals (at least a year) occur between the observations, and it would seem that he did not always employ the same bars, so that his investigations permit but a limited conclusion in regard to the law of the variations of elasticity as modified by temperature. Finally, the dilatation caused by temperature was disregarded in the numerical values given by Kupffer, a source of serious errors in view of the method of observation which was employed.

A few of the results derived by Kupffer were obtained by means of torsional vibrations. Among these are the coefficients of temperature for the elasticity of an iron wire (piano cord), of a copper wire and of a brass wire. After having applied to these values the correction required for the dilatation of the vibrating disc, the decrease of the elasticity for 1° expressed in parts of the total elasticity is found to be

for	iron,	0·00055	(" mem." S. 446)
"	copper,	0·00082	S. 464
"	brass,	0·00039	(S. 467)

These values agree tolerably well with those found by us, with the exception of the copper wire, in regard to which it is possible that the divergence may be caused by the circumstance that we employed a chemically pure metal.

5. *The absolute moduli of elasticity of the iron, copper and brass wires calculated from torsional vibrations and from the velocity of sound.*

To determine from our investigations the absolute moduli of torsional elasticity, it is necessary to know the dimensions and mass of the wires, as well as the moment of inertia of the vibrating weight.

The latter is to be calculated from the mass of the perforated lead cylinder =1818 grs. and the interior radius =0·34 c. m. and the exterior radius =5·07 c. m. It is found to be

$$\frac{1818}{2}\left(\overline{5{\cdot}07}^2+\overline{0{\cdot}34}^2\right)=23470 \text{ grs. } \square \text{ c. m.}$$

To this must be added the moment of inertia = 40 grs. □ c. m. of the connecting pieces and the mirror, which was calculated from their size and form. The total moment of inertia therefore is

$$K=23510 \text{ grs. } \square \text{ c. m.}$$

Further was determined

	Iron.	Copper.	Brass.
The length of the wire,	l=20·80	20·75	20·80 c. m.
The mass of one c. m. length,	m=0·00301	0·00655	0·00403 grs.
The density,	Δ=7·82	9·00	8·41
The time of vibration at 20°,	t=17·35	12·23	20·55 sec.
The radius of the wire,	r=0·0111	0·0152	0·0123 c. m.

The modulus of torsion T is derived from these data by means of the formula, p. 357,

$$T = \frac{\pi^2 K}{g} \frac{l}{mr^2 t^2}.$$

It is found to be

for iron $=444$ kilometers.
" copper $=217$ "
" brass $=190$ "

If it is desired to follow the definition customary in practice, and to refer the modulus to the section instead of to the mass of the unity of length, these numbers must be multiplied by the corresponding densities; whereby they become

for iron,	3470	$\frac{\text{kilogrammes}}{\text{quadratmillimeter.}}$
" copper,	1950	"
" brass,	1600	"

The velocity of sound was measured in the same species of wires by stretching them in a Weber's monochord, and by varying the length, comparing the longitudinal tone with tuning forks. The normal tone of the set of tuning forks was determined by comparison with two normal tuning forks of Appunn.

The modulus of dilatation of the wire (i. e. the length of a wire of the same species possessing the weight necessary to double the original length), is obtained from the velocity of sound c by means of the formula $A = \frac{c^2}{g}$, where g denotes the acceleration due to gravity. The value A′ ordinarily employed in practice, and made use of by Wertheim, (i. e., the weight which hung on a wire whose section is unity would double the length), is obtained as above by multiplication with the density. Thus it was found

	Velocity of sound.	Modulus of elasticity.	
		A.	A′.
Iron,	5050 meters.	2580 kilometers.	20310 $\frac{\text{kgr.}}{\text{quad. mill.}}$
Copper,	3640 "	1350 "	12140 "
Brass,	3380 "	1170 "	9810 "

Wertheim gives A′ for iron wire $=19445$, copper $=12536$, brass $=9000\ \frac{\text{kgrs.}}{\square\ \text{m.m.}}$

According to these observations, the moduli of torsion bear to the moduli of dilatation, for all three wires very nearly the same ratio. The ratio $\frac{A}{T}$ is

for iron,	5·85
" copper,	6·23
" brass,	6·15
Mean,	6·07

The deviations of these values from the mean are perhaps due to errors of observation, especially in the determination of the densities, since in order to be sure of investigating the same material as in the original experiments, but small masses could be employed. It is possible too that the deviation of the mean from the whole number 6, is to be sought in these errors.

Without entering on a more extended investigation of this point, and without discussion as to whether the customary theory of elasticity is applicable to such thin wires, or whether the lack of isotropy, or the relations of superficies, forbid this application, it may be remarked that according to the theory

$$\frac{A}{T}=4(1+\mu),$$

where μ denotes the ratio of the transversal contraction to the longitudinal dilatation. By a comparison of the torsional and longitudinal vibrations, there would result accordingly for our three wires very nearly the same value for μ. If it is assumed that $4(1+\mu)=6$ there results $\mu=\frac{1}{2}$. This is the extreme value permitted by the theory, which would correspond to a volume unchanged by dilatation.

It may suffice to have indicated the fact. It would lead us too far from the chief object of our investigation to develope the subject more in detail.

ART. XXXVIII.—*On the Oxy-Calcium Light as applied to Photo-Micrography;* by Lieut. Col. J. J. WOODWARD, Assistant Surgeon, U. S. Army. Report to the Surgeon General of the U. S. Army, dated June 4, 1870.

SINCE the preparation of my report of January 4, 1870, on the use of the Magnesium and Electric lights in Photo-micrography, I have made some experiments with the Oxy-calcium, or Hare's light, as a source of illumination for the same purpose, and have succeeded in obtaining excellent pictures with powers as high as a thousand diameters. This result appears to me of considerable importance, both because of the comparative cheapness of this light, and because the apparatus for its production is so common as to be practically within the reach of every microscopist. In addition to these advantages the oxy-calcium light possesses the quality of steadiness to a greater degree than either the electric or the magesium lamp and requires much less trouble and skill to manage.

For the purposes of my experiments, I made the hydrogen as I consumed it, in a Hare's self-regulating generator, by the action of dilute sulphuric acid on scraps of ordinary sheet-zinc.

The oxygen was sometimes made in the usual way from chlorate of potassa, sometimes purchased compressed in iron cylinders; in either case it was transferred to a large sheet-iron gasometer for use. The gases were burned under a pressure equal to a column of water fourteen inches high. I used for lamp one of the first-class magic-lanterns, manufactured by J. W. Queen & Co. (No. 924, Chestnut street, Philadelphia, Pennsylvania,) in which the disc of lime is revolved by clockwork before the burning jet of gas and a fresh surface constantly presented to the flame. I simply removed from the lantern the lens intended to magnify the image on the slides, when the apparatus is in ordinary use, and allowed the cone of light proceeding from the large condenser of the instrument to fall upon the achromatic condenser of the microscope, in the same manner as described and figured for the magnesium lamp in my report of January 4th, a reference to which will render any description of the arrangement of the microscope and of the sensitive plate unnecessary in this place.

I employed the ammonio-sulphate cell, as I do in taking Photo-micrographs with other sources of light, but found I could dispense with the ground glass which is necessary in photographing so many objects, if sunlight or the electric lamp is employed; a large portion of the lime disc being luminous, the resulting mixed divergent pencil, like that obtained from the magnesium lamp, does not produce the interference phenomena which result when tissues and many other objects are illuminated by powerful parallel rays. This circumstance, however, renders the calcium light inferior to the sun and the electric lamp, in the resolution of the Nobert's plate and certain lined test objects.

I did not find the time of exposure differed materially from what I had given in making photographs of the same objects with the magnesium lamp, and the pictures produced were not inferior to these in quality. This arose from the fact that the greater steadiness of the calcium light permitted the use of condensers which concentrated the light to a greater degree than I had found advantageous with the magnesium lamp, and not from equality in the actinic power of the two sources of illumination. I have recently made some experiments with the view of obtaining positive information with regard to the comparative actinic energy of the electric, magnesium and calcium lamps which I employ. For this purpose all condensers being removed the divergent pencil proceeding from each lamp in turn was permitted to fall, for the space of five seconds, on an exposed circular portion of a sensitive plate thirty feet distant.

The whole operation was completed in less than a minute, when the plate being developed in the ordinary way three circular spots appeared as the results of the exposures. The spot produced by the electric light was intensely black, that by the magnesium of a rich middle-tint, while the circle impressed by the calcium light was extremely pale. Want of time prevented me from continuing these experiments and obtaining as I desired numerical values for the relative actinic powers of these sources of illumination under definite conditions; this I have however regretted the less, as the actual energy of the naked flames is not really the measure of their availability in photo-micrography; here the question of steadiness, involving, as it does, the possibility of great concentration, plays a most important part and materially modifies the result.

So far as I know, the Calcium light has never before been successfully employed as the source of illumination for making Photo-micrographs in this country. My friend Dr. R. L. Maddox, however, writes me that it has been experimented with in England by Drs. Abercrombie and Wilson. He thinks they used powers as high as an eighth with pleasing results. This information has directed my attention to the essay of Dr. Wilson in the Popular Science Review for 1867, volume vi, page 54, in which that gentleman gives in detail the process employed by himself and Dr. Abercrombie. He experimented with an oil lamp and with the Oxy-calcium and magnesium lights: "I can scarcely think it would be used now that the more active light of magnesium is within the reach of every one." And of the magnesium: "The light fails only in steadiness, and if some means could be devised for burning the metal uniformly and at a fixed point nothing would be left to desire." Dr. Beale (How to work with the Microscope, 4th edition, p. 248) tells us that some of the pictures of these gentlemen were remarkably good, "they possess a peculiar delicacy in the half tones and the shadows, with much roundness of the objects, but the definition, as might be expected, does not quite equal in some of the finest markings, prints obtained from sun negatives." A perusal of Dr. Wilson's paper will show that my process differs from his in the use of the following precautions: the interpolation of an ammonio-sulphate cell to exclude the non-actinic rays, the use of lenses specially made for photography for all powers from the ⅛th down, the use of much larger condensers to concentrate the light and so to shorten the exposure, and in the case of the magnesium light, in the use of a clock-work lamp to increase the steadiness of the illumination. Each of these points are in my judgment essential to obtain the best results.

I learn from the same letter of Dr. Maddox that he had himself made experiments with the magnesium lamp some time before those of Abercrombie and Wilson. He used powers as high as a fifth, and appears to have obtained better results than I supposed any one had done prior to the publication of my report. He gives me the following account of his experience: "The first picture I took with the magnesium light was done in a very rude way. An inch and a quarter of wire was held in a small spirit flame and advanced by hand as burnt. The objective was Beck's $\frac{2}{3}$ds, the object the sycamore-leaf insect, and about $\frac{3}{8}$ths of an inch of wire remained after use. I sent a print, with a sun print of the same to the British Journal of Photography, and in the number for July 1, 1864, you will find some remarks by myself and the editors. Now to try and meet any error that might arise from what we may term want of correction, I used the $\frac{2}{3}$ds with the correcting lens, which is excellent for sunlight; the picture was soft, full of half tone, but wanted, as in other pictures I had seen by artificial light, the decision of definition in the outlines." "After this I used the $\frac{1}{5}$th with the little apparatus sketched in Beale's book (page 275) and which I venture to think, embraces all that is required for its use, provided the condenser has its focus at the burning point, and that the reflector has the same." With the $\frac{1}{5}$th, fibers of cotton and muscular fibrillæ of boiled shrimp, with several other objects were taken, but I did not use any higher power, nor indeed pay much attention to the subject as I gave the preference to the sunlighted prints and negatives." I give these extracts with great pleasure as showing the experience in this direction of one of the most distinguished laborers in the field of Photo-micrography, and regret that I was not acquainted with them at the time of publishing my first report. The method of Dr. Maddox, however, differed from mine in the same essential points as that of Abercrombie and Wilson, and the peculiar fitness of the magnesium light for photographing the animal tissues and those objects generally, which require the use of ground glass when sunlight is employed, would appear to have escaped the observation of these accomplished gentlemen, and to have remained unnoted until the publication of my report.

In conclusion I append to this paper two illustrative photographs. The first, which represents the 6th square of the Möllers type-plate of the diatomaceæ, taken with Wales's 1½ inch objective, arranged to give thirty-five diameters, will serve for comparison with the photographs of the same object with the same lens taken by sunlight and by the electric and magnesium lamps, which were published with my former report. The second represents the Navicular Lyra, taken with the Powell and Lealand's immersion $\frac{1}{16}$ arranged to magnify 1000 diameters.

ART. XXXIX.—*On the Secular Perturbations of the Planets;* by ASAPH HALL.

IN view of the recent consideration in geological speculations of the secular inequalities of the excentricity of the earth's orbit, it may be worth while to state briefly the method of treating secular perturbations, and our present knowledge of the subject.

Denoting by e, π, φ, and θ the excentricity, the longitude of the perihelion, the inclination of the orbit and the longitude of its ascending node, it has been found easier in the discussion of the problem to substitute for these quantities four others which are simple functions of them; and thus are assumed

$$h=e \sin \pi. \qquad p=\text{tang } \varphi \sin \theta.$$
$$l=e \cos \pi. \qquad q=\text{tang } \varphi \cos \theta.$$

Neglecting terms of the third and higher orders of the excentricities and inclinations in that part of the development of the perturbative function from which the secular perturbations arise, linear differential equations of the first order are established containing the first differential co-efficients of h, l, p and q with respect to the time; and it is by the integration of these equations that the secular perturbations are obtained. By this process also the equations are separated into two distinct classes admitting of separate treatment; the one class containing the differential co-efficients of h and l and the solution furnishing the values of the excentricities and the longitudes of the perihelia, and the other containing those of p and q and the solution giving the values of the inclinations and the longitudes of the ascending nodes. In this way the discussion of the problem is much simplified. The numerical co-efficients in the differential equations depend on the semi-major axes of the orbits, and on the masses of the planets. Considering the eight principal planets of our solar system, there will be of course eight values of h, l, p, and q, and these are usually distinguished by the addition of accents to the symbols.

In order to effect the integration, Lagrange, whose method is still followed, assumed

$$h=\text{N} \sin (g\,t+\beta), \qquad l=\text{N} \cos (g\,t+\beta),$$
$$h'=\text{N}' \sin (g\,t+\beta) \qquad l'=\text{N}' \cos (g\,t+\beta), \qquad \text{\&c.}$$

Differentiating these equations and substituting the value of $\frac{dh}{dt}, \frac{dh'}{dt}, \frac{dl}{dt}$, &c., in the differential equations and then eliminating the ratios of the coefficients N, N$'$, N$''$, &c., a numerical equation is obtained for the determination of g. This equation will rise to the degree denoted by the number of planets considered, and in our solar system will be of the eighth degree. If we denote by g_1, g_2, g_3 - - - - g_8 the roots of this equation the general integrals will be

$$\begin{aligned} h&=N_1 \sin(g_1t+\beta_1)+N_2 \sin(g_2t+\beta_2)+ \dots +N_8 \sin(g_8t+\beta_8),\\ l&=N_1 \cos(g_1t+\beta_1)+N_2 \cos(g_2t+\beta_2)+ \dots +N_8 \cos(g_8t+\beta_8),\\ h'&=N'_1 \sin(g_1t+\beta_1)+N'_2 \sin(g_2t+\beta_2)+ \dots +N'_8 \sin(g_8t+\beta_8),\\ l'&=N'_1 \cos(g_1t+\beta_1)+N'_2 \cos(g_2t+\beta_2)+ \dots +N'_8 \cos(g_8t+\beta_8),\\ &\&c., \qquad\qquad \&c. \end{aligned} \qquad (1)$$

The arbitrary quantities N_1, N'_1 - - - - β_1, β_2, &c., are determined by the initial values of h and l. The solution for p and q is quite similar to that for h and l.

The conditions necessary for the stability of the system are, first, that the eight roots of the equation for g shall be real and unequal, in order that outside the circular functions there may be no terms containing the time as a factor or exponent and which would therefore increase indefinitely; and secondly, it is necessary that the coefficients N may not be great in order that the excentricity may not increase so as to render divergent the series which have been assumed in the solution to be rapidly convergent.

The actual numerical solution by several eminent astronomers, Lagrange, Pontecoulant and Leverrier, their results being essentially accordant in this respect, shows with a great degree of probability that our solar system is a stable one, the law of gravitation alone being considered; although to speak with certainty on this point an analytical solution is to be desired. But when it is required to compute for very remote epochs the values of the elements of the orbits the co-efficients g which in equations (1) are multiplied by the time must be carefully considered. These coefficients depend on the assumed masses of the planets, and are generally determined by neglecting terms of the third order. The most complete investigation of this subject is that given by Leverrier in the Connaissance des Tems for 1843 and 1844, and reproduced with some additions in the memoirs of the Paris Observatory for 1856. In this work, which is a masterpiece of astronomical calculation, Leverrier shows that terms of the third order may produce corrections of the values of g amounting to three or four tenths of a second. Probable uncertainties in the assumed values of the masses of the planets may give rise to errors of nearly two tenths of a second. Hence, if we consider the forms of the general integrals (1), we shall readily see that for very remote epochs, distant by millions of years, our calculations must be very untrustworthy; since when the time is great the errors in the values of g may completely change the character of the circular functions.

For a satisfactory solution of this problem, certainly one of the most interesting in astronomy, our knowledge of the masses of the planets must be greatly advanced. In the case of the interior planets it appears that we must wait patiently until the theory of their own motions, or the motion of some one of the periodical comets, shall furnish the data for an exact determination of their masses. The masses of Mars and Jupiter will in time be very accurately known from the theories of some of the minor planets. But in the case of Saturn, Uranus and Neptune it appears to me that the instrumental means are already at hand for making an accurate determination of their masses, and a more complete investigation of the theories of their satellites. When the novel and entertaining observations with the spectroscope have received their natural abatement and been assigned their proper place, it is to be hoped that some of the powerful telescopes recently constructed may be devoted to this class of observations, where a rich and an ample field awaits the skillful observer. One could not wish a better example than the beautiful work of Bessel on the satellites of Jupiter.

August 2, 1870.

ART. XL.—*Farmer's Theorem discussed;* by FRED. E. STIMPSON.

TOWARD the close of Prof. Silliman's paper, "On the relation between the intensity of light produced from the combustion of illuminating gas and the volume of gas consumed" (this Jour., xlix, 17), is the following:—"A comparison of the foregoing results will show that the coincidences, with the requirements of the theorem of Farmer are, within the limits assigned, too numerous, and too closely accordant, to be considered as otherwise than pointing clearly to its general truth."

What I propose to examine now is, whether the data given in the paper referred to, do warrant the conclusion reached.

According to the data given for the first experiment, two lights were made equal, so that the disk of a Bunsen Photometer stood midway between the flames, and the consumptions were found to be 3·66 feet per hour.

"The screen was then moved upon the bar to a point just four times as far from one flame as it was from the other, i. e., the bar being 100 inches, the screen stood at 80, i. e., as 1 : 4. The light from the distant burner was then increased, until the disk again showed as an equality of illumination. On reading the rate of the gas consumed by the two burners respectively. one gave 3·66 cubic feet and the other 7·32 cubic feet."

Now from this it will be seen that the ratio of consumption was as 1 to 2, but the ratio of the lights being as the squares of the distances from the disk, becomes $200^2 : 80^2$ or 1 : 16, and the result on the paper should stand,

$$3{\cdot}66^4 : 7{\cdot}32^4 = 1 : 16 \text{ and not}$$
$$3{\cdot}66^2 : 7{\cdot}32^2 = 1 : 4.$$

In other words, the lights were proportional to the fourth powers of the consumptions and not to the squares.*

In experiments 2, 3 and 4, the ratio of the squares of consumption is nearer to the ratio of the lights than the simple ratio of the consumptions, but in experiment 2, the ratio of the 2·67 powers of the consumption almost exactly expresses the ratio of the lights. In experiment 3 the ratio of the squares is too small, and in experiment 4 it is too great; while in experiment 5, the simple ratio is certainly much nearer than the ratio of the squares.

The following table will, I think, suffice to show these various relations:

No. Exp't.	Rate of consumption per hour.	Intensity found by experiment.	Intensity by old law.	Intensity by law of the squares.	Intensity by the 2·67 power of consumption.	Difference between calculated and observed results.		
						Old law.	Law of squares.	2·67 power.
2	3·30	1·	1·	1·	1·	0	0	0
	4·35	2·1	1·32	1·73	2·098	−0·78	−0·37	−0·002
	5·136	3·2	1·55	2·42	3·222	−1·65	−0.78	+0·022
	5·556	4·0	1·68	2·83	3·995	−2·32	−1·17	−0·005
3	3·72	1·	1·	1·	----	0	0	----
	4·884	2·	1·312	1·72	----	−0·628	−0·28	----
	6·000	3·	1·61	2·60	----	−1·39	−0·40	----
	7·218	4·	1·94	3·76	----	−2·06	−0·24	----
4	4·5	1·	1·	1·	----	0	0	----
	9·519	4·	2·114	4·46	----	− 1·886	+0·46	----
5	5·16	1·85	1·85	1·85	----	0	0	----
	10 06	4·00	3·70	5·72	----	− 0·30	+1·72	----

In experiment 6, the reductions have been made by applying the old rule to the correction for the candle in both cases; the correction by Farmer's Theorem being applied only to the consumption of the gas, and so applied gives the most concordant results, the difference shown by the old rule however, (1·34 candles) is no greater than might have occurred between two observations, even if the consumption in both cases had been

---

[* Since the above was written, I have been informed that the error is not in the proportion of ("1 to 4") but in the statement of the position of the disk upon the bar. It should read *twice* as far from one flame as the other, i. e., the screen stood at 66⅔, and thus the experiment supports the theorem.]

equal, i. e., five feet per hour. But this experiment alone certainly does not warrant the conclusion reached, viz:—"That this theorem applies with equal force to the weight of sperm consumed by the standard candle as to the volume of the gas burned in equal times," because the correction has *not* been applied to the candle either here or in any other observation given in Prof. Silliman's paper.

The confirmation obtained by experimenting upon Peytona, Albert and Wollongong gas depends upon the assumption that the true candle power of a rich gas can be obtained by mixing it in definite proportions, with another gas whose illuminating power is known, and deducing from the observed candle power of the mixture, the candle power of the rich gas; until this assumption has been proved to be correct, it is of course useless in establishing Farmer's Theorem.

The next proof offered is drawn from a tabular statement in Sugg's Gas Manipulation. Prof. Silliman says, "By this statement the burner in question produced from five cubic feet of gas exactly 15 (14?) candle power, but when reduced to 4·5 cubic feet consumption the candle when 'corrected to the standard quality of gas by proportion' was only 11·93 candles. The values of the 'correction' referred to can only be conjectured, but assuming that the observation made the uncorrected rendering 11·32 candles (a very probable quantity), we find that the law of the squares of consumption then makes the ratio as follows:—$4{\cdot}5^2 : 11{\cdot}32 = 5^2 {\cdot} 14$." The assumption here made is not at all necessary because we can find the exact value of the 'correction' for the gas by reversing the proportion used, thus:—$5 : 11{\cdot}93 = 4{\cdot}5 : 10{\cdot}73$, and now applying Farmer's Theorem the ratio becomes $4{\cdot}5^2 : 10{\cdot}73 = 5^2 : 13{\cdot}24$ only. The relation can be exactly expressed by the ratio $4{\cdot}5^{\frac{5}{2}} : 10{\cdot}73 = 5^{\frac{5}{2}} : 13{\cdot}997 = 14$.

The last proof offered is drawn from Audoin and Berard's experiments. But after several fruitless efforts to obtain the results, as given under the head of "Intensities by law of the squares of consumption," I am forced to conclude that these figures are incorrect through some inaccuracy in applying that law. The two tables referred to may be found in the author's original memoir on pages 439 and 441 of Annales de Physique et de Chimie, vol. lxv, 1862. In these experiments they compared the burning of two batwing burners at different rates of consumption, with a "Bengel Argand" whose rate of consumption was nearly constant. The first three columns of the table below give their experimental data.

TABLE I. *Burner of the fifth series—slit* $\frac{1}{39}$ *inch wide.*

Consumption of the Batwing under trial.	Consumption of the Bengel Argand.	Comparative intensities. The Bengel burner being 100	Intensties by old formula or direct ratio of consumption.	Difference between calculated and observed results.	Intensities for law of the squares of consumption.	Difference between calculated and observed results.
Liters per hour.	Liters per hour.					
			l'		l''	
211	100	120	99·38	−0·62	82·31	− 17·69
189	96	110	97·64	−2·36	86·38	− 13·62
180	103	100	100·00	0·00	100·00	0·0
156	104	90	104·85	+4·85	122·17	+ 22·17
142	104	80	102·39	+2·39	131·06	+ 31·06
124	101	70	99·65	−0·35	141·84	+ 41·84
102	101	60	103·82	+3·82	179·68	+ 79·68
88	102	50	99·30	−0·70	197·19	+ 97·19
68	100	40	102·79	+2·79	264·20	+164·20
57	102	30	90·73	−9·27	293·40	+193·40

TABLE II. *Burner of the same series—slit* $\frac{1}{63}$ *inch wide.*

Consumption of the Batwing under trial.	Consumption of the Bengel Argand.	Comparative intensities. The Bengel burner being 100.	Intensities by old formula or direct ratio of consumption.	Difference between calculated and observed results.	Intensities for law of the squares of consumption.	Difference between calculated and observed results.
Liters per hour.	Liters per hour.					
264	106	200	100·4	+ 0·4	50·3	− 49·7
236	105	180	100·1	+ 0·1	55·6	− 44·4
208	105	160	100·9	+ 0·9	63·5	− 36·5
182	105	140	100·9	+ 0·9	72·8	− 27·2
152	104	120	102·6	+ 2·6	87·7	− 12·3
130	104	100	100·0	0·0	100·0	0 0
112	104	80	92·8	− 7·2	107·8	+ 7·8
90	104	60	86·6	−13·4	125·1	+ 25·1
75	104	50	86·6	−13·4	150·2	+ 50·2
66	104	40	78·8	−22·2	155·2	+ 55·2
43	104	20	60·5	−39·5	182·8	+ 82·8
28	104	10	46·4	−53·6	215·5	+115·5

From the first table it will be seen that the two burners gave equal light (intensity 100), when the batwing consumed 180 liters per hour, and the standard 103 liters. Correcting the first line of this table for these consumptions, by the old rule
we have for the batwing $211 : 180 = 120 : l$
and for the standard $103 : 100 = l : l'$;
the total correction for both
will be therefore $211 \times 103 : 180 \times 100 = 120 \times l : l \times l'$;
the l cancels out and leaves
the proportion $211 \times 103 : 180 \times 100 = 120 : l'$.
The second line of this table
becomes $189 \times 103 : 180 \times 96 = 110 : l$
and so on.

Now applying Farmer's Theorem, the proportions become

$$(211)^2 \times (103)^2 : (180)^2 \times (100)^2 = 120 : l''$$
$$(189)^2 \times (103)^2 : (180)^2 \times (96)^2 = 110 : l''.$$

The value of l' and l'' from these proportions will be found in the 4th and 6th columns of the above table.

It will be seen that in the second table the burners gave equal light for consumptions of 130 and 104; these numbers have been used therefore in the corrections for that table.

The experiments show that the last term of the proportions should be 100. The tables show that by the old formula, this term becomes about 100 for every experiment of the first table, and for the first seven experiments of the second table, the greatest difference being 7·2; and for the remaining five experiments, the old formula gives the best approximate results except for the last one, and here the old law gives a result which must be *multiplied* by 2·15 to make it correct, and Farmer's Theorem gives a result that must be *divided* by 2·15 to make it correct.

From a perusal of these various results, I am led to disagree with Prof. Silliman, and say that 'Farmer's Theorem' is not proven, and that the law of the squares does *not in general* give any closer results than the old law of the direct ratio; though I entirely and heartily concur with him in the conclusion, *that every photometric observer should recognize the importance of bringing the consumption of gas and sperm to the agreed standard*, when attempting to give the true candle power of any gas.

It is much to be desired that experimenters should turn their attention to the matter of the relation of consumption of gas to illuminating power, and I sincerely hope that Mr. Farmer will not let the matter rest here, but will make and publish further observations upon the same subject.

Messrs. Audoin and Berard in their valuable experiments, to determine the best burner for the city of Paris, proved that for every consumption of gas there is a burner which is best suited for that consumption. Now by the proper selection of a burner for small consumptions, some simple relation may yet be found between consumption and illuminating power.

In the above, reference has been made only to experiments detailed in Prof. Silliman's communication. I have, however, made a number of experiments myself upon the same subject, besides collating the results of some sixty or more independent observations, which have been published during the last fifteen years, and the results are curious, instructive and unexpected. There are a few among them to which Farmer's Theorem might be applied, quite a number to which the old law will apply; though many of them require a *modification* of the old law.

I hope in due time to prepare a paper giving some of these results.

April, 1870.

ART. XLI.—*Note on Mr. Stimpson's Paper on Farmer's Theorem;* by B. SILLIMAN.

MR. STIMPSON'S criticism on the first experiment of my paper was induced by an obvious numerical error in the statement of the data, which Mr. S. has himself corrected in a foot note on p. 373. The experiment properly stated exactly sustains the theorem.

I admit that in Experiment No. 2, the exponent comes nearer to the third power than to the square, being 2·689, 2·638 and 2·669; in the three cases average 2·661. But it also indicates that the illuminating power of the standard 3·30 cubic feet was very imperfect. The experiments, however, appear to me to demonstrate clearly the radical inaccuracy of the old rule for photometric calculations, and that some ratio near to or greater than the square gives often more trustworthy results than the old rule.

In Experiment No. 3, the exponents are respectively 2·552, 2·297 and 2·092, coming in the last very near the square. It shows that 4·88 cubic feet consumption gave the best results, and also that 3·72 and 7·219 cubic feet consumption gave very nearly the same degree of intensity of combustion.

In Experiment No. 4, the exponent 1·85 power for a consumption of 9·519 consumption, shows an imperfect combustion of the fish-tail burner employed.

In Experiment No. 5, the experimental conditions were wholly unfavorable to accuracy, owing to an inequality of pressure unavoidable in the experimental method adopted, there being one inch pressure on the 10·06 c. f. consumption, and only half an inch on the 5·16 c. f. It is well known to all photometric observers how important a low pressure and an equal pressure is to the results obtained. It was hardly fair to Mr. Farmer to have quoted this trial, but I was desirous of exhibiting the entire range of observation, bad as well as good.

As in Experiment No. 6, the consumption of sperm was in the two tests very nearly uniform; the difference would be very trifling if the correction had been applied. Nor is it by any means so certain that Farmer's Theorem applies to the candle as I supposed when the remarks quoted by Mr. Stimpson were made, since, if a candle burns much over 120 grains it smokes ("tails off"), and then there is an end of all accuracy, and the observation must be rejected; since there is an imperfect combustion giving an increase in consumption but not in intensity.

Mr. Stimpson rejects all the data given by me which are founded upon the determination of the intensity of a rich gas by the 'method of mixtures,' because, as he says, the accuracy of this method has not yet been demonstrated by experiment. But this objection ceases to have force now that experiments, made by Mr. Farmer and myself lately, prove that this method is worthy of confidence. Inasmuch as the most important case for the use of Farmer's Theorem is that of gas too rich to be burned in the standard burner on a basis of 5 cubic feet per hour, it is striking off by all means the most valuable portion of my contribution to photometrical methods if it could be shown that the 'method of mixtures' was untrustworthy. I am, therefore, glad of this occasion to reiterate my confidence in this method, and to refer the reader curious in such matters to a brief paper of mine upon this subject, which will be found on page 379.

I care very little whether the results of experiment shall show, when they are sufficiently accumulated, that the ratio of consumption of gas is to the intensity produced as the squares of consumption in a given case, or in some other ratio greater than a simple ratio. I have desired chiefly to call attention to the general untrustworthiness of all photometric observations which are made with volumes of gas much less than the normal standard adopted, when these results are calculated on the simple ratio of the consumptions. It is only by the accumulation of carefully conducted experimental data that a law can be established, and these data are now pretty rapidly accumulating.*

* In the proceedings of the American Association for Advancement of Science, Salem meeting, I presented the matter referring to *Sugg's manipulation* in a different form, giving the results of the observations with his Argand burner in a tabular form as follows:

No. obs.	Cubic feet.	Observations corrected by old rule candles.	Differences.	Uncorrected observations.	Observations corrected by Farmer's Theorem.	Differences.
1.	5·	14·00		14·00	14·00	
2.	4·9	13·78	0·22	13·504	14·060	0·556
3.	4·8	13·74	0·26	13·190	14·312	1·132
4.	4·7	13·30	0·70	12·502	14·148	1·646
5.	4·6	13·04	0·96	11·996	14·191	2·105
6.	4·5	11·93	2·07	10·738	13·255	2·517

The mean candle power of the 6th column is 13·99 candles; difference 0·01 candle.

The following will show the fractional power required to bring the uncorrected observations (column 5) to 14·00 candles.

2. $4{\cdot}9^{1{\cdot}83} : 13{\cdot}504 :: 5^{1{\cdot}83} : 14{\cdot}00$ candles.
3. $4{\cdot}8^{1{\cdot}47} : 13{\cdot}\ 90 :: 5^{1{\cdot}47} : 14{\cdot}00$ "
4. $4{\cdot}7^{1{\cdot}85} : 12{\cdot}502 :: 5^{1{\cdot}85} : 14{\cdot}00$ "
5. $4{\cdot}6^{1{\cdot}86} : 11{\cdot}996 :: 5^{1{\cdot}36} : 14{\cdot}00$ "
6. $4{\cdot}5^{2{\cdot}52} : 10{\cdot}737 :: 5^{2{\cdot}52} : 14{\cdot}00$ "

This table shows that the 3rd and 6th tests have not been good ones, while the 2d, 3d, 4th and 5th tests fall a little below the square or 2d power, and the 6th test is considerably more.

New Haven, July, 1870.

ART. XLII.—*On the Determination of the Photometric Power of a rich gas by dilution with a poor gas of known value: the "method of mixtures;"* by B. SILLIMAN.

IN a paper on 'Farmer's Theorem,'* I have given several examples of the method of determining the intensity or photometric power of a rich gas by diluting it with several times its own volume of a poorer gas of known intensity, and then calculating its value from the increment of intensity. Having demonstrated in the paper before mentioned the worthlessness of all determinations of the intensity of gases of high illuminating power made by burning them in volumes less than five cubic feet, and then calculating their intensity by the rule of three up to that volume, I have shown how much more exact results were obtained when the results were calculated upon the theorem of Mr. Farmer; this greater exactness being predicated largely upon the confirmation drawn from parallel observations upon the same gases when measured by the method of mixtures. The results thus obtained having, however, been questioned by Mr. Stimpson,† on the ground that the method itself had not been experimentally demonstrated, I have undertaken lately, in connection with Mr. Farmer, to make some experiments calculated to test its accuracy.

The results which go to support the accuracy of the method were obtained with the use of a new photometric apparatus, constructed for the Manhattan Gas Co., under my direction, by Sugg of London, and which was designed to embrace all the best approved features which recent experience has indicated in photometry. A discussion of these details would be out of place in this connection. Before detailing our results, it will be proper to present the method of determination of intensity for gas of high illuminating power as practiced by Mr. Farmer at the Manhattan Gas Works in New York, and which I have called *the method of mixtures.*

To find the candle power of a gas having, for example, an intensity greater than 20 candles, mix the rich gas of unknown power with a poorer gas of known power in such proportions that the intensity of the mixture shall not be greater than 20 candles power, when consumed at the agreed rate of not over five cubic feet per hour. Then to compute the candle power (intensity) of the rich gas,—

---

* This Journal, II, xlix, 17; also Proceedings of American Association for Advancement of Science, Salem meeting, 1869, p. 149.

† See page 272, this volume.

Let a=the percentage or volume of gas of low intensity.
" b=the intensity in candles of the gas of low intensity.
" c=the percentage or volume of rich gas used in the mixture.
" d=the intensity in candles of the mixture as observed.
" x=the intensity in candles of the rich gas required.

Hence,
$$\frac{ab+cx}{a+c}=d \qquad \frac{(d-b)a}{c}+d=x$$
$$ab+cx=ad+cd$$
$$cx=ad+cd+ab.$$

And this expression is stated arithmetically in the following

Rule:—Subtract the intensity of the poor gas from the intensity of the mixture; multiply the remainder by the volume of poor gas; divide the product by the volume of rich gas; add to the quotient the intensity of the mixed gas, and the sum is the intensity of the rich gas sought.

Now when we reflect that in any given illuminating gas we have always a certain volume of non-luminous combustible gas, as the substratum to which is added, according to its source and mode of treatment, a variable volume of illuminants, it is no unwarranted assumption to say that the illuminants (chiefly olefines) are *diluted* by the non-illuminants. It is agreed, on all hands, that hydrogen, marsh gas and carbonic oxyd, which together form the mass of the non-luminous substratum of all illuminating gas, have of themselves, when pure, no luminosity, and when burned at ordinary atmospheric pressures and corresponding temperatures, that they may in fact be called, as to luminosity, neutral. It can hardly be questioned that the intensity which these neutral gases may assume in a given mixture must depend, under the same ordinary conditions before mentioned, upon the amount and kind of olefines they may take up in the destructive distillation of the coal or other hydrocarbons used in making gas. If these assumptions are true, we ought to be able to demonstrate them by experiment, by commingling certain volumes of a gas of known intensity with a neutral gas of no intensity. These experimental conditions would be met by using carbonic oxyd as the neutral gas, or better still, the mixture of carbonic oxyd and hydrogen resulting from the decomposition of vapor of water at a high temperature in contact with highly ignited carbon, as in the hydrocarbon gas process. But in default of any convenient means of obtaining these gases, we had recourse to hydrogen gas evolved by the action of diluted sulphuric acid upon zinc in a large self-regulating generator of hydrogen. It is well known that hydrogen thus made is not absolutely non-luminous, but it is sufficiently so for photometric purposes. The avidity of hydrogen for all the olefines, however, renders it difficult to obtain entirely satisfactory results with the use of this gas, provided it is passed through gas pipes and holders which have been previously used for the transmission of coal gas, since however carefully one may rinse out these tubes by hydrogen, there may yet cling, probably, some small trace of the condensed illuminants to the walls of the tubes, which imparts a trifling intensity to the hydrogen passing through them.

To obtain a supply of rich gas of uniform intensity, 100 lbs. of Albertite were coked until 810 cubic feet of gas had been taken from it of a density of ·498. This gas was purified, collected and preserved in a separate gas holder. Its intensity was determined,

1st. By Farmer's Theorem = 30·49 candles.
2d. " mixing with poor gas = 30·95 "
3d. " simple ratio = 26·20 "

In determining the intensity of this gas by the method of mixtures, 20 per cent by volume of Albert gas was mingled with 80 per cent of 10·6 candle gas obtained from a poor coal. The mixture had an intensity of 14·67 candles. Hence,

$$(14{\cdot}67-10{\cdot}60)\times 80\div 20+14{\cdot}67=30{\cdot}95 \text{ candles}$$

for the intensity of the Albert Gas by the method of mixtures.

1st Experiment.

Taken 75 volumes of coal gas =14·75 candles, obs'd.
" 25 " hydrogen = ----
" 100 " mixture =10·22 " "
If hydrogen=0 this mixture should have given 11·06 " calc.
Hence there is an error of observation of ·84 of a candle, which makes the value of the hydrogen apparently below zero.

2d Experiment.

Taken 75 volumes of coal gas =14·25 candles, obs'd.
" 25 " hydrogen = ----
" 100 " mixture =10·35 " "
If hydrogen=0 this mixture should have given 10·69 " calc.
Hence there is an error of observation = ·25 " which again makes the value of the hydrogen, apparently a little below zero.

