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SIR DAVID BREWSTER, K.H. LL.D. F.R.S.L. & E. &c.
SIR ROBERT KANE, M.D., F.R.S., M.R.I.A.
WILLIAM FRANCIS, PH.D. F.L.S. F.R.A.S. F.C.S.
JOHN TYNDALL, F.R.S. &c.

> "Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus u apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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Page 200, line 9 from top, *for* 0° =100 *read* 0° = 226.

PLATES.

THE
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[FOURTH SERIES.]

JANUARY 1862.

I. *On Chinese Astronomical Epochs.*
By Archdeacon J. H. Pratt, *M.A.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the absence of authentic history, astronomy sometimes lends valuable aid in enabling us to fix dates by independent means, if certain facts have been handed down to us regarding the positions of the heavenly bodies. There are two eras in Chinese history which it has been attempted to fix in this manner:—one, the reign of the emperor Tcheou-kong, said to have lived about 1100 b.c.; the other, that of the emperor Yao, many centuries earlier, about 2357 b.c. The traditions regarding these persons are vague and altogether uncertain. My object in the present communication is to show what degree of reliance can be placed on the astronomical determinations. However perfect the methods may be which modern science puts into our hands, the results to which they lead us can be of no value if the data are not sufficient and also trustworthy.

On the determination of the Era 1100 b.c.

2. M. Gaubil, a jesuit missionary at Pekin, sent to Paris in 1734 a MS. of Chinese astronomical observations, which Laplace published in the *Connaissance des Tems* for 1809. The oldest observations which Laplace considered to be of any value for astronomical purposes (as he there tells us) are two observations of the length of shadow cast by a gnomon at the summer and winter solstices in the time of Tcheou-kong at a place Tching-tcheou, called also Loyang and Hon-an-fou. The latitude of this place was observed by the missionaries in 1712 three times, and found to be 34° 52′ 8″ by one observation, 34° 46′ 15″ by the second, and 34° 43′ 15″ by the third, the last being considered the best. The vertical style or gnomon was 8 feet (pieds) high,

and the shadows were 1 foot 5 inches (pouces) and 13 feet long, there being 10 pouces in 1 pied. These data give at once, by a table of tangents, 79° 7′ 11″ and 31° 18′ 42″ for the altitudes of the sun. To these Laplace applies corrections for refraction, parallax, and the sun's semidiameter, and makes them finally 79° 6′ 52″ and 31° 18′ 48″. Half the sum and half the difference of these should give the colatitude of the place of observation and the obliquity of the ecliptic. They give the latitude =34° 47′ 10″, and the obliquity =23° 54′ 2″. This latitude is equal to the mean of the three observations mentioned above, but is greater by 4′ than the best of the three. Laplace shows, by a formula in *Mécanique Céleste*, that 23° 51′ 58″ was the obliquity in 1100 B.C. This differs by 2′ 4″ from that obtained from the observations, which, at the rate of 48″ a century (the mean decrease of obliquity, see Herschel's 'Astronomy,' art. 640), would throw the date back to 1358 B.C. Laplace thinks the obliquity deduced from the observations as perfect an accordance as could be desired, "seeing the uncertainty which this kind of observations presents, especially because of the ill-defined limit of the shadow" (*Con. des Tems*, 1809, pp. 433,434).

3. It is the extent of uncertainty arising from this cause which I wish now to determine. Let h be the height of the style, s and w the lengths of the shadows at the summer and winter solstices, l and ϕ the latitude and obliquity, α and β the altitudes of the sun. Then

$$90-l=\frac{1}{2}\tan^{-1}\frac{h}{s}+\frac{1}{2}\tan^{-1}\frac{h}{w},\quad \phi=\frac{1}{2}\tan^{-1}\frac{h}{s}-\frac{1}{2}\tan^{-1}\frac{h}{w};$$

$$\therefore\ \delta l=\frac{1}{2}\,\frac{h^2}{h^2+s^2}\,\frac{\delta s}{h}+\frac{1}{2}\,\frac{h^2}{h^2+w^2}\,\frac{\delta w}{h}=\frac{1-\cos 2\alpha}{4}\,\frac{\delta s}{h}+\frac{1-\cos 2\beta}{4}\,\frac{\delta w}{h}.$$

Similarly,

$$\delta\phi=-\frac{1-\cos 2\alpha}{4}\,\frac{\delta s}{h}+\frac{1-\cos 2\beta}{4}\,\frac{\delta w}{h}.$$