The want of sufficient storage room for hydrogen and the necessity of many repetitions, and much care in manipulation to avoid errors of quantity in the synthesis of mixtures make experiments of this sort tedious, and my other avocations have prevented my multiplying them as much as is desirable. I think, however, that most photometric observers will agree with me that it is safe to conclude, from these two experiments, that the action of hydrogen in gaseous mixtures is simply that of a diluant. We might make a thousand observations by the means now at command, and not obtain one with an exact 0 for hydrogen. A very trifling error in the admeasurement greatly influences the result.

We may therefore safely conclude, as it appears to me—

1st. That in all illuminating gas we have a certain substratum of non-luminous gas, holding in solution a variable volume of luminous gas (olefines).

2d. That when a gas is too rich in illuminants to permit of accurate photometric admeasurement by the usual standards of intensity, it may be diluted with a poor gas of known value and volume to such a standard as is consistent with the accurate employment of the usual photometric apparatus, its true value being then calculated from the known values employed.

P. S. I find in the manuscript records of the Manhattan Gas Company, mention of four experiments made many years since by Mr. Schultz, chemist of that company, in which he mixed 5 per cent and 10 per cent of hydrogen with gas of very high illuminating power. The results are less satisfactory than they would have been, had the volume of hydrogen employed been much larger and the intensity of the coal gas not over 15 candles. Moreover at that time the means of admeasurement at the command of the observer in the laboratory of the Manhattan Company of small volumes of gas were much less exact than they now are. The results obtained are as follows:

1st Experiment.

Taken	90	volumes of coal gas	=22·53	candles,	observed.
“	10	“ hydrogen	=----		
“	100	“ mixture	=19·11	“	“
If the hydrogen is taken as 0		“	=20·27	“	calculated.
Showing apparent error of			=− 1·11	“	“

2d Experiment.

Taken	95	volumes of coal gas	=22·53	candles,	observed.
“	5	“ hydrogen	= ----		
“	100	“ mixture	=20·35	“	“
If the hydrogen is taken as 0		“	=21·39	“	calculated.
Apparent error			=−1·04	“	“

3d Experiment.

Taken	90	volumes coal gas	=20·83	candles,	observed.
“	10	“ hydrogen	= ----		
“	100	“ mixture	=19·44	“	“
If hydrogen is taken= 0		“	=18·29	“	calculated.
Apparent error,			+ ·15	“	“

4th Experiment.

Taken	95	volumes of	coal gas	=20·83	candles,	observed.
"	5	"	hydrogen	= ----		
"	100	"	mixture	=20·52	"	"
If hydrogen is taken	=0		"	=19·79	"	calculated.
Apparent error,				+ ·73	"	"

New Haven, Sept. 1870.

Art. XLIII.—*Address of Thomas Henry Huxley, at the meeting of the British Association at Liverpool, on the* 14*th of Sept.*, 1870.*

It has long been the custom for the newly installed President of the British Association for the Advancement of Science to take advantage of the elevation of the position in which the suffrages of his colleagues had, for the time, placed him, and, casting his eyes around the horizon of the scientific world, to report to them what could be seen from his watch-tower; in what directions the multitudinous divisions of the noble army of the improvers of natural knowledge were marching; what important strongholds of the great enemy of us all, ignorance, had been recently captured; and, also, with due impartiality, to mark where the advanced posts of science had been driven in, or a long-continued siege had made no progress.

I propose to endeavor to follow this ancient precedent, in a manner suited to the limitations of my knowledge and of my capacity. I shall not presume to attempt a panoramic survey of the world of science, nor even to give a sketch of what is doing in the one great province of biology, with some portions of which my ordinary occupations render me familiar. But I shall endeavor to put before you the history of the rise and progress of a single biological doctrine; and I shall try to give some notion of the fruits, both intellectual and practical, which we owe, directly or indirectly, to the working out, by seven generations of patient and laborious investigators, of the thought which arose, more than two centuries ago, in the mind of a sagacious and observant Italian naturalist.

It is a matter of every-day experience that it is difficult to prevent many articles of food from becoming covered with mould; that fruit, sound enough to all appearance, often contains grubs at the core; that meat, left to itself in the air, is apt to putrefy and swarm with maggots. Even ordinary water, if allowed to stand in an open vessel sooner or later becomes turbid and full of living matter.

The philosophers of antiquity, interrogated as to the cause of these phenomena, were provided with a ready and a plausible

---

* From "Nature," of Sept. 15.

answer. It did not enter their minds even to doubt that these low forms of life were generated in the matters in which they made their appearance. Lucretius, who had drunk deeper of the scientific spirit than any poet of ancient or modern times except Goethe, intends to speak as a philosopher, rather than as a poet, when he writes that "with good reason the earth has gotten the name of mother, since all things are produced out of the earth. And many living creatures, even now, spring out of the earth, taking form by the rains and the heat of the sun." The axiom of ancient science, "that the corruption of one thing is the birth of another," had its popular embodiment in the notion that a seed dies before the young plant springs from it; a belief so wide spread and so fixed, that Saint Paul appeals to it in one of the most splendid outbursts of his fervid eloquence:—"Thou fool, that which thou sowest is not quickened, except it die."*

The proposition that life may, and does, proceed from that which has no life, then, was held alike by the philosophers, the poets, and the people, of the most enlightened nations, eighteen hundred years ago; and it remained the accepted doctrine of learned and unlearned Europe, through the middle ages, down even to the seventeenth century.

It is commonly counted among the many merits of our great countryman, Harvey, that he was the first to declare the opposition of fact to venerable authority in this, as in other matters; but I can discover no justification for this wide-spread notion. After careful search through the "Exercitationes de Generatione," the most that appears clear to me is, that Harvey believed all animals and plants to spring from what he terms a "*primordium vegetale*," a phrase which may nowadays be rendered "a vegetative germ;" and this, he says, is "*oviforme*," or "egg-like;" not, he is careful to add, that it necessarily has the shape of an egg, but because it has the constitution and nature of one. That this "*primordium oviforme*" must needs, in all cases, proceed from a living parent is nowhere expressly maintained by Harvey, though such an opinion may be thought to be implied in one or two passages; while, on the other hand, he does, more than once, use language which is consistent only with a full belief in spontaneous or equivocal generation. In fact, the main concern of Harvey's wonderful little treatise is not with generation, in the physiological sense, at all, but with development; and his great object is the establishment of the doctrine of epigenesis.

The first distinct enunciation of the hypothesis that all living matter has sprung from pre-existing living matter, came from a contemporary, though a junior, of Harvey, a native of that country fertile in men great in all departments of human activity,

---

*1 Corinthians, xv, 36.

which was to intellectual Europe, in the sixteenth and seventeenth centuries, what Germany is in the nineteenth. It was in Italy, and from Italian teachers, that Harvey received the most important part of his scientific education. And it was a student trained in the same schools, Francesco Redi—a man of the widest knowledge and most versatile abilities, distinguished alike as scholar, poet, physician, and naturalist—who, just two hundred and two years ago, published his "Esperienza intorno alla Generazione degl' Insetti," and gave to the world the idea, the growth of which it is my purpose to trace. Redi's book went through five editions in twenty years; and the extreme simplicity of his experiments, and the clearness of his arguments gained for his views, and for their consequences, almost universal acceptance.

Redi did not trouble himself much with speculative considerations, but attacked particular cases of what was supposed to be "spontaneous generation" experimentally. Here are dead animals, or pieces of meat, says he; I expose them to the air in hot weather, and in a few days they swarm with maggots. You tell me that these are generated in the dead flesh; but if I put similar bodies, while quite fresh, into a jar, and tie some fine gauze over the top of the jar, not a maggot makes its appearance, while the dead substances, nevertheless, putrefy just in the same way as before. It is obvious, therefore, that the maggots are not generated by the corruption of the meat; and that the cause of their formation must be a something which is kept away by gauze. But gauze will not keep away aëriform bodies, or fluid. This something must, therefore, exist in the form of solid particles too big to get through the gauze. Nor is one long left in doubt what these solid particles are: for the blowflies, attracted by the odor of the meat, swarm round the vessel, and, urged by a powerful but in this case misleading instinct, lay eggs out of which maggots are immediately hatched upon the gauze. The conclusion, therefore, is unavoidable; the maggots are not generated by the meat, but the eggs which give rise to them are brought through the air by the flies.

These experiments seem almost childishly simple, and one wonders how it was that no one ever thought of them before. Simple as they are, however, they are worthy of the most careful study, for every piece of experimental work since done, in regard to this subject, has been shaped upon the model furnished by the Italian philosopher. As the results of his experiments were the same, however varied the nature of the materials he used, it is not wonderful that there arose in Redi's mind a presumption, that in all such cases of the seeming production of life from dead matter, the real explanation was the introduction of living germs from without into that dead matter. And thus the hypothesis that living matter always arises by the agency of pre-existing living matter, took definite shape; and had, henceforward, a right to be considered and a claim to be refuted, in each particular case, before the production of living matter in any other way could be admitted by careful reasoners. It will be necessary for me to refer to this hypothesis so frequently, that, to save circumlocution, I shall call it the hypothesis of *Biogenesis;* and I shall term the contrary doctrine—that living matter may be produced by not living matter—the hypothesis of *Abiogenesis.*

In the seventeenth century, as I have said, the latter was the dominant view, sanctioned alike by antiquity and by authority; and it is interesting to observe that Redi did not escape the customary tax upon a discoverer of having to defend himself against the charge of impugning the authority of the Scriptures; for his adversaries declared that the generation of bees from the carcase of a dead lion is affirmed, in the Book of Judges, to have been the origin of the famous riddle with which Samson perplexed the Philistines:—

> "Out of the eater came forth meat,
> And out of the strong came forth sweetness."

Against all odds, however, Redi, strong with the strength of demonstrable fact, did splendid battle for Biogenesis; but it is remarkable that he held the doctrine in a sense which, if he had lived in these times, would have infallibly caused him to be classed among the defenders of "spontaneous generation." "Omne vivum ex vivo," "no life without antecedent life," aphoristically sums up Redi's doctrine; but he went no further. It is most remarkable evidence of the philosophic caution and impartiality of his mind, that although he had speculatively anticipated the manner in which grubs really are deposited in fruits and in the galls of plants, he deliberately admits that the evidence is insufficient to bear him out; and he therefore prefers the supposition that they are generated by a modification of the living substance of the plants themselves. Indeed, he regards these vegetable growths as organs, by means of which the plant gives rise to an animal, and looks upon this production of specific animals as the final cause of the galls and of at any rate some fruit. And he proposes to explain the occurrence of parasites within the animal body in the same way.

It is of great importance to apprehend Redi's position rightly; for the lines of thought he laid down for us are those upon which naturalists have been working ever since. Clearly, he held *Biogenesis* as against *Abiogenesis;* and I shall immediately proceed, in the first place, to inquire how far subsequent investigation has borne him out in so doing.

But Redi also thought that there were two modes of Biogenesis. By the one method, which is that of common and ordinary occurrence, the living parent gives rise to offspring which passes through the same cycle of changes as itself—like gives rise to like; and this has been termed *Homogenesis.* By the other mode the living parent was supposed to give rise to offspring which passed through a totally different series of states from those exhibited by the parent, and did not return into the cycle of the parent; this is what ought to be called *Heterogenesis*, the offspring being altogether, and permanently unlike the parent. The term Heterogenesis, however, has unfortunately been used in a different sense, and M. Milne-Edwards has therefore substituted for it *Xenogenesis*, which means the generation of something foreign. After discussing Redi's hypothesis of universal Biogenesis, then, I shall go on to ask how far the growth of science justifies his other hypothesis of Xenogenesis.

The progress of the hypothesis of Biogenesis was triumphant and unchecked for nearly a century. The application of the microscope to anatomy in the hands of Grew, Leeuwenhoek, Swammerdam, Lyonet, Vallisnieri, Reaumur, and other illustrious investigators of nature of that day, displayed such a complexity of organization in the lowest and minutest forms, and everywhere revealed such a prodigality of provision for their multiplication by germs of one sort or another, that the hypothesis of Abiogenesis began to appear not only untrue, but absurd; and, in the middle of the eighteenth century, when Needham and Buffon took up the question, it was almost universally discredited.

But the skill of the microscope-makers of the eighteenth century soon reached its limit. A microscope magnifying 400 diameters was a *chef d'œuvre* of the opticians of that day; and at the same time, by no means trustworthy. But a magnifying power of 400 diameters, even when definition reaches the exquisite perfection of our modern achromatic lenses, hardly suffices for the mere discernment of the smallest forms of life. A speck, only $\frac{1}{25}$th of an inch in diameter, has, at 10 inches from the eye, the same apparent size as an object $\frac{1}{10000}$th of an inch in diameter, when magnified 400 times; but forms of living matter abound, the diameter of which is not more than $\frac{1}{40000}$th of an inch. A filtered infusion of hay, allowed to stand for two days, will swarm with living things, among which, any which reaches the diameter of a human red blood-corpuscle, or about $\frac{1}{3200}$th of an inch, is a giant. It is only by bearing these facts in mind, that we can deal fairly with the remarkable statements and speculations put forward by Buffon and Needham in the middle of the eighteenth century.

When a portion of any animal or vegetable body is infused in water, it gradually softens and disintegrates; and, as it does so, the water is found to swarm with minute active creatures, the so-called Infusorial Animalcules, none of which can be seen, except by the aid of the microscope; while a large proportion belong to the category of smallest things of which I have spoken, and which must have all looked like mere dots and lines under the ordinary microscopes of the eighteenth century.

Led by various theoretical considerations which I cannot now discuss, but which looked promising enough in the lights of that day, Buffon and Needham doubted the applicability of Redi's hypothesis to the infusorial animalcules, and Needham very properly endeavored to put the question to an experimental test. He said to himself, if these infusorial animalcules come from germs, their germs must exist either in the substance infused, or in the water in which the infusion is made, or in the superjacent air. Now the vitality of all germs is destroyed by heat. Therefore, if I boil the infusion, cork it up carefully, cementing the cork over with mastic, and then heat the whole vessel by heaping hot ashes over it, I must needs kill whatever germs are present. Consequently, if Redi's hypothesis hold good, when the fusion is taken away and allowed to cool, no animalcules ought to be developed in it; whereas, if the animalcules are not dependent on pre-existing germs, but are generated from the infused substance, they ought, by-and-by, to make their appearance. Needham found that, under the circumstances in which he made his experiments, animalcules always did arise in the infusions, when a sufficient time had elapsed to allow for their development.

In much of his work Needham was associated with Buffon, and the results of their experiments fitted in admirably with the great French naturalist's hypothesis of "organic molecules," according to which, life is the indefeasible property of certain indestructible molecules of matter, which exist in all living things, and have inherent activities by which they are distinguished from not living matter. Each individual living organism is formed by their temporary combination. They stand to it in the relation of the particles of water to a cascade, or a whirlpool; or to a mould, into which the water is poured. The form of the organism is thus determined by the reaction between external conditions and the inherent activities of the organic molecules of which it is composed; and, as the stoppage of the whirlpool destroys nothing but a form, and leaves the molecules of the water, with all their inherent activities intact, so what we call the death and putrefraction of an animal, or of a plant, is merely the breaking up of the form, or manner of association, of its constituent organic molecules, which are then set free as infusorial animalcules.

It will be perceived that this doctrine is by no means identical with *Abiogenesis*, with which it is often confounded. On this hypothesis, a piece of beef, or a handful of hay, is dead only in a limited sense. The beef is dead ox, and the hay is dead grass; but the "organic molecules" of the beef or the hay are not dead, but are ready to manifest their vitality as soon as the bovine or herbaceous shrouds in which they are imprisoned are rent by the macerating action of water. The hypothesis therefore must be classified under Xenogenesis, rather than under Abiogenesis. Such as it was, I think it will appear, to those who will be just enough to remember that it was propounded before the birth of modern chemistry, and of the modern optical arts, to be a most ingenious and suggestive speculation.

But the great tragedy of Science—the slaying of a beautiful hypothesis by an ugly fact—which is so constantly being enacted under the eyes of philosophers, was played, almost immediately, for the benefit of Buffon and Needham.

Once more, an Italian, the Abbé Spallanzani, a worthy successor and representative of Redi in his acuteness, his ingenuity, and his learning, subjected the experiments and the conclusions of Needham to a searching criticism. It might be true that Needham's experiments yielded results such as he had described, but did they bear out his arguments? Was it not possible, in the first place, that he had not completely excluded the air by his corks and mastic? And was it not possible, in the second place, that he had not sufficiently heated his infusions and the superjacent air? Spallanzani joined issue with the English naturalist on both these pleas, and he showed that if, in the first place, the glass vessels in which the infusions were contained were hermetically sealed by fusing their necks, and if, in the second place, they were exposed to the temperature of boiling water for three-quarters of an hour,* no animalcules ever made their appearance within them. It must be admitted that the experiments and arguments of Spallanzani furnish a complete and a crushing reply to those of Needham. But we all too often forget that it is one thing to refute a proposition, and another to prove the truth of a doctrine which, implicitly or explicitly, contradicts that proposition, and the advance of science soon showed that though Needham might be quite wrong, it did not follow that Spallanzani was quite right.

Modern chemistry, the birth of the latter half of the eighteenth century, grew apace, and soon found herself face to face with the great problems which biology had vainly tried to attack without her help. The discovery of oxygen led to the laying of the foundations of a scientific theory of respiration, and to an examination of the marvellous interactions of organic substances with oxygen. The presence of free oxygen appeared to be one of the conditions of the existence of life, and of those

---

* See Spallanzani, "Opere," vi, pp. 42 and 61.

singular changes in organic matters which are known as fermentation and putrefaction. The question of the generation of the infusory animalcules thus passed into a new phase. For what might not have happened to the organic matter of the infusions, or to the oxygen of the air, in Spallanzani's experiments? What security was there that the development of life which ought to have taken place had not been checked or prevented by these changes?

The battle had to be fought again. It was needful to repeat the experiments under conditions which would make sure that neither the oxygen of the air, nor the composition of the organic matter, was altered in such a manner as to interfere with the existence of life.

Schulze and Schwann took up the question from this point of view in 1836 and 1837. The passage of air through red-hot glass tubes, or through strong sulphuric acid, does not alter the proportion of its oxygen, while it must needs arrest or destroy any organic matter which may be contained in the air. These experimenters, therefore, contrived arrangements by which the only air which should come into contact with a boiled infusion should be such as had either passed through red-hot tubes or through strong sulphuric acid. The result which they obtained was that an infusion so treated developed no living things, while if the same infusion was afterwards exposed to the air such things appeared rapidly and abundantly. The accuracy of these experiments has been alternately denied and affirmed. Supposing them to be accepted, however, all that they really proved was that the treatment to which the air was subjected destroyed, *something* that was essential to the development of life in the infusion. This "something" might be gaseous, fluid, or solid; that it consisted of germs remained only an hypothesis of greater or less probability.

Contemporaneously with these investigations a remarkable discovery was made by Cagniard de la Tour. He found that common yeast is composed of a vast accumulation of minute plants. The fermentation of must or wort in the fabrication of wine and of beer is always accompanied by the rapid growth and multiplication of these *Torulæ.* Thus fermentation, in so far as it was accompanied by the development of microscopical organisms in enormous numbers, became assimilated to the decomposition of an infusion of ordinary animal or vegetable matter; and it was an obvious suggestion that the organisms were, in some way or other, the causes both of fermentation and of putrefaction. The chemists, with Berzelius and Liebig at their head, at first laughed this idea to scorn; but in 1843, a man then very young, who has since performed the unexampled feat of attaining to high eminence alike in Mathematics, Physics, and Physiology—I speak of the illustrious Helmholtz—reduced the matter to the test of experiment by a method alike elegant and conclusive. Helmholtz separated a putrefying or a fermenting liquid from one which was simply putrescible or fermentable by a membrane which allowed the fluids to pass through and become intermixed, but stopped the passage of solids. The result was, that while the putrescible or the fermentable liquids became impregnated with the results of the putrescence or fermentation which was going on on the other side of the membrane, they neither putrefied (in the ordinary way) nor fermented; nor were any of the organisms which abounded in the fermenting or putrefying liquid generated in them. Therefore the cause of the development of these organisms must lie in something which cannot pass through membranes; and as Helmholtz's investigations were long antecedent to Graham's researches upon colloids, his natural conclusion was that the agent thus intercepted must be a solid material. In point of fact, Helmholtz's experiments narrowed the issue to this: that which excites fermentation and putrefaction, and at the same time gives rise to living forms in a fermentable or putrescible fluid, is not a gas and is not a diffusible fluid; therefore it is either a colloid, or it is a matter divided into very minute solid particles.

The researches of Schroeder and Dusch in 1854, and of Schroeder alone, in 1859, cleared up this point by experiments which are simply refinements upon those of Redi. A lump of cotton-wool is, physically speaking, a pile of many thicknesses of a very fine gauze, the fineness of the meshes of which depends upon the closeness of the compression of the wool. Now, Schroeder and Dusch found, that, in the case of all the putrefiable materials which they used (except milk and yolk of egg), an infusion boiled, and then allowed to come into contact with no air but such as had been filtered through cotton-wool, neither putrefied nor fermented, nor developed living forms. It is hard to imagine what the fine sieve formed by the cotton-wool could have stopped except minute solid particles. Still the evidence was incomplete until it had been positively shown, first, that ordinary air does contain such particles; and, secondly, that filtration through cotton-wool arrests these particles and allows only physically pure air to pass. This demonstration has been furnished within the last year by the remarkable experiments of Professor Tyndall. It has been a common objection of Abiogenists that, if the doctrine of Biogeny is true, the air must be thick with germs; and they regard this as the height of absurdity. But Nature occassionally is exceedingly unreasonable, and Professor Tyndall has proved that this particular absurdity may nevertheless be a reality. He has demonstrated that ordinary air is no better than a sort of stir-about of excessively minute solid particles; that these particles are almost wholly destructible by heat; and that they are strained off, and the air rendered optically pure by being passed through cotton-wool.

But it remains yet in the order of logic, though not of history, to show that among these solid destructible particles there really do exist germs capable of giving rise to the development of living forms in suitable menstrua. This piece of work was done by M. Pasteur, in those beautiful researches which will ever render his name famous; and which, in spite of all attacks upon them, appear to me now, as they did seven years ago,* to be models of accurate experimentation and logical reasoning. He strained air through cotton-wool, and found, as Schroeder and Dusch had done, that it contained nothing competent to give rise to the development of life in fluids highly fitted for that purpose. But the important further links in the chain of evidence added by Pasteur are three. In the first place he subjected to microscopic examination the cotton-wool which had served as strainer, and found that sundry bodies clearly recognizable as germs, were among the solid particles strained off. Secondly, he proved that these germs were competent to give rise to living forms by simply sowing them in a solution fitted for their development. And, thirdly, he showed that the incapacity of air strained through cotton-wool to give rise to life, was not due to any occult change effected in constituents of the air by the wool, by proving that the cotton-wool might be dispensed with altogether, and perfectly free access left between the exterior air and that in the experimental flask. If the neck of the flask is drawn out into a tube and bent downward: and if, after the contained fluid has been carefully boiled, the tube is heated sufficiently to destroy any germs which may be present in the air which enters as the fluid cools, the apparatus may be left to itself for any time, and no life will appear in the fluid. The reason is plain. Although there is free communication between the atmosphere laden with germs and the germless air in the flask, contact between the two takes place only in the tube; and as the germs cannot fall upward, and there are no currents, they never reach the interior of the flask. But if the tube be broken short off where it proceeds from the flask, and free access be thus given to germs falling vertically out of the air, the fluid which has remained clear and desert for months, becomes, in a few days turbid and full of life.

These experiments have been repeated over and over again by independent observers with entire success; and there is one

---

* "Lectures to Working Men on the Causes of the Phenomena of Organic Nature." 1863.

very simple mode of seeing the facts for oneself, which I may as well describe.

Prepare a solution (much used by M. Pasteur, and often called "Pasteur's solution") composed of water with tartrate of ammonia, sugar, and yeast-ash dissolved therein.* Divide it into three portions in as many flasks; boil all three for a quarter of an hour: and, while the steam is passing out, stop the neck of one with a large plug of cotton-wool, so that this also may be thoroughly steamed. Now set the flasks aside to cool, and when their contents are cold, add to one of the open ones a drop of filtered infusion of hay which has stood for twenty-four hours, and is consequently full of the active and excessively minute organisms known as *Bacteria.* In a couple of days of ordinary warm weather the contents of this flask will be milky from the enormous multiplication of *Bacteria.* The other flask, open and exposed to the air, will, sooner or later, become milky with *Bacteria*, and patches of mould may appear in it; while the liquid in the flask, the neck of which is plugged with cotton-wool, will remain clear for an indefinite time. I have sought in vain for any explanation of these facts, except the obvious one, that the air contains germs competent to give rise to *Bacteria*, such as those with which the first solution has been knowingly and purposely inoculated, and to the mould-*Fungi.* And I have not yet been able to meet with any advocate of Abiogenesis who seriously maintains that the atoms of sugar, tartrate of ammonia, yeast-ash, and water, under no influence but that of free access of air and the ordinary temperature, rearrange themselves and give rise to the protoplasm of *Bacterium.* But the alternative is to admit that these *Bacteria* arise from germs in the air; and if they are thus propagated, the burden of proof that other like forms are generated in a different manner, must rest with the assertor of that proposition.

To sum up the effect of this long chain of evidence:—

It is demonstrable that a fluid eminently fit for the development of the lowest forms of life, but which contains neither germs, nor any protein compound, gives rise to living things in great abundance if it is exposed to ordinary air, while no such development takes place if the air with which it is in contact is mechanically freed from the solid particles which ordinarily float in it, and which may be made visible by appropriate means.

It is demonstrable that the great majority of these particles are destructible by heat, and that some of them are germs or living particles, capable of giving rise to the same forms of life as those which appear when the fluid is exposed to unpurified air.

---

* Infusion of hay treated in the same way yields similar results; but as it contains organic matter, the argument which follows cannot be based upon it.

It is demonstrable that inoculation of the experimental fluid with a drop of liquid known to contain living particles, gives rise to the same phenomena as exposure to unpurified air.

And it is further certain that these living particles are so minute that the assumption of their suspension in ordinary air, presents not the slightest difficulty. On the contrary, considering their lightness and the wide diffusion of the organisms which produce them, it is impossible to conceive that they should not be suspended in the atmosphere in myriads.

Thus the evidence, direct and indirect, in favor of *Biogenesis* for all known forms of life must, I think, be admitted to be of great weight.

On the other side, the sole assertions worthy of attention are that hermetically sealed fluids, which have been exposed to great and long-continued heat, have sometimes exhibited living forms of low organization, when they have been opened.*

The first reply that suggests itself is the probability that there must be some error about these experiments, because they are performed on an enormous scale every day with quite contrary results. Meat, fruits, vegetables, the very materials of the most fermentable and putrescible infusions are preserved to the extent, I suppose I may say, of thousands of tons every year, by a method which is a mere application of Spallanzani's experiment. The matters to be preserved are well boiled in a tin case provided with a small hole, and this hole is soldered up when all the air in the case has been replaced by steam. By this method they may be kept for years without putrefying, fermenting, or getting mouldy. Now this is not because oxygen is excluded, inasmuch as it is now proved that free oxygen is not necessary for either fermentation or putrefaction. It is not because the tins are exhausted of air, for *Vibriones* and *Bacteria* live, as Pasteur has shown, without air or free oxygen. It is not because the boiled meats or vegetables are not putrescible or fermentable, as those who have had the misfortune to be in a ship supplied with unskillfully closed tins well know. What is it, therefore, but the exclusion of germs? I think that Abiogenists are bound to answer this question before they ask us to consider new experiments of precisely the same order.

And in the next place, if the results of the experiments I refer to are really trust-worthy, it by no means follows that Abiogenesis has taken place. The resistance of living matter to heat is known to vary within considerable limits, and to depend, to some extent, upon the chemical and physical qualities of the surrounding medium. But if, in the present state of

---

* For a full account of the most recent series of experiments of this description, see Dr. H. C. Bastian's paper in NATURE, No. xxxv, p. 170; No. xxxvi. p. 193; and No. xxxvii, p. 219.

science, the alternative is offered us, either germs can stand a greater heat than has been supposed, or the molecules of dead matter, for no valid or intelligible reason that is assigned, are able to rearrange themselves into living bodies, exactly such as can be demonstrated to be frequently produced in another way, I cannot understand how choice can be, even for a moment, doubtful.

But though I cannot express this conviction of mine too strongly, I must carefully guard myself against the supposition that I intend to suggest that no such thing as Abiogenesis ever has taken place in the past or ever will take place in the future. With organic chemistry, molecular physics, and physiology yet in their infancy, and every day making prodigious strides, I think it would be the height of presumption for any man to say that the conditions under which matter assumes the properties we call "vital" may not, some day, be artificially brought together. All I feel justified in affirming is that I see no reason for believing that the feat has been performed yet.

And looking back through the prodigious vista of the past, I find no record of the commencement of life, and therefore I am devoid of any means of forming a definite conclusion as to the conditions of its appearance. Belief, in the scientific sense of the word, is a serious matter, and needs strong foundations. To say, therefore, in the admitted absence of evidence, that I have any belief as to the mode in which the existing forms of life have originated, would be using words in a wrong sense. But expectation is permissible where belief is not; and if it were given me to look beyond the abyss of geologically recorded time to the still more remote period when the earth was passing through physical and chemical conditions, which it can no more see again than a man can recall his infancy, I should expect to be a witness of the evolution of living protoplasm from not living matter. I should expect to see it appear under forms of great simplicity, endowed, like existing fungi, with the power of determining the formation of new protoplasm, from such matters as ammonium carbonates, oxalates and tartrates, alkaline and earthy phosphates, and water, without the aid of light. That is the expectation to which analogical reasoning leads me; but I beg you once more to recollect that I have no right to call my opinion anything but an act of philosophical faith.

So much for the history of the progress of Redi's great doctrine of Biogenesis, which appears to me, with the limitations I have expressed, to be victorious along the whole line at the present day.

As regards the second problem offered to us by Redi, whether Xenogenesis obtains, side by side with Homogenesis; whether, that is, there exist not only the ordinary living things, giving rise to offspring which run through the same cycle as themselves, but also others, producing offspring which are of a totally different character from themselves, the researches of two centuries have led to a different result. That the grubs found in galls are no product of the plants on which the galls grow, but are the result of the introduction of the eggs of insects into the substance of these plants, was made out by Vallisnieri, Reaumur, and others, before the end of the first half of the eighteenth century. The tapeworms, bladderworms, and flukes continued to be a stronghold of the advocates of Xenogenesis for a much longer period. Indeed, it is only within the last thirty years that the splendid patience of Von Siebold, Van Beneden, Leuckart, Küchenmeister, and other helminthologists, has succeeded in tracing every such parasite, often through the strangest wanderings and metamorphoses, to an egg derived from a parent, actually or potentially like itself; and the tendency of inquiries elsewhere has all been in the same direction. A plant may throw off bulbs, but these, sooner or later, give rise to seeds or spores, which develop into the original form. A polyp may give rise to Medusæ, or a pluteus to an Echinoderm, but the Medusa and the Echinoderm give rise to eggs which produce polyps or plutei, and they are therefore only stages in the cycle of life of the species.

But if we turn to pathology, it offers us some remarkable approximations to true Xenogenesis.

As I have already mentioned, it has been known since the time of Vallisnieri and of Reaumur, that galls in plants, and tumors in cattle, are caused by insects, which lay their eggs in those parts of the animal or vegetable frame of which these morbid structures are outgrowths. Again, it is a matter of familiar experience to everybody that mere pressure on the skin will give rise to a corn. Now the gall, the tumor, and the corn are parts of the living body, which have become, to a certain degree, independent and distinct organisms. Under the influence of certain external conditions, elements of the body, which should have developed in due subordination to its general plan, set up for themselves and apply the nourishment which they receive to their own purposes.

From such innocent productions as corns and warts, there are all gradations to the serious tumors which, by their mere size and the mechanical obstruction they cause, destroy the organism out of which they are developed; while, finally, in those terrible structures known as cancers, the abnormal growth has acquired powers of reproduction and multiplication, and is only morphologically distinguishable from the parasite worm, the life of which is neither more nor less closely bound up with that of the infested organism.

If there were a kind of diseased structure, the histological elements of which were capable of maintaining a separate and independent existence out of the body, it seems to me that the shadowy boundary between morbid growth and Xenogenesis would be effaced. And I am inclined to think that the progress of discovery has almost brought us to this point already. I have been favored by Mr. Simon with an early copy of the last published of the valuable "Reports on the Public Health," which, in his capacity of their medical officer, he annually presents to the Lords of the Privy Council. The appendix to this report contains an introductory essay "On the Intimate Pathology of Contagion," by Dr. Burdon Sanderson, which is one of the clearest, most comprehensive, and well-reasoned discussions of a great question which has come under my notice for a long time. I refer you to it for details and for the authorities for the statements I am about to make.

You are familiar with what happens in vaccination. A minute cut is made in the skin, and an infinitesimal quantity of vaccine matter is inserted into the wound. Within a certain time a vesicle appears in the place of the wound, and the fluid which distends this vesicle is vaccine matter, in quantity a hundred or a thousandfold that which was originally inserted. Now what has taken place in the course of this operation? Has the vaccine matter, by its irritative property, produced a mere blister, the fluid of which has the same irritative property? Or does the vaccine matter contain living particles, which have grown and multiplied where they have been planted? The observations of M. Chauveau, extended and confirmed by Dr. Sanderson himself, appear to leave no doubt upon this head. Experiments, similar in principle to those of Helmholtz on fermentation and putrefaction, have proved that the active element in the vaccine lymph is non-diffusible, and consists of minute particles not exceeding $\frac{1}{20000}$ of an inch in diameter, which are made visible in the lymph by the microscope. Similar experiments have proved that two of the most destructive of epizootic diseases, sheep-pox and glanders, are also dependent for their existence and their propagation upon extremely small living solid particles, to which the title of *microzymes* is applied. An animal suffering under either of these terrible diseases is a source of infection and contagion to others, for precisely the same reason as a tub of fermenting beer is capable of propagating its fermentation by "infection," or "contagion," to fresh wort. In both cases it is the solid living particles which are efficient; the liquid in which they float, and at the expense of which they live, being altogether passive.

Now arises the question, are these microzymes the results of *Homogenesis*, or of *Xenogenesis*; are they capable, like the *To-*

rulæ of yeast, of arising only by the development of preëxisting germs; or may they be, like the constituents of a nutgall, the results of a modification and individualization of the tissues of the body in which they are found, resulting from the operation of certain conditions? Are they parasites in the zoological sense, or are they merely what Virchow has called "heterologous growth?" It is obvious that this question has the most profound importance, whether we look at it from a practical or from a theoretical point of view. A parasite may be stamped out by destroying its germs, but a pathological product can only be annihilated by removing the conditions which give rise to it.

It appears to me that this great problem will have to be solved for each zymotic disease separately, for analogy cuts two ways. I have dwelt upon the analogy of pathological modification, which is in favor of the xenogenetic origin of microzymes; but I must now speak of the equally strong analogies in favor of the origin of such pestiferous particles by the ordinary process of the generation of like from like.

It is, at present, a well-established fact that certain diseases, both of plants and of animals, which have all the characters of contagious and infectious epidemics, are caused by minute organisms. The smut of wheat is a well-known instance of such a disease, and it cannot be doubted that the grape-disease and the potato-disease fall under the same category. Among animals, insects are wonderfully liable to the ravages of contagious and infectious diseases caused by microscopic *Fungi*.

In autumn, it is not uncommon to see flies, motionless upon a window-pane, with a sort of magic circle, in white, drawn round them. On microscopic examination, the magic circle is found to consist of innumerable spores, which have been thrown off in all directions by a minute fungus called *Empusa muscæ*, the spore-forming filaments of which stand out like a pile of velvet from the body of the fly. These spore-forming filaments are connected with others which fill the interior of the fly's body like so much fine wool, having eaten away and destroyed the creature's viscera. This is the full-grown condition of the *Empusa*. If traced back to its earlier stages, in flies which are still active, and to all appearance healthy, it is found to exist in the form of minute corpuscles which float in the blood of the fly. These multiply and lengthen into filaments, at the expense of the fly's substance; and when they have at last killed the patient, they grow out of its body and give off spores. Healthy flies shut up with diseased ones catch this mortal disease and perish like the others. A most competent observer, M. Cohn, who studied the development of the *Empusa* in the fly very carefully, was utterly unable to discover in what manner the smallest germs of the *Empusa* got into the fly. The spores could not be made to give rise to such germs by cultivation; nor were such germs discoverable in the air, or in the food of the fly. It looked exceedingly like a case of Abiogenesis, or, at any rate, of Xenogenesis; and it is only quite recently that the real course of events has been made out. It has been ascertained, that when one of the spores falls upon the body of a fly, it begins to germinate and sends out a process which bores its way through the fly's skin; this, having reached the interior cavities of its body, gives off the minute floating corpuscles which are the earliest stage of the *Empusa.* The disease is "contagious," because a healthy fly coming in contact with a diseased one, from which the spore-bearing filaments protrude, is pretty sure to carry off a spore or two. It is "infectious" because the spores become scattered about all sorts of matter in the neighborhood of the slain flies.

The silkworm has long been known to be subject to a very fatal and infectious disease called the *Muscardine.* Audouin transmitted it by inoculation. This disease is entirely due to the development of a fungus, *Botrytis Bassiana,* in the body of the caterpiller; and its contagiousness and infectiousness are accounted for in the same way as those of the fly-disease. But of late years a still more serious epizoötic has appeared among the silkworms; and I may mention a few facts which will give you some conception of the gravity of the injury which it has inflicted on France alone.

The production of silk has been for centuries an important branch of industry in Southern France, and in the year 1853 it had attained such a magnitude that the annual produce of the French sericulture was estimated to amount to a tenth of that of the whole world, and represented a money-value of 117,000,000 of francs, or nearly five million sterling. What may be the sum which would represent the money-value of all the industries connected with the working up of the raw silk thus produced is more than I can pretend to estimate. Suffice it to say that the city of Lyons is built upon French silk as much as Manchester was upon American cotton before the civil war.

Silkworms are liable to many diseases; and even before 1853 a peculiar epizoötic, frequently accompanied by the appearance of dark spots upon the skin (whence the name of "Pébrine" which it has received), had been noted for its mortality. But in the years following 1853 this malady broke out with such extreme violence, that, in 1858, the silk-crop was reduced to a third of the amount which it had reached in 1853; and, up till within the last year or two, it has never attained half the yield of 1853. This means not only that the great number of people engaged in silk growing are some thirty millions sterling poorer than they might have been; it means not only that high prices have had to be paid for importing silkworm eggs, and that, after investing his money in them, in paying for mulberry-leaves and for attendance, the cultivator has constantly seen his silkworms perish and himself plunged in ruin ; but it means that the looms of Lyons have lacked employment, and that for years enforced idleness and misery have been the portion of a vast population which, in former days, was industrious and well to do.

In 1858 the gravity of the situation caused the French Academy of Sciences to appoint Commissioners, of whom a distinguished naturalist, M. de Quatrefages, was one to inquire into the nature of this disease, and, if possible, to devise some means of staying the plague. In reading the report* made by M. de Quatrefages in 1859, it is exceedingly interesting to observe that his elaborate study of the Pébrine forced the conviction upon his mind that, in its mode of occurrence and propagation, the disease of the silkworm is, in every respect, comparable to the cholera among mankind. But it differs from the cholera, and so far is a more formidable disease, in being hereditary, and in being under some circumstances, contagious as well as infectious.

The Italian naturalist, Filippi, discovered in the blood of the silkworms affected by the strange disease a multitude of cylindrical corpuscles, each about $\frac{1}{6000}$ of an inch long. These have been carefully studied by Lebert, and named by him *Pansistophyton ;* for the reason that in subjects in which the disease is strongly developed, the corpuscles swarm in every tissue and organ of the body, and even pass into the undeveloped eggs of the female moth. But are these corpuscles causes, or mere concomitants, of the disease? Some naturalists took one view and some another ; and it was not until the French Government, alarmed by the continued ravages of the malady, and the inefficiency of the remedies which had been suggested, dispatched M. Pasteur to study it, that the question received its final settlement ; at a great sacrifice, not only of the time and peace of mind of that eminent philosopher, but, I regret to have to add, of his health.†

But the sacrifice has not been in vain. It is now certain that this devastating, cholera-like Pébrine is the effect of the growth and mutiplication of the *Panhistophyton* in the silkworm. It is contagious and infections because the corpuscles of the *Panhistophyton* pass away from the bodies of the diseased caterpillars, directly or indirectly, to the alimentary canal of healthy silkworms in their neighborhood ; it is hereditary, because the corpuscles enter into the eggs while they are being formed, and consequently are carried within them when they are laid ; and

---

* Etudes sur les Maladies Actuelles des Vers à Soie, p. 53.

† In Nature No. xxxvi, p. 181, will be found a *résumé*, by Prof. Tyndall, of Pasteur's investigations of the silkworm disease.

for this reason, also, it presents the very singular peculiarity of being inherited only on the mother's side. There is not a single one of all the apparently capricious and unaccountable phenomena presented by the Pébrine, but has received its explanation from the fact that the disease is the result of the presence of the microscopic organism, *Panhistophyton.*

Such being the facts with respect to the Pébrine, what are the indications as to the method of preventing it? It is obvious that this depends upon the way in which the *Panhistophyton* is generated. If it may be generated by Abiogenesis, or by Xenogenesis, within the silkworm or its moth, the extirpation of the disease must depend upon the prevention of the occurrence of the conditions under which this generation takes place. But if, on the other hand, the *Panhistophyton* is an independent organism, which is no more generated by the silkworm than the mistletoe is generated by the oak or the apple-tree on which it grows, though it may need the silkworm for its development in the same way as the mistletoe needs the tree, then the indications are totally different. The sole thing to be done is to get rid of and keep away the germs of the *Panhistophyton.* As might be imagined, from the course of his previous investigations, M. Pasteur was led to believe that the latter was the right theory; and, guided by that theory, he has devised a method of extirpating the disease. which has proved to be completely successful wherever it has been properly carried out.

There can be no reason, then, for doubting that, among insects, contagious and infectious diseases, of great malignity, are caused by minute organisms which are produced from preëxisting germs, or by homogenesis; and there is no reason, that I know of, for believing that what happens in insects may not take place in the highest animals. Indeed, there is already strong evidence that some diseases of an extremely malignant and fatal character to which man is subject, are as much the work of minute organisms as is the Pébrine. I refer for evidence of this to the very striking facts adduced by Professor Lister in his various well-known publications on the antiseptic method of treatment. It seems to me impossible to rise from the perusal of those publications without a strong conviction that the lamentable mortality which so frequently dogs the footsteps of the most skillful operator, and those deadly consequences of wounds and injuries which seem to haunt the very walls of great hospitals, and are, even now, destroying more men than die of bullet or bayonet, are due to the importation of minute organisms into wounds, and their increase and multiplication; and that the surgeon who saves most lives will be he who best works out the practical consequences of the hypothesis of Redi.

I commenced this Address by asking you to follow me in an attempt to trace the path which has been followed by a scientific idea, in its long and slow progress from the position of a probable hypothesis to that of an established law of nature. Our survey has not taken us into very attractive regions; it has lain, chiefly, in a land flowing with the abominable, and peopled with mere grubs and mouldiness. And it may be imagined with what smiles and shrugs, practical and serious contemporaries of Redi and of Spallanzani may have commented on the waste of their high abilities in toiling at the solution of problems which, though curious enough in themselves, could be of no conceivable utility to mankind. Nevertheless you will have observed that before we had traveled very far upon our road there appeared, on the right hand and on the left, fields laden with a harvest of golden grain, immediately convertible into those things which the most sordidly practical of men will admit to have value, viz: money and life.

The direct loss to France caused by the Pébrine in seventeen years cannot be estimated at less than fifty millions sterling; and if we add to this what Redi's idea, in Pasteur's hands, has done for the wine-grower and for the vinegar-maker, and try to capitalise its value, we shall find that it will go a long way towards repairing the money losses caused by the frightful and calamitous war of this autumn. And as to the equivalent of Redi's thought in life, how can we over-estimate the value of that knowledge of the nature of epidemic and epizoötic diseases, and consequently of the means of checking, or eradicating, them, the dawn of which has assuredly commenced?

Looking back no further than ten years, it is possible to select three (1863, 1864, and 1869) in which the total number of deaths from scarlet-fever alone amounted to ninety thousand. That is the return of killed, the maimed and disabled being left out of sight. Why, it is to be hoped that the list of killed in the present bloodiest of all wars will not amount to more than this! But the facts which I have placed before you must leave the least sanguine without a doubt that the nature and the causes of this scourge will, one day, be as well understood as those of the Pébrine are now; and that the long-suffered massacre of our innocents will come to an end.

And thus mankind will have one more admonition that "the people perish for lack of knowledge;" and that the alleviation of the miseries, and the promotion of the welfare, of men must be sought, by those who will not lose their pains, in that diligent, patient, loving study of all the multitudinous aspects of Nature, the results of which constitute exact knowledge, or Science. It is the justification and the glory of this great meeting that it is gathered together for no other object than the advancement of the moiety of science which deals with those phenomena of nature which we call physical. May its endeavors be crowned with a full measure of success.

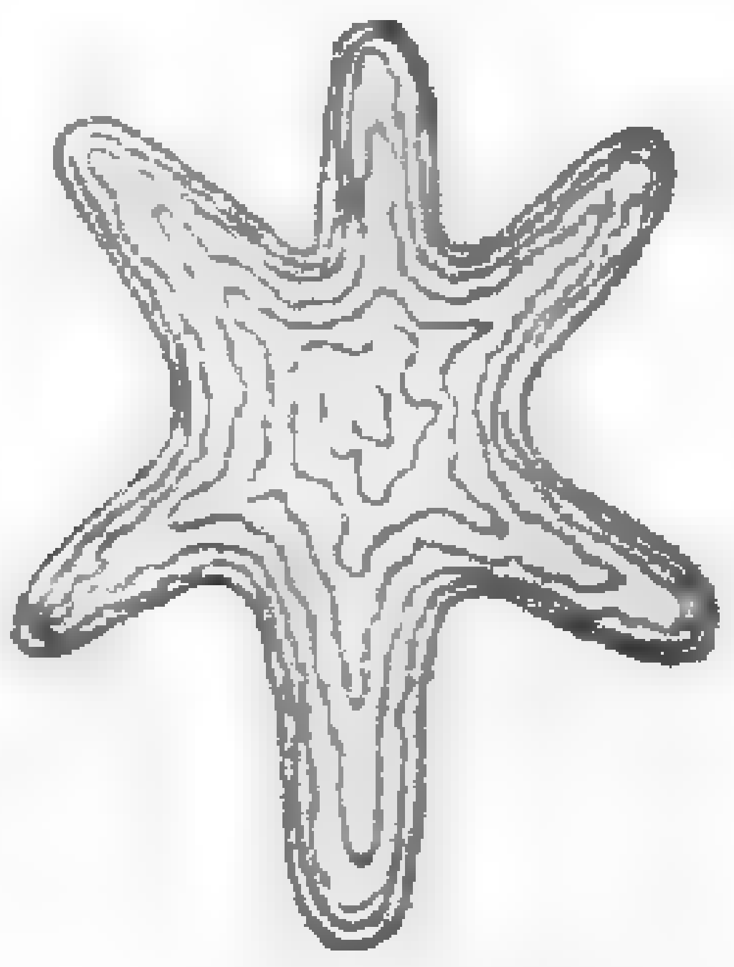
ART. XLIV. — *The Hail-storm of June 20th*, 1870; by Rev. HORACE C. HOVEY, M.A.

THIS remarkable storm swept along a path about thirty miles wide, and extending from Troy, N. Y., to Bangor, Me., though it was not everywhere accompanied by hail.

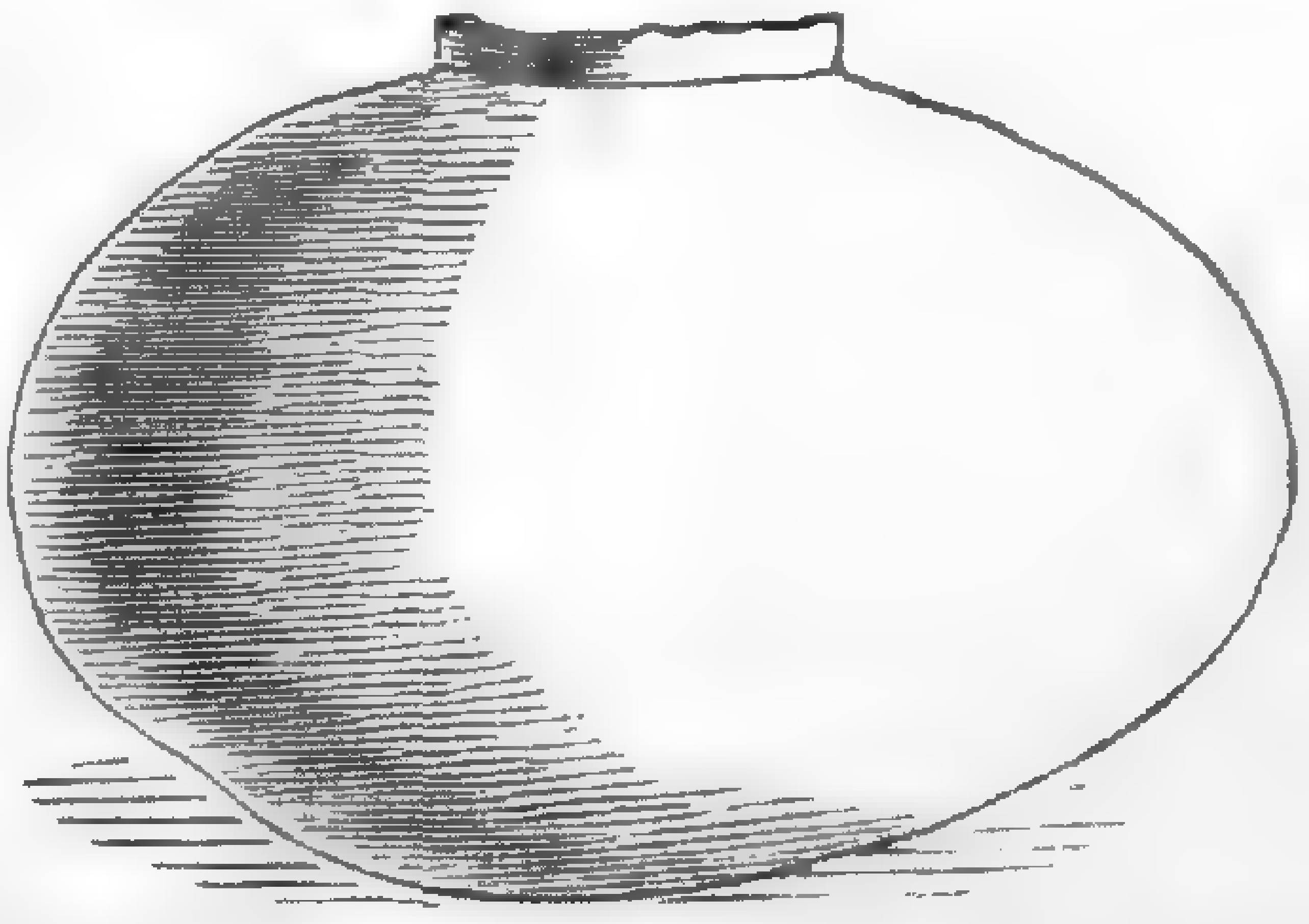
My point of observation was in Northampton, Mass., which was in the central line of the storm.

At sunrise the atmosphere was obscured by fog, which was partially dispersed at a later hour. The day was sultry. At noon the thermometer indicated 88° in the shade. At 3 P. M. a vast mass of dark-green cloud rolled up from the N. W., while lateral currents seemed to set in, forcing the clouds at first into confusion, but afterwards into a well-defined vortex, or spout. The electrical detonations were frequent and sharp. No rain preceded the hail, though it fell copiously after a few minutes. The first hail-stones were about one inch in diameter, and seemed to fall from a greater height, and with more force, than those that fell subsequently. The latter were probably nearer the center of the vortex, and so had their downward motion restrained by that which was lateral. The first that fell were, most of them, on striking the ground, instantly buried out of sight. If they struck on a rocky surface they were dashed in pieces, or else rebounded to a considerable height in in the air. Had their larger successors been driven by a corresponding force, nothing could have survived their assault. The smaller hail-stones were generally flattened spheres, though sometimes in rude stellar forms, (fig. 1). But the largest ones were symmetrical ovoids; each being surmounted, however, by a roughened crown, (fig. 2). The dimensions and weight of three specimens are given, with such accuracy as could be secured by the means at hand. These are but samples of thousands that fell till the earth was covered with ice. The first was, in long diameter, 3⅜ inches; short diameter, 2⅛ inches; weight 7 ounces. The second was 3½ inches by 2¼; weight 8 ounces. The third was 4 inches by 2⅜; weight 10 ounces.

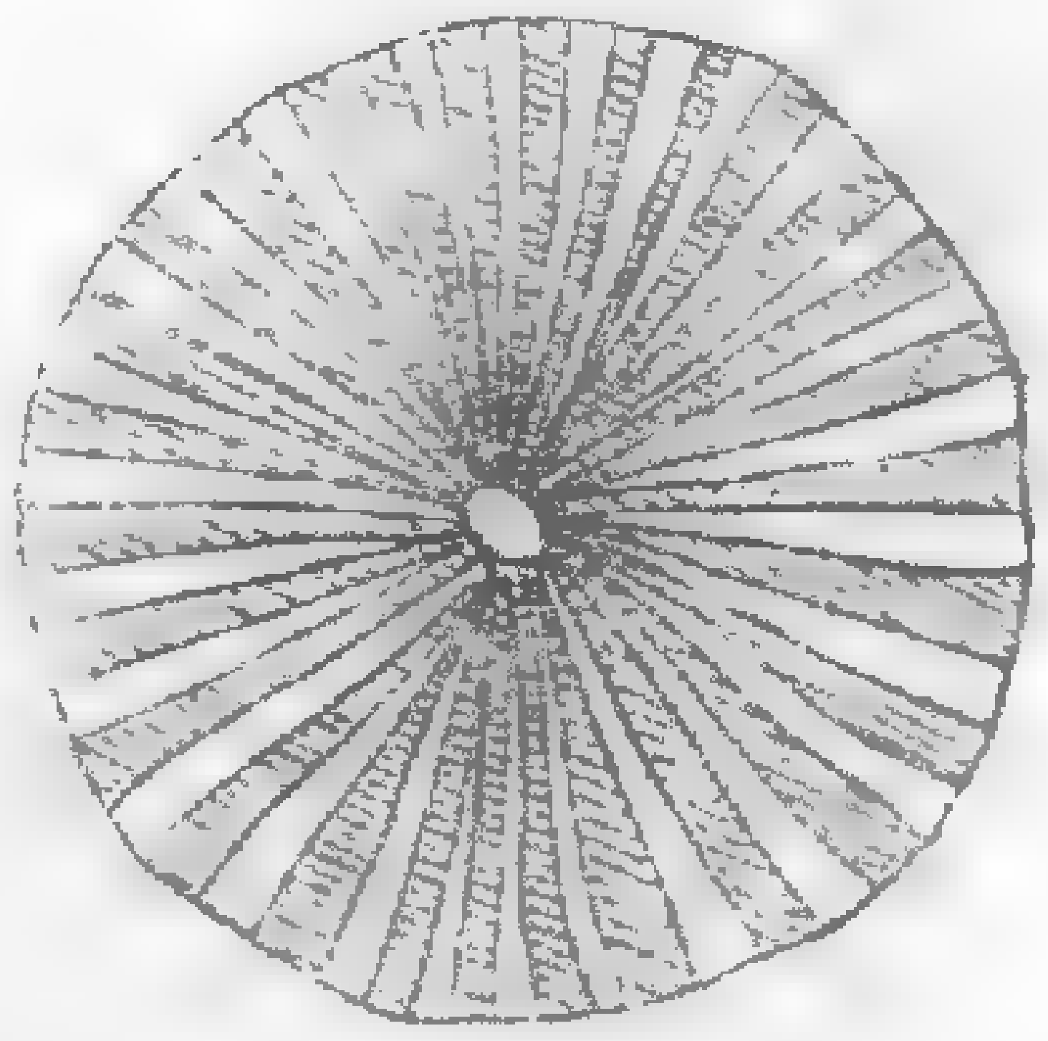
1.



2. 3½″ × 2¼″.

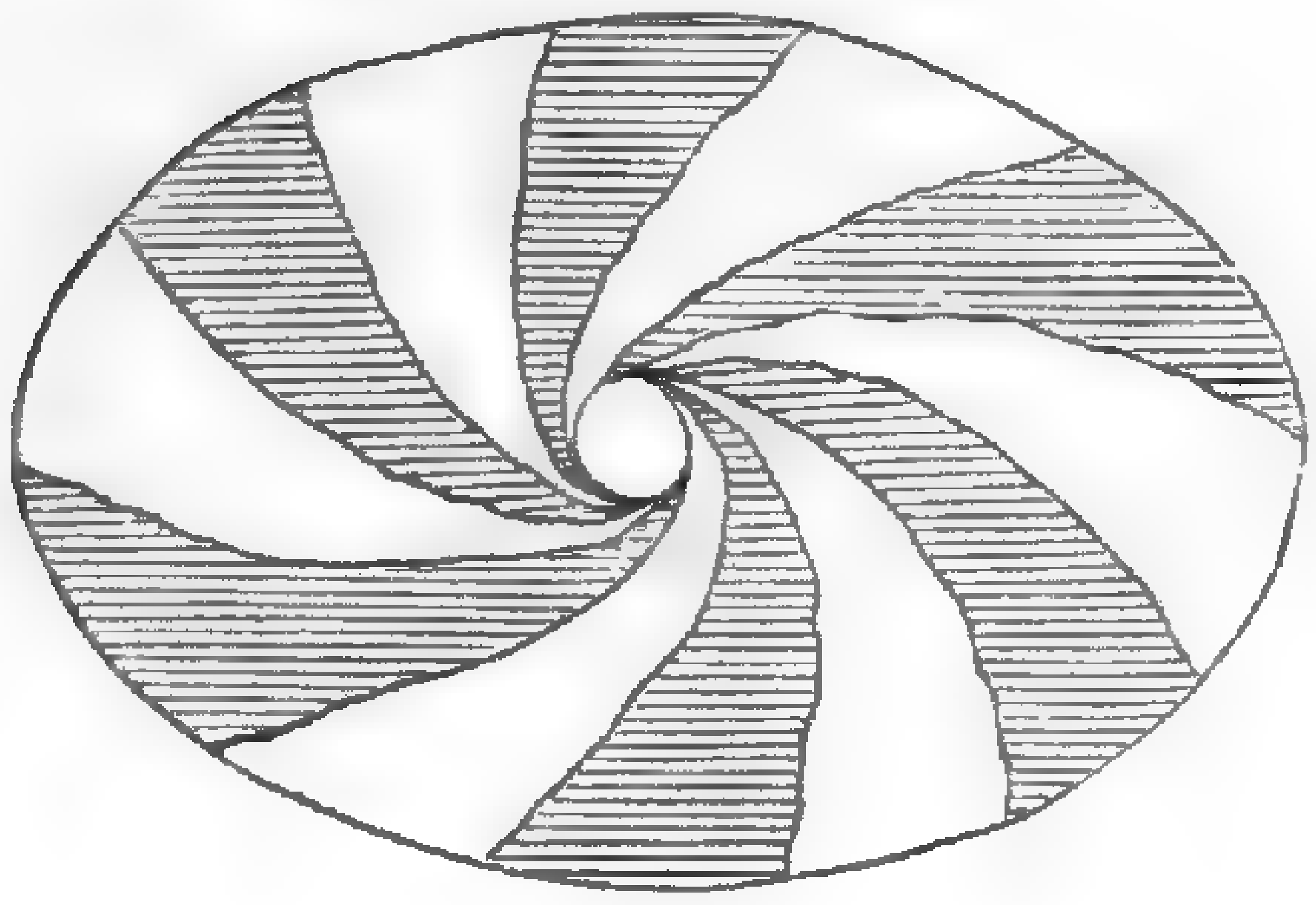


3. 2″ diam.

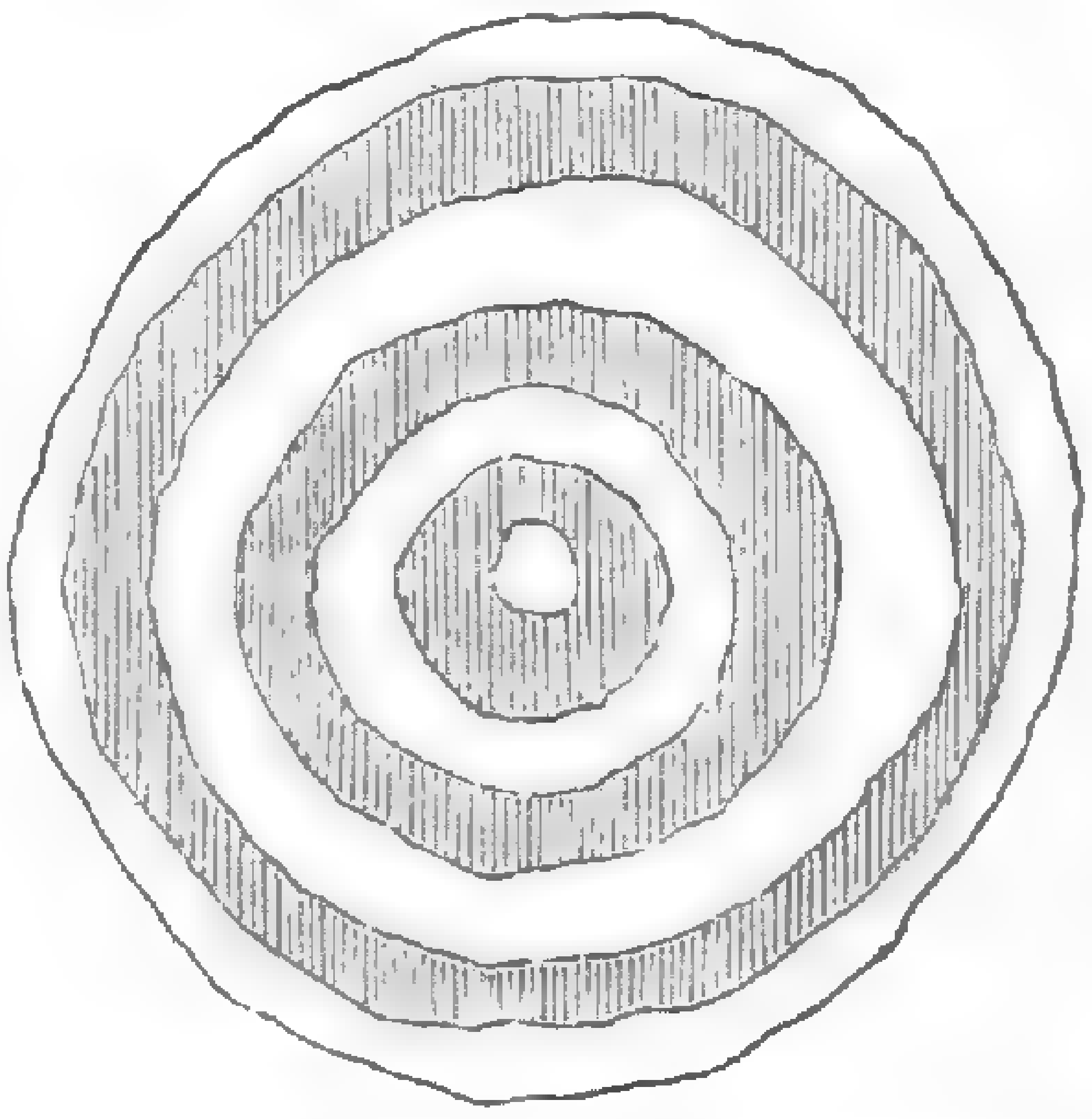


This monster, a foot in circumference, did not entirely melt away for six hours after it fell! The ice in all the hail-stones was peculiarly hard and compact. Interesting structural peculiarities were noted. Hail-stones of stellar form were always transparent and homogeneous. The spheroids were covered with an opaque coating, and had likewise an opaque center. On being bisected some of them showed a radiated structure, the alternate rays being white and clear, (fig. 3). The largest hail-stones had an axis of white ice, half an inch in diameter, around which the alternate layers were arranged in spiral convolutions, (fig. 4). The most common form was in concentric layers, like the coats of an onion, still alternating opaque and transparent; but the edges were finely serrated, like the stripes in some species of agate, (fig. 5). In one hailstone I counted thirteen of these layers, indicating that it had passed through as many strata of snowy and vaporous cloud.

4. 3½″ × 2¼″.



5. 3″ diam.



After a lull in the storm, for half an hour, there was a second fall of hail, but much lighter than the first.

The damage done by such a war of the elements cannot easily be ascertained. Vegetation suffered greatly. In some cases men and animals were wounded. The icy missles not only broke thousands of pains of glass, but also in many instances the window-blinds and sash. In a few cases weather-worn house-roofs were pierced.

Peoria, Ill., July 25, 1870.

Art. XLV.—*Photograph of a Solar Prominence*; by Prof. C. A. Young, of Dartmouth College.

The following is from a letter to the editors dated Sept. 28th, 1870:—

I have just succeeded, with the help of our skillful artist Mr. H. O. Bly, in obtaining a photograph of one of the solar prominences, a copy of which I enclose. It was taken through the hydrogen line, near G, by opening the slit of the spectroscope and attaching a small camera to its eye-piece. As a *picture* of course it amounts to very little. It required an exposure of three minutes and a half, and the polar axis of the telescope being imperfectly adjusted, the clock-work failed to follow perfectly, so that no detail is visible, and the picture will not bear much magnifying. I am convinced, however, that by using a more sensitive collodion, and taking proper pains with the adjustment of the instrument, satisfactory photographs of these curious objects may be obtained.

I may add that the spectroscope employed has the dispersive power of 13 prisms of flint, each with an angle of 55°.

With it I observed this afternoon in the spectrum of a spot, the reversal of the following lines, viz: C, D_1, D_2, D_3, 1474 K, (very faint), b_1, b_2, b_3, b_4, F, 2796 K, (Hγ), and *h* (Hδ). b_3 was most conspicuous after C_1, D_3, and F.

ART. XLVI.—*Contributions to Zoölogy from the Museum of Yale College.* No. 8.—*Descriptions of some New England Nudibranchiata;* by A. E. VERRILL.

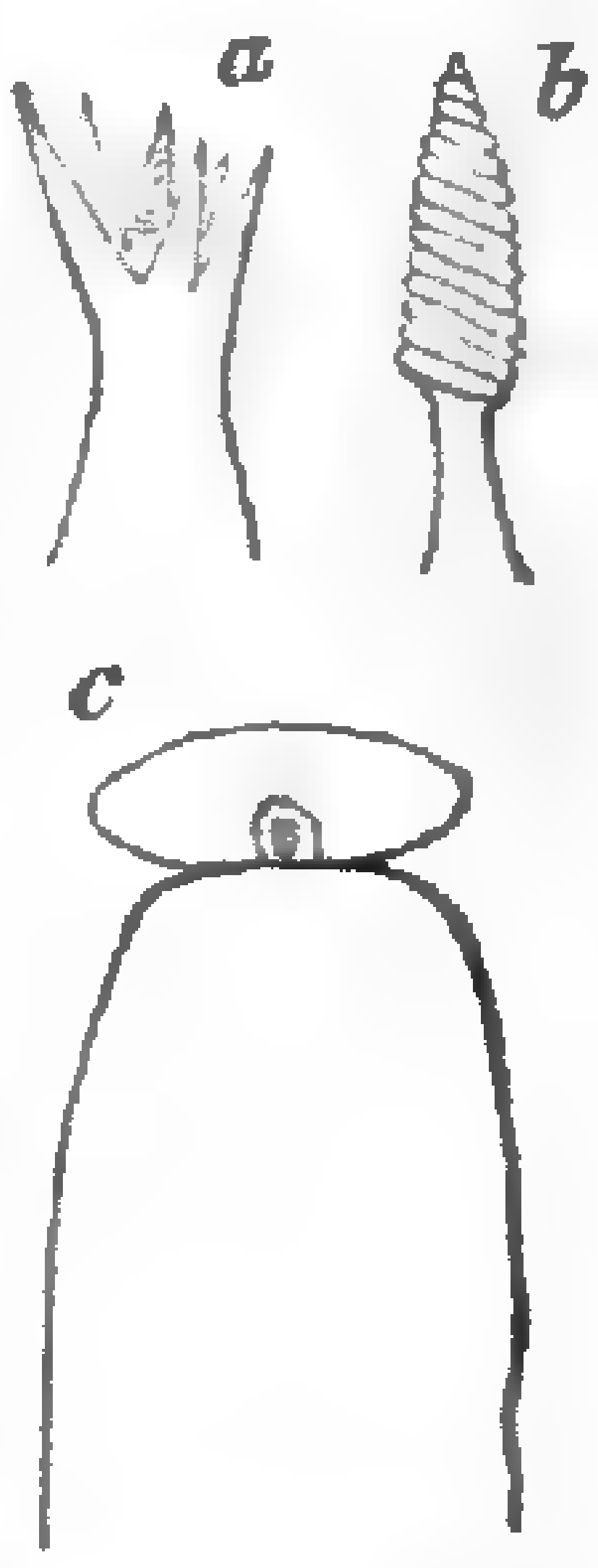
DURING a dredging expedition to Eastport, Me., and Grand Menan, the past season, in company with Mr. Oscar Harger and C. H. Dwinelle, students in the Sheffield Scientific School, the following very interesting species was obtained. Many other Nudibranchs were also observed, most of which are well known species.

Dendronotus robustus, sp. nov. Figure 1.

Body stout, about 2 inches long; ·5 broad, and about the same in height, somewhat quadrangular, tapering posteriorly, but much less acute than in *D. arborescens*, as well as much stouter throughout. Branchiæ in about six pairs, those of the three first pairs with a supplementary one of nearly the same size arising separately outside of, but close to their bases; on the fourth pair these originate from the base as large branches, and on the following ones they are more distinctly branches, arising from the sides near the bases of the branchiæ. The branchiæ are diffusely arborescent and very much subdivided, the divisions taking place very rapidly, the branches being more equal in length and more spreading than in *D. arborescens*, and do not have the long, slender and acute main branches seen in that species. The sheaths of the tentacles (figure 1, *a*) are round and stout, about ·4 of an inch long and ·12 in diameter, and are destitute of any lateral branches; they divide at top into five simple, round, smooth, tapering, acute divisions, of which the two posterior ones are longest. The tentacles (figure

1, *b*) are about equal in length to the lobes of the sheath, the pedicle forming about half of the visible part; the terminal portion suddenly enlarges at first, becomes somewhat conical, and tapers to an obtuse point; it has ten or twelve oblique plications. Front of head with numerous (about thirty) sparingly branched appendages arranged in two series. In the upper series there are about ten, the outer ones being largest; these have stout stems with a few conical, tapering branches, mostly on the lower side, which are tipped with sulphur-yellow. Below these are numerous unequal, smaller, and more simple appendages, about ten on each side, part of which are forked at the end, while others are simple and papilliform and surround the expanded oral disk; all are tipped with yellow. The oral disk (figure 1, *c*) is tranversely elliptical. The foot is nearly as broad as the body (·4 of an inch), and can be adapted for clasping by infolding the edges.

1.



Color pale grayish, thickly sprinkled with small yellow spots, which become less numerous on the oral appendages and sheaths of the tentacles.

Whale Cove, Grand Menan, on sea-weeds in a pool near low-water mark. One specimen only, found by Mr. Oscar Harger.

Dendronotus arborescens Alder and Hancock (*D. Reynoldsii* Couthouy) differs widely from this species in having a very narrow foot; an elevated compressed body, which is more slender and more acute behind; a much smaller number (about ten or twelve) appendages in front of the head, of which the six upper ones are larger and much more branched, and the four lower ones very small; the gills longer and the branches more unequal, while the lowest branch on the outside arises from the side, above the base, even on the front pairs; and in having more clavate tentacles, with longer and branched lobes to their sheaths, while the sheaths also have a large, arborescent, gill-like branch originating from the outer side toward the base. By the last character alcoholic specimens can easily be distinguished. Both species occurred together in the same pool.

Doris bifida, sp. nov.

Outline broad oval, widest anteriorly, 1 inch long by ·5 broad, in extension, back very convex, mantle covered with numerous, scattered, prominent, pointed papillæ. Tentacles rather long, thickest in the middle, the outer half strongly plicated, but with a smooth tip, the base surrounded by small papillæ. Gills retractile into a single cavity, united together by a partial web,

Figure 1.—*Dendronotus robustus* Verrill; *a*, tentacle sheath, natural size; *b*, tentacle, enlarged; *c*, oral disk and anterior part of the foot, natural size; from living specimen by A. E. Verrill.

deeply frilled, much subdivided, bipinnate, the subdivisions fine and slender. Foot very broad, in extension projecting back beyond the mantle about a quarter of an inch, slightly tapering, rounded and slightly notched at the end. Oral disk or veil crescent-shaped, the front a little prominent, the sides extended backward and forming a curve continuous with that of the foot.

Color dark purplish brown, sprinkled with white specks; tentacles deep brown, specked with white, tips yellowish; gills purplish at base, the edges and tips yellow; foot similar in color to mantle, but lighter.

Eastport, Me., at low-water mark, under stones, Aug. 19, 1868.

Onchidoris tenella.

Doris tenella Ag., in Gould, Invert. of Mass., Ed. 2, p. 229, Pl. xx, figures 289, 290, 293, 1870.

Specimens of this rare and imperfectly known species were obtained under stones in a large pool at low-water mark, near Eastport, Me.

The largest one was ·35 of an inch long, and ·20 to ·25 broad, according to the position. The outline is oval, elliptical, or oblong, in different states of extension, and the edges of the mantle are often rolled inward. The back is strongly convex, the surface thickly covered with small conical papillæ, which are strengthened by numerous minute white spicula. The tentacles are rather long, oblong, scarcely tapering, with numerous transverse laminæ, which cover nearly the whole length, the tip with a small obtuse papilla; the base is surrounded by a short sheath, with the edge divided into five small conical papillæ or teeth, the two anterior ones largest. Branchiæ nine, seven principal ones with two very small ones posteriorly; the larger ones are short, thick, lanceolate, with short lateral lobes. In the center of the branchial circle there is a small brownish papilla. The foot is long-oval, tapering behind and rounded in front, about half as wide as the mantle, and very much shorter. The oral disk is short and broad, subtriangular, with a very obtuse angle in front.

Color of the upper surface yellowish white, the papillæ mostly tipped with yellow, but some with flake-white; tentacles lemon-yellow with lighter tips; branchiæ yellowish white, edged and tipped with lemon-yellow, the yellow tint conspicuous in partial contraction; foot yellowish white; mouth and edge of oral disk bright yellow.

Onchidoris grisea.

Doris grisea Stimpson MS., in Gould, op. cit., p. 232, Pl. xx, figures 292, 295.

This species occurred under the same circumstances as the preceding, and more commonly. The color was generally clear white, sometimes tinged with pale sulphur-yellow, in some parts.

Onchidoris pallida.

Doris pallida Ag., in Gould, op. cit., p. 229, Pl. xx, figs. 284, 287, 288, 291.

This species was dredged in 20 fathoms in Eastport harbor. It has much larger tubercles than either of the preceding.

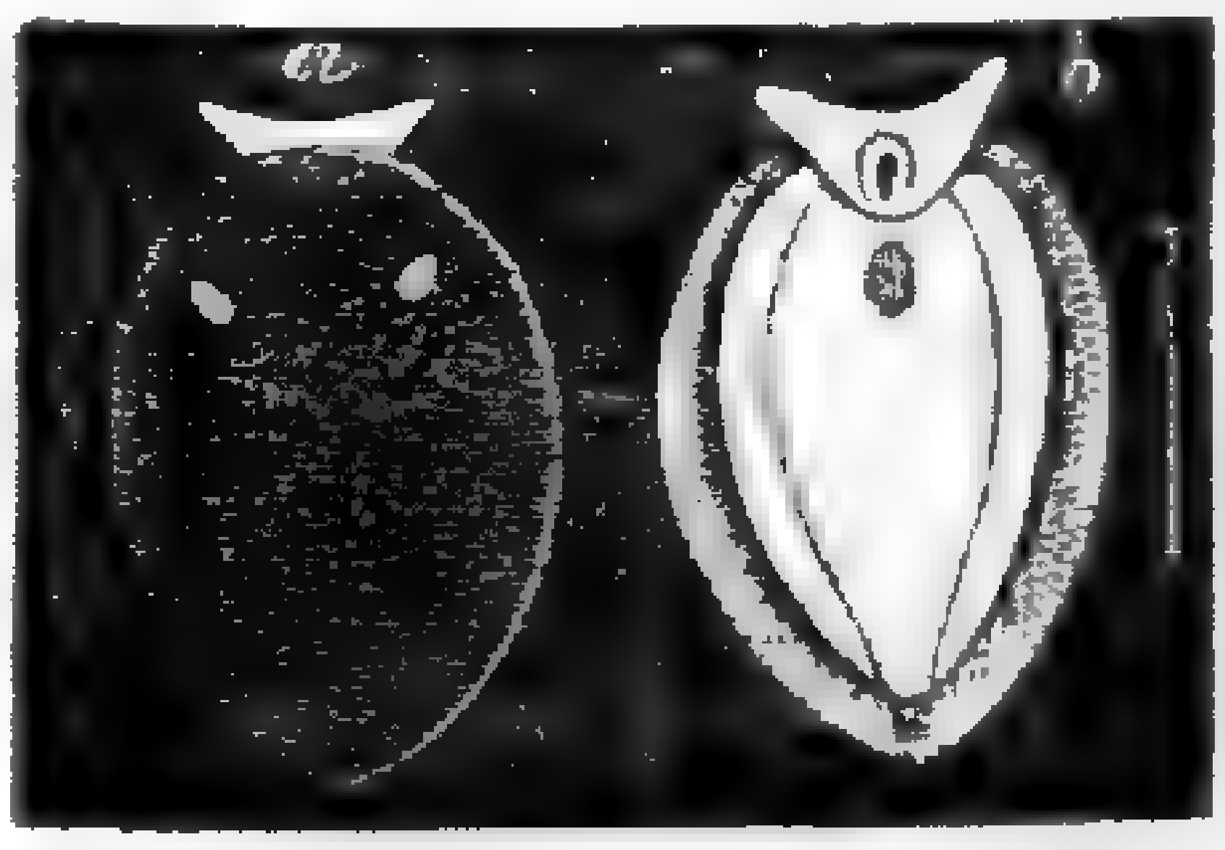
Doridella, gen. nov.

Body covered with an ample, smooth mantle, oval, convex. Dorsal tentacles retractile, without sheaths. Head prominent, the lateral angles prolonged anteriorly as short oral palpi or tentacles. Foot broad, cordate. Branchiæ posterior, in the groove between the mantle and foot.

Doridella obscura, sp. nov. Figures 2 and 3.

Form broad oval, ·3 of an inch long and ·2 broad; back convex, smooth. Foot broad, cordate in front. Oral disk broad, emarginate or with concave outline in front; The angles somewhat produced, forming short, tentacle-like organs, which in extension project beyond the front edge of the mantle. Dorsal tentacles small, stout, retractile. Color of body blackish, lighter toward the edge, as if covered with nearly confluent black spots, the whitish ground color showing between them; foot, oral disk, and dorsal tentacles white; the central part of body, beneath, bright yellow.

2.