Put

$\alpha=79^\circ\ 6'\ 52''$, $\beta=31^\circ\ 18'\ 48''$, $\cos 2\alpha=-0{\cdot}92867$, $\cos 2\beta=0{\cdot}45978$

$$\therefore\ \delta l=0{\cdot}482\frac{\delta s}{h}+0{\cdot}365\frac{\delta w}{h}=27^\circ{\cdot}6\frac{\delta s}{h}+20^\circ{\cdot}9\frac{\delta w}{h},$$

and

$$\delta\phi=-27^\circ{\cdot}6\frac{\delta s}{h}+20^\circ{\cdot}9\frac{\delta w}{h}.$$

The recorded lengths of the shadows contain no fractions of an inch. It may therefore be supposed that fractions equal to, or less than half an inch, were thought too trifling to observe; or the undefined appearance of the shadow made greater nicety impracticable. Put, therefore, δs and δw each equal to half an inch, in excess or in defect, as errors to which the measure of the shadows is liable. It then appears that the latitude and the obliquity, determined from these observations, will be free from error only within the limits $-0^\circ{\cdot}25$ and $+0^\circ{\cdot}25$, that is, within a range of $0^\circ{\cdot}5$. Now this variation in the obliquity, at the rate of $48''$ in a century, is equivalent to a range of $30' \times 60 \div 48'' = 37\frac{1}{2}$ centuries! This astronomical observation, therefore, really gives no independent information whatever regarding its date. All we can gather is, that if History points out that 1100 B.C. was the era when this observation was made, Astronomy presents no obstacle to this determination.

On the determination of the Era 2357 B.C.

4. An attempt has been made by M. Biot (see *Journal des Savants,* 1840, 1859) to fix the date of the Emperor Yao by reconstructing the celestial sphere (as he imagines it to have been at that time), and reasoning from the change in the position of the equinoxes. He fixes the date at 2357 B.C. I will briefly explain his process, and then show in what I think it inconclusive. He states that the ancient Chinese astronomers divided the equator into 28 unequal parts (called *sieu* or mansions) by declination circles drawn through certain stars chosen for the purpose. M. Biot has evidently bestowed much attention on the subject, and has endeavoured to identify these stars, which the ancient Chinese astronomers are supposed to have used so far back as 4000 years ago! In the next page, in Tables I. and II., I have gathered together some of his results, and in the two following pages some further calculations, the use of which will be explained.

In Table I. are given the names of these twenty-eight stars, and their positions at the era, and the consequent widths of the mansions. (The figures in the second column show the *magnitudes* of the stars.) In the choice of stars two things surprise one: (1) that their intervals in Æ are so very unequal, and (2) that in many instances such unimportant stars are chosen. Thus, while the width of the fifth mansion is more than 30°, the width of the third is less than 3°. Indeed the stars at the beginning and end of the third mansion are so near in Æ, that in the column of longitudes for 1750 A.D. the fourth star *falls behind* the third! For this reason apparently M. Biot goes far back into time past for his epoch, that the line joining the two stars may have as large a projection as possible on the equator of the time, so as to give the greatest advantage to this, at best, very ill-conditioned mansion. The fourth is also a narrow mansion, though not quite so narrow as the third. It will be seen that it is the introduction of the star λ Orion (instead of some other, perhaps between Nos. 4 and 5) which makes these mansions so narrow.

TABLE I.