Savin Rock, near New Haven, Oct. 28, 1868,—E. T. Nelson.

The eggs, laid in confinement, were very small, pale yellow, numerous, arranged in an open coil, (figure 3).

This is the only Nudibranch hitherto discovered in the vicinity of New Haven. It appears to be allied to *Phyllidia* and *Fryeria*, which are usually referred to the Tectibranchs.

3.

ART. XLVII.—*On a recent Earthquake at Bogota*; by the Hon. S. A. HURLBUT, U. S. Minister to Columbia.

[The following communication respecting an earthquake recently felt at Bogota, observed by the Hon. S. A. Hurlbut, U. S. Minister to Columbia, has been kindly furnished to us by the Secretary of the Smithsonian Institution, to whom it was addressed.]

WE have had rather an unusual phenomenon at this place in a remarkably well developed earthquake. At about 10 minutes before 10 P. M. of the evening of the 4th of April (Saturday), and without any previous warning that we had noticed, there

Figure 2.—*Doridella obscura* Verrill, enlarged two diameters; *a*, upper surface; *b*, lower surface.

Figure 3.—Eggs of *D. obscura*, enlarged two diameters.

occurred first, a moderate shock not of any peculiar force and consisting of a single vibration ; this was momentary. In about two minutes afterward a very sharp movement took place, giving the impression of a lateral motion from north to south. The table on which I had my elbow at the time seemed to recede about 1½ to 2 inches, quiver an instant and return to its place: the beams of the houses creaked like the timbers of a ship in heavy weather. Doors and windows flew open. Those who were in bed at the time seemed to feel it much more, and the effect of the vibrations was to make many "sea sick." This shock, they tell me, was the sharpest known here since 1826. I cannot learn of any damage done to buildings in the city.

The unquietness of the earth continued from the time mentioned until nearly 11 P. M., with a species of shuddering motion scarcely perceptible unless one were lying down. There was heard with each shock, a peculiar muffled rushing sound, not as clear and distinct as the movement of wind, but something like it. At the moment of the principal shock I looked at my watch and found the time to be ten minutes of ten—Bogota time. Time however, here, is not well regulated, as the observatory possesses no instruments and is neglected. The direction of the movement was very distinct from the north to the south. As earthquakes rarely have their centers in Columbia and are generally the result of action in Ecuador, it may be advisable to connect this observation with notices from that country. I believe there is but one volcano in action in Colombia-Puracè.

Some nights since we noticed for two hours after sunset in the west, and nearly in the range of Tolima, a well defined column or line of light, on the Cordillera. This bore about due west. The character of the light I could not determine.

Legation of the United States, Bogota, June 6, 1870.

Art. XLVIII.—*Discovery of a new Planet, the* 112*th, named Iphigenia;* by Dr. C. H. F. Peters, of the Litchfield Observatory of Hamilton College. Letter to the Editors, dated Clinton, Oneida Co., N. Y., Sept. 22, 1870.

I have the pleasure to communicate the following observations upon an asteroid discovered on the night of the 19th inst.

1870	Ham. Coll. m.t.	App. A.R.	App. Decl.	
	h m	h m s	° ′	
Sept 19.	15 30 —	1 2 35	+10 16 —	(approx. by estimation).
" 20.	14 57 28	1 1 51·54	+10 13 30·8	10 comp. W. 0h 1079.
" 21.	10 48 22	1 1 12·24	+10 10 58·9	10 comp. Sohj. 374.

The planet is about 11th magnitude, receives the number (112), and I have already given a name to it, *Iphigenia*, while that found on Aug. 14, (111), has been called *Ate*, with regard to the simultaneous events in Europe.

Art. XLIX.—*Geological Explorations in China;* by Baron von Richthofen. In a letter to Prof. J. D. Whitney, dated Peking, Aug. 20th, 1869, and communicated by him for this Journal.

I promised you, a few days ago, a more detailed account of the geological results of my travels in Manchuria and the province of Chi-li than I wished to give before having visited at least some of the localities near Peking examined before by Pumpelly. I have done this and am now acquainted with the most important formations occurring in the neighborhood of this capital.

The southern province of Manchuria has the name Shing-King and is divided by the Liao river into Liao-tung and Liao-hsi (meaning East and West of the Liao). The course of my travels from May to July was as follows: from Chifu by sea to Nin-chang, at the mouth of the Liao; thence by land down the western and up the southeastern coast of Liao-tung, to the frontier of Corea, then to the northeast, along this frontier, and to Mukden; from Mukden I went to Peking by Kin-chan and Yung-ping.

There are certain circumstances which render the geology of northern China difficult. In the first place the extent of the country is very great, and the character of the formations changes no less in the different provinces of it than it does in other regions of the globe. One is easily inclined, on a hasty tour of reconnoissance, to compare the strata in various regions on lithological grounds. But if I bear in mind the erroneous conclusions arrived at in the European Alps, by geologists who endeavored to determine the age of the sedimentary formations on the strength of their lithological resemblance to the formations of other parts of Europe, I think I cannot be cautious enough in this new country. Lithological analogy can here be used as a safe guide only when carefully traced from province to province and so on to remote regions. Then there is the apparent absence of any great geological events creating disturbances simultaneously over the whole region. Ancient deposition continued in one place while it was interrupted in another by the dislocation of those strata previously deposited. Another difficulty is the scarcity of fossils. I dare say that, with the exception of a few plants of the Coal-measures, I have discovered all localities of fossils now known to exist. Yet, if it is considered that I never received any knowledge of their existence (excepting Lake Tai-hu) not even the slightest hint, from either native or foreigner, but that I had to discover every fossil myself in hurrying through the country, it will still be found surprising, that the number of known fossiliferous localities is so great. I believe that China will, on a closer examination, contribute largely to the knowledge of the most ancient animal life on the globe. There is another difficulty caused by the recurrence, at different levels, of strata which bear a close similarity to each other. This relates chiefly to certain quartzose sandstones of a reddish or yellow color when converted into quartzite. They have often a thickness of thousands of feet, and, as they are less affected by denudation than many other stratified rocks, they frequently compose mountain ranges for themselves, without offering any clue for determining their stratigraphical position. This is also true, though in a far less degree, of the limestones. In regard to these, as also to the knowledge of the occurrence of fossils, the analogy with the history of the exploration of the European Alps is striking. One cannot be astonished that the first explorer of Chinese geology, though a most admirable observer, should have distinguished only one great limestone formation in China, and referred it all to that age (namely, the Devonian) to which all the fossils then known (namely, those which are sold in drug stores) were believed to belong; just as "the Alpine limestone" was until not very long ago a comprehensive term used to designate all the limestones occurring at different geological levels in the Alps. You may recollect that already my observations on the Yang-tse induced me to distinguish there at least three limestone formations. Since then I have come to the conclusion that it will not be difficult, on detailed examination, to establish in other regions a greater number at distinct levels.

It is for the various reasons mentioned, that I examined every mountainous country independently from what I had seen before, applying new terms for the different formations observed, and tried to establish the analogy between different regions only after having completed the exploration of each. The series of formations, as established on the Yang-tse, has found thereby a great deal of additional support and, I think, will prove to be a near approximation to the true order of succession.

In my two previous letters (March 1st and May 8th) I mentioned my having found Carboniferous fossils on the Yang-tse and in Shantung, in certain limestones and shales which occur there in close connection with the Coal-measures, or indeed form part of them. They mark there a very distinct and remarkable horizon. I did not find any corresponding fossils in Manchuria or Chili. There, however, I was fortunate enough to collect sufficient material for determining the age of a formation which corresponds, stratigraphically and lithologically, with the lowest ("Matsu") limestone on the Yang-tse and the formation mentioned as No. 2 of Shantung in my letter of May 8th. It has a thickness of many thousand feet, and is, lithologically, exceedingly varied. It consists of endless alternations of red micaceous argyllite and brown sandstone, with strata of limestone, the former two prevailing in the lower half, the latter in the upper portion. The limestone itself presents a large number of varieties, and one can distinguish, on purely lithological grounds, several horizons, the order of succession of which is similar in wide regions. One of these horizons is marked by black oölitic limestone, which, together with some other conspicuous varieties of limestone, extends from the Yang-tse to Liao-tung and to Peking (twelve degrees of latitude and ten of longitude).

On the frontier of Corea, this oolitic limestone abounds in the remains of Trilobites and small brachiopods, chiefly of the Orthis and Lingula families. The sediments were there deposited in inlets of the ancient sea between ridges consisting chiefly of gneiss, granite and quartzite. The accumulation of the shells of trilobites which took place in these protected abodes is astonishing. I collected quite a number of species, but nothing adequate to the material which I saw, as several circumstances, chiefly the danger of being cut off by torrents swelled by the copious rains of the season, made it necessary to hurry over the most prolific localities. Yet, I hope that it will now be possible to determine the age of one of the most important formations of Northern China. It predominates, in bulk, over all other sedimentary formations of Lio-tung and extends thence into Corea. In Liao-tsi, it appears to have been removed by denudation to a great extent. It appears only in places, and even in these it is often covered by porphyries. Again it takes a dominent part in the structure of the country in the province of Chi-li, chiefly between Yung-ping-pu and Peking, becoming, however, more and more metamorphosed with the approach to the latter city. Its limestones (that is, its upper portion) form the narrows which lead to the Nankan pass. They are intensely altered, and traversed by innumerable dykes of intrusive rocks. What Pumpelly describes as the Hwaingan beds, are probably the lower strata of this same great formation.

In Liao-tung, as in Shan-tung, the oolitic limestones carrying the trilobites, are overlain by a great thickness of limestone, which is immediately followed by the Coal-measures. In Liao-tung, I found no fossils in this limestone, excepting numerous Ammonites and Orthoceratites, which cannot be determined. They occur immediately below the coal-bearing strata, the conformable superposition of which on the limestone I observed in a number of places. There are localities, such as northwestern Shan-tung and southern Liao-tung, where there is an apparently uninterrupted series of sedimentary deposits, commencing with sandstones and shales thousands of feet lower than the oolitic limestone and ending with the Coal-measures. Every layer appears to be conformable to that which it overlies, but no such parallelism exists between the lower and upper portions of the series, a gradual change of inclination marking the former gradual changes of level. It is not improbable that in these localities the entire series of formations, from the Silurian to the Coal-measures, is represented. It is in these regions that the order of succession will have to be studied. There are, however, more numerous instances where the outbreaks first of granitic and then of porphyric rocks, which extended evidently over long periods, have created repeated disturbances. The vicinity of Peking has been among the theaters of the most intense eruptive action. But even here the Coal-measures are conformable to the underlying limestones, in all instances which came under my observation.

I am unable to pronounce at present an opinion on the relation of the northern Coal-measures to those of Shan-tung and the lower Yang-tse. The remains of plants which I collected in nearly every one of the northern coal-fields will probably help to elucidate this question. But, judging on mere stratigraphical grounds, I cannot help thinking, that the Coal-measures of Liao-tung and Liao-hsi, and the lower portion of those in the neighborhood of Peking, will not differ much in age, either among themselves, or as compared with the Coal-measures of Middle China, It is, however, a noteworthy fact, that the coal formation of Peking has an extraordinary development. I had only occasion to see those coal-bearing strata which lie immediately on the limestone. West of Peking, these strata, together with the limestone and a few thousand feet of superincumbent deposits, have undergone an intense metamorphism, the coal being converted into anthracite. It appears, from Mr. Pumpelly's description, that the Chaitung Coal-measures (which I did not visit) occupy a, geologically, much higher level. Altogether it may be safe to conclude, that in China, as in other countries, the deposition of coal and intervening sediments continued during a considerable period, in which it shifted to different regions. Yet, I must confess, that comparison on stratigraphical grounds makes it difficult to believe that any portion of the Coal-measures of Northern China should be of so recent age as Dr. Newberry was inclined to conclude on the strength of the vegetable remains which he determined.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the influence of electricity on air and oxygen as a means of producing ozone.*—Houzeau has drawn the following conclusions from a great number of estimations of ozone obtained by means of Ruhmkorff's apparatus.

(1.) The production of ozone is greater in air renewed from time to time than in confined air.

(2.) It is greater at the negative than at the positive pole.

(3.) The production of ozone increases only up to a certain point with the duration of the electric action.

(4.) The ozone increases with the electric intensity.

(5.) The ozone diminishes when the distance which separates the electrodes increases.

(6.) The production of ozone varies with the length or surface of the electrodes.

(7.) Other conditions being equal, the production of ozone is greater by utilizing the effect of the two electrodes.

(8.) The production of ozone is equally manifested, out of direct contact with the air, with metallic electrodes, when these last are surrounded for their whole lengths with tubes of thin glass play-ing the part of insulating sheaths, whether the extremities of these tubes are closed or not.

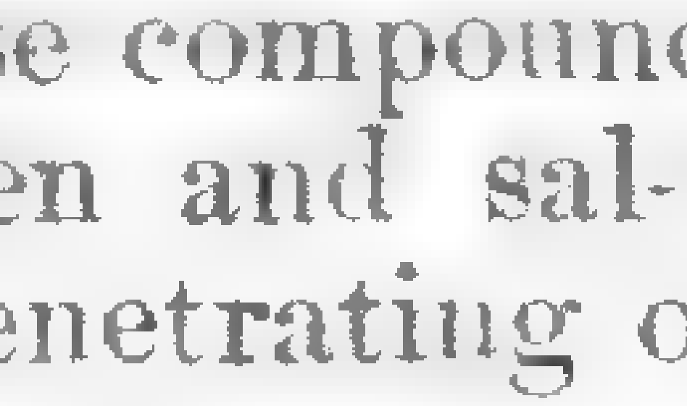
(9.) Still the production of ozone resulting from the passage of air over the naked metallic electrodes (direct contact with the platinum wires) is greater than that which arises from the passage of the air round the same electrodes, when sheathed and closed, (no direct contact of the air with the naked metallic electrodes).

(10.) With closed sheathed electrodes the production of ozone varies equally with the length or surface of the metallic electrodes.

(11.) The production of ozone increases considerably with a diminution of the temperature at which the electrification of the air is effected.

(12.) All conditions being equal, the quantity of ozone produced with a definite volume of oxygen, is always much more considerable (about eight or ten times) than that furnished by the same volume of air.

(13.) The ozone produced by the obscure electrification of air, is accompanied by small quantities of nitrous compounds, while that which is furnished by pure oxygen under the same circumstances, contains only traces. By attention to the conditions above described, the author has been able to construct a new apparatus, which he calls an *ozonizer*, and with which, according to his statement, quantities of ozone hitherto unknown, may be prepared. The apparatus is not described in the paper from which our extract is taken.—*Comptes Rendus*, lxx, 1286. W. G.

2. *Researches on Platinum.*—SCHÜTZENBERGER has communicated to the Academy of Sciences further investigations of the remarkable compounds of platinum with carbonic oxyd, already noticed in this Journal, and has in addition described some new series of great interest and theoretical importance. Setting out with the view that the two compounds already described, $CO = PtCl_2$, and  $\begin{matrix}CO\\|\\CO\end{matrix}{>}PtCl_2$,* may be regarded as the chlorids of two diatomic radicals, the author found that ammonia unites with each compound, forming the two new chlorids represented respectively by the formulas,

$$N_2H_6 . CO . PtCl_2, \text{ and } N_2H_6 . (CO)_2 . PtCl_2.$$

When heated these compounds fuse, yielding metallic platinum, nitrogen, hydrogen and sal-ammoniac, together with a volatile liquid having a penetrating odor which appears to be chlorid of formyl, COHCl. The compound $CO . PtCl_2$ absorbs dry ethylene and forms a yellow crystalline body, which has probably the formula $C_2H_4 . CO . PtCl_2$, and may be regarded as corresponding to the dicarboxyl compound $(CO)_2 . PtCl_2$, C_2H_4 replacing CO. When heated above 95° C. this body gives off chlorhydric acid, while a dark colored body remains insoluble in water and having the formula $C_2H_3Cl . CO . PtCl_2$, so that it contains monochlorinated ethylene in place of ethylene.

Phosphorous chlorid, PCl_3, like carbonic oxyd, unites readily

---

* Pt=197 C=12 O=16.

with platinous chlorid, forming a solid reddish-yellow substance, volatile with difficulty, fusing at about 200°, and soluble in phosphorous chlorid, from which it crystallizes in cooling. The composition of this body is represented by the formula $PCl_3 . PtCl_2$. The same substance is formed by heating one atom of platinum with one molecule of phosphoric chlorid, $PCl_5 + Pt = PCl_3 . PtCl_2$, and also by the action of PCl_3 upon $CO . PtCl_2$, CO being given off. The compound $PCl_3 . PtCl_2$ dissolves in water with a yellow color, forming a new complex acid, $P(HO)_3 PtCl_2$, and differs essentially from the body described by Baudrimont, which according to that chemist, has the formula $PCl_5 . PtCl_2$, $(Pt = 98{\cdot}5)$. In a later communication to the Academy, Schützenberger gives the formulas and names of a series of very remarkable substances which he has obtained from the primitive, $PCl_3 . PtCl_2$; these are as follows:

$PCl_3 . PtCl_2$ chlorid of trichloro-phosphoplatinum.
$P(HO)_3 . PtCl_2$ phospho chloro-platinous acid.
$P(A_9O)_3 . PtCl_2$ silver salt of the same.
$P(C_2H_5O)_3 . PtCl_2$ chlorid of trioxethyl-phospho platinum.
$P(C_2H_5O)_3PtO . H_2O$ hydrate of oxyd of platinum.
$P(C_2H_5O)_3PtO . N_2O_3$ nitrate of oxyd " "
$P(C_2H_5O)_3 . PtN_2H_4 . 2HCl$ chlorhydrate of trioxethyl-phosphoplatin-diamin.
$P_2Cl_6 . PtCl_2$ chlorid of hexachloro-diphosphoplatinum.
$P_2(HO)_6 . PtCl_2$ diphospho-chloroplatinous acid.
$P_2(AgO)_6 . PtCl_2$ silver salt.
$P_2(C_2H_5O)_6 . PtCl_2$ chlorid of hexoxethyl-diphospho platinum.
$P_2(C_2H_5O)_6 . PtN_2H_4 . 2HCl$ " " " diamine.
$P_2(C_2H_5O)_3Cl_3PtCl_2$ chlorid of trichloro-trioxethyl-diphospho platinum.
$P_2 (C_2H_5O)_3 (HO)_3 Pt Cl_2$ diphospho-trioxethyl-chloro-platinous acid.

Schützenberger points out the analogy between these compounds and the ammonia-platinum bases. In a third note he gives a method of isolating the radicals contained in the above described compounds. By treating the alcoholic solutions of the two chlorids $P(C_2H_5O)_3PtCl_2$, and $P_2(C_2H_5O)_6PtCl_2$, with zinc, viscid black masses are obtained, having respectively the formulas $P(C_2H_5O)_3Pt$ and $P_2(C_2H_5O)_6Pt$, and combining directly with chlorine to reproduce the original chlorids.—*Comptes Rendus*, lxx, 1287, 1414; lxxi, 69. W. G.

3. *On new derivatives of triethylphosphine.*—Cahours and Gal have studied the product of the action of platinic chlorid upon triethylphosphine, and have obtained a compound having the formula,[*] $P(C_4H_5)_3PtCl$, which they regard as the equivalent of the green salt of Magnus in the ammonia series. This body crystallizes from its solution in ether in voluminous transparent prisms of an amber yellow color. An alcoholic solution of this salt heated to 100° for several hours in sealed tubes, yields crystals of an iso-

---

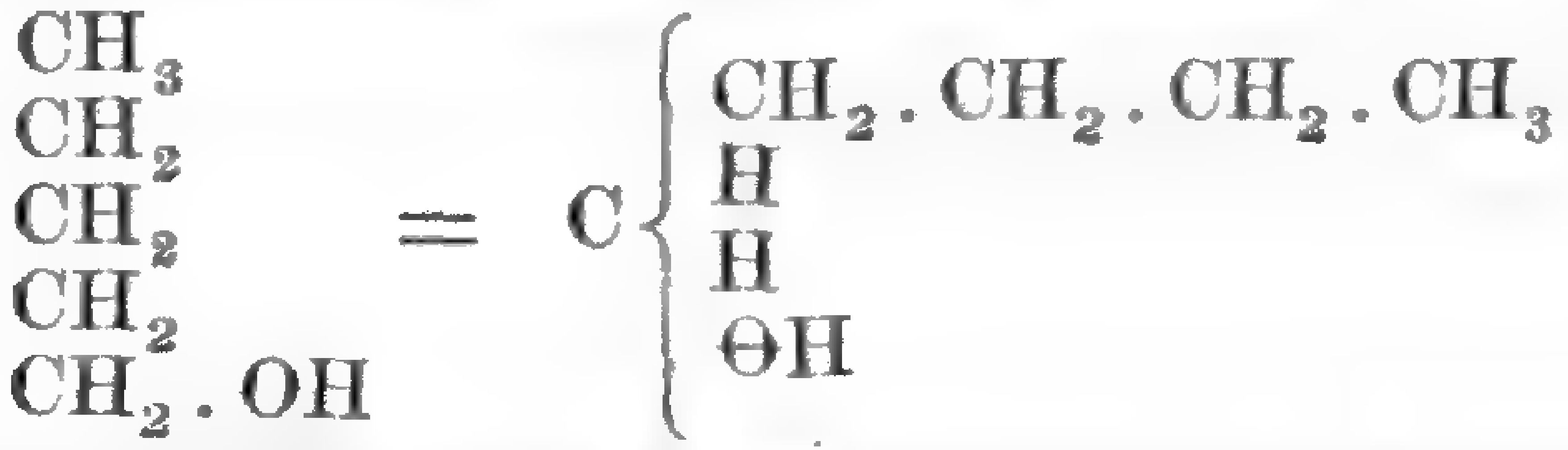
[*] $C = 6$ $O = 8$ $Pt = 98{\cdot}7$.

meric white salt. When the yellow salt is boiled with water and triethylphosphine, a colorless salt is formed, which has the formula, $[P(C_4H_5)_3]$ PtCl and which the authors consider to be the phosphorous analogue of the salt of Reiset. It loses an atom of triethylphosphine when long kept, and leaves the white modification of the chlorid first described. With argentic oxyd and water it yields a strongly alkaline liquid; with the chlorids of gold and platinum well defined salts. Palladium forms a compound similar to the first described salt of platinum, and also forming amber yellow prisms. This does not, however, unite with another atom of triethylphosphine. When triethylphosphine is added to a solution of auric chlorid, $AuCl_3$, in alcohol a colorless crystalline salt is obtained which has the formula, $P(C_4H_5)_3AuCl$. Compounds of an exactly analogous composition were obtained with the chlorid of platinum, palladium and gold, and triethylarsine.—*Comptes Rendus*, lxx, 1380, lxxi, 208. W. G.

4. *On silico-propionic acid.*—By the simultaneous action of zinc-ethyl and sodium upon ethyl-silicic monochlorhydrine, $SiCl(OC_2H_5)_3$, FRIEDEL and LADENBURG have obtained a liquid boiling at 158°·5 C. and having the formula, $Si(C_2H_5)(OC_2H_5)_3$, which they term tribasic silico-propionic ether. A concentrated solution of caustic potash does not set free in this compound the silicon in the form of silicic oxyd $SiΘ_2$, but gives a product having the formula, $SiC_2H_5O_2H$, which however cannot be obtained in this way in a state of purity. By heating silico propionic ether at 180° C. in a closed tube with chlorid of acetyl, the authors obtained a mixture of acetic ether and a body having the formula $SiC_2H_5Cl_3$. By treating with water the part of this liquid which boils at 90°–110°, chlorhydric acid and a white gelatinous body are formed; this last is the hydrate of silico-propionic acid. When dried at 100° the acid forms a white amorphous powder greatly resembling silicic oxyd, but easily distinguished from it by its combustibility. When heated it burns like tinder, disengaging combustible gases. The acid is insoluble in water, but dissolves in hot concentrated caustic potash, and is not precipitated from this solution by HCl, but only by NH_4Cl, like SiO_2, the residue after evaporation being unchanged silico-propionic acid. The new substance appears therefore to be a weak acid analogous to silicic acid, and presents the first known case of a silicic acid containing carbon. Its formula shows that it contains the group, SiO_2H, which may be termed silicoxyl, and which is the analogue of carboxyl, CO_2H. It is easy to see also that it forms one term of a group of homologous acids.—*Comptes Rendus*, lxx, 1407. W. G.

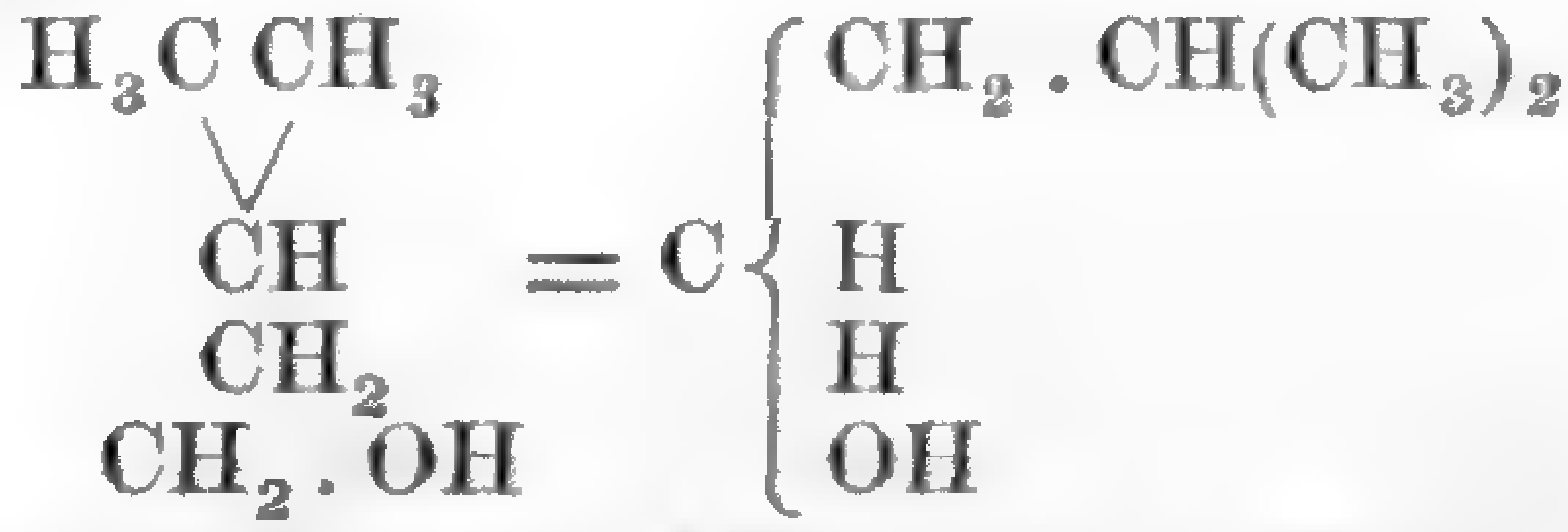
5. *On normal amylic alcohol.*—LIEBEN and ROSSI have succeeded in obtaining synthetically the normal amylic alcohol, which bears the same relation to the alcohol already known which normal butylic alcohol bears to that obtained by fermentation. Normal cyanid of butyl yields normal valeric acid, which greatly resembles the acid already known, but which has an odor more closely resembling that of butyric acid. It boils at 184°–185° at 736^{mm}. When normal calcic valerate is mixed with normal calcic formate,

and the mixture is distilled in small portions at a time, valeric aldehyd is obtained, boiling at 102° C. This aldehyd, by the action of nascent hydrogen, yields the normal amylic alcohol. The alcohol much resembles that obtained by fermentation, but has a higher boiling point, 137° C. under a pressure of 740^{mm}. The authors have prepared from it the chlorid, bromid, iodid and acetate of amyl, all of which possess higher boiling points than the corresponding ordinary amylic ethers. The constitution of the normal alcohol must be expressed by the formula,



$$\begin{matrix} \mathrm{CH_3} \\ \mathrm{CH_2} \\ \mathrm{CH_2} \\ \mathrm{CH_2} \\ \mathrm{CH_2 \, . \, OH} \end{matrix} = \mathrm{C}\begin{cases} \mathrm{CH_2 \, . \, CH_2 \, . \, CH_2 \, . \, CH_3} \\ \mathrm{H} \\ \mathrm{H} \\ \Theta\mathrm{H} \end{cases}$$

while common amylic alcohol has probably the formula attributed to it by Erlenmeyer,



The authors promise a detailed description of the normal valeric acid and its salts.—*Comptes Rendus*, lxxi, 369. W. G.

6. *Transformation of the fatty acids into the corresponding alcohols.*—Saytzeff has given a method of passing from the fatty acids to the corresponding alcohols, which possesses much interest. An amalgam of sodium of 3 per cent is to be introduced into a flask, and a mixture of one molecule of the chlorid and two of the acid introduced, the mixture being cooled. After 12 hours water is added, the liquid distilled, and the distillate saturated with potassic carbonate. The product separated is the ether formed by the fatty acid with the corresponding alcohol. This may then be saponified by potash. In this manner the author prepared propylic and butylic alcohols.—*Bull. de la Société Chimique*, xiii, p. 51. W. G.

II. GEOLOGY AND MINERALOGY.

1. *Laurentian Rocks of Nova Scotia;* by Rev. Dr. Honeyman, F.G.S., &c.—In this communication I propose to make a few observations bearing upon remarks made by Dr. Hunt on the above subject, in the July number of the American Journal of Science.

While I was engaged in the service of the Canadian Survey, ascertaining the extent of the distribution of the Upper and Middle Silurian rocks of Arisaig, I unexpectedly came upon a band of crystalline rocks, of considerable thickness and of great lithological variety. There was a succession of different kinds of diorites and hornblendic rocks, traversed with veins of quartz and many granular limestones. These extended about two miles along the shore, stretching at the same time both into the sea and land. After these came a thick band of ophites and ophio-calcites. Succeeding these were bands of diorites and hornblendic slates, with quartz veins containing mica in crystals, and then white feldspar rock of considerable thickness with green veins, and a thick band of rose-colored feldspar. I was convinced that I had discovered a series of Laurentian rocks. In geological position it is unquestionably inferior to the Arisaig series, which ranges from the Medina and Oneida through the Clinton, Niagara (?), Lower Helderberg, and possibly the Devonian, into the Lower Carboniferous. There appeared to me to be a sufficient lithological resemblance, between these rocks and those of Canada and Newfoundland Laurentian, to warrant the conclusion. Dr. Hunt gives me the credit of having profited by his suggestions, and the descriptions of Laurentian rocks given by the Canadian Survey, and assures the readers of the Journal, that these first brightened my ideas in regard to the geological age of the rocks in question. Dr. Hunt, however, knows right well that I had other and much better means of acquiring the requisite knowledge. I spent the greater part of six months in the Paris Exhibition, in the immediate vicinity of the Canadian Survey collection of rock specimens, from the Laurentian rocks of Canada and Newfoundland, and those who know me know that I am not in the habit of allowing such good opportunities to pass unimproved. After Mr. Richardson had finished the arrangement of these rocks in the Canadian department of the Exhibition, when there was no geologist in the Canadian and Newfoundland courts, my duties as Executive Commissioner in the Nova Scotia department requiring my constant attendance, I had frequent occasion to direct the attention of English and Continental geologists to the rock specimens referred to; so that I was not altogether ignorant of their character and appearance.

Thus schooled I considered the rocks discovered to be *Laurentian*. The first to whom I communicated the discovery was H. R. Hill, Esq., High Sheriff of Antigonish, who had often accompanied me in my Arisaig trips, and whose knowledge of the Silurian rocks of Arisaig and the places where fossils occur is somewhat intimate. To him I declared positively that I had found the Laurentian rocks in the neighborhood of our old geological field; at the same time I gave him specimens of these rocks. Shortly after I left the field on account of the inclemency of the weather. On my way home I had to pass through New Glasgow; I called upon Sir W. Logan at his hotel and showed him specimens of the ophite and ophiocalcite. He appeared to hail them as an important discovery, declared them to be of Quebec age, and recommended a search for chromic iron in the locality. When Prof. Hind inspected the specimens which I had in the Museum, he appeared then to regard them as Sir W. Logan had done, and in his report on the Waverly Gold Fields he mentions the discovery as a discovery of Quebec rocks. In the month of December following, I received a letter from Sir W. Logan, addressed to the care of W. A. Hendry, Esq., Deputy Commissioner of Nova Scotia Crown Lands, a gentleman who has had considerable field experience as a geologist, to whom I communicated the contents of the letter. In this letter Sir W. Logan says "after all it is not serpentine you have discovered. Dr. Hunt says it is agalmatolite or dysyntribite and it proves nothing; the rocks may be of Upper Silurian or Devonian age." I was not prepared to maintain that the ophite was not agalmatolite or dysyntribite; simple test proved that the ophiocalcite was undoubtedly a calcite. I could not for a moment admit that the strata in question were Upper Silurian or Devonian. Any one knowing the locality could not fail to be convinced that they were underlying the Upper and Middle Silurian of Arisaig, and therefore might be Lower Silurian, Huronian, or Laurentian, but certainly nothing more recent I had a specimen of the ophiocalcite—ophiocalcite it is now acknowledged to be—polished on two opposite sides by Mr. Wesley, marble worker. In returning it to me he said he had partly polished it by means of acid. It turned out to be a very beautiful and peculiar specimen. I examined its surface by a combination of lenses having a considerable magnifying power, and observed what appeared to me to be structure. I had often examined the magnificent specimens of Eozonal serpentine which were exhibited in the Canadian department of the Paris Exhibition of 1867, which were afterward deposited in the Ecole des Mines of Paris. I also had received from a Bohemian geologist in Paris a specimen of ophiocalcite with *Eozoon Bohemicum.* I compared this with the Nova Scotian specimen; I became impressed with the conviction that if the Bohemian was Eozonal so was the Nova Scotian, and that this was another evidence of the Laurentian age of the Nova Scotian rocks. In my simplicity and ignorance of facts advanced in the Eozonal controversy, I did not know that serpentines of *all ages* might contain *species* of the genus Eozoon. I pointed out the peculiar structure of the Nova Scotian specimen and its resemblance to that of the Bohemian to Mr. Hendry, already referred to. He expressed doubts on the matter of structure in both, as an unbeliever in the Eozoon might do. About the same time Prof. Lawson of Dalhousie College and University, Halifax, came to the Museum and asked if I had a specimen of the *Eozoon Canadense;* I told him that I had not, but that I had a specimen of the *Eozoon Bohemicum* and what might be called the *Eozoon Nova-Scoticum.* He examined them, but replied that he wanted to see the real *Eozoon Canadense* Mr. Hendry confirms the above statements as far as he is referred to, in the presence of Dr. How of Kings College and University, Windsor, to whose friendship and kindness I am indebted for the loan of a copy of the number of the American Journal of Science, containing Dr. Hunt's article already referred to.

In the spring of 1869, I visited Montreal and took with me the polished specimen of ophiocalcite. I gave it to Dr. Hunt in the lapidary's workshop in Gabriel street. A day or two after, when I was engaged in the same place talking with Sir W. Logan, Dr. Hunt entered with the specimen referred to in his article, and stated to Sir W. Logan in my presence to the effect that Dr. Honeyman had discovered a very interesting series of rocks; that the specimens tempted him to go to see them, and that he believed them to be the Laurentian. Sir W. objected, saying that the specimen was not serpentine. Dr. Hunt assured him that it was ophiocalcite; that he had examined it; that the white effervesced with acid. He also named some of the constituents; I do not recollect what they were. Sir W. and Dr. Hunt left. I heard nothing more about the matter until my very kind host, Dr. Dawson, on returning from a visit to Gabriel street, told me, in his library, that Dr. Hunt had said to him that certain rocks that I had discovered were of Laurentian age; but that he had advised him to consider them first of all Devonian. All that I replied was, I heard him say so. For certain private reasons I expressed no opinion on the subject. When I returned to Halifax I arranged my representative collection of Nova Scotia rocks in the Provincial Museum under my charge, thus: Arisaig ophite, ophiocalcite and porphyritic diorite, at the bottom of the series; granites, new gneissoid rocks of Prof. Hind, next; andalusite rock and slates next; and thus they had remained until the present time. Many a time have I pointed out these rocks to visitors who take an interest in such matters, and told them the lowest ones were Laurentian. I have certainly not been silent, as Dr. Hunt informs the readers of the American Journal of Science, although the sound of my voice may not have reached Gabriel street, Montreal. I could name many witnesses to attest this.

I would now observe that, from one cause or other, I had never met with Prof. Hind since the time already referred to, until a day or two after he had read a paper on his discovery of the Laurentian rocks, before the Nova Scotia Institute of Natural Science. He came to meet Dr. Lawson in the Museum. He commenced discussing the subject of his discovery. I then turned his attention to my collection of rocks; showed him the Laurentian of Arisaig, and gave him a detailed account of the opinions expressed in regard to them. He informed me that he had announced his discovery of the Laurentian gneissoid in Sherbrooke and elsewhere, to Sir Roderick Murchison, Sir W. Logan, &c. Not long after a letter was inserted in the Halifax Morning Chronicle, addressed by Prof. Hind to the Hon. Robert Robertson, M.E.C., Commissioner of Mines and Public Works, dated Windsor, 10th Feb., 1870, from which I give the following extracts: "Under date Montreal, Feb. 3, 1870, Dr. Hunt informs me that recent microscopic examination of some of the specimens sent by Dr. Honeyman, has revealed *well-defined* '*Eozoon Canadense.*'" "This," continued Dr. Hunt, "must, I conceive, be *conclusive evidence of their Laurentian age.*" "Dr. Dawson, in a separate communication, confirms the identification of the Eozoon."

I find, however, that in the discussion that followed the reading of the paper, "On the Laurentian rocks of Arisaig, N. S.," addressed to the Geological Society of London, Dr. Dawson remarked that the Arisaig Eozoon was different from that of Canada; that the *Eozoon Bohemicum* belonged to a formation more recent than the Laurentian, and that the Arisaig Eozoon did not prove the rocks to be Laurentian. *Vide* printed abstract of the proceedings of the Geological Society of London.

I do not regard the discovery of Eozoon as the only evidence of the Laurentian age of the Arisaig rocks. I consider that *relative* position, in connection with *lithological* character, must have something to do with the determination of their age. Besides, is it not possible that, as there appear to be *new* different species of Eozoon belonging to different countries and different ages, that there may be different species in different countries, but still of the same age, and still that the Nova Scotian species is still Laurentian.

Another extract from Prof. Hind's letter: Dr. Hunt says, in his letter already referred to, "A line from the Laurentian of Malignant Cove (Arisaig) to that in Newfoundland, will pass through Cape Breton, and we can now look for limestone and Eozoon there." Is it not possible that the thought may have occurred to some one else besides Dr. Hunt? Perhaps Dr. Hunt has forgotten something that happened during the Exhibition in Paris of 1867. He came into the Nova Scotian Court, accompanied by Prof. Lesley of Philadelphia; he found among the polished specimens of marble a green one which arrested his attention; he asked where it came from, and was told that it came from Cape Breton, (having been furnished by Mr. Hendry already referred to.) This specimen also attracted the attention of Prof. Wyville Thompson, an Eozoonal unbeliever, who was a member of the International Jury; he said that it had Eozoonal structure; he wished to have it and got it; he took it to London where it made no small stir among the believers in the Eozoon, and gave occasion for a little ingenious explanation on the part of Dr. Hunt; so I was informed.

Another extract from Prof. Hind's letter: "It is thus, that Dr. Honeyman's opinions have been beautifully verified, but it would have greatly enhanced the gratification which Dr. Honeyman must feel if the announcement had been made a year and a half ago."