No. of mansion.	Star at its commencement.	Latitude.	Longitude.	Declination.	Right ascension.	Width of mansion.
		1750 A.D.		2357 B.C.		2357 B.C.
1	η Pleiades...... 3	+ 4° 2′	56° 31′	+ 3° 10′	358° 30′	10° 25′
2	ε Taurus 3, 4	− 2 36	64 58	+ 0 30	8 55	18 5
3	λ Orion......... 4	−13 24	80 13	− 3 38	27 0	2 43
4	δ Orion 2	−23 35	78 52	−13 32	29 43	3 36
5	μ Gemini 3	− 0 51	91 49	+12 11	33 19	30 35
6	θ Cancer 5, 6	− 0 47	122 15	+20 28	63 54	6 38
7	δ Hydra 4	−12 25	126 49	+ 9 44	70 32	17 5
8	α Hydra 2	−22 24	143 48	+ 1 13	87 37	7 39
9	39ν_1Hydra 5	−26 5	152 13	− 2 29	95 16	16 40
10	α Crater 4	−22 43	170 15	− 0 39	111 56	17 27
11	γ Corvus 3	−14 29	187 15	+ 4 4	129 23	16 12
12	α Virgo......... 1	− 2 2	200 21	+12 12	145 35	12 0
13	κ Virgo......... 4	+ 2 56	211 0	+13 9	157 35	8 58
14	α_2 Libra 4	+ 0 22	221 36	+ 6 46	166 33	14 5
15	π Scorpio...... 4	− 5 26	239 27	− 5 39	180 38	5 2
16	σ Scorpio...... 3, 4	− 4 0	244 19	− 6 17	185 40	3 5
17	μ_2 Scorpio...... 4	−15 21	252 46	−20 1	188 45	17 49
18	γ_2 Sagittarius 3, 4	− 6 57	267 47	−18 3	206 34	9 52
19	φ Sagittarius 3, 4	− 3 55	276 41	−18 19	216 26	26 29
20	β Capricornus 3	+ 4 37	300 33	−16 23	242 55	8 24
21	ε Aquarius ... 4	+ 8 7	308 14	−14 12	251 19	11 55
22	β Aquarius ... 3	+ 8 38	319 55	−14 49	263 14	10 10
23	α Aquarius ... 3	+10 41	329 52	−12 58	273 24	18 48
24	α Pegasus ... 2	+19 25	349 59	− 2 39	292 12	16 11
25	γ Pegasus...... 2	+12 36	5 40	− 6 17	308 23	9 13
26	ζ Andromeda 4	+17 37	17 6	+ 1 35	317 36	15 19
27	β Aries......... 3	+ 8 29	30 29	− 2 42	332 55	10 48
28	35 Aries 4	+11 17	43 27	+ 4 42	343 43	14 47
Mean..	....	......	......	− 2 28		

TABLE II.

Position of Equinoxes and Solstices, 2357 B.C.

1.	η Pleiades......	1° 30′	behind Vernal Equinox.
8.	α Hydra	2 23	behind Summer Solstice.
15.	π Scorpio......	0 38	before Autumnal Equinox.
22.	β Aquarius ...	6 46	behind Winter Solstice.

TABLE III.

No. of mansion.	Declination.	Right ascension.	Width of mansion.	Declination.	Right ascension.	Width of mansion.
	In the year 1729 B.C.			In the year 1100 B.C.		
1	+ 7° 53′	5° 23′	10° 50′	+11° 21′	13° 28′	10° 53′
2	+ 4 14	16 13	18 1	+ 7 32	24 21	17 55
3	− 0 9	34 14	2 23	+ 2 29	42 16	1 58
4	−10 13	36 37	4 29	− 7 35	44 14	5 39
5	+15 24	41 6	31 24	+17 48	49 53	32 4
6	+22 8	72 30	5 55	+22 51	81 57	5 13
7	+11 27	78 25	16 34	+11 23	87 10	15 53
8	+ 1 26	94 59	7 24	+ 0 47	103 3	7 9
9	− 2 46	102 23	16 45	− 3 51	110 12	16 47
10	− 2 1	119 8	17 41	− 4 1	126 59	17 51
11	+ 1 1	136 49	16 27	− 1 6	144 50	16 31
12	+ 6 9	153 16	11 55	+ 5 29	161 21	11 53
13	+ 9 42	165 11	8 43	+ 6 11	173 14	8 41
14	+ 3 6	173 54	14 1	− 0 27	181 55	14 9
15	− 9 26	187 55	6 40	−12 52	196 4	5 10
16	− 7 32	194 35	1 32	−13 25	201 14	3 39
17	−23 52	196 7	18 57	−27 4	204 53	18 32
18	−21 28	215 4	9 26	−24 10	223 25	10 13
19	−21 22	224 30	25 42	−23 37	233 38	26 46
20	−17 59	250 12	9 20	−18 55	260 24	8 8
21	−15 25	259 32	12 0	−15 43	268 32	11 57
22	−15 11	271 32	10 0	−14 50	280 29	9 47
23	−12 46	281 32	18 3	−11 43	290 16	17 21
24	+ 0 27	299 35	16 20	+ 0 47	307 37	16 24
25	− 3 58	315 55	8 50	− 1 9	324 1	8 25
26	+ 4 21	324 45	15 29	+ 7 21	332 26	15 30
27	+ 0 38	340 14	10 38	+ 4 5	347 56	10 54
28	+ 8 18	350 52	14 31	+11 51	358 50	14 38
Mean ..	− 2 26	……	……	− 2 48		

TABLE IV.

Position of Equinoxes and Solstices.

	2357 B.C.	1729 B.C.	1100 B.C.
Vernal Equinox ...	1° 30′ before No. 1.	5° 23′ behind No. 1.	1° 10′ before No. 28.
Summer Solstice ...	2 23 before No. 8.	4 59 behind No. 8.	2 50 before No. 7.
Autumnal Equinox.	0 38 behind No. 15.	7 35 behind No. 15.	1 55 behind No. 14.
Winter Solstice ...	6 46 before No. 22.	1 32 behind No. 22.	1 28 before No. 21.

Table V.

Nos. of mansions compared.	Their differences of Right Ascension.		
	2357 B.C.	1729 B.C.	1100 B.C.
1 and 15	182° 8′	182° 32′	182° 36′
2 „ 16	176 45	178 24	176 53
3 „ 17	161 45	161 53	162 37
4 „ 18	176 51	178 27	179 11
5 „ 19	183 7	183 24	183 45
6 „ 20	179 1	177 42	178 27
7 „ 21	180 47	181 7	181 22
8 „ 22	175 37	176 33	177 26
9 „ 23	178 8	179 9	180 4
10 „ 24	180 16	180 27	180 38
11 „ 25	179 0	179 6	179 11
12 „ 26	172 1	171 29	171 5
13 „ 27	175 20	175 3	174 42
14 „ 28	177 10	176 58	176 55

M. Biot attempts to illustrate the correctness of his list of stars, though so irregularly distributed and in many instances so inferior in importance, by stating that there is evidently a law in their selection, and that a narrow mansion in one part of the heavens corresponds to a narrow mansion in the opposite part; so also with the wider mansions; so that the stars, at the epoch, were situated in pairs on the same meridian.

5. It is by the application of this test, which is to some extent approximately true, that I think I can detect a flaw which destroys the necessity of passing so far back into past time as the twenty-fourth century B.C. In the first of the three columns in Table V., I have given the differences of Æ for 2357 B.C. gathered from M. Biot's results in Table I. It will there be seen that the pairs of stars deviate from being on the same meridian by an average = about 3°, *if we except* the third pair in which the deviation is as much as 18° 15′. There must be some reason for this exception. The fact is, both the limiting stars of that mansion are exceptional; No. 3 being so near Nos. 2 and 4 (as I have already pointed out), and No. 17 having so large a declination. (No. 6 has as large a declination, see Table I.; but that star is close upon the ecliptic, which No. 17 is not, which may be some reason for its use, there being no nearer star.) These two stars, moreover, are both small, being only of the fourth order of magnitude. I conclude, therefore, for these reasons that they have been wrongly determined. If λ Orion is rejected, the necessity for pushing back the epoch so far is removed.

6. In Tables III., IV., V. I have given the results of calculations I have made for two other epochs, viz. 1100 B.C. and 1729 B.C. (halfway between 1100 B.C. and 2357 B.C.), in order to see