As might be expected this observation caused some excitement in Gabriel street. In order to rebut the above implied charge, doubtless, Dr. Hunt finds fault with me for not expressing my views, relative to the age of the rocks in question, in my official report. I answer, I studiously avoided expressing any opinion on controverted points in my report, and made it a simple record of facts. As far as I recollect, I mentioned the discovery of the rocks *simpliciter*. It appears that my alleged silence is considered by Dr. Hunt as "*simply incomprehensible.*" I think I can bring the matter to the level of the comprehension of even a less acute person than Dr. Hunt. I have already shown that the term *silence*, if meant literally, is not a proper term to use in the case, as I have not been *silent* on the subject. If by silence he means that I had not addressed a communication on the subject to any scientific Society or Journal, until the publication of Prof. Hind's letter, Dr. Hunt has hit the mark. My defense is, I had not been able on account of the inclemency of the weather and lateness of the season, to examine the rocks in a manner so thorough and satisfactory as was desirable; I did not think there was any great hurry in the matter, as I did anticipate the *great discoveries* of my excellent friend Prof. Hind. I have not yet had the desired opportunity, and yet I have had to write on the subject: first, to the Geological Society of London at the request of Prof. Hind; second, to the American Journal of Science, at the instigation of Dr. Hunt.

2. *Descriptions of Fossils collected by the U. S. Geological Survey under the charge of Clarence King, Esq.* (Proc. Acad. Nat. Sci., Philad., No. 1, 1870).—Mr. MEEK prefaces his numerous descriptions of new species by the following observations, addressed to Prof. Joseph Leidy.

I send herewith, to be presented for publication in the Proceedings of the Academy, descriptions of a few of the fossils brought in by the United States Geological Survey under the direction of Clarence King, Esq. You will please state, in presenting the paper, that the Trilobites described in it from Eastern Nevada, are decidedly Primordial types, and, so far as I know, the first fossils of that age yet brought in from any locality west of the Black Hills. Mr. King's collections also establish the fact that the rich silver mines of the White Pine district occur in Devonian rocks, though the Carboniferous is also well developed there. The Devonian beds of that district yet known by their fossils, seem mainly to belong to the upper part of the system. Mr. King, however, has a few fossils from Pinon Station, Central Nevada, that appear to belong to the horizon of the Upper Helderberg limestone of the New York series.

The Tertiary fossils described in this paper, from the region of Hot Spring Mountains, Idaho, came from an extensive and interesting fresh-water Lacustrine deposit, and are all distinct specifically, and some generically, from all the other Tertiary fossils yet brought from the far west. Two of the species belong to the existing California genus Carinifex, or some closely allied group, while another beautifully sculptured species was thought, by Mr. Tryon, to whom I sent a specimen of it, to be possibly a true *Melania*, and allied to existing Asiatic forms.

It is an interesting fact, that among all of our fresh-water Tertiary shells from this distant internal part of the Continent, neither the beaks of the bivalves, nor the apices of the spire in the univalves, is ever in the slightest degree eroded; even the most delicate markings on these parts being perfectly preserved, if not broken by some accident. From this fact it may be inferred that the waters of the lakes and streams of this region, during the Tertiary epoch, were more or less alkaline, as is the case with many of those there at the present day.

These descriptions, as well as others that I expect to send you soon, are merely preliminary and will be re-written, and presented with full illustrations, now in course of preparation, in Mr. King's report of his survey.

3. *Discovery of a Mastodon.*—On Friday last, Mr. Fletcher Correll, a farmer residing one and one-half miles southeast of Illiopolis, in this county, was digging a well upon his place, when, at the depth of about four feet from the surface, he struck a hard sub-

stance, at first supposed to be a stone or piece of wood imbedded in the earth; but upon digging farther, it was discovered to be the remains of a mastodon.

The bones were in a fair state of preservation, and exhibited a dark, spongy, porous appearance. One of the tusks, which was broken in removing it from its long resting place, proved to be, when measured, nearly ten feet in length, and twenty nine inches in circumference three feet from the lower end. The other tusk, and the main portion of the skeleton, are now being lifted from the earth, and will probably be added to the collection of fossils now being made by our state Geologist, Professor Worthen.

The part of this huge creature which was exhibited in our office in the presence of Professor Worthen, was a piece of the lower left jaw, about two feet in length, and at least the same in circumference. It contained, in a fine state of preservation, one of the great jaw-teeth.—*Daily State Journal, of Springfield, Illinois, Sept. 7. Received from C. L. Conkling.*

4. *Description of the Cavern of Bruniquel, and its organic Contents;* by Professor OWEN. Part I, Human and Equine Remains. 55 pp. 4to, with six plates. (Phil. Trans., 1870; communicated to the Royal Soc., June, 1864).—Professor Owen, after describing the human remains, draws from them the following conclusions:

"They exemplify the distinctive characteristics of the human genus and species, as decidedly as do the corresponding parts of the present races; they show most affinity with the oldest Celtic types, the cranium being oval, and rather dolicho- than brachy-cephalic in its general proportions; the cranial capacity or brain corresponds with that of uneducated Europeans of Celtic origin, and exceeds that of the average Australian aboriginals."

Some of the bones accompanying the human remains—those of the deer especially—are covered with drawings, representing the heads of horses and showing much artistic skill; and from these draughtsmen of the cave-dwelling people, we thus learn that the horse of the era, *Equus spelæus*, had short pointed ears, and that the stallions had beard-like hairs; and from the antler of a reindeer, found in another cavern, that of La Madelaine, in Dordogne, we have the additional fact that the tails of these early horses were short and furnished with long hairs to their base, having "cauda undique setosa," instead of "cauda extremitate setosa," a fact repeated seven times on the antler. The horse is of the same species that occurs in certain quaternary beds in France, for example, those near the Tour de Juvillac, Püy-de-Dome, and belongs to the restricted genus *Equus*, and not to that including the zebra and ass (*Asinus*). Prof. Owen concludes with the following paragraph:

"No satisfactory evidence of an aboriginal feral *Equus caballus* has yet been obtained by the Naturalist. No specimen of such exists in any Museum. The doubts expressed by Forster and Pallas as to the alleged wild horses of the Ukraine, viz: that they might be descendants from strayed domestic horses, have not yet been cleared up. I believe the illustrations contained in the present paper to be the best, if not sole, evidences of the wild originals of some of our domesticated breeds. Like the alleged wild horses of Prussia, those of Aquitaine, in the time of the flint-armed hunters and cave-dwellers, were doubtless "shy and difficult of capture, but very good venison."

The above memoir is followed by another, under the same cover, entitled *Supplementary Remains of Equines from Central and South America referable to* Equus conversidens *Ow.*, Equus tau *Ow.*, and Equus arcidens *Ow.* In this paper Professor Owen, besides discussing the characters of the different kinds of fossil horse in the regions referred to, treats of their relations to the extinct genera *Macrauchenia*, *Nesodon*, and *Toxodon*, and illustrates the subject with numerous figures on six lithographic plates. He observes that according to the facts made out, the most ancient Equines of the South American continent present modifications of the upper molars, and more especially of the lower molars, which supply an additional link between the Equines with other perissodactyle Ungulates, and the group including *Toxodon* and the other genera above mentioned.

5. *The North American Lakes as Chronometers of Post-glacial time.*—Dr. E. Andrews, the author of this paper, noticed at p. 264 of this volume, states, in a recent letter, that his method of calculation eliminates the error alluded to in the notice, due to the wearing away of the sands. He says that in the calculation, "the first element is the age of the lower beach, which is obtained by ascertaining the age of its cotemporary terrace of erosion; this settles the approximate age of the lower beach without liability to error from attrition of sand. The age of the upper beaches is deduced by comparison of their relative masses with the lower. Now as the attrition was going on at about the same rate at all ages, the relative loss by attrition would be strictly proportioned in all the beaches, and then so far as time is concerned would be unchanged."

6. *Descriptions of new Fossil Shells of the Upper Amazon;* by T. A. Conrad, (Am. J. of Conch., Oct. 10, 1870).—Mr. Conrad describes several species of shells from the deposit at Pebas, on the Amazon, visited by Prof. Orton, (see p. 294 of this volume). They include a number of species of *Pachydon*, Gabb, and a bivalve near *Mulleria*, besides gasteropods of the genera *Isæa* Conrad, *Hemisinus*, *Dyris* Conrad, *Neritina*, *Bulimus*. The Bulimus is the only land shell in the collection. The species "may have lived either in fresh or brackish water, but are certainly not of marine origin." They appear to be all extinct, and if so, they "cannot be later than the Tertiary."

7. *Restoration of a Flying Dragon* (Dimorphodon macronyx *O.*) *from fossil remains in the Lias-cliffs of Dorsetshire;* by Prof. Owen, F.R.S. (Pages 463–502 of Pal. Soc. volume for 1869). With two plates. London, 1870.—Prof. Owen's memoir contains a full and detailed account of this remarkable Pterosaur, and illustrates it by admirable lithographs. He does not agree with Mr. H. G. Seeley with regard to the relations of Pterosaurs to Birds.

8. *Remarks on Prof. Owen's Monograph on Dimorphodon;* by H. G. Seeley, F.G.S., (from the Ann. Mag. Nat. Hist., Aug., 1870. 24 pp.)—Mr. Seeley in this paper presents his objections to the views of Prof. Owen on the Dimorphodon.

9. *Geological Charts.*—The second edition of von Dechen's general geological chart of Germany, France and England and the adjoining countries, was published at Berlin in 1869, and a geological chart of Germany by the same able author in 1870.

The Royal "Comitato Geologico," of Italy have announced that they will publish, the coming year, a small geological map of Italy.

10. *Carte Géologique du versant Occidental de L'Oural;* by Valérien de Möller. 1869. Scale $\frac{1}{840,000}$. A beautiful colored geological chart of the Western or European slope of the Urals.

11. *Second Annual Report upon the Geology and Mineralogy of the State of New Hampshire;* by C. H. Hitchcock, Prof. Geol. and Min. in Dartmouth College. 38 pp. 8vo. Manchester, N. H. 1870.

III. BOTANY AND ZOOLOGY.

1. *Martius, Flora Brasiliensis:* fasc. xlix, *Cyatheaceæ et Polypodiaceæ.* Exposuit J. G. Baker.—The other suborders of Filices were worked up by J. W. Sturm, and were published in 1859. To illustrate the different ways in which these two fern-authors regard species, one may look at the genus *Aneimia*, of which Mr. Baker in the "Synopsis Filicum" recognizes but twenty-six species for the whole world, while Sturm describes not less than thirty-one species peculiar to Brazil, besides thirteen common to Brazil and some other countries, and five more not yet reported from Brazil, but likely to be found there. Mr. Baker seems to diminish the number of species at every step; many that were recognized in the "Synopsis" being reduced to synonyms in the work now under consideration. For instance, *Lomaria L'Herminieri* and *L. lanceolata*, placed in different groups in the Synopsis, are now made into one species. So, too, *Blechnum Fendleri* is joined to *B. longifolium*, and *Polypodium* (*Phegopteris*) *rigidum* to *Aspidium aculeatum*. *Gymnogramme calomelanos* swallows up *G. tartarea*, having long ago digested *G. chrysophylla, Massoni, Peruviana*, &c., and many other such instances might easily be pointed out. But all this reduction seems to have been done only after patient study over many specimens, and it is gratifying to see this emphatic protest against the flood of new species with which we are deluged by continental authors.* The author's industry and re-

---

* The publication in Linnæa by Dr. Kuhn of manuscript names left by the late Dr. Mettenius, many of which the maturer judgment of the latter would doubtless have withheld, gives to Botany several hundred "new species;"—a few examples of them from one genus, by no means a large one, are subjoined.—*Pellæa intermedia*, founded on Mr. Wright's New Mexican No. 825, is said to be "perhaps a hybrid between *P. sagittata* and *P. flexuosa*," forms which in all probability both belong to *P. cordata; Pellæa myrtillifolia*, is the Chilian form of *P. andromedæfolia; Pellæa microphylla* is the new name for the New Mexican specimens of *P. pulchella*, and *Pellæa glabella* rests upon St. Louis and Wisconsin specimens of *P. atropurpurea*, with an unusually glabrous stipe and rhachis.

search have unearthed a few long buried and forgotten names for well known ferns:—*Pteris geraniæfolia* Raddi (1825) was found by Dr. Kuhn to be the same as *Pteris Pohliana* of Presl. (1822), and now we must go back twelve years more to *Pteris concolor* Langsdorff and Fisher (1810). Can any one find a yet more ancient name for it? It is gratifying to see, in a note, that the African *Cheilanthes Kirkii* is now confessed to be probably an adiantopsoid variety of this same species, the only difference being in the interrupted instead of continuous involucre.

Synonymy given by Mr. Baker is ample without being profuse: the characters are well drawn up, and the descriptions clear. There are several pages of letter press devoted to the geographical distribution of Brazilian Cytheaceæ and Polypodiaceæ. If the orders elaborated by Dr. Sturm could have been included in the notes and tables relating to geographical distribution, it would have been very acceptable to Pteridologists, but, as Mr. Baker remarks, the former suborders were written under such a very different theory of species that they could not be joined with the present ones in any kind of synoptical table. There are thirty-eight plates of Nature-printed fragments of Fern-fronds, borrowed from Ettingshausen, and thirty-two more cut on stone, from drawings by Fitch, Hipsley and Kunze, with a single one done under the eye of Dr. Eichler at Munich. D. C. E.

Oxford Botanists is the theme of a discourse pronounced by Prof. Lawson,—the successor in the professorial chair of the worthies he commemorates,—at the recent Congress held at Oxford by the Royal Horticultural Society. The history begins with the foundation of the Botanic Garden by Henry Earl of Danby, in 1632, and with the installation of *Jacob Bobart*, the first curator. It gives a biography of *Dr. Morison* the first professor, appointed in 1669; of his successor, *Jacob Bobart*, the second; of *Dr. Sherard*, or "Sherwood" who re-endowed the chair, and of *Dillenius* whose appointment he provided for in his will; of *Dr. Humphrey Sibthorp*, the next incumbent, and his distinguished son, *Dr. John Sibthorp*, who succeeded his father in 1784, and died at the age of 38 in 1796, bequeathing new endowments and the foundation of the associated chair of Rural Economy, after providing for the publication of the sumptuous *Flora Græca.* His successor *Dr. Williams* is mentioned; and an interesting notice of the late incumbent and patron, *Dr. Daubeny*, brings down the history to the present time.

Dr. Hasskarl, who has long been studying the *Commelynaceæ*, has just issued a monograph of the Indian species, especially of the Archipelago, with occasional genera and species of other parts of the world; an octavo of 182 pages, published at Vienna. Even in this partial representation of the order over forty genera are admitted! Some valuable criticisms of Dr. Hasskarl's work in this order will be found in a paper by Mr. Clarke, mentioned below.

The Journal of the Linnean Society, Nos. 54, 55, issued together in September of this year. The first and the most important, as well as the longest, article is

A Revision of the Genera and Species of Herbaceous Capsular Gamophyllous Liliaceæ, by J. G. Baker, F.L.S., &c. The adjectives here employed distinctly limit the portions of the great Liliaceous family, to which Mr. Baker here devotes himself. That he intends to go through the order in this way seems probable, and it is desirable. In treating incidentally of the limitation of *Liliaceæ*, he refers without qualification, to "characters which are universally regarded as of ordinal value, in the extrorse anthers, separated styles and septicidally dehiscent capsules of *Colchicaceæ*," apparently under the impression that these characters and their converse, run harmoniously parallel throughout. The exceptions, however, which have already been noted and placed upon record, and the way in which some of these opposed characters blend, may upon consideration show that so complete a separation is hardly practicable; and indeed the attempt has been abandoned by at least three botanists already, two of them authors of British Floras, and of whose views Mr. Baker would be likely to be particularly well-informed. *Helonias* and *Chamælirium*, on one hand, are loculicidal in dehiscence, while some near Japanese relatives of Helonias have united styles, and *Tofieldia* has introrse anthers. Then the two elements which constitute extrorsion or introrsion of the anthers may not coincide. The anthers of *Uvularia*, being adnate, are plainly extrorse, yet the lines of dehiscence are almost exactly lateral. If, then, we regard the fixation rather than the lines of dehiscence as the more essential, *Lilium* itself comes into the category of extrorse anthers (as first noticed, long ago, by Dr. Chapman), for its anthers are extrorsely attached although slightly introrsely dehiscent. *Medeola* is in the same case, while *Trillium* is nearly as in *Uvularia*. Mr. Baker brings the plants he deals with under 27 genera, including our *Hesperocallis*, which had escaped notice when the Clavis was printed. As to this very rare and little-known plant, he expresses a natural doubt as to its generic distinctness from *Hemerocallis*, which inhabits a widely distant part of the world. We are now able to state that, like *Hemerocallis*, the blossoms open in the morning and close at night. This is on the authority of Mr. Schott, who proves to have been the first discoverer of the plant. Moreover, which is of more consequence, he collected and has supplied us with the "bulb." It really is a *tunicated bulb*; and so the genus falls out of the tribe *Hemerocallideæ*, and seemingly into Mr. Baker's *Odontostemoneæ*, for which we would like to have Mr. Baker substitute the name of *Hesperocallideæ*. Our further interest in this revision, as regards N. American Botany, centers in *Brodiæa* and its allies. In this genus he (as well as Dr. Hooker, in Bot. Mag.) retains the remarkable *Brevoortia* of Mr. Wood, which, it appears, was first collected by the late Mr. Lobb; and he follows up the idea which this species suggested by reducing Torrey's *Stropholirion* likewise to *Brodiæa*. On the other hand he departs from Bentham's view, and refers all the hexandrous species, and we suppose justly, to *Milla*, which swallows up *Triteleia*, *Calliprora*, and even *Hesperoscordum*. The latter name is accidentally printed *Hesperocordium* in the conspectus; and further on, *Hesperoscordium.* The latter is perhaps an intended (and a laudable) innovation, as it is hard to pronounce the original name, as must needs be, with the accent on the penult.

On the Commelynaceæ of Bengal, by C. B. Clarke; another paper in this fasciculus of no small importance for the correct understanding of the characters of these difficult plants.

Notes on some Algæ found in the North Atlantic Ocean, by Dr. Dickie. The results of the examination of the contents of a bottle which was filled in the midst of the Atlantic, lat. 12° N., long. 21° 40′ W. There Capt. Mitchell, on the 24th Nov. 1867, found a patch of sea, 12 or 14 miles across, closely studded with a green substance, some of which he gathered. It is thought to have been wafted there from the American side by the Gulf-stream and the African current. The principal interest to us is in the following statement:—

"The substance thus picked up by Capt. Mitchell was in excellent condition, and, besides three *Algæ*, contained numerous fragments, more or less decayed, of wood, both endogenous and exogenous; *seedling plants several inches long, all with a pair of cotyledons, roots, and terminal bud, quite fresh ;* small fruits partially decayed, evidently one-seeded legumes; intermixed were various microscopical crustacea, and a common oceanic insect, one of the *Hydrodromideæ*, genus *Halobates ;* on some of the pieces of drift wood were numerous elliptical ova, of a deep orange color, mixed with which was growing the smallest of the three Algæ now to be described."

Leaves do not absorb moisture from the atmosphere in the state of vapor, according to a series of elaborate experiments by Unger, and afterward by Duchartre,—some of which were published several years ago,—confirmed recently by those of Prillieux.

The latter reaches this conclusion upon ascertaining that when a leafy branch, flaccid from evaporation, is suspended in moist air and recovers its freshness—as every one knows it promptly does—it does not gain in weight, but even loses. The same occurs with leaves furnished with petiole, the cut end sealed with wax. The conclusion is that the moisture by which the foliage is freshened comes not from the surrounding air but from the lower part of the branch, or from the leaf-stalk, the upper and foliaceous parts gaining at the expense of the lower. To test this, let the experiment be tried with single leaves destitute of leaf-stalk. Duchartre made a portion of his experiments with epiphytes, both Orchids and *Bromeliaceæ*, and concluded that the same applied to the aerial roots as well as the foliage. There was no gain in weight except when liquid water reached the leaves, roots, or other portion of the surface, when absorption was promptly indicated by increase in weight: and it appeared that this absorption of liquid water hardly took place by the leaves unless these were immersed or kept long and thoroughly wet. This again corresponds with some older experiments which went to show that syringed foliage did not absorb the liquid. As to aqueous vapor, there being free communication by the stomata between the air outside of the leaf and that within its intercellular spaces and passages, and the thin walls of the parenchyma-cells freely allowing vapor of water to pass out, it is very difficult to believe that it will not equally pass inward under appropriate conditions. Hence we cannot yet regard the question as concluded. The editor of the Gardeners' Chronicle (Sept. 17) pertinently suggests that "it does not appear to have been remembered that the density of aqueous vapor is very much less than that of dry air. If the latter be estimated at 1000, that of aqueous vapor is 625 only,—a circumstance that ought to be taken into consideration in such experiments." But if leaves continue so to absorb aqueous vapor, they ought to condense or appropriate it, along with carbonic acid gas, and so increase manifestly in weight.

An Elementary Course of Botany, Structural, Physiological and Systematic, by the late Prof. Henfrey, which was originally published (by Van Voorst) in 1857, is this year reproduced in a second edition, "revised and in part rewritten" by Dr. Maxwell T. Masters, F.R.S., &c.—The death of Prof. Henfrey, while still a young man, was a sad loss to physiological botany in Great Britain. As a vegetable anatomist he was becoming truly eminent. Our estimate of the value of this, his last work—on the whole very favorable—was recorded at the time in a somewhat extended review of it in the number of this Journal for November, 1857 (vol. xxiv, II, p. 434–440.)

The preparation of a second edition could hardly have been consigned to better hands than those of Dr. Masters, who has evinced special aptitude for morphological work, which was not Prof. Henfrey's *forte*. The alterations and additions by the present editor are not specially indicated: those which we have discovered are well adapted to bring the work up to the time, and to render it a valuable text-book. But we are bound to maintain that it might have been more thoroughly "revised and rewritten" to great advantage. It could hardly be expected that criticisms made a dozen years ago, and on this side of the Atlantic, would come under the notice of a recent editor, yet it is curious to observe that, out of a series of more than twenty misapprehensions or errors in detail (individually of small moment, no doubt), which were indicated in this Journal, chiefly in the morphological part, all but two (and one of these a grammatical slip) stand unaltered in the new edition. The sole alteration is in the statement which attributed axillary tendrils to the Vine. We do not include some criticisms of another order, relating to mere theoretical points, upon which views which have long prevailed are retained;—such, for instance, as that which unquestioningly regards the so-called radicle of the embryo as root, although in one part of the work its nature as hypocotyledonary stem is implied in a particular instance. That this part of the embryo represents the first joint of stem is a doctrine which seems to be ignored in England, but generally accepted out of it. A. G.

2. *Anatomisch-Systematische Beschreibung der Alcyonarien. Erste Abtheilung; die Pennatuliden, erste Hälfte*, von A. Kölliker. Quarto, with ten plates. Frankfort, 1870.—There are very few groups in the animal kingdom which have been more carefully studied, during several years past, than the Halcyonoid Polyps, or concerning which our information has more rapidly increased, both in respect to their structure and the number and variety of their forms. The Pennatulacea, especially, have been carefully studied and described by several able naturalists, and the number of species has been increased to an extent that is truly surprising, considering the very few species known a dozen years ago, and their comparative rarity. Numerous species and genera were made known in the works of Herklots, Bleeker, Richiardi, and others. But in this work Prof. Kölliker has undertaken a complete revision, both anatomical and descriptive, of the entire group, and in the part already published has added a great number of new and interesting forms. His work is of greater interest from the fact that he has been able to examine the collections in nearly all the principal museums of Europe, including the types of most previous writers.

This first part includes an anatomical description of the whole group, with an analytical table of the classification adopted, while most of the volume is devoted to very careful and elaborate anatomical descriptions of the genera: *Pteroeides*, *Godeffroyia* (nov.), *Sarcophyllum*, *Pennatula*, *Leioptilum*, *Ptilosarcus* and *Halisceptrum*, and their included species, of which a large proportion are admirably illustrated. In the genus *Pteroeides* 43 species are described, of which 29 are regarded as new, although a few of them are probably identical with some of those described in the nearly contemporary work of Richiardi, (we have not learned the precise date of either). Numerous varieties of some of the species are also named and carefully described. The polymorphic forms of the polyps in all the genera and species are thoroughly discussed

The following is the classification adopted:

First Tribe, Pennatuleæ; Second Tribe, Renillaceæ; Third Tribe, Veretillidæ.

Tribe, Pennatuleæ.

1st Family, Penniformes.

1st Subfamily, Pteroidinæ, including the genera *Pteroeides* Herkl., *Godeffroyia* Köll., *Sarcophyllum* Köll.

2nd Sub-family, Pennatulinæ, including *Pennatula* L., *Leioptillum* Verrill, *Ptilosarcus* Gray, *Halisceptrum* Herkl.

2nd Family, Virgularieæ, including *Virgularia* Lam., *Stylatula* Verrill, *Pavonaria* Köll., *Scytalium* Herkl., *Funiculina* Lam., *Halipteris* Köll. V.

3. *The External and Internal Parasites of Man and Domestic Animals*; by A. E. Verrill. 140 pp. 8vo, 85 wood-cuts. (From the Report of the Connecticut Board of Agriculture), Hartford, 1870.—The first part of this work is devoted to the external parasites and contains descriptions of the various species of fleas, lice,

ticks, bot-flies, bed-bugs, mites, and acari, often troublesome to our domestic animals, as well as to man himself, together with accounts of their habits, effects, and remedies. Several pages are also devoted to introductory remarks on the structure of insects and their classification.

The second part is devoted to the internal parasites, and contains a general account of the parasitic worms, and their classification, a series of lists of those found respectively in man, the dog, cat, sheep, cattle, horse, hog, and poultry; and detailed descriptions of the forms, anatomy, metamorphoses, habits, effects, and remedies of some of the most important species, many of which are illustrated by wood-cuts. Among those most fully discussed are the *Trichina spiralis;* the pork and beef tape-worms of man; the *Tœnia cœnurus* or water-brain of sheep; the *T. echinococcus* or hydatids of man, sheep, etc.; the *Fasciola hepatica* or liver-fluke of sheep and cattle; the *Syngamus trachealis* or gape-worm of chickens, etc., and many others of importance.

4. *On the Eared Seals* (*Otariadæ*), *with detailed descriptions of the North Pacific species;* by J. A. ALLEN. *Together with an account of the Habits of the Northern Fur Seal* (*Callorhinus ursinus*); by CHARLES BRYANT. 108 pp. 8vo, with three plates. From the Bulletin of the Museum of Comparative Zoology, vol. ii, no. 1. Cambridge, Mass., August, 1870.—This is a very interesting and important contribution to our knowledge of the seals of the Pacific and Southern oceans. It includes a résumé of the recent literature of the subject; a chapter on the affinities, distinctive characters, variations due to age and sex, etc., and a conspectus of the genera and species; detailed descriptions of *Eumetopias Stelleri* Peters, based on two perfect skins and two ligamentary skeletons; of *Zalophus Gillespii* Gill, based on two skulls: and of *Callorhinus ursinus* Gray, based on six skins and four complete and two incomplete skeletons, representing both sexes and the young and old. Mr. Bryant contributes a very full and interesting account of the habits of the last species, with descriptions of the islands that they inhabit, the modes of capture, etc. Much information, some of it new, is also given from various other sources, concerning the habits of this and the other species. The geographical distribution of the family is also discussed on page 42.*

The plates and several wood-cuts are devoted chiefly to the skulls and teeth. V.

5. *Record of American Entomoloy for the year* 1869. Edited by A. S. PACKARD, Jr., M.D. Naturalist's Book Agency, Salem, Mass.—This, like the Record for 1868, is a work that should be in the hands of every entomologist who hopes to keep up with the

---

* It is here stated that "the habitat of no species, so far as certainly known, quite reaches the tropics," but a skull of *Otaria jubata* in the Harvard Anatomical Museum labeled as from "Arica, Peru," is mentioned, with a doubt as to the correctness of the locality. I may add that in the Museum of Yale College there is a large adult skull of the same species, collected by Prof. F. H. Bradley much farther north, at Zorritos, Peru (about lat. 5° S.) where the marine fauna is eminently tropical and nearly identical with that of Panama.—A. E. V.

progress of the science. It contains references to the writings of fifty-two authors, with indications of the species referred to, including 335 described during the year as new, in American works.

6. *Antero-posterior Symmetry, with special reference to the Muscles of the Limbs;* by ELLIOTT COUES, A.M., M.D., Ph.D.—Dr. Coues takes up the idea, well illustrated by Dr. Jeffries Wyman, of antero-posterior polarity in the animal system, and points out the facts sustaining it in the Muscles of the Limbs. His valuable discussion of the subject runs through the several recent numbers of "The Medical Record," (published in New York), from June 1 to September 1; Article v, in the series, is contained in the last mentioned number.

7. *On the Hypothesis of Evolution;* by EDWARD D. COPE. (From Lippincott's Magazine, for July, Aug. and Sept., 1870.)—Prof. Cope has brought an extensive knowledge of science to bear on the question of evolution in animal life, and presents some original views. The subject is too large a one for discussion in this place.

IV. ASTRONOMY.

1. *Elements of the new planet Ate;* by Dr. C. H. F. PETERS. (Communicated by the author in a letter dated, Litchfield Observatory of Hamilton College, Clinton, N. Y., October 23d, 1870.—The following elements of the planet *Ate* (111) were derived from intervals of 18 days, viz: from observations of Aug. 14, Sept. 1 and 19:—

Epoch: 1870. Sept. 0·0. Berlin mean time.

$$\begin{array}{l}
M_0 = 205^\circ\ 17'\ 21''{\cdot}0\\
\left.\begin{array}{l}
\pi = 122\ \ 53\ \ \ 7{\cdot}3\\
\Omega = 306\ \ 26\ \ 28{\cdot}4\\
i = \ \ \ 5\ \ \ 1\ \ 21{\cdot}4
\end{array}\right\}\text{Mean Equ. 1870·0.}\\
\varphi = \ \ \ 5\ \ 49\ \ 10{\cdot}6\\
\mu = 858''{\cdot}392\\
\log a = 0{\cdot}4108808.
\end{array}$$

They represent an observation obtained last night still within a few seconds.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *On the Chemistry of the Bessemer process.*—The following is an abstract of the paper on the Chemistry of the Bessemer process, read before the American Association for the Advancement of Science, at its Troy meeting, by Lieut. C. E. Dutton, U. S. A.

After some general statements concerning the chemical composition of crude pig iron, Lieutenant Dutton passed to discuss the theoretical changes possible when air is brought in contact with it in a melted state. According to his view, the silicon is first oxydized, then the phosphorus, next the manganese and sulphur, and lastly, the carbon. Chemically, there is nothing new in the Bessemer process; it may be said to be "the employment of entirely new mechanical methods and appliances for effecting old and familiar reactions." The process itself was then minutely described. Since the sulphur is but partially removed and the phosphorus not at all, the iron selected must be free from these substances. It must also contain at least two per cent of silicon. This iron is melted in a cupola furnace, run into the converter—a charge being 12,000 pounds—and the blast turned on. "The first change is in the oxydation of iron and silicon. The silicon becomes silicic acid and enough of the iron oxydizes to satisfy the affinities of the acid and does not decompose during the remainder of the blast. It is during this stage of the conversion that the remarkable heat of the conversion is developed. It will be remembered that when silicon oxydizes, it takes up three equivalents of oxygen. Carbon takes up only one in this process, becoming carbonic oxyd. It is a common error to suppose that any very great quantity of heat is generated by the combustion of the carbon,—that is, as compared with that derived from the silicon. * * * The heat generated by the silicon burning to silicic acid will be found by the application of the coefficients and formulæ of the mechanical theory of heat, to be from two and one-half to three times greater than that generated by the burning of the carbon to carbonic oxyd. Another circumstance of importance is that the silicic acid remains as a dense fluid in the converter, no part of its heat being lost, except such as is carried out of the converter by the atmospheric nitrogen; and none is rendered latent by converting it into vapor; while the carbon is vaporized, a physical change absorbing much heat, and the vapor thus formed is carried out of the converter at a very high temperature. Hence will be seen the necessity of employing irons containing high percentages of silicon. At least two per cent of this element is essential, any less quantity being insufficient to generate heat enough to keep the iron thoroughly liquid and fluent until the end of the casting process. It is often asserted that irons for Bessemer conversion must be 'gray irons,' as they are called; i. e., irons rich in carbon. Now, although this happens as a rule, to be true enough, it is apt to lead to misapprehension. The fact that Bessemer pig irons are carbonized varieties is an accident, and not an essential feature. What is essential is that it should contain a large quantity of silicon, and very little—indeed, the least possible—of sulphur, phosphorus, and manganese. Now, a pig-iron containing much silicon and no sulphur or manganese, is pretty sure to contain a high percentage of carbon, as all smelters are aware. This fact is a feature of the blast-furnace, and almost without exception. If an iron could be produced with much silicon, a little carbon, no phosphorus, it would I think, be not altogether unsuited to the Bessemer treatment. In a word, the quantity of carbon is approximately immaterial, except so far as it implies proper conditions with respect to other elements. The main element required is the silicon and not the carbon." Of phosphorus, "the arch-enemy of the iron-maker, but the very scourge and pestilence of the steel-maker,"—fifteen-thousandths of one per cent ruining Bessemer metal past all remedy,—Lieut. Dutton said: "I have already ventured the opinion that phosphorus increases its affinity for iron with every increase of heat; at least relatively if not absolutely. The fact seems to be, absolutely. If we accept it, we can instantly explain what seem, otherwise, to be many anomalies and contradictions. It will explain to us why, in the great heat of the blast-furnace, it leaves every other combination and enters the iron; why, in the much lower heat of the puddling furnace it seems to waver between staying with the iron, or forming a new alliance with oxygen, ready to choose either, at the influence of any third substance which may affect the question; why in the Bessemer process it clings to the iron with a desperate tenacity which nothing seems able to resolve. These three facts, then, are all of them formidable: 1st. That a minute quantity of phosphorus is capable of working terrible injury, and that it is omnipresent throughout nature; 2d. That whatever quantities of it are charged into the blast-furnace, as fuel, flux, or ore, are almost wholly concentrated in the resulting pig-iron; and 3d. That no portion is eliminated in the Bessemer converter." The sudden change of the flame at the close of the conversion Lieut. Dutton thus explained: "When two combustibles are intermixed, like oxygen and hydrogen, or hydrocarbon gas, it is well known that the relative proportion of the two elements in the mixture influences the readiness with which they combine. Thus oxygen and hydrogen cannot combine explosively unless their proportions lie within certain definite and rather narrow limits. May not the same law hold good in the present case? It is certain, or nearly certain, that the iron either does not oxydize in the bath during the blow, except in quantity sufficient to furnish a base for the acids present; or, if it oxydizes beyond that, it is immediately reduced again, leaving little or no free oxyd of iron in the bath. But after the change of flame all this is reversed and iron oxydizes rapidly and freely, and remains undecomposed, while the residual traces of the other elements as suddenly cease to oxydize rapidly. I freely grant that in referring this back to what is supposed to be a conceded, but unexplained law, we are merely putting the question in another, a more general, and more abstract shape:—still it is, in a qualified sense, an explanation." The theory of the action of the spiegeleisen, run in after the blast is stopped is next discussed, and also the quality of the metal produced, which Lieut. D. calls a "cast wrought-iron." The paper closed with some comparisons of Bessemer with other metal, and a discussion of the uses for which Bessemer metal is most valuable. G. F. B.

2. *Earthquake of October 20th*, 1870.—On Thursday morning, Oct. 20th, an earthquake vibration was felt throughout Canada, and the northern part of the United States from Maine to Iowa. It seems to have been more severe in Canada and in New England. In many places the shock was sufficient to throw down chimnies, crack the walls of buildings, and do other damage. It was remarkably severe for the region of country visited.

At New Haven, as well as in many other places, there were two distinct series of vibrations. Prof. Twining has carefully collected information from several persons as to the time of the occurrence and duration of the vibrations. The beginning of the first shock was at 11^h 19^m 45^s A. M. New Haven mean time. It lasted ten seconds, and its individual vibrations were about two-thirds of a second in duration, or one and one-third second for a complete double vibration. After an interval of five seconds there was a second series like the first, lasting eleven seconds.

The motion was not a simple oscillation but there was a rocking motion, indicating a vertical component in the movement of the earth. The vibrations were not severe enough to arrest universal attention, though multitudes felt a peculiar sensation without recognizing the cause. The direction of the vibration was N.N.E. and S.S.W.

At Cambridge, Mass., according to Prof. Winlock, the direction was about 10° north of east, as determined by the appearance of the sides of a vessel containing milk.

Mr. Farmer, at Boston, gives 11^h 25^m 37^s for the time of the ending of the vibrations, Cambridge mean time. This would imply that the shock reached Boston a minute and three-fourths earlier than New Haven.

At Cleveland, Ohio, several clocks were stopped by the earthquake, each indicating very nearly 10^h 45^m A. M. This is approximately the instant at which the shock reached New Haven.

It is reported that the shock reached Quebec 30 seconds before it did Montreal. the telegraph operator of the former city being in the act of inquiring of the operator in the latter one respecting the earthquake when it arrived at Montreal. These data seem to show that the general progress of the wave was from North to South.

Slight vibrations were felt as far south as Richmond, Va., and as far west as Dubuque, Iowa. H. A. N.

3. *Kansas Natural History Society.*—(From the Secretary of the Society.)—The third annual meeting of this Society was held in Lawrence, Kansas, the first week in September, in the University Building. Gen. John Fraser, President of the State University, delivered a lecture, the first evening of the session, "On the Aims, Organization and Advantages of Scientific Associations," in which he recommended an enlargement of the scope ot the Society to cover the whole field of scientific observation and investigation. On the following evening Rev. John H. Barrows, of Arvonia, delivered an address in the Presbyterian church on "Hugh Miller, or The Workingman's Education." At the close of the lecture President Fraser, who spent his youth in Cromarty, gave some personal recollections of the great geologist.

The following papers were read before the Society: "A Catalogue of all the Plants of Kansas, as far as observed," by Prof. J. H. Carruth, of Lawrence. The catalogue contains about 500 species—30 being added by members of the association. "On the Fishes of the Kansas River, as observed at Lawrence," by Prof.

F. H. Snow, of Lawrence. This paper describes 27 species of fish. "On the Internal Heat of the Earth," by Rev. John D. Parker, of Burlington. "On a Comparison of the Coals of Kansas with other Western Coals," by Prof. Wm. H. Saunders, M.D., of Lawrence. "On the Saurian Formation and Moss Agate of Kansas," by Prof. B. F. Mudge, of Manhattan. Prof. Mudge exhibited to the Society some fine specimens gathered during his recent visit to the Rocky Mountains, among which was a fine, well-preserved Saurian.

Officers of the Society for the current year: Gen. John Fraser, President; B. F. Mudge, Vice President; John D. Parker, Secretary and Librarian; Frank H. Snow, Treasurer; B. F. Mudge and F. H. Snow, Curators. Curators of Departments: B. F. Mudge, Geology; F. H. Snow, Entomology; Wm. H. Saunders, Chemistry; John D. Parker, Meterology; J. H. Carruth, Botany.

4. *Corrections of errata in the "Notes on the structure of the Crinoidea, &c."*—In page 226, in the 20th line from the top, for "*Spænonites*," read "*Sphæronites*;" page 228, in the 5th line, for "Miller," read Müller;" page 230, in the 17th line for "ovo-anal," read "oro-anal;" in page 232, for "Crinoidea," read "paleozoic Crinoidea;" and in page 233, 11th line, for "interradial," read "internal." I desire also to express my obligations to Prof. Alexander Winchell, State Geologist of Michigan, and to Mr. Charles Wachsmuth of Burlington, Iowa, for their kind assistance in lending me a large number of beautifully preserved specimens.

E. BILLINGS.

OBITUARY.