whether there is any special reason for so remote an epoch as 2357 B.C. being selected. I can see no special reason. (1) Even if λ Orion is not rejected, it will be seen that the width of the third mansion is 2° 23′ and 1° 58′ at those two later dates, and these are not so inferior to 2° 43′, the width in 2357 B.C., as to induce any great preference for that epoch. (2) By comparing the columns of declinations in Tables I. and III., it will be seen that the twenty-eight stars lie very much alike with reference to the equators of the three epochs, without any decided advantage for 2357 B.C. In all of them the mid-line is south of the equator; the distance is 2° 28′, 2° 26′, 2° 48′ in the three cases. (3) M. Biot points out that the arrangement of stars chosen by the Chinese is equatorial, and not ecliptical; and he well illustrates this by showing that α Hydræ has been chosen, though only of the second magnitude, as the eighth star in preference to the much brighter star Regulus in the same hour-angle, but on the ecliptic and about 24° from the equator. But he departs from this idea when he draws an argument for his ancient epoch from the circumstance that η in Pleiades is the *first* of the twenty-eight stars, and that therefore when the system was chosen that star must have been the vernal equinox. The Pleiades, in their sevenfold group, are so conspicuous and marked an object, that it is very easy to understand that, being chosen as one of the twenty-eight determining stars, they should be fixed upon, for that reason, as the point of departure. At the three dates in my Tables, the declination of η is only 3° 10′, 7° 53′, 11° 21′; the largest of which is smaller than that of twelve of the twenty-eight stars as laid down by M. Biot for 2357 B.C. In passing backwards to obtain as great a width as possible for his third mansion, he seems to have stopped short at η in Pleiades for no other reason than that, being first of the equatorial series, it might also be the equinox; whereas by going further back he might have somewhat further widened his third mansion. (4) M. Biot also observes that four of the chosen stars, the 1st, 8th, 15th, and 22nd (see Table II.), fall very near the equinoxes and solstices of 2357 B.C., and that this is an argument in favour of that epoch. But my Table IV. shows that in 1100 B.C. four others of the twenty-eight stars, also at equal intervals in point of number, viz. the 7th, 14th, 21st, and 28th, were still nearer to the equinoxes and solstices. So that no argument can be drawn from this source in favour of his epoch: rather the contrary. He says that the four stars he names are mentioned in the Chou-king*, an ancient work on

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* For what is known of this work Chou-king, or Shoo-king, see *Encyc. Brit.*, word *China*, p. 640: also see "History of Astronomy" in 'Library of Useful Knowledge.'

astronomy, as being at the equinoxes and solstices in the time of Yao. As one of the points (see Table II.) is wrong by 6° 46′, this commits the Chou-king to an error of 487 years. Moreover, these traditions are not to be relied upon as facts. We have in Indian astronomy an instance of an ancient conjunction of sun, moon, and planets stated as if an observed phenomenon, whereas it is clear that the idea (which is false in fact) was come at by calculating backwards, and that upon defective data. (5) Another thing which M. Biot appears to think confirmatory of his epoch is this. He states that the Chinese used to observe with care the motion of the circumpolar stars in the Dragon, the Great Bear, and the Lesser Bear, and two in Lyra; and conjectures that the position of the declination-circles passing through them at that epoch influenced the astronomers in the choice of the stars which define the twenty-eight mansions. Eleven, however, of the mansions have none of these circles passing through them, and the angular distances of the others from the nearest stars vary through all degrees of magnitude between 0° 8′ and 7° 26′; of these approximate conjunctions on the same meridian twelve take place at the inferior passage of the circumpolar star. There does not appear to be anything at all remarkable in these approximate relations. Other positions of the pole and other epochs may be no doubt found where even a nearer approximation of the kind exists. (6) Another argument of M. Biot in favour of his epoch is, that a star is spoken of by Chinese astronomers as the "Unity of the Heavens," which name is supposed to indicate that it was at the pole of the equator when first so designated: and the French chronologist Freret thinks the star must be α in the Dragon, though M. Biot thinks it may be another star close to it. It is very easy to show that, as the longitude of this star was in 1750 A.D. 153° 54′, it must have been $63{\cdot}9 \times 72 = 4601$ years before that epoch (that is, 2850 B.C.) when it was at the pole, or at its nearest point only a few minutes from it. This is 500 years before M. Biot's epoch. In this time, however, it would not have moved away more than about 2° 46′, and therefore might still be regarded as the pole-star. But this shows the uncertainty of such means of fixing dates, even by the best astronomical means, if the data are not precise. The star would continue within a distance of 2° 46′, taking both sides of the pole, for no less a period than 1000 years. The fact, therefore, of its being regarded as the pole-star, if such errors are allowed (and we see larger errors allowed in this approximation to a system), would not fix the date within 1000 years. There is a tradition that the Chaldee astronomers had observed α Draconis in the pole. It is quite possible that such a circumstance might be handed down, one so easily observed, even from antediluvian times. But it is still more probable that it has been calculated backwards, a matter of no difficulty when once the precession of the equinoxes was known, and the epoch when it occurred assigned with more or less accuracy according to the means of calculation.

7. This reconstruction of the celestial sphere in the time of the Emperor Yao, even supposing he was a historical character, appears to me to be based upon such uncertain data as to make it altogether untrustworthy. If the data were more exact, the case would be different. Unless, however, reliable data can be procured, it seems a pity to prostrate the science of astronomy by using it in such a cause, lending its name and high character to prop up the conclusions of vague and uncertain traditions, enveloped too often in fable and falsehood.