Capt. James Pedersen, whose name is associated with numerous new and rare species of corals, echinoderms, shells, etc., described in this Journal and elsewhere, as well as with the very extensive collections which he has contributed to the Museum of Yale College, and of which portions have been distributed to many other Museums, both in this country and in Europe, died in San Francisco, Aug. 19th. He was born at Christiania, Norway, February 24th, 1811. His father, Sars Pedersen, and his ancestors for several generations, were sea-faring men, and he seems to have early developed the same taste. In 1822, and for several years subsequently, he sailed in his father's vessels between Christiania and Copenhagen, and by his visits to the celebrated museums and galleries of the latter city, he acquired a taste for the rare and curious in nature and art, which in later years led him to become an enthusiastic and intelligent collector of objects of Natural History. In 1828–9 he studied navigation and took his diploma, and afterward sailed to St. Petersburg. In 1830 he came to this country, and subsequently made New York his home. He sailed for 19 years between New York and Havana, New Orleans, Vera Cruz, Honduras, etc., but in 1849 he took his vessel to California, and since then has spent most of his time in various parts of the Pacific and Indian Oceans, being for some time on the South American coast, and some years in China and Japan. The last years of his life were spent at La Paz, Lower California, where he engaged to a considerable extent, in the pearl fishery, and from whence he sent the extensive zoölogical collections which adorn the Museum of Yale College. These collections are no doubt the largest that have ever been made in the Gulf of California, and when fully described will add greatly to the knowledge of that rich fauna. He was obliged to leave La Paz last December, on account of failing health and the excessive heat, but after his arrival in San Francisco, was still mindful of the interests of science, and continued to send valuable collections as often as he was able. He was a thoroughly temperate, Christian man, who has done much in his humble way, to advance science. His collections will be an enduring monument to his memory.

Dr. W. A. **Miller**, the author of "Miller's Elements of Chemistry," and of various memoirs on chemical subjects published in the Philosophical Transactions and elsewhere, died at Liverpool, of apoplexy, on the 30th of September. He was born at Ipswich in December, 1817. In 1861 he was elected Treasurer of the Royal Society. He occupied for some years the office of President of the Chemical Society, and was one of the assayers to the Royal Mint.

Dr. A. **Mattheissen**, a chemist of high promise, and one of the examiners of the University of London, died recently, in his 39th year.

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *Transactions of the Connecticut Academy of Arts and Sciences*, Vol. II, Part I. New Haven, August, 1870. 210 pp. 8vo, 7 lithog. plates. Price $3.—This number includes: Notice of the Crustacea collected by Prof. C. F. Hartt on the coast of Brazil in 1867, by Sidney I. Smith; On the Geology of the New Haven Region, with special reference to the origin of some of its topographical features, by James D. Dana; Notes on American Crustacea, No. 1, Ocypodoidea, by S. I. Smith; On some alleged specimens of Indian Onomatopæia, by J. H. Trumbull; On the Molluscan fauna of the later Tertiary of Peru, by E. T. Nelson. Several of these papers have already been noticed when the author's copies have been distributed. The last, illustrated by two plates, is of special interest as throwing some light on the ancient marine fauna of the Pacific coast of Central America, and its relations to that of the existing faunæ of the same coast and of the Atlantic coast, thus aiding in solving the question of a connection, more or less ancient, between the two oceans across the Isthmus. In this paper numerous new species are described from a formation very rich in fossils near Zorritos, Peru. The formation is probably of Miocene or even earlier age, since only one or two species appear identical with living forms, but the genera are nearly all identical with the common genera now living on the same coast, and the species are often representative of or analogous to living forms of the same region, being apparently much more nearly related to the existing species of Panama, than to either the Miocene or living species of the West Indies. V.

2. *A Treatise on Elementary Geometry;* by Prof. WILLIAM CHAUVENET. Lippincott & Co., Philadelphia.—The reputation of Prof. Chauvenet in science, and the knowledge of his experience as a teacher, will draw peculiar attention to this work upon Elementary Geometry.

In the methods of demonstration Prof. Chauvenet resembles Legendre more than Euclid, though he differs widely from both. He uses the *reductio ad absurdum* rarely, and the various modes of superposition quite extensively. In his treatment of ratios he uses algebraic symbols. The difficulties of incommensurable ratios are met by the early introduction of the idea and theory of limits. He does not adopt the *direction* theory of parallels, believing that it does not meet all difficulties, especially in solid geometry. He introduces the idea of *loci* early in the first Book. To *symmetrical* figures he gives special place and importance. He adds two appendices, the first containing exercises upon the several books, and the second an introduction to modern geometry.

As a preparation for the study of the theory of transversals and other branches of modern Geometry, the introduction of algebraic symbols in elementary demonstrations is of great value. The modern Geometry has greater power than that of Euclid for two reasons: first, it uses the *plus* and *minus* of algebra; and second, it implicitly uses the roots of quadratic equations, real and imaginary.

On the other hand, the use of algebraic symbols in elementary geometry, has, we believe, a disadvantage. The statement of the steps of the syllogism is not so lucid.

The introduction of the idea of limits early in geometry is a good feature of the work. Students have in decimal fractions and their applications been accustomed to incommensurable ratios, and we believe that such early use is not premature.

For the sake of discipline in clear and distinct statement and close consecution of thought, we admit a special liking to the first six books of Euclid. But Prof. Chauvenet's work more nearly satisfies our idea of a good geometry than any other modern text-book. H. A. N.

3. *Grundzüge einer allgemeinen Theorie der Oberflächen in synthetischer Behandlung;* von LUDWIG CREMONA. Translated into German by M. Curtze. Berlin, 1870. (S. Calvary & Co.)—This volume is ostensibly a translation of the memoirs of the distinguished Italian Geometer, but it is in fact much more. Rather more than half of the volume is new matter, added by Prof. Cremona, or under his supervision.

The work is an extension to surfaces of Prof. Cremona's method of dealing with plane curves, as shown in his memoir published by the Bologna Academy, *Introduzione ad una teoria geometrica delle curve piane.* This important work had previously been translated by M. Curtze into German.

These German translations render Cremona's important contributions to pure Geometry much more accessible than they are in the original Italian. H. A. N.

4. *A historical and descriptive narrative of the Mammoth Cave of Kentucky*, including explanations of the causes concerned in its formation, its Atmospheric conditions, its Chemistry, Geology, Zoology, etc.; with full scientific details of the eyeless Fishes; by W. STUMP NORWOOD, M.D. 225 pp. 12mo, with illustrations. Philadelphia, 1870. (J. B. Lippincott & Co.)—All future visitors to the Mammoth Cave will have occasion to thank Dr. Norwood for the service he has rendered the tourist. The book contains several interior views of the cave, which give some idea of the wonders within, although a very inadequate one. They should be supplemented by a good map of the extensive windings of the cavern. The author estimates the extent of the various avenues and mazes of the Mammoth Cave at over one hundred and fifty miles. An instrumental survey would probably shorten this assumed distance, but we know from our own wanderings in it that its extent is immense. The chapter on the eyeless fishes is made up mostly from the writings of Agassiz and Wyman, and presents the facts on the subject in an attractive form, well adapted to instruct and interest the general reader.

5. *Tent life in Siberia, and Adventures among the Koraks and other Tribes in Northern Kamtchatka and Northern Asia;* by GEORGE KINNAN. 425 pp. 12mo, with a map. New York, 1870. (G. P. Putnam & Sons, London; S. Low, Son and Marston).—A very readable and instructive narrative of travel in a little known region of the globe, is this neat volume of Mr. Kinnan's. The costly and disastrous enterprise of the Western Union Telegraph Company, designed to establish electrical communication between America and Asia through Kamtchatka and Northern Asia, has been by no means fruitless in scientific interest. Already Messrs. Whymple and Dall have given us instructive and valuable contributions upon their wanderings in British Columbia and Alaska. Mr. Kinnan makes no claim to any special devotion to scientific investigation, but his narrative gives a clear and lively account of the physical and social conditions of a country rarely visited, and full of curious interest. His volume well repays perusal, and is obviously the work of a good observer and careful historian, who is never tedious, and has the agreeable art of carrying his reader along with him.

6. *Report of the Superintendent of the U. S. Coast Survey, showing the progress of the Survey during the year* 1867. 344 pp. 4to, with 28 maps.—This volume, besides the Report of the Superintendent, Prof. Peirce, contains, among the articles in its Appendix, a Report on Transatlantic determination of Longitude, by Dr. B. A. Gould; on comparison of Meters, by Dr. F. A. P. Barnard and M. Tresca; on a new form of Reflector for geodetic signals, by J. E. Hilgard; on the Tides and Currents of Hell Gate, by Henry Mitchell; on Soundings in the Gulf Stream, by H. Mitchell; on the Fauna of the Gulf Stream, by L. F. Pourtales; on Alaska Territory, by G. Davidson; obituary of Alexander Bache.

7. *Treatise on the power of Water as applied to drive Flour Mills and to give motion to Turbines and other Hydrostatic*

Engines; by JOSEPH GLYNN, F.R.S., member of the Institute of Civil Engineers of London, etc. 3d ed., revised and enlarged. 162 pp. 12mo, with numerous illustrations. New York, 1869. (D. Van Nostrand).—This is a popular and practical treatise on water as a motive power. It is simple in its explanations and mathematics, and is well illustrated.

8. *Archives of Science and Transactions of the Orleans Co. Society of Natural Sciences.*—This first number of the Archives of a new Natural History Society, at Newport, Vermont, contains the following papers: On the characters and customs of the Pawnees, by Rev. T. E. Ranney; Qualitative Analysis of the Mineral Springs of Essex Co., Vt., by H. A. Cutting; on the Indian History of Northern Vermont, by Wm. W. Grout, Esq.; Meteorological register, by J. M. Currier; on a new mounting for microscopic objects, H. A. Cutting.

9. *The American Entomologist and Botanist.*—This St. Louis Journal will be suspended during the year 1871, to be resumed with the commencement of the following year.

10. *Geology and Revelation, or the Ancient History of the Earth, considered in the light of Geological facts and Revealed religion,* with illustrations; by the Rev. GERALD MOLLOY, D.D., Prof. Theol. Roy. Coll. St. Patrick, Maynooth. 380 pp. 12mo. New York, 1870. (G. P. Putnam & Sons.)—A handsome American edition of the excellent work noticed at page 151 of this volume.

Zeitschrift der Oesterreichischen Gesellschaft für Meteorologie; redigirt von Dr. C. Jelinck und Dr. J. Hann. 4th volume. Vienna, 1869.

Results of Astronomical and Meteorological Observations made at the Radcliffe Observatory, Oxford, in the year 1867, under the superintendence of Rev. Robert Main, M.A., Radcliffe observer. Vol. xxvii.

The Portable Transit Instrument in the Vertical of the Pole Star; translated from the original memoir of Wm. Döllen, by Cleveland Abbe, Director of the Cincinnati Observatory. 48 pp. 8vo. Washington, 1870.

Annual Report of the Director (C. Abbe) of the Cincinnati Observatory. 20 pp. 8vo. June, 1870.

Catalogue of known species, recent and fossil, of the Family Marginellidæ; by John H. Redfield. 56 pp. 8vo. From the Annals of the Lyc. Nat. Hist. New York.

Catalogue of the Birds of Chemung Co., N. Y.; by W. H. Gregg, M.D. 14 pp. 8vo. Elmira, N. Y. From the Proceedings of the Elmira Academy of Sciences.

PROCEEDINGS AMER. PHILOSOPHICAL SOC., PHILADELPHIA. VOL. XI, No. 83.—p. 245. Notices and descriptions of Fossils, from the Marshall group of the Western States, with Notes on Fossils from other formations; *A. Winchell.*—p. 261, On some Etheostomine Perch from Tennessee and North Carolina; *E. D. Cope.*—p. 271, On some Reptilia of the Cretaceous formation of the U. S.; *E. D. Cope.*—p. 284. Extent of the order Pythonomorpha in the Cretaceous rocks of the U. S.; *E. D. Cope.*—p. 285, Fourth contribution to the History of the Fauna of the Miocene and Eocene periods of the U. S.; *E. D. Cope.*—p. 295. Adocus, a genus of Cretaceous Emydidæ; *E. D. Cope.*—p. 299, Periods of certain Meteoric Rings; *D. Kirkwood.*—p. 301, Contributions to a grammar of the Muskokee language; *D. G. Brinton.*—p. 313, Comparison of Mechanical Equivalents; *P. E. Chase.*

PROCEEDINGS ACADEMY NAT. SCI. PHILADELPHIA, No. 2, 1870.—p. 65, Anomalous organs of generation in a hog; *J. Leidy.*—p. 65, Fossil Rhinoceros; *J. Leidy.*—p. 66. Remarks on some Mammalian remains, and on Hadrosaurus and Thespesius; *J. Leidy.*—p. 70, Fossil fishes from the Rocky Mts.; *J. Leidy.*—p. 73, Fossil Ruminant from Iowa; *J. Leidy.* Cranium of an Owl; *T. H. Streets.*—p. 74, New Saurian; *J. Leidy.*—Description of Grasshoppers from Colorado; *C. Thomas.*—p. 84, Huxley's Classification of Birds; *T. H. Streets.*—p. 89, A new Leech; *J. Leidy.*—p. 92, New Fishes from the Upper Amazon and Napo rivers; *T. Gill.*

Electrotype Copy No.1 by G. Mathiot U.S.C.S.

U.S. COAST SURVEY

A.D. BACHE Supdt.

DIAGRAMS SHOWING THE RESULTS

of the

GULF STREAM EXPLORATIONS

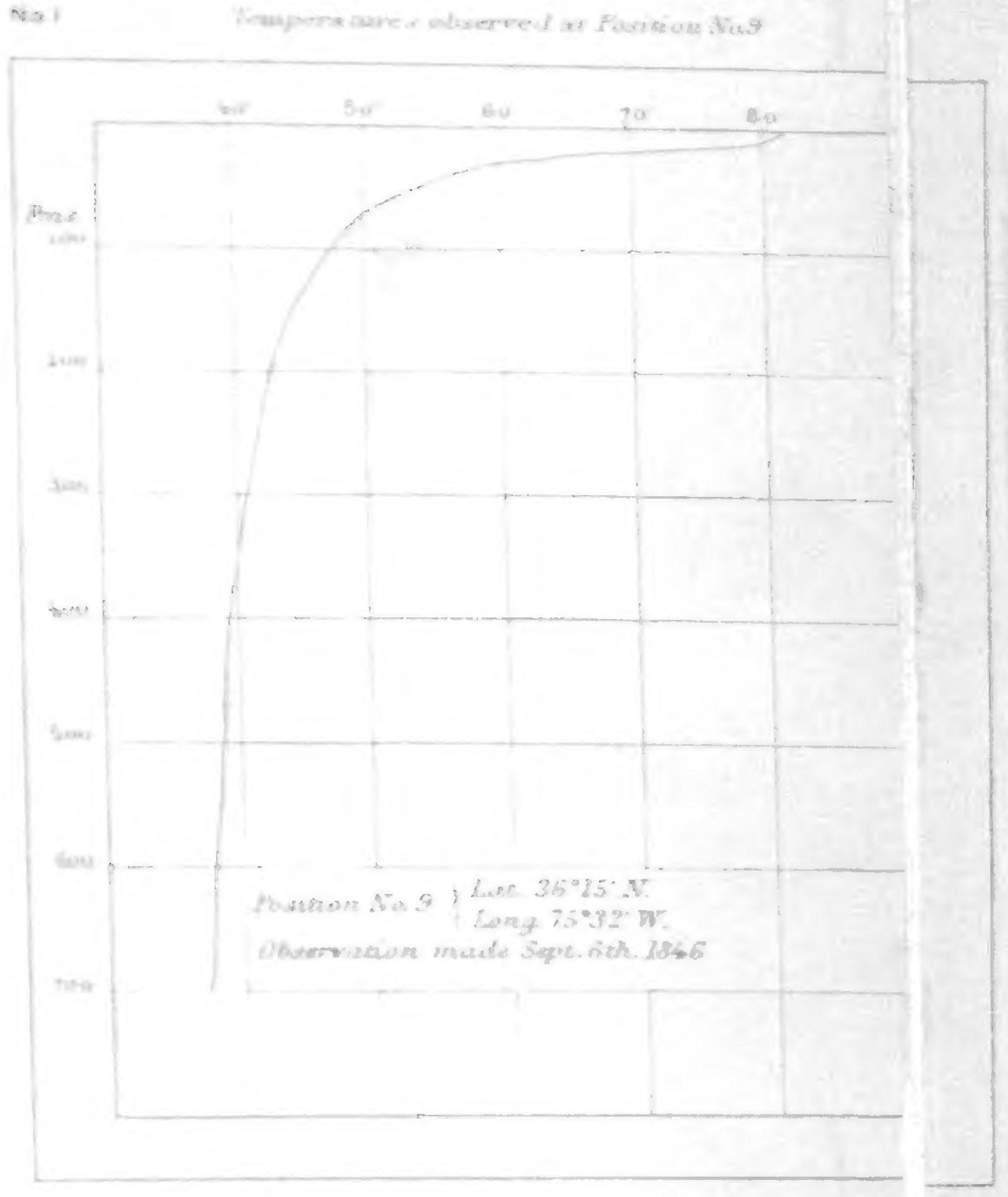
From

Observations made in 1845 '46 '47 '48 & '53

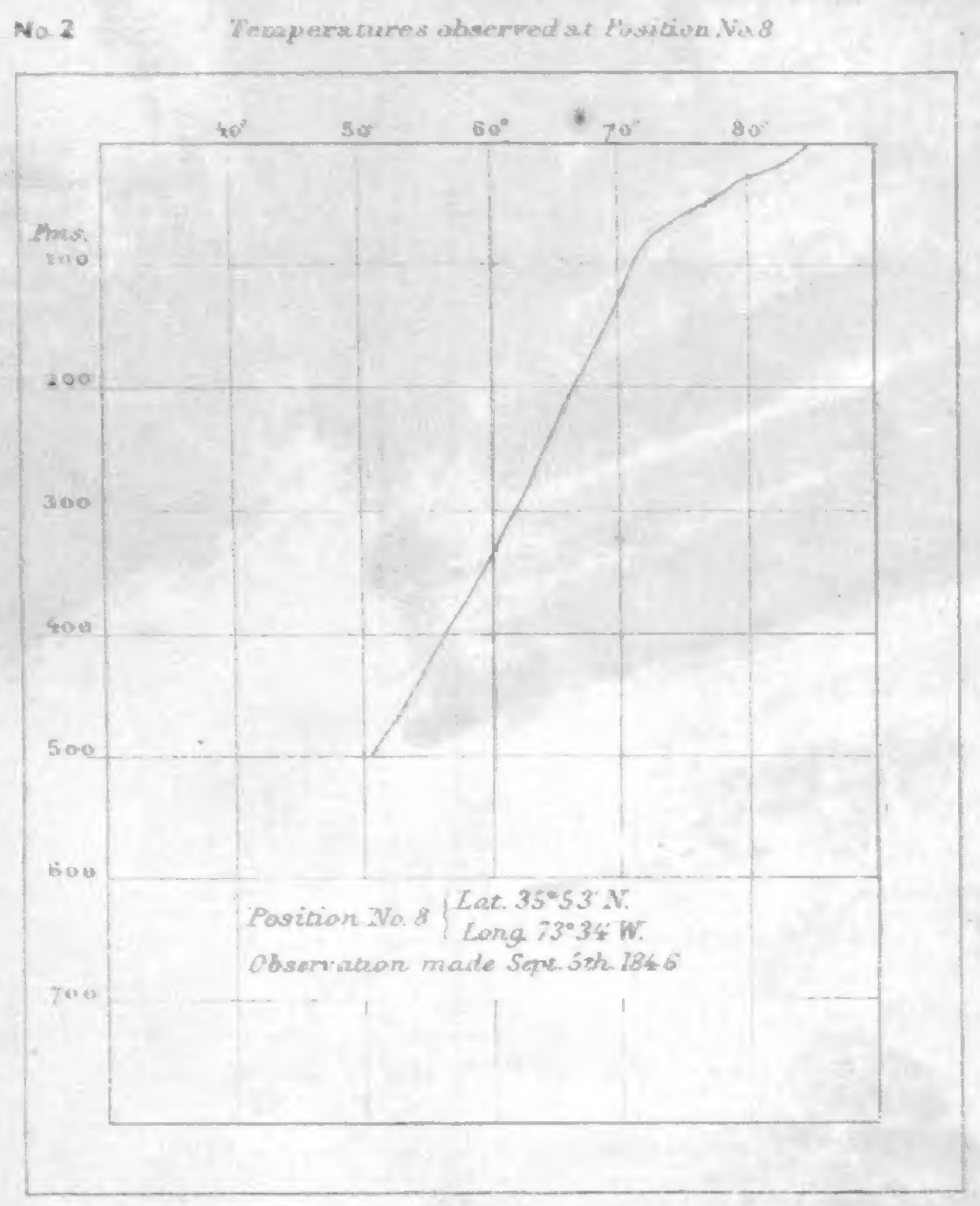
1854

SECTION OFF CAPE HENRY

No. 1 Temperatures observed at Position No. 9

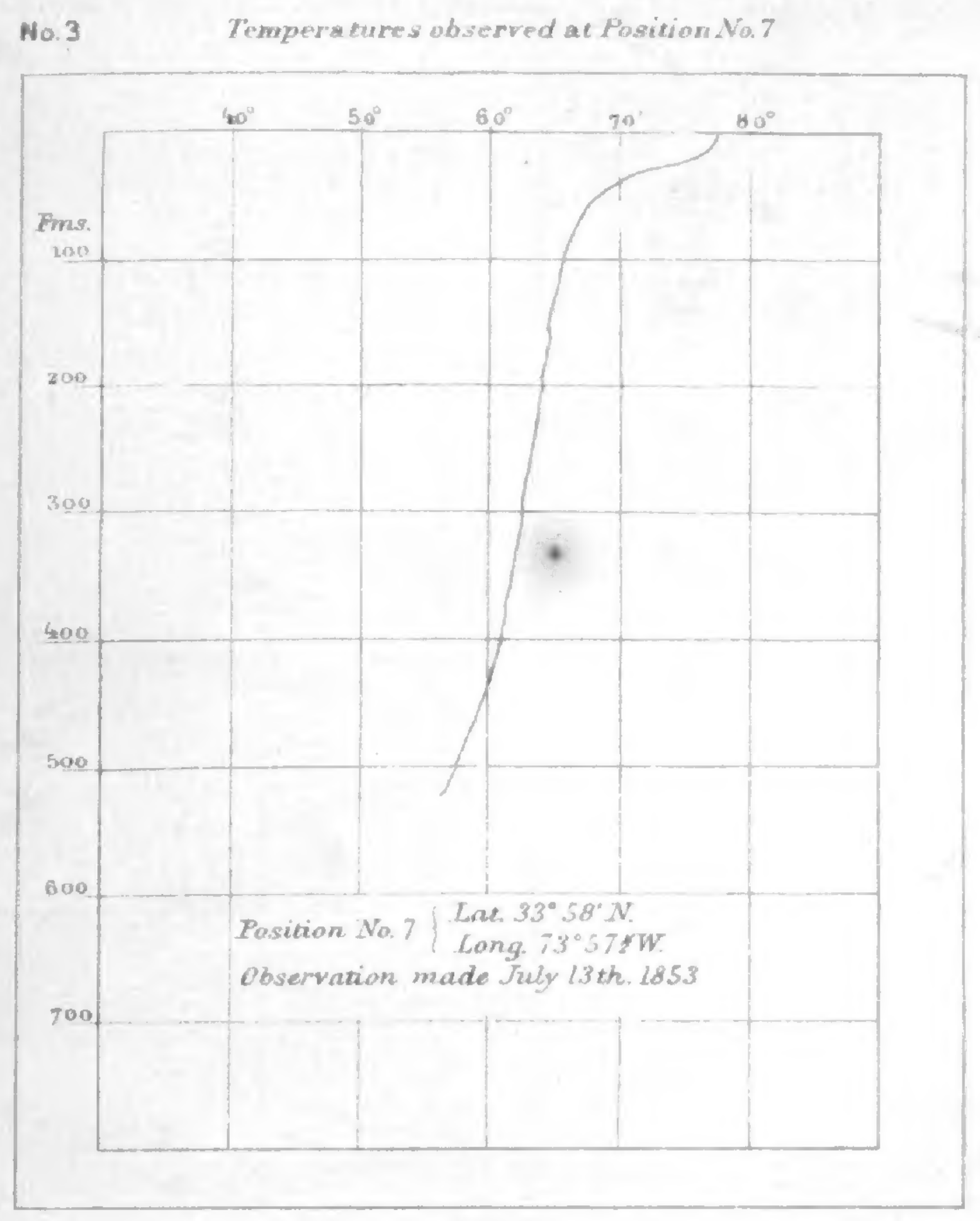


No. 2 Temperatures observed at Position No. 8



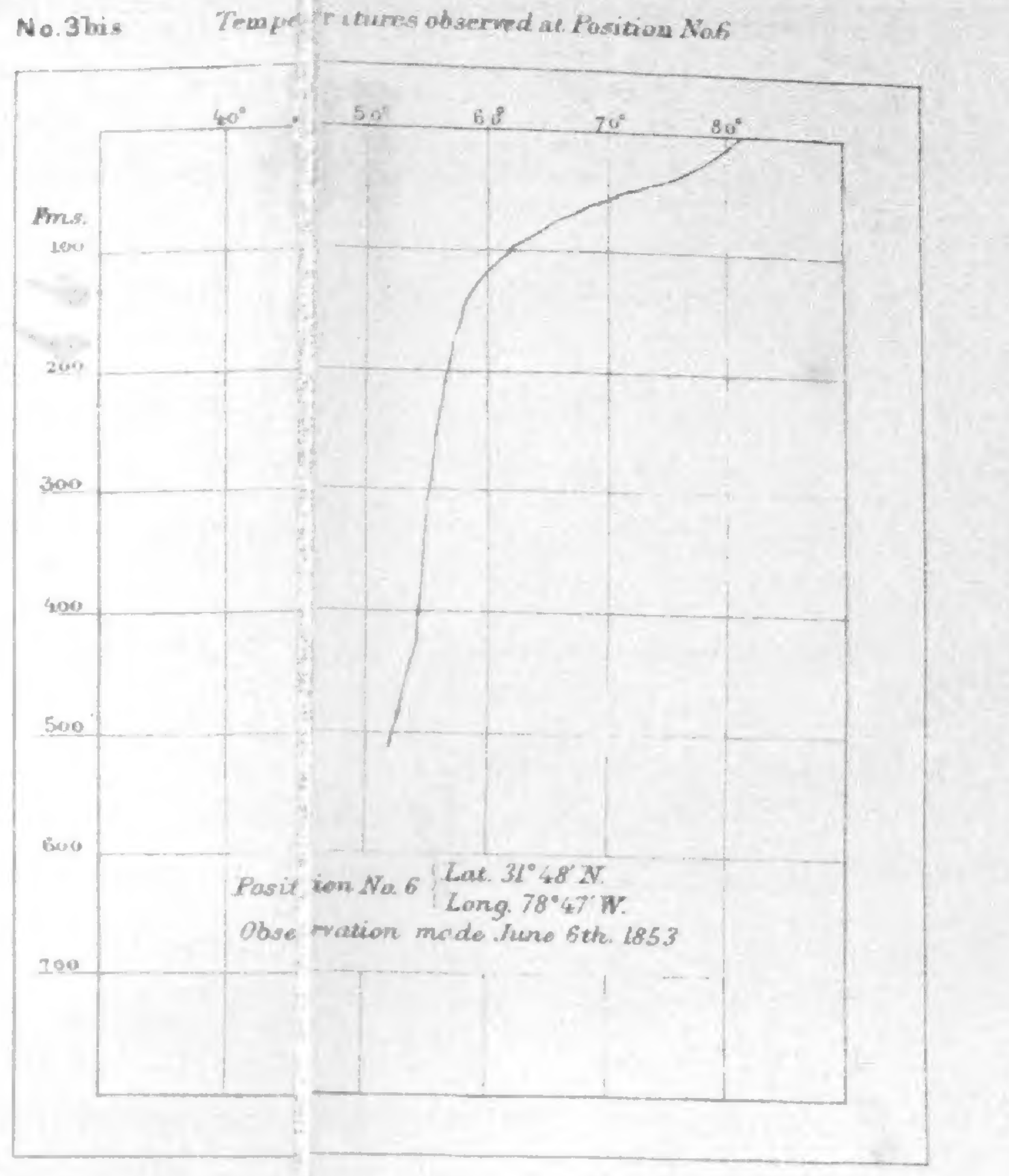
SECTION OFF CAPE HATTERAS

No. 3 Temperatures observed at Position No. 7



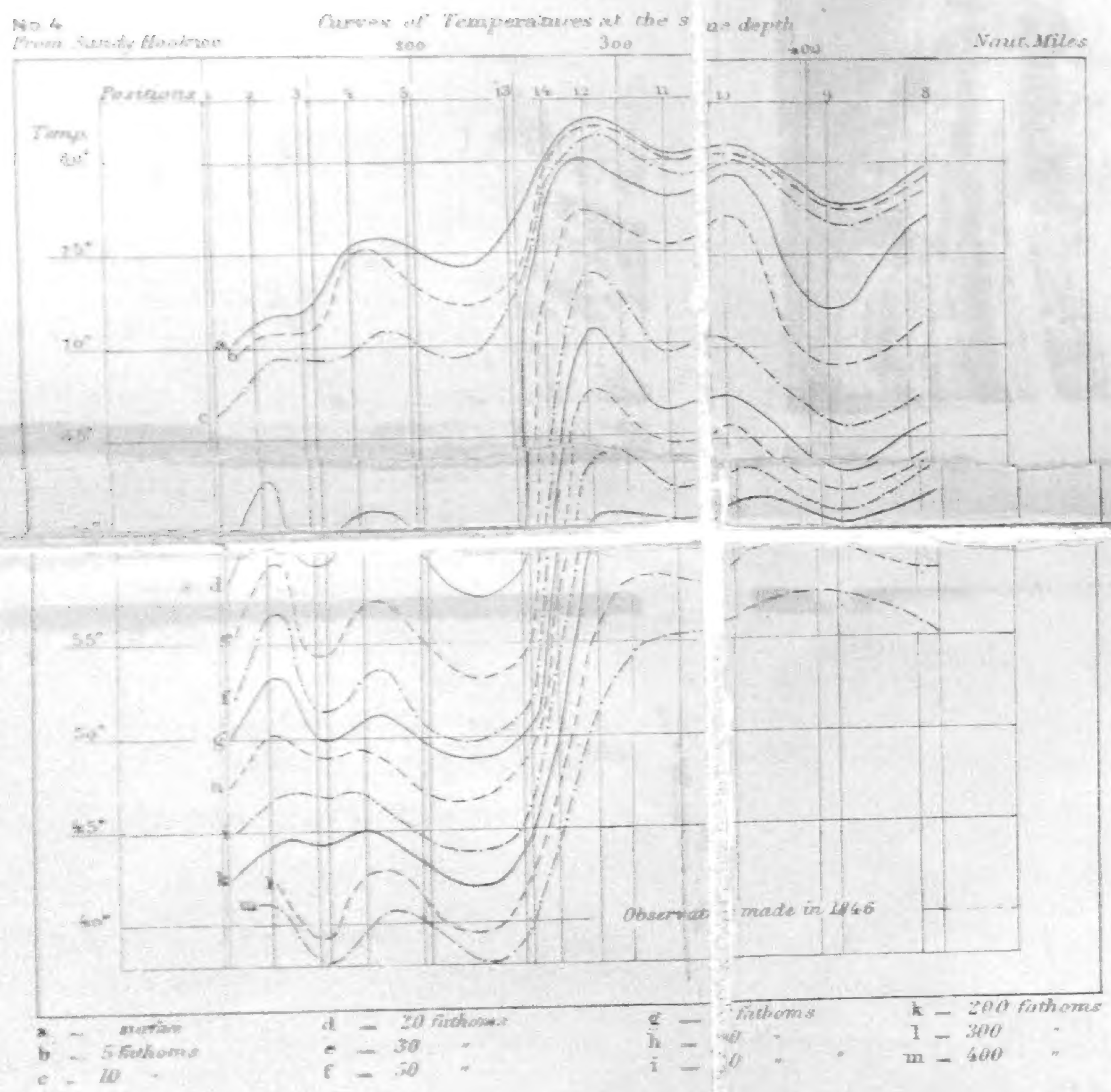
SECTION OFF CHARLESTON

No. 3 bis Temperatures observed at Position No. 6

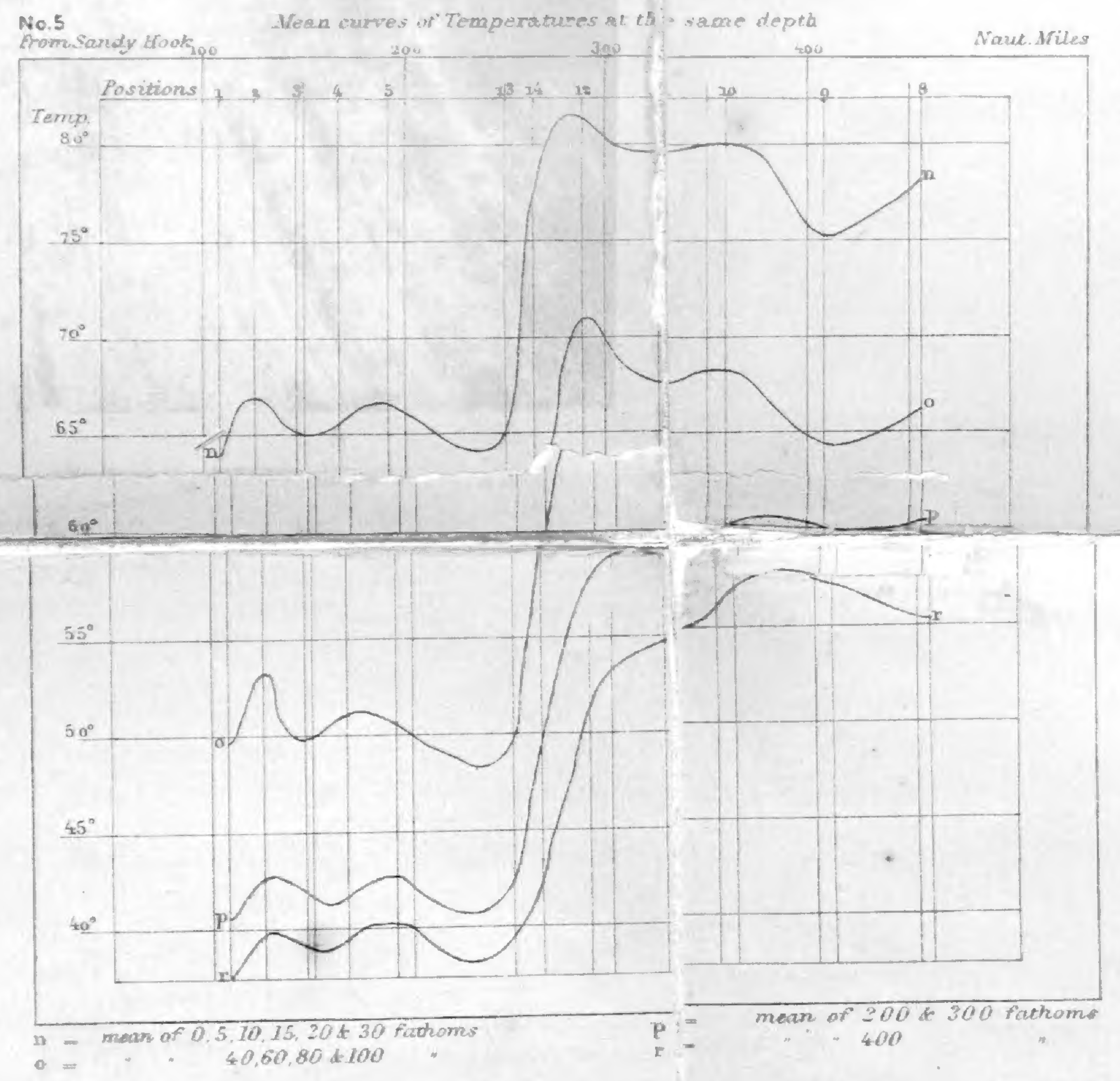


SECTION OFF SANDY HOOK

No. 4 Curves of Temperatures at the same depth

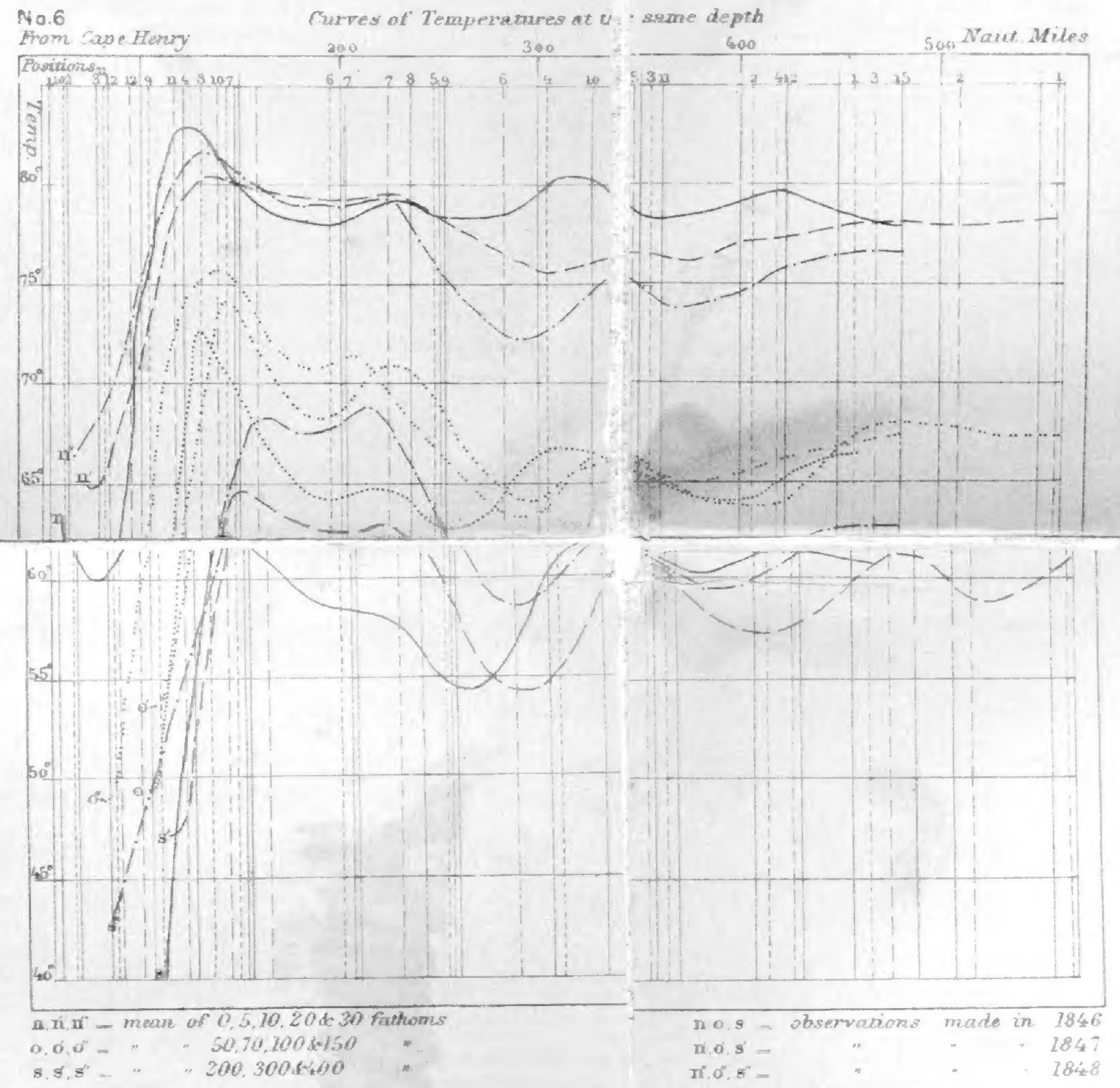


No. 5 Mean curves of Temperatures at the same depth

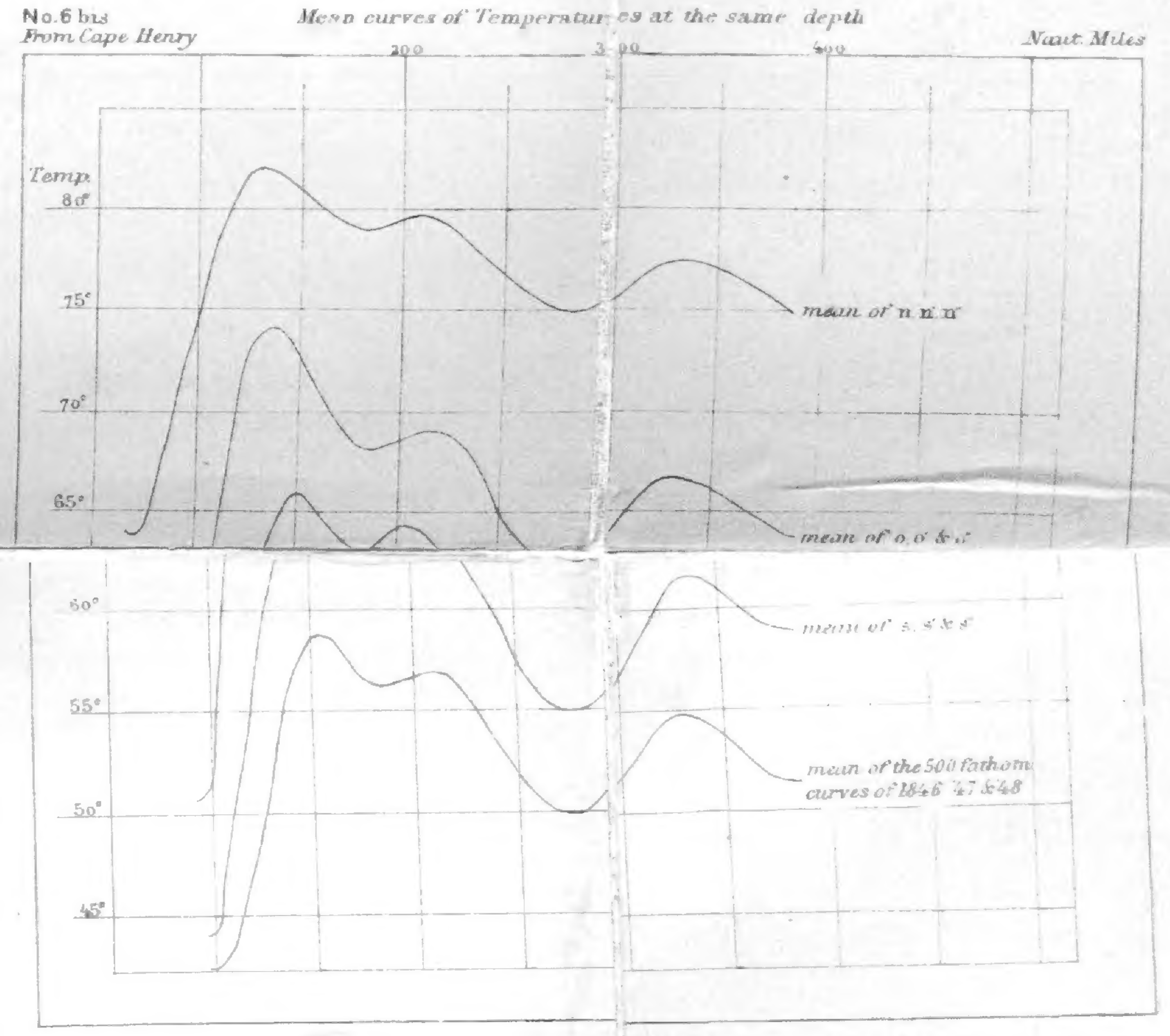


SECTION OFF CAPE HENRY

No. 6 Curves of Temperatures at the same depth

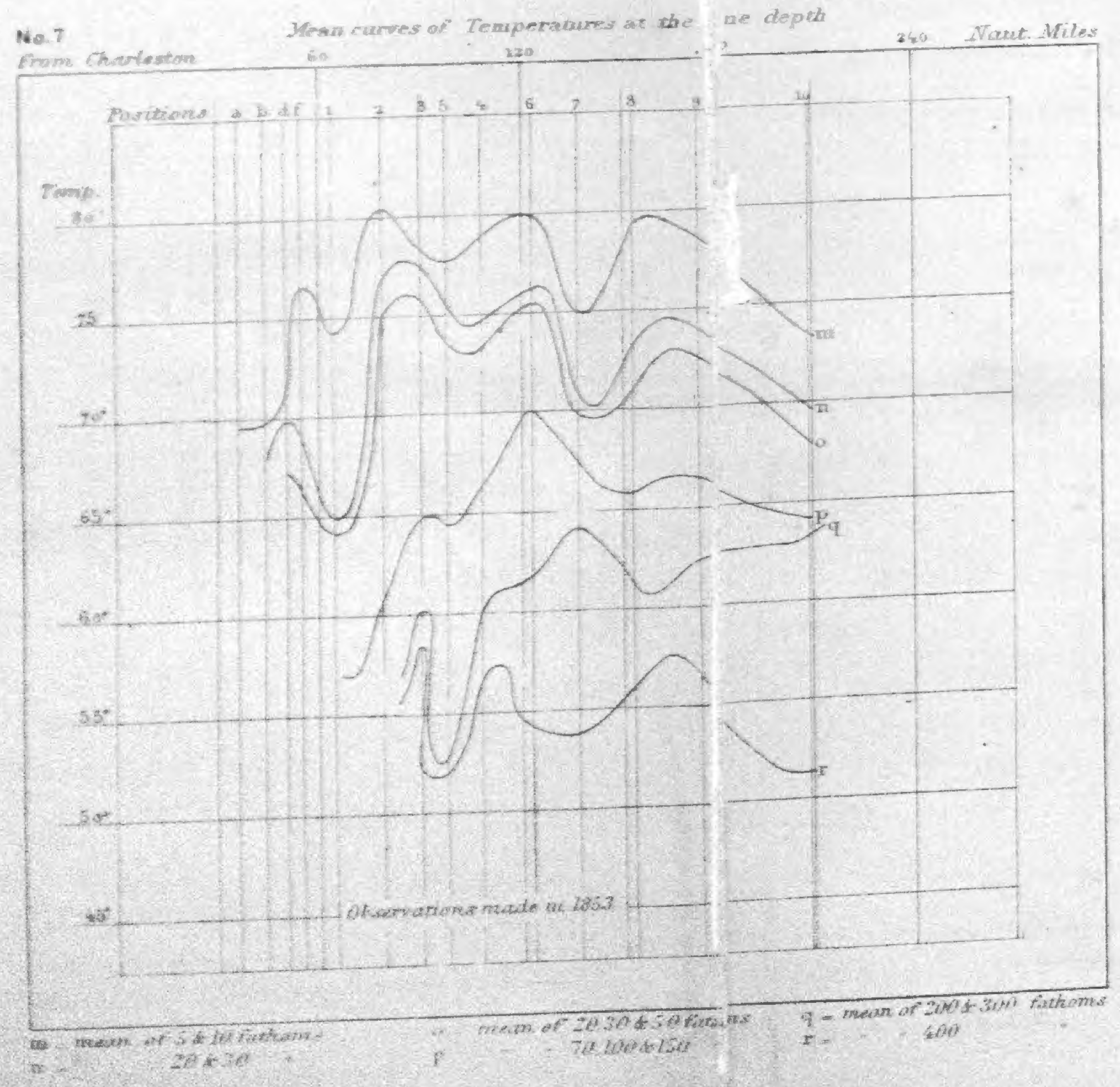


No. 6 bis Mean curves of Temperatures at the same depth



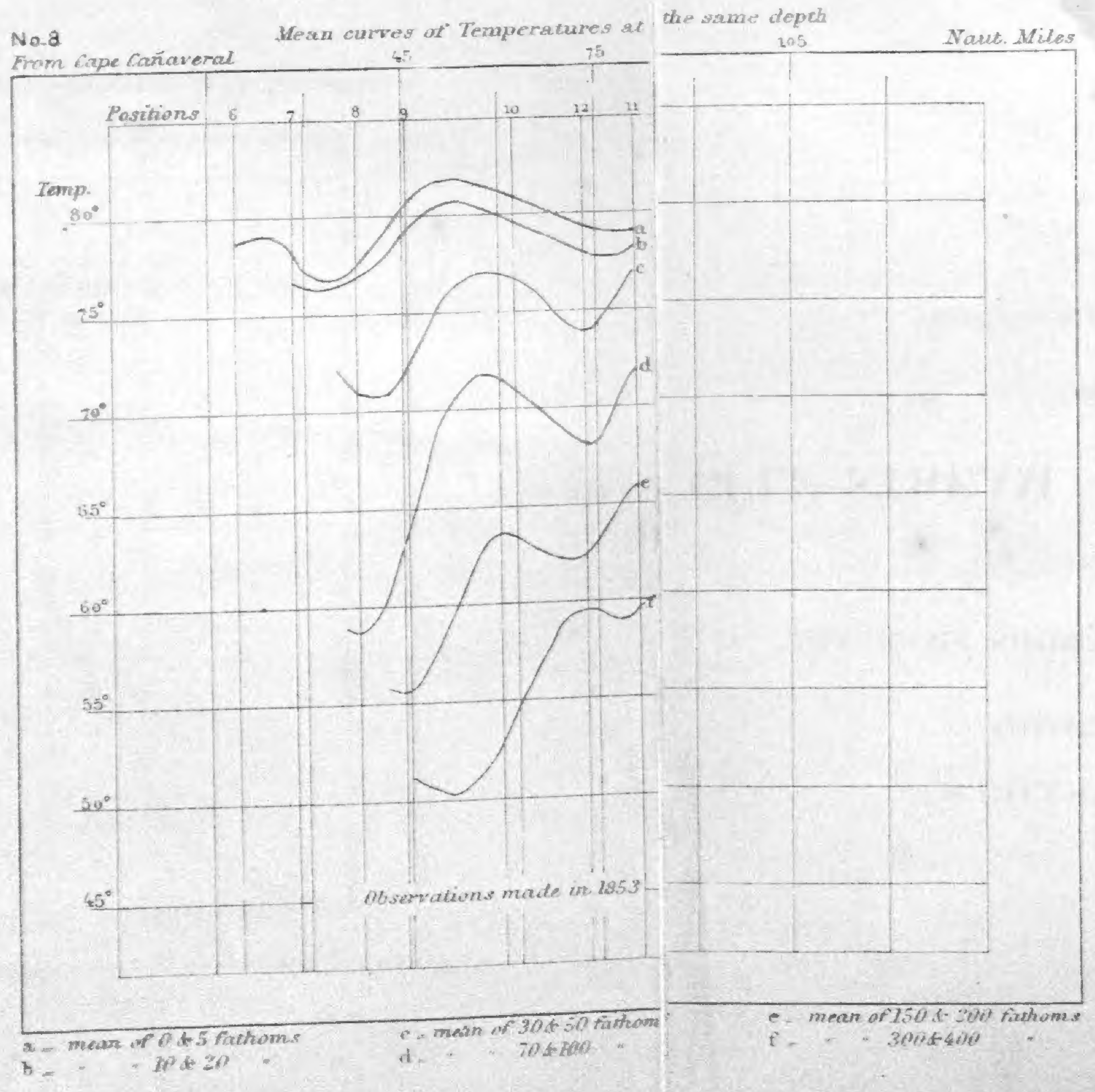
SECTION OFF CHARLESTON

No. 7 Mean curves of Temperatures at the same depth



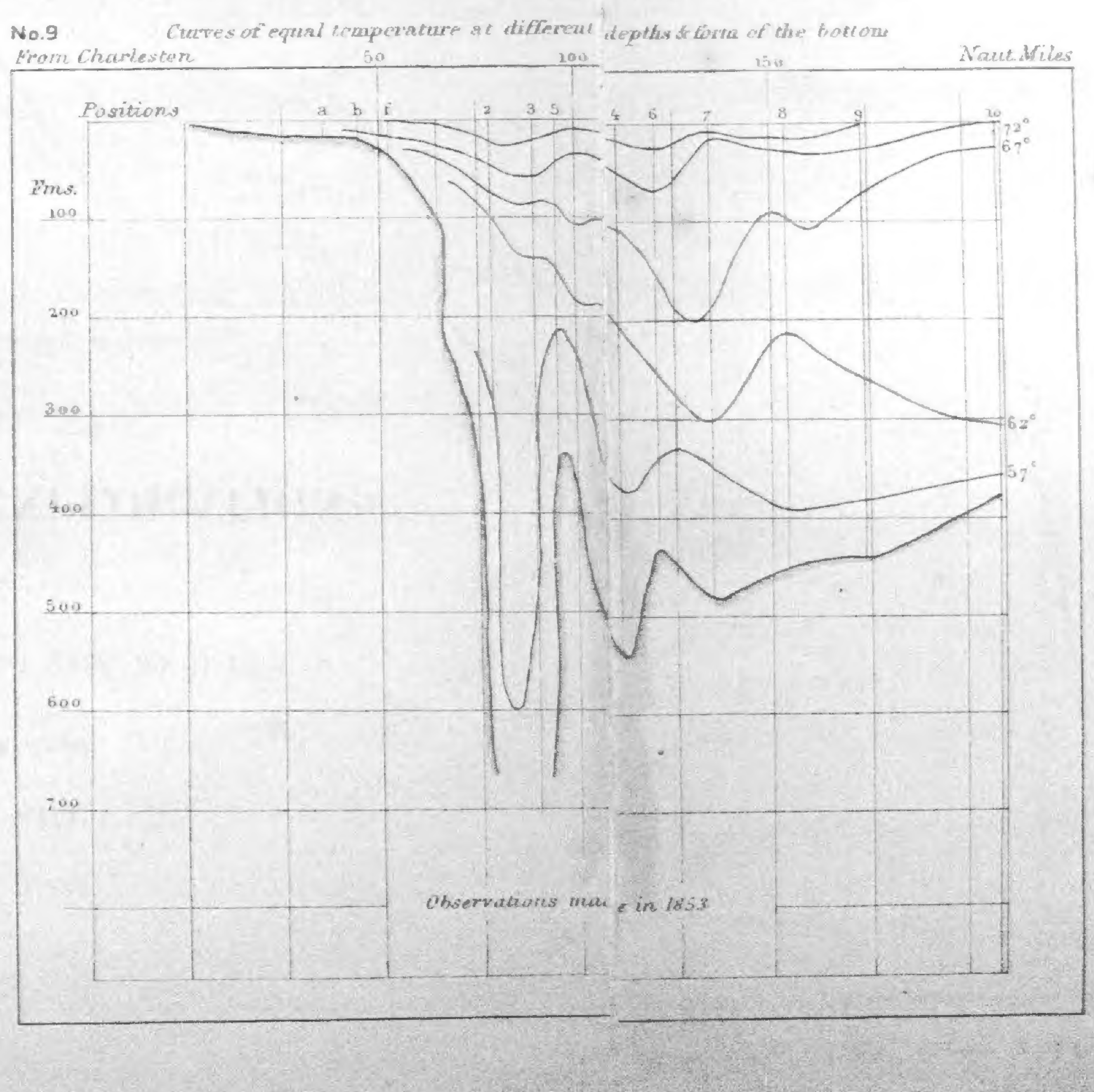
SECTION OFF CAPE CAÑAVERAL

No. 8 Mean curves of Temperatures at the same depth



SECTION OFF CHARLESTON

No. 9 Curves of equal temperature at different depths & form of the bottom



No. 10 Comparative curves of the cold wall

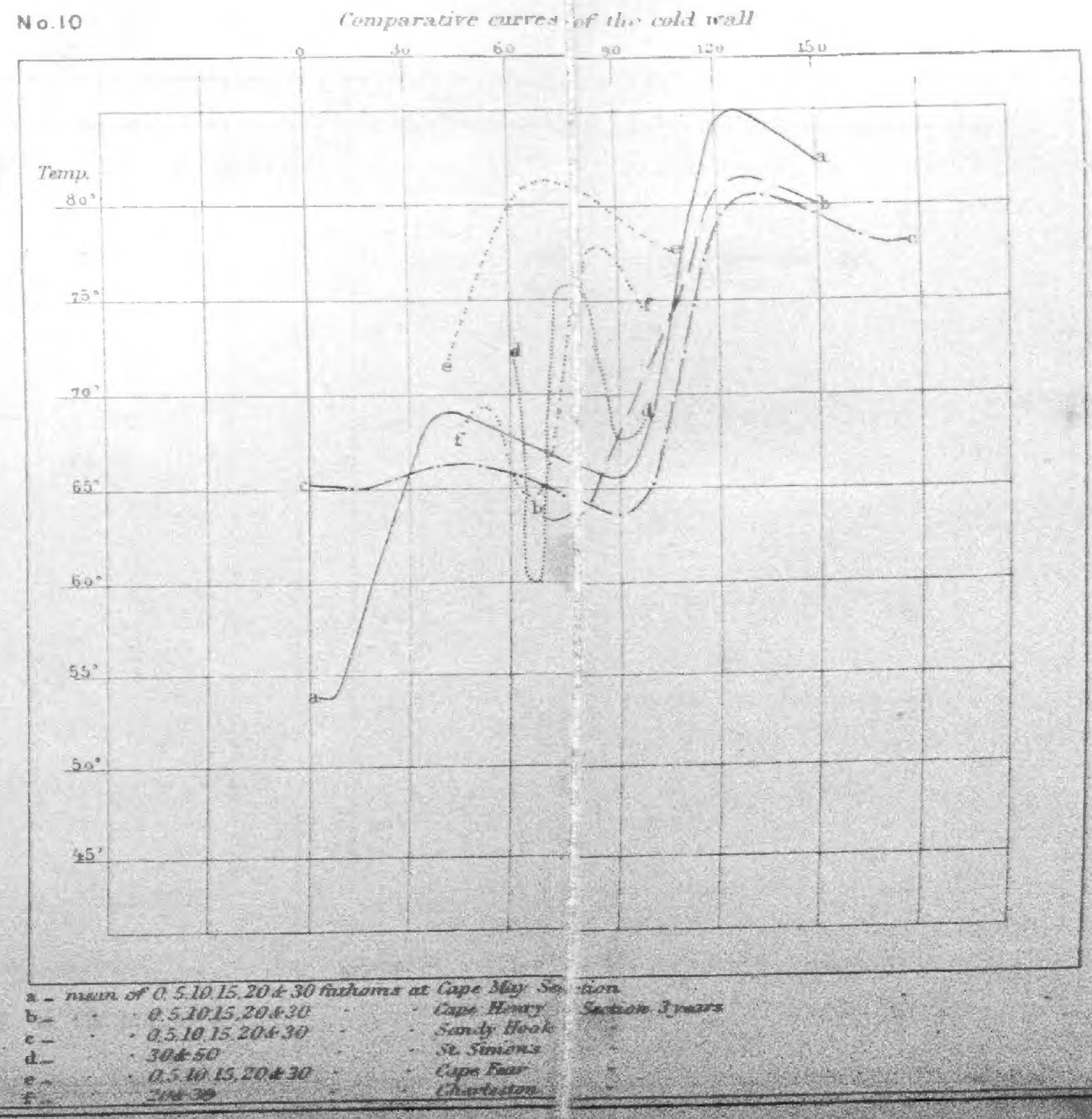


Plate B

Electrotype Copy No 1 by G. Mathiot U.S.C.S.

U.S.COAST SURVEY

A.D.BACHE Supdt.

DIAGRAMS SHOWING THE RESULTS

of the

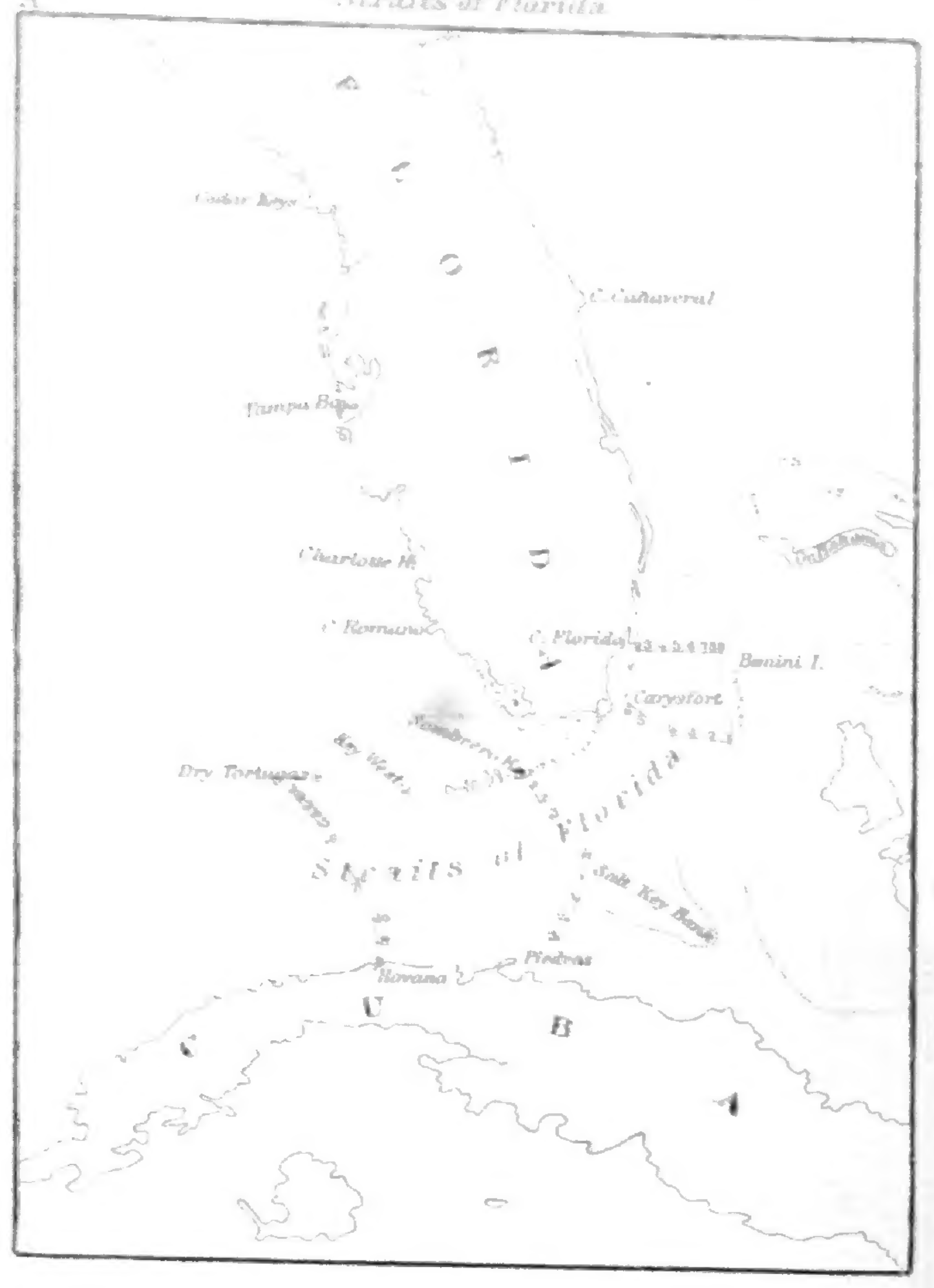
GULF STREAM EXPLORATIONS

From

Observations made in 1845 '46 '47 '48 '53 '55 '58 & '59

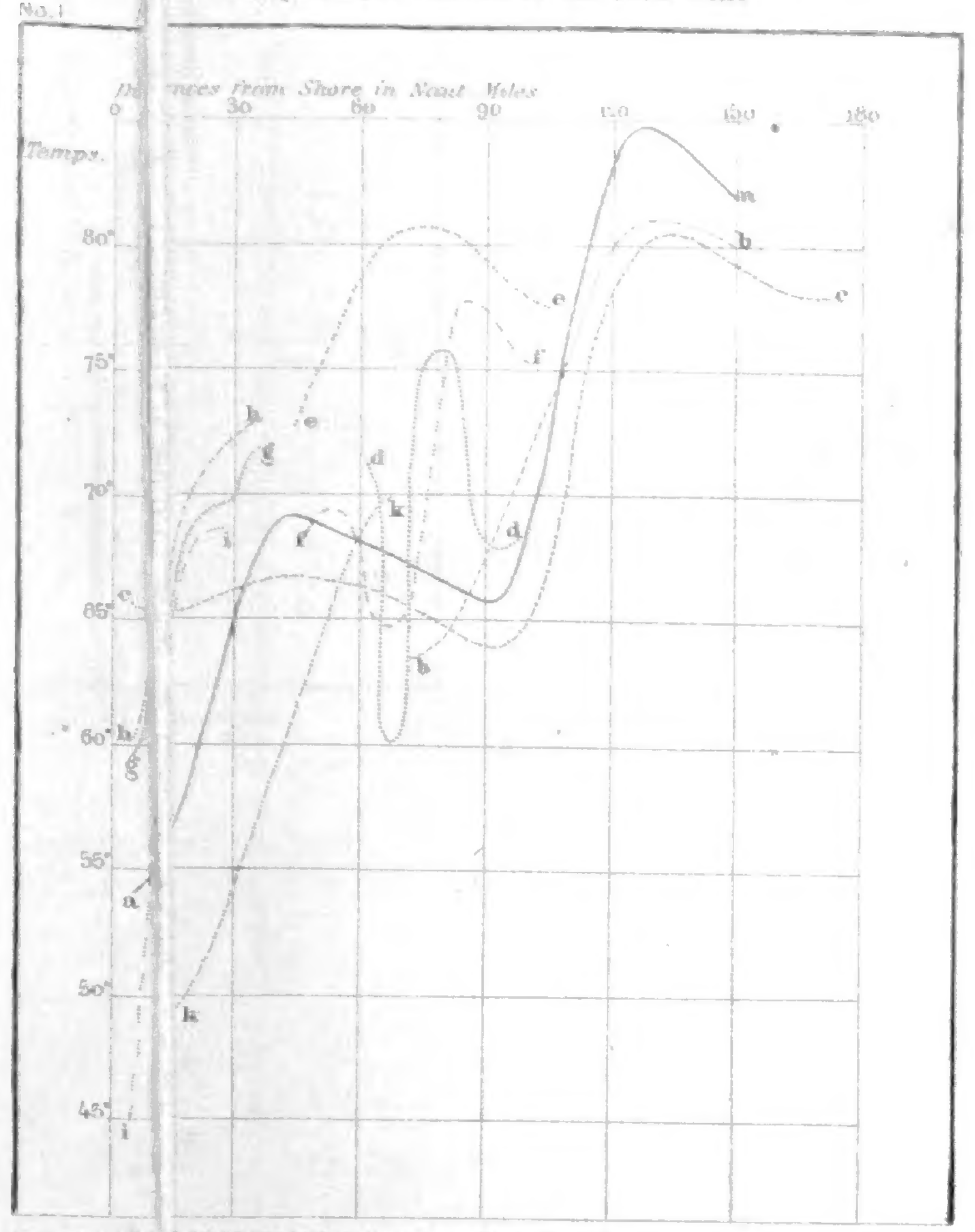
1859

CHART showing the lines of Sections run in the Straits of Florida.



No.1

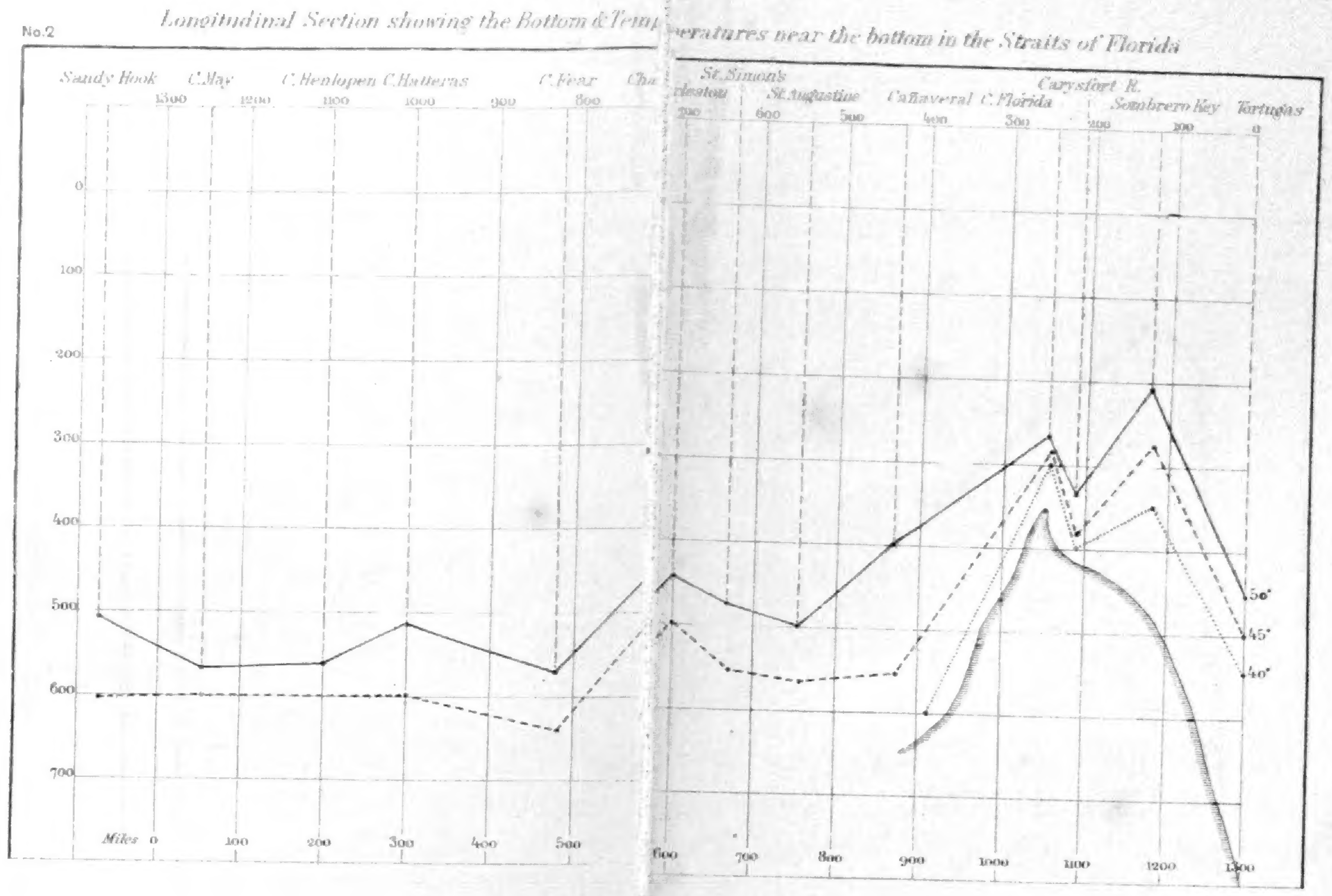
Comparative curves of the cold wall



a mean of 0,5,10,15,20 & 30 fathoms at Cape May Section
b - - 0,5,10,15,20 & 30 - - Cape Henry - 3 years
c - - 0,5,10,15,20 & 30 - - Sandy Hook -
d - - 20 & 30 - - St. Simon's -
e - - 0,5,10,15,20 & 30 - - Cape Fear -
f - - 20 & 30 - - Charleston -
g - - 16 - - Cape Florida -
h - - 16 - - Carysfort Lt.Ho. -
i - - 70 & 100 - - Sombrero Key -
k - - 200 - - Tortugas -

No.2

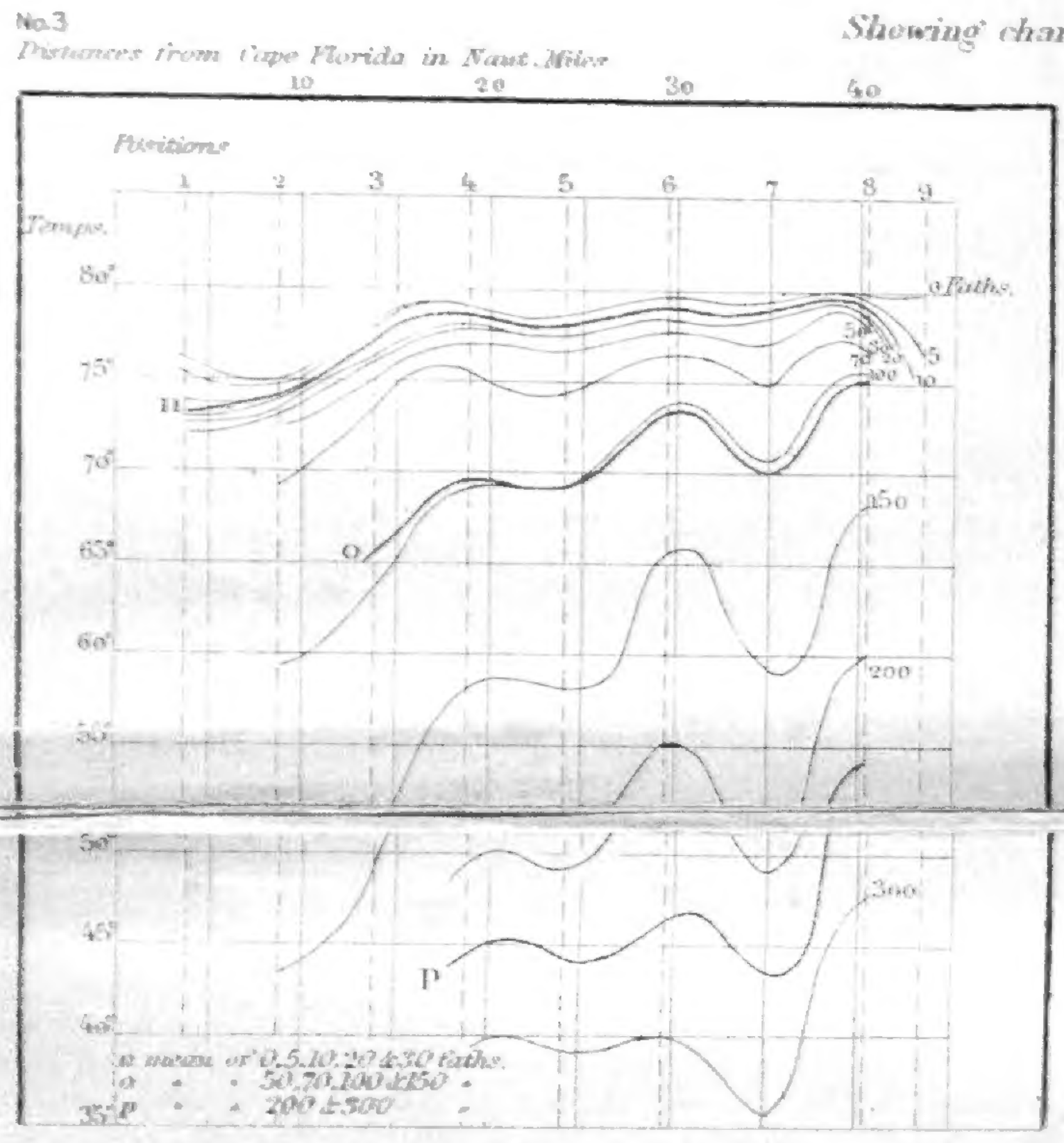
Longitudinal Section showing the Bottom & Temperatures near the bottom in the Straits of Florida



Sections off Cape Florida and Carysfort Lt.Ho. Diagrams of Sections

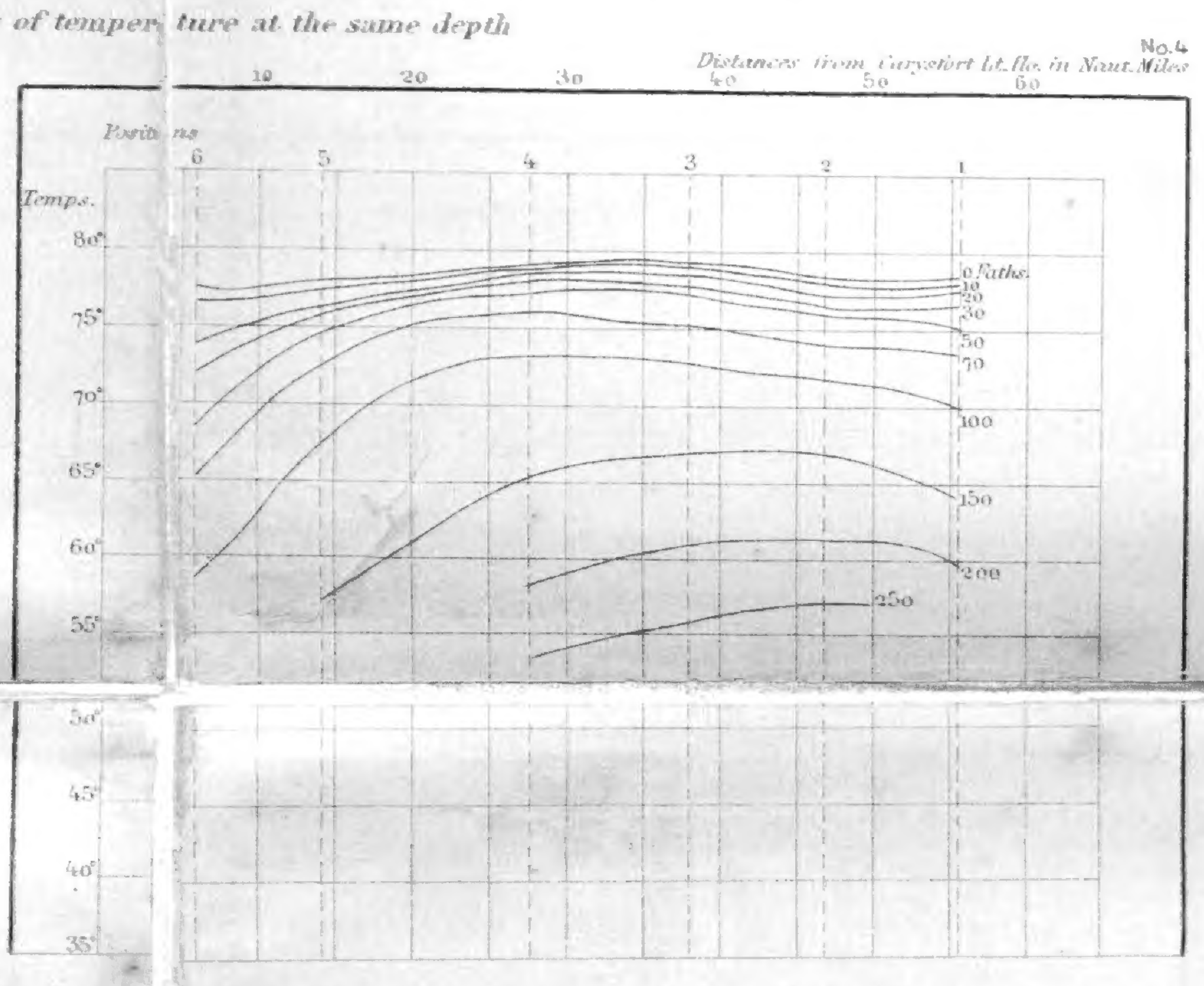
Showing changes of temperature at the same depth