J. H. Pratt.

Calcutta, October 19, 1861.

II. *Experiments on the Comparative Effects of Nocturnal Radiation from the Surface of the Ground and over a large Sheet of Water. By* Professor Marcet *of the Academy of Geneva*[*].

It is acknowledged that about the period of sunset, provided the sky be clear, the temperature of the air in contact with the earth's surface is cooler than that of the atmosphere at a certain height above the ground. This fact was first noticed by Pictet and Six, towards the end of the last century; but as the theory of radiant heat had not yet been established, no satisfactory explanation of the phenomenon was offered until 1814, when Dr. Wells published his valuable 'Essay on Dew.' Many years afterwards (in 1842) a series of observations, on the same subject, I had made in the neighbourhood of Geneva, was published in the eighth volume of the *Mémoires de la Société de Physique et d'Histoire Naturelle*, tending to prove that during clear and calm nights the atmosphere becomes gradually warmer on ascending above the surface of the earth, until a certain height be attained, which varies according to circumstances, but is generally not less than from thirty to forty yards. The observations I made in 1842 have just been fully confirmed by an elaborate treatise on the subject published by Professor C. Martins of Montpellier[†]. The results obtained are, no doubt, attributable to the gradual cooling of the earth's surface arising from its nocturnal radiation into empty space; which radiation, when the sky is clear, is not compensated by the transmission of

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[*] Communicated by the Author.

[†] Vide *Mémoires de l'Académie des Sciences et Belles Lettres de Montpellier*, vol. v.

caloric from the higher regions of the atmosphere. The cooling of the surface of the earth naturally gives rise to a corresponding diminution of temperature of the stratum of air in its immediate vicinity; the effect is transmitted to the stratum above, though naturally in a less degree, and so on from one stratum to another, until a height be attained at which the temperature of the atmosphere is found to be equal to that of the stratum of air in contact with the earth.

I had often thought of inquiring whether the effects of nocturnal radiation, tending as they do to produce a gradual increase of temperature on ascending above the earth's surface, are entirely dependent on the radiation of the ground, properly so called, or whether they would be equally perceptible above a large sheet of water, such as the sea or a lake. The exceptionally fine clear weather of October last afforded me a favourable opportunity for making some experiments on the effects of nocturnal radiation from the surface of the lake of Geneva.

Let me, however, be allowed to remark that experiment alone could determine to what extent a large surface of water is capable of producing, by the radiation of its caloric, the whole, or at least a part of the effects due to the nocturnal radiation of the earth. Water, it is well known, possesses a considerable radiating power; Leslie, in his 'Researches on Heat,' found it to be equal to that of lampblack, and superior to that of paper. It is not, therefore, because water does not radiate sufficiently that we can be authorized to conclude *à priori* that the nocturnal increase of temperature is not as likely to take place over a liquid surface as over the solid ground; but there is another fact, depending upon the peculiar constitution of liquids, which must also be taken into consideration. The particles of liquids, it is well known, are essentially moveable, and their different strata subject to a constant interchange of position when affected by the slightest changes of temperature. The consequence is, that the moment the upper surface of a given extent of water has begun to cool by the effect of nocturnal radiation, it will become denser than the stratum immediately below it; it will therefore descend and be replaced by this stratum, which, becoming heavier in its turn, will be replaced by the following, and in the same way successively from one stratum to the other; so that in fact there is no reason why the temperature of the surface of the water should undergo any appreciable change. Under these circumstances it will, I think, be admitted that the effect of nocturnal radiation (inasmuch as it would tend to lower the temperature, first of the surface of the water, and next that of the stratum of air in immediate contact with this surface) will become, if not entirely imperceptible, at least far less apparent than on land. The following experiments made during last October on the Lake of Geneva entirely corroborate this view.

Three Centigrade mercurial thermometers, capable of showing the tenth part of a degree, were fixed at different heights round a vertical pole about 16 feet long. Each thermometer was attached to the extremity of a horizontal rod fixed to the pole, and sufficiently long to ensure the thermometers being suspended, not over the boat from which the experiments were to be made, but directly over the surface of the water. One of the thermometers was placed at a height of about 3 inches above the surface of the water, the second at about 6 feet, and the third at 15 feet. Three series of observations were made during the evenings of the 26th and 28th of October, at about 600 yards from land, under the most favourable circumstances—the sky being beautifully clear, and the surface of the lake unruffled by the slightest breeze. The observations were commenced a quarter of an hour before sunset, and renewed every half hour until three-quarters of an hour after sunset. The following is the average result of the observations noted during the evening of the 26th of October:—

Temperature of the atmosphere 3 inches above the water.	11°·65 C.
„ „ 6 feet „ .	11·62
„ „ 15 feet „ .	11·80
Temperature of the water at the surface of the lake .	12

The average result obtained during the evening of the 28th was as follows:—

Temperature of the atmosphere 3 inches above the water.	11°·35 C.
„ „ 6 feet „ .	11·29
„ „ 15 feet „ .	11·32
Temperature of the surface of the water .	12·75

The consequence to be drawn from these results is, I apprehend, that the comparative temperature of the successive strata of air above the surface of the lake up to the height of 15 feet, undergoes no sensible change from the effects of nocturnal radiation. The almost imperceptible differences indicated by the thermometers in no case exceeding a few hundredths of a degree, may be fairly attributed to errors arising from accidental currents, which it is difficult to guard against completely in observations of this nature.

The following is the mean result of comparative observations made at the same moment in the centre of a large field about 700 yards from the borders of the lake:—

Temperature of the surface of the ground . . .	6°·98 C.
Temperature of the air 3 inches above the ground .	8·00
„ „ 6 feet „ .	9·10
„ „ 15 feet „ .	9·65

Finally, comparative observations made simultaneously on the borders of the lake, within a few feet of the water, gave the following result:—

Temperature of the surface of the gravel . . .	9°·90 C.
Temperature of the air 3 inches above the ground.	10·40
” ” 6 feet ” .	10·55
” ” 15 feet ” .	10·62

showing that the immediate neighbourhood of the water is sufficient to modify the results generally obtained on land.

The following conclusions may, I think, be safely drawn from the foregoing observations:—

1. The gradual increase of temperature occurring on ascending through the lower strata of the atmosphere, which appears constantly to prevail on land about and after sunset, is not apparent above a large surface of water.

2. The immediate vicinity of a large sheet of water is sufficient to modify to a considerable extent the effects of the nocturnal radiation of the earth, and thereby materially diminish the increase of temperature observed under ordinary circumstances on ascending above the surface of the ground.

3. One cannot help being struck by the great difference (amounting to between 2 and 3 Centigrade degrees) constantly observed between the temperature of the atmosphere a few feet above the ground, and that of the air at the same height above a large sheet of water.

III. *On Physical Lines of Force.* *By* J. C. Maxwell, *F.R.S., Professor of Natural Philosophy in King's College, London*[*].

Part III.—*The Theory of Molecular Vortices applied to Statical Electricity.*

In the first part of this paper[†] I have shown how the forces acting between magnets, electric currents, and matter capable of magnetic induction may be accounted for on the hypothesis of the magnetic field being occupied with innumerable vortices of revolving matter, their axes coinciding with the direction of the magnetic force at every point of the field.

The centrifugal force of these vortices produces pressures distributed in such a way that the final effect is a force identical in direction and magnitude with that which we observe.

In the second part[‡] I described the mechanism by which these rotations may be made to coexist, and to be distributed according to the known laws of magnetic lines of force.

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[*] Communicated by the Author.

[†] Phil. Mag. March 1861.

Phil. Mag. April and May 1861.

I conceived the rotating matter to be the substance of certain cells, divided from each other by cell-walls composed of particles which are very small compared with the cells, and that it is by the motions of these particles, and their tangential action on the substance in the cells, that the rotation is communicated from one cell to another.

I have not attempted to explain this tangential action, but it is necessary to suppose, in order to account for the transmission of rotation from the exterior to the interior parts of each cell, that the substance in the cells possesses elasticity of figure, similar in kind, though different in degree, to that observed in solid bodies. The undulatory theory of light requires us to admit this kind of elasticity in the luminiferous medium, in order to account for transverse vibrations. We need not then be surprised if the magneto-electric medium possesses the same property.

According to our theory, the particles which form the partitions between the cells constitute the matter of electricity. The motion of these particles constitutes an electric current; the tangential force with which the particles are pressed by the matter of the cells is electromotive force, and the pressure of the particles on each other corresponds to the tension or potential of the electricity.

If we can now explain the condition of a body with respect to the surrounding medium when it is said to be "charged" with electricity, and account for the forces acting between electrified bodies, we shall have established a connexion between all the principal phenomena of electrical science.