No.3



n mean of 0,5,10,20 & 30 faths.
o - - 50,70,100 & 150 -
p - - 200 & 300 -

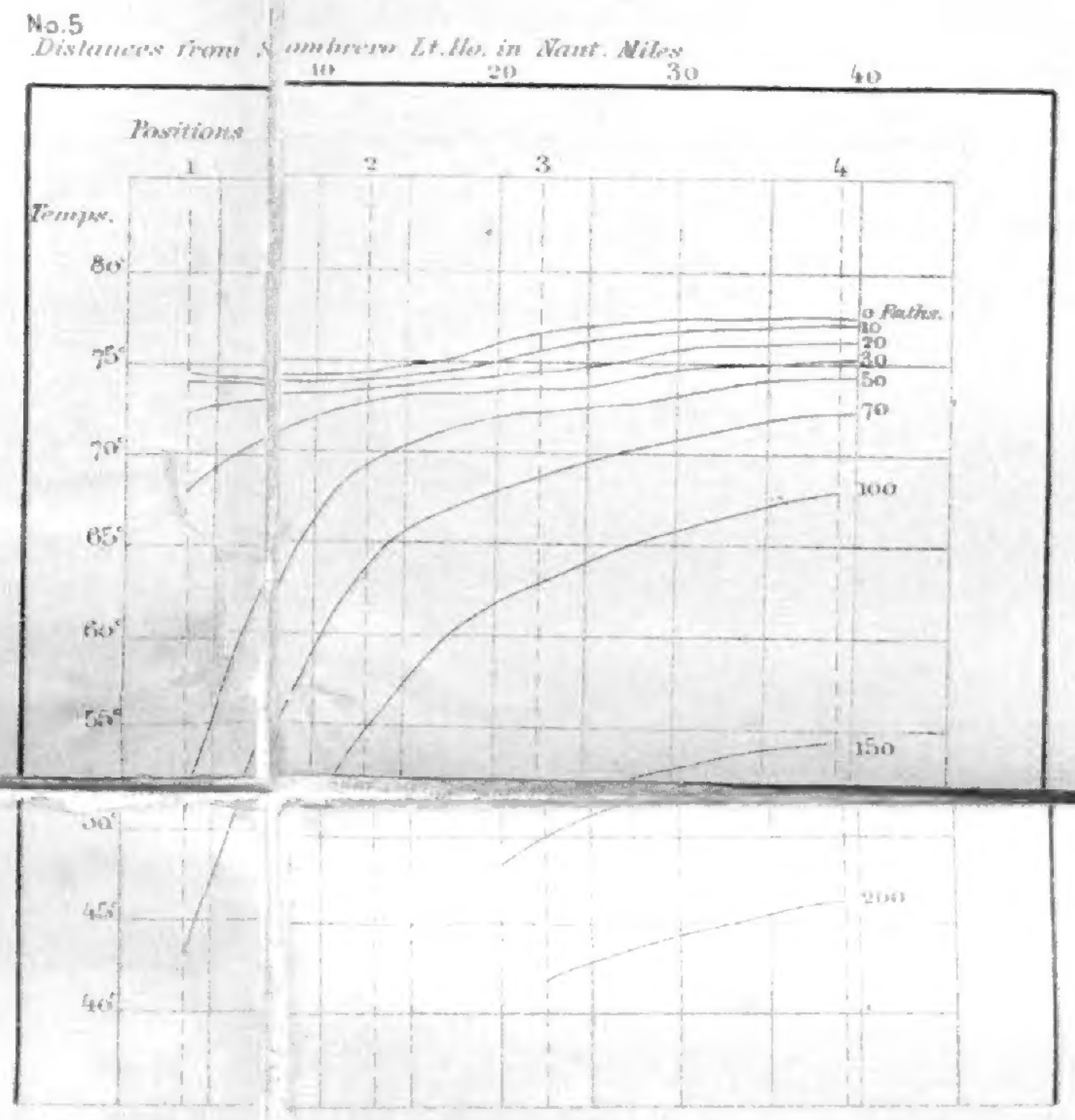
No.4



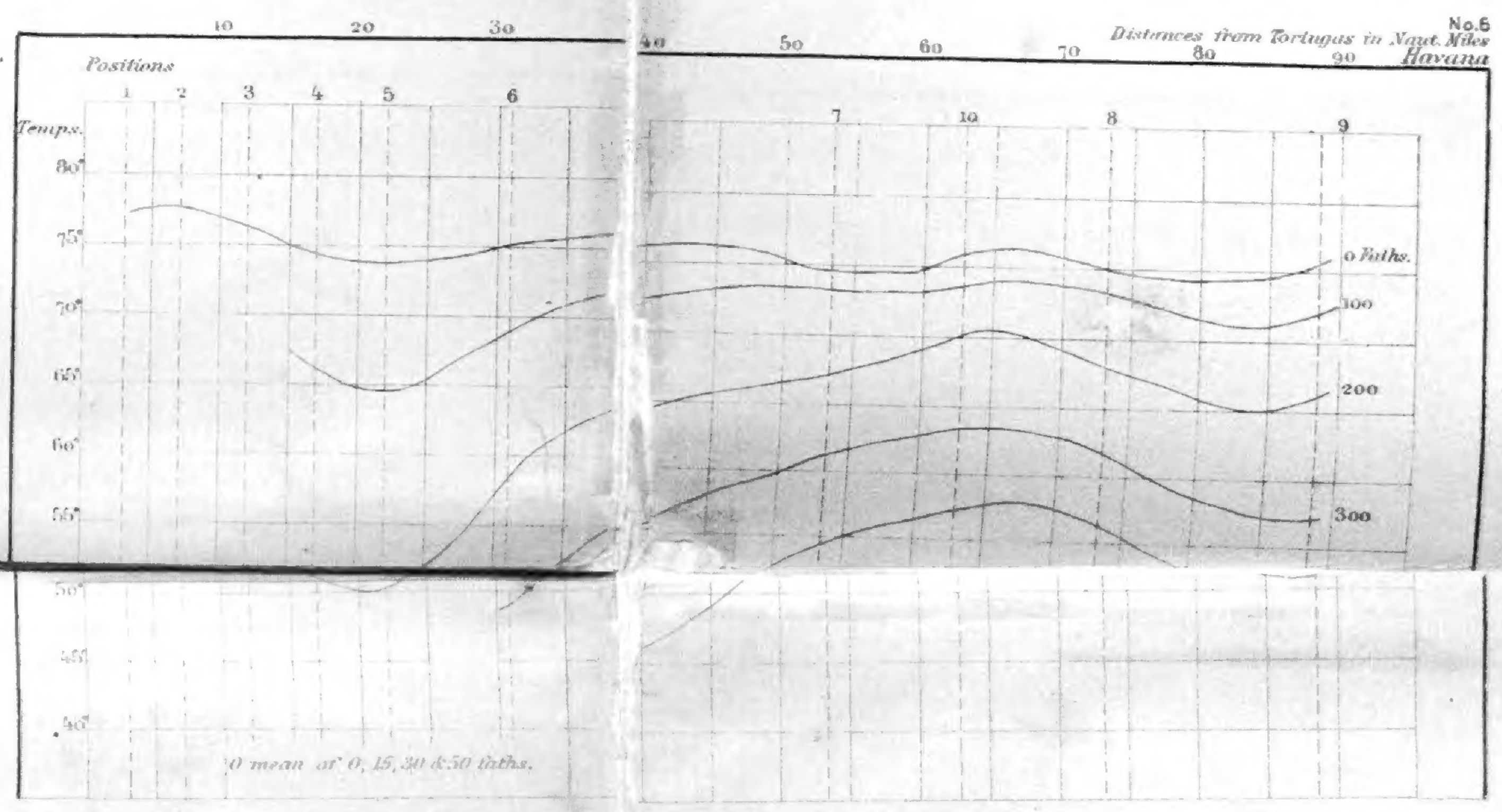
Sections off Sombrero Lt.Ho. and Tortugas. Diagrams of Sections

Showing changes of temperature at the same depth

No.5



No.6



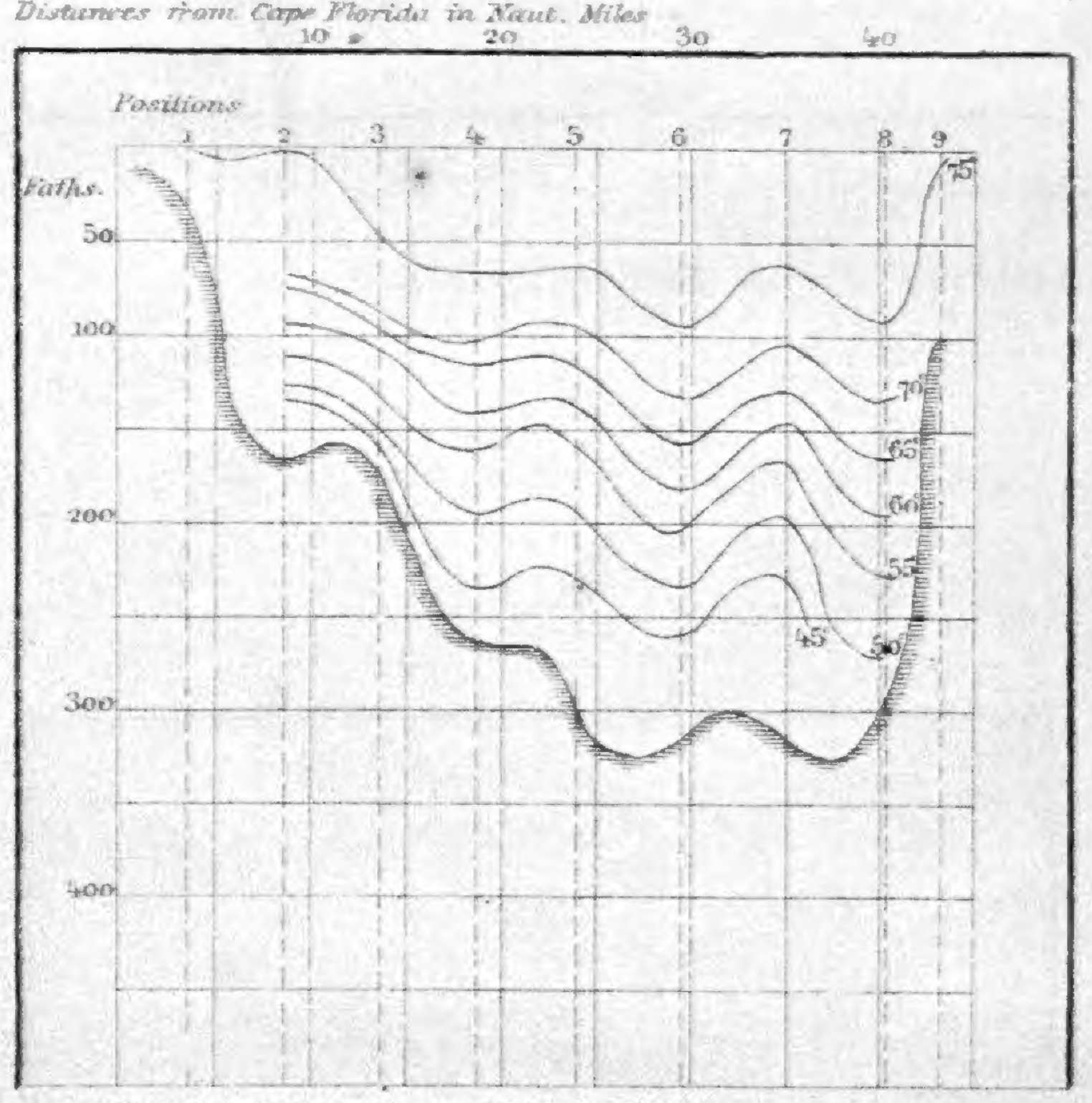
0 mean of 0,15,30 & 50 faths.

Section 1

Section 2

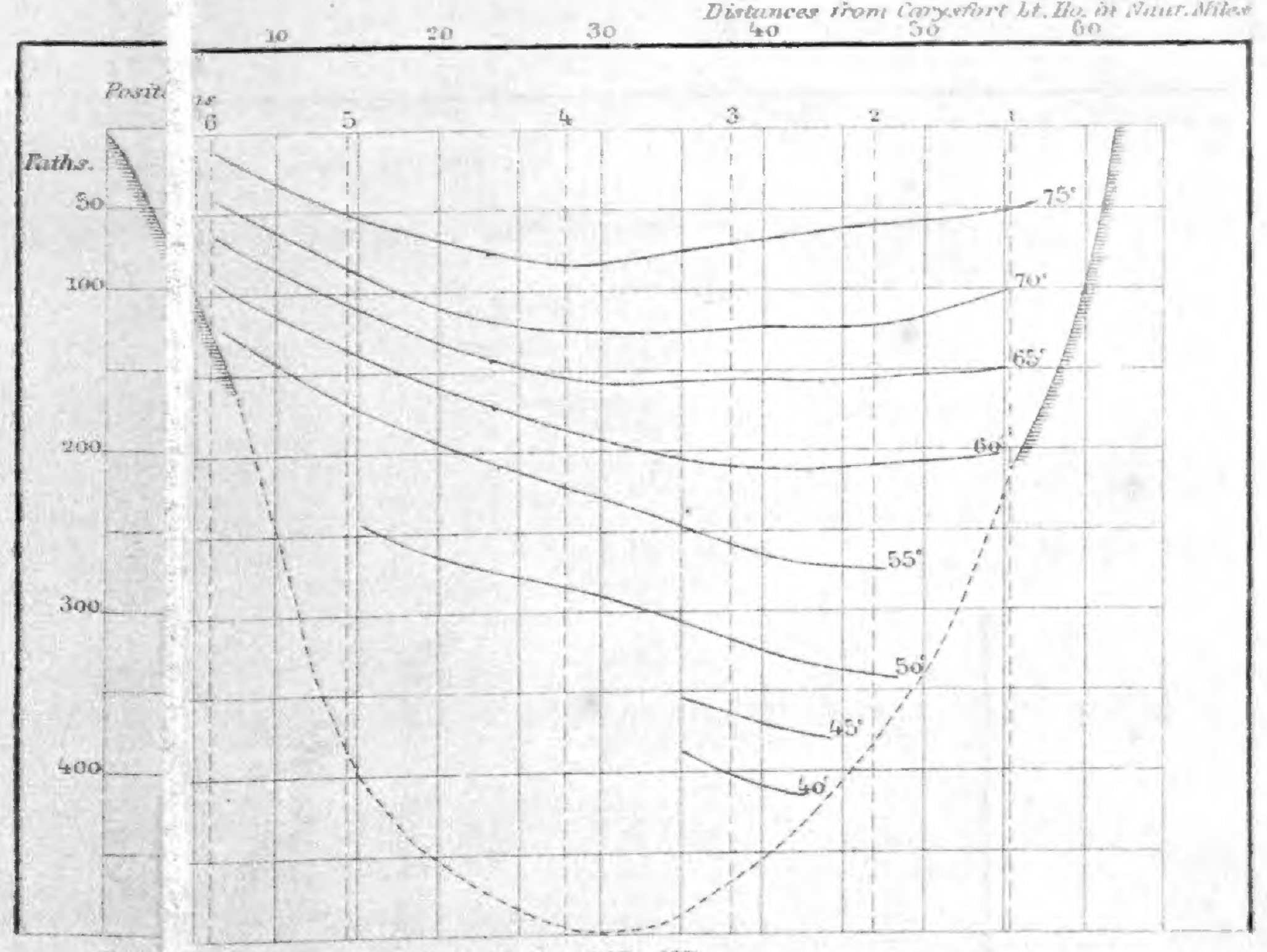
Showing bottom and changes of depth for the same temperature

No.7



The observations were made in May 1855

No.8



The observations were made in April and May 1859

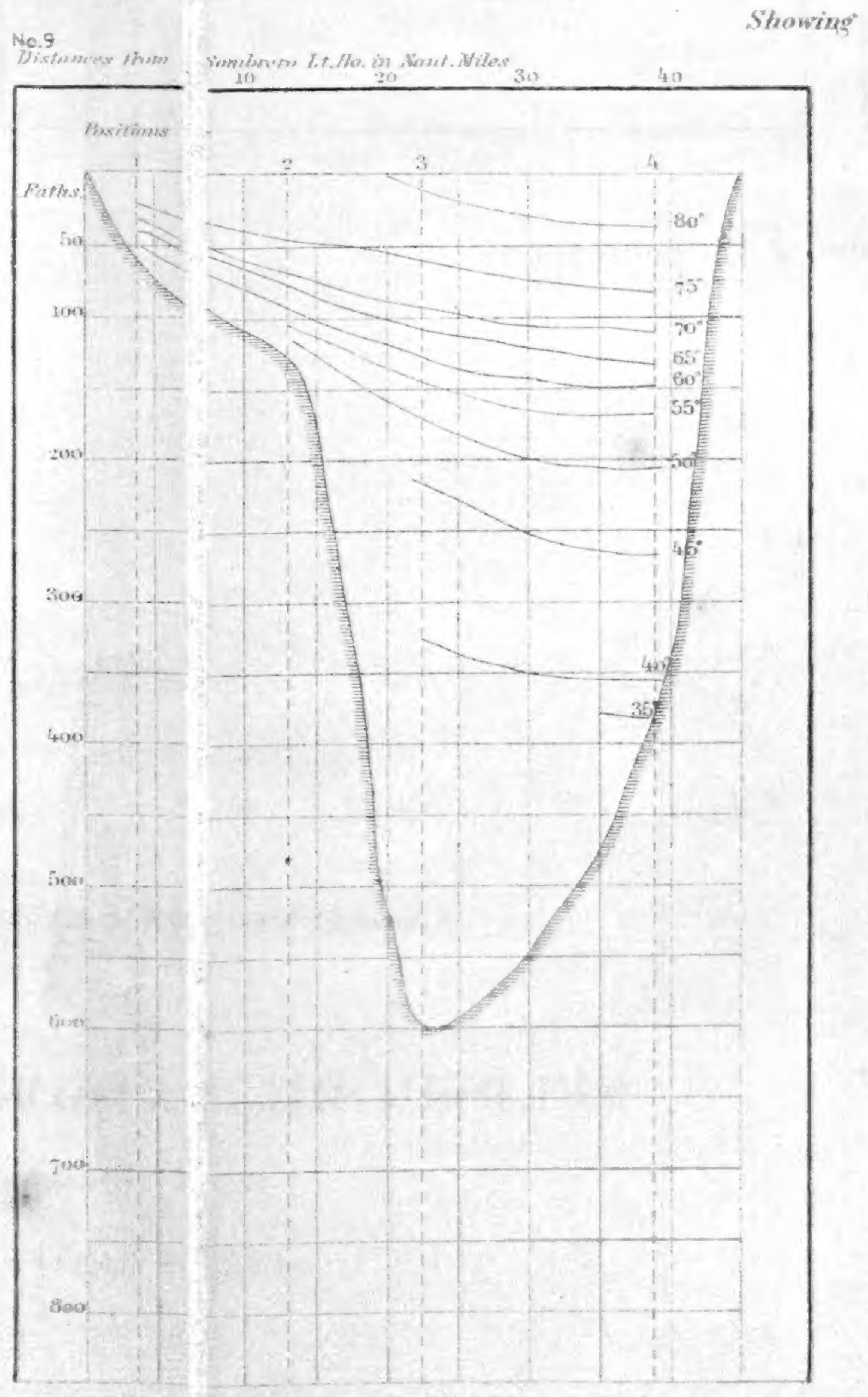
by Lieut. Comdg. T.A.Craven U.S.N. Asst. in the Coast Survey

Section 3

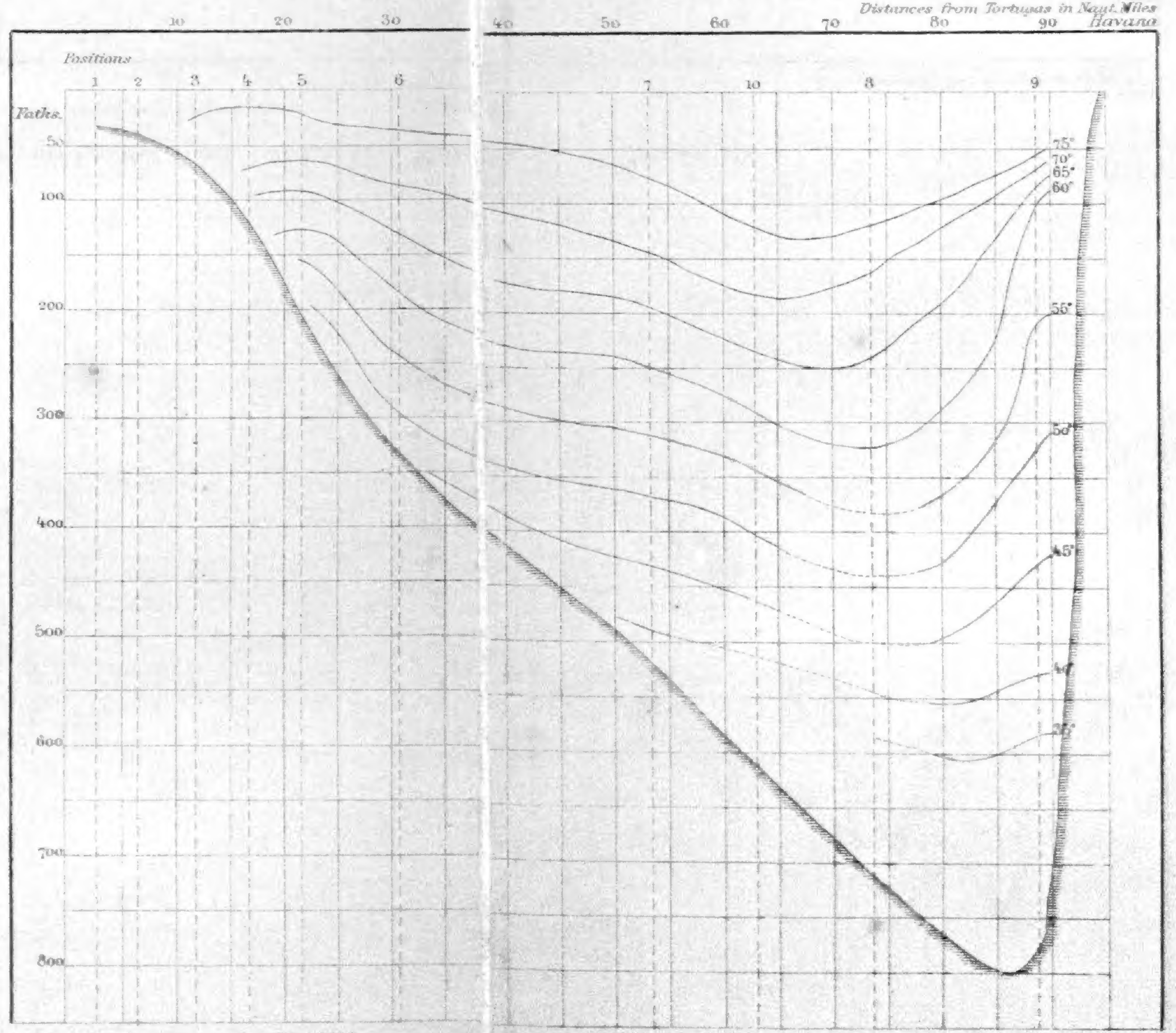
Section 4

Showing bottom and changes of depth for the same temperature

No.9



No.10



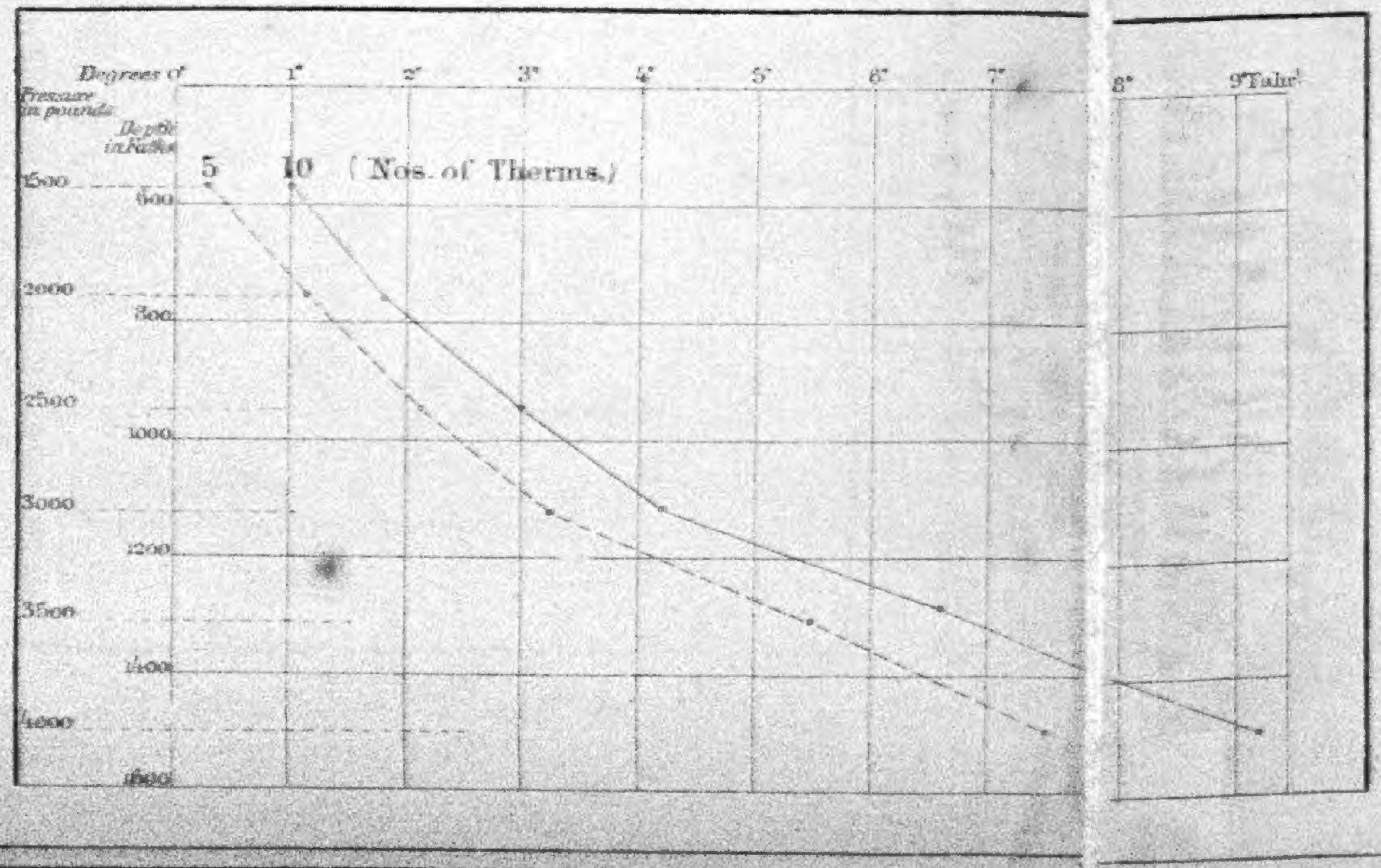
The observations were made in April 1859

The observations were made in May 1858

by Commander B.F.Sands U.S.N. Asst. in the Coast Survey

No.11

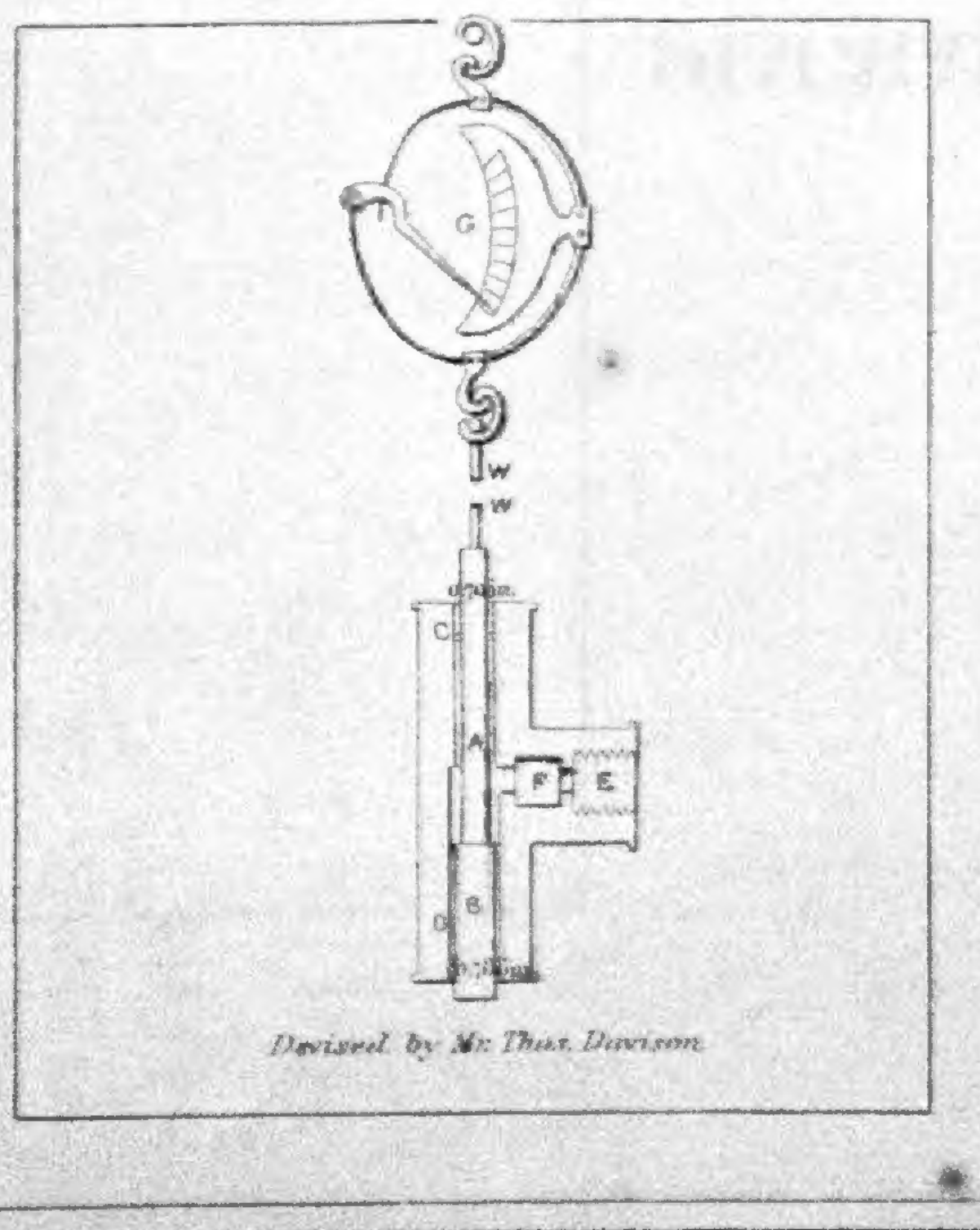
Diagram showing the effect of pressure upon Saxton's Deep Sea Thermometer



GAUGE

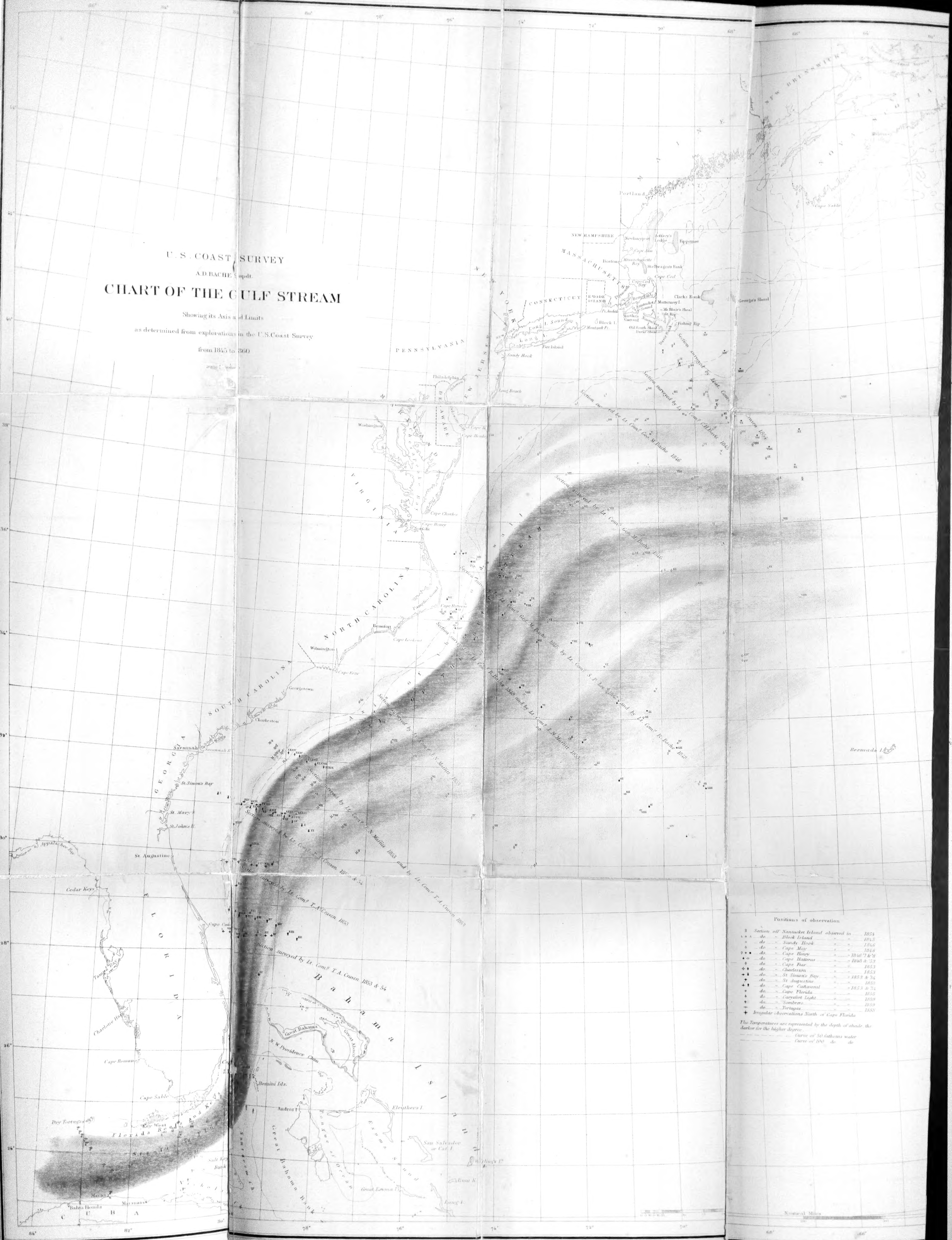
No.12

for measuring the pressure in hydraulic cylinders



Devised by Mr. Thos. Davison

Plate III



U. S. COAST SURVEY

A. D. BACHE Supdt.

CHART OF THE GULF STREAM

Showing its Axis and Limits

as determined from explorations in the U.S. Coast Survey

from 1845 to 1860

Positions of observation

8	Section off Nantucket Island observed in	1854
⸤ ● ⅄	.... do. ..... " .. Block Island ..... " .... "	1845
○	... do ..... " .. Sandy Hook ..... " .... "	1846
♁	... do ..... " .. Cape May ..... " .... "	1846
♀ + ●	.... do. ..... " .. Cape Henry ..... " .... "	1846 '7 & '8
♦ ⊕	.... do ..... " .. Cape Hatteras ..... " .... "	1848 & '53
♦	... do ..... " .. Cape Fear ..... " .... "	1853
✦ ♦	.... do. ..... " .. Charleston ..... " .... "	1853
⇾ ✠	.... do ..... " .. St Simon's Bay ..... " .... "	1853 & '54
✝	... do ..... " .. St Augustine ..... " .... "	1853
✢ ✠	... do. ..... " .. Cape Cañaveral ..... " .... "	1853 & '54
●	do ..... " .. Cape Florida ..... " .... "	1855
♦	do ..... " .. Carysfort Light ..... " .... "	1859
♦	do ..... " .. Sombrero ..... " .... "	1859
⇾	do ..... " .. Tortugas ..... " .... "	1858
✦	Irregular observations North of Cape Florida	

The Temperatures are represented by the depth of shade, the darker for the higher degree.

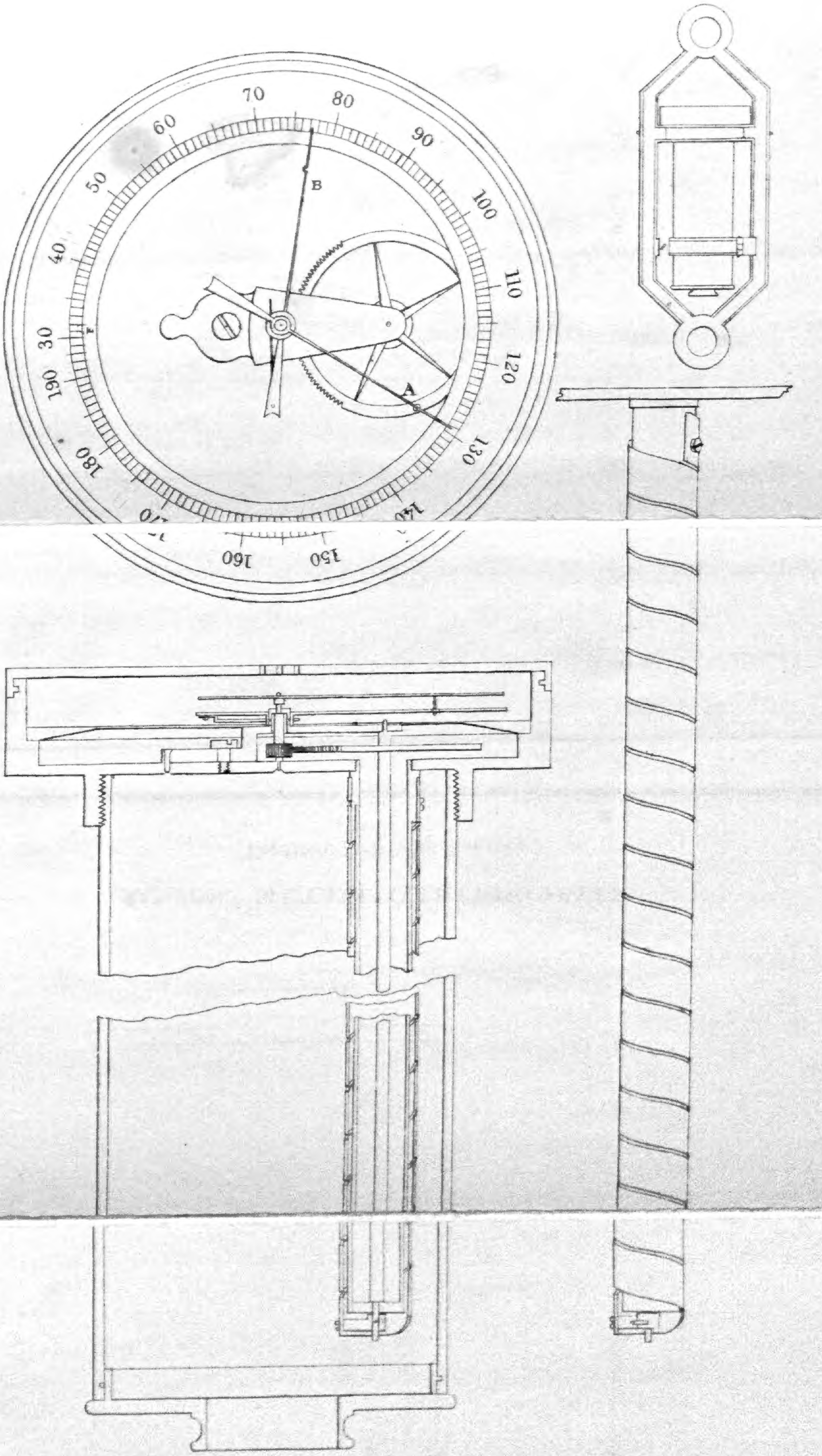
.... Curve of 50 fathoms water

——— ——— ——— Curve of 100 do do

Red^d. Dr^n. by L. D. Williams

Eng^d by W. Ogilvie & App. F. W. Benner

Plate IV



SAXTON'S METALLIC THERMOMETER.

For Deep Sea Temperatures