We know by experiment that electric tension is the same thing, whether observed in statical or in current electricity; so that an electromotive force produced by magnetism may be made to charge a Leyden jar, as is done by the coil machine.

When a difference of tension exists in different parts of any body, the electricity passes, or tends to pass, from places of greater to places of smaller tension. If the body is a conductor, an actual passage of electricity takes place; and if the difference of tensions is kept up, the current continues to flow with a velocity proportional inversely to the resistance, or directly to the conductivity of the body.

The electric resistance has a very wide range of values, that of the metals being the smallest, and that of glass being so great that a charge of electricity has been preserved* in a glass vessel for years without penetrating the thickness of the glass.

Bodies which do not permit a current of electricity to flow through them are called insulators. But though electricity does

* By Professor W. Thomson.

not flow through them, electrical effects are propagated through them, and the amount of these effects differs according to the nature of the body; so that equally good insulators may act differently as dielectrics*.

Here then we have two independent qualities of bodies, one by which they allow of the passage of electricity through them, and the other by which they allow of electrical action being transmitted through them without any electricity being allowed to pass. A conducting body may be compared to a porous membrane which opposes more or less resistance to the passage of a fluid, while a dielectric is like an elastic membrane which may be impervious to the fluid, but transmits the pressure of the fluid on one side to that on the other.

As long as electromotive force acts on a conductor, it produces a current which, as it meets with resistance, occasions a continual transformation of electrical energy into heat, which is incapable of being restored again as electrical energy by any reversion of the process.

Electromotive force acting on a dielectric produces a state of polarization of its parts similar in distribution to the polarity of the particles of iron under the influence of a magnet†, and, like the magnetic polarization, capable of being described as a state in which every particle has its poles in opposite conditions.

In a dielectric under induction, we may conceive that the electricity in each molecule is so displaced that one side is rendered positively, and the other negatively electrical, but that the electricity remains entirely connected with the molecule, and does not pass from one molecule to another.

The effect of this action on the whole dielectric mass is to produce a general displacement of the electricity in a certain direction. This displacement does not amount to a current, because when it has attained a certain value it remains constant, but it is the commencement of a current, and its variations constitute currents in the positive or negative direction, according as the displacement is increasing or diminishing. The amount of the displacement depends on the nature of the body, and on the electromotive force; so that if h is the displacement, R the electromotive force, and E a coefficient depending on the nature of the dielectric,

$$\mathrm{R}=-4\pi\mathrm{E}^2h\,;$$

and if r is the value of the electric current due to displacement,

$$r=\frac{dh}{dt}.$$

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* Faraday, 'Experimental Researches,' Series XI.

† See Prof. Mossotti, "Discussione Analitica," *Memorie della Soc. Italiana* (Modena), vol. xxiv. part 2. p. 49.

These relations are independent of any theory about the internal mechanism of dielectrics; but when we find electromotive force producing electric displacement in a dielectric, and when we find the dielectric recovering from its state of electric displacement with an equal electromotive force, we cannot help regarding the phenomena as those of an elastic body, yielding to a pressure, and recovering its form when the pressure is removed.

According to our hypothesis, the magnetic medium is divided into cells, separated by partitions formed of a stratum of particles which play the part of electricity. When the electric particles are urged in any direction, they will, by their tangential action on the elastic substance of the cells, distort each cell, and call into play an equal and opposite force arising from the elasticity of the cells. When the force is removed, the cells will recover their form, and the electricity will return to its former position.

In the following investigation I have considered the relation between the displacement and the force producing it, on the supposition that the cells are spherical. The actual form of the cells probably does not differ from that of a sphere sufficiently to make much difference in the numerical result.

I have deduced from this result the relation between the statical and dynamical measures of electricity, and have shown, by a comparison of the electro-magnetic experiments of MM. Kohlrausch and Weber with the velocity of light as found by M. Fizeau, that the elasticity of the magnetic medium in air is the same as that of the luminiferous medium, if these two coexistent, coextensive, and equally elastic media are not rather one medium.

It appears also from Prop. XV. that the attraction between two electrified bodies depends on the value of E^2, and that therefore it would be less in turpentine than in air, if the quantity of electricity in each body remains the same. If, however, the *potentials* of the two bodies were given, the attraction between them would vary inversely as E^2, and would be greater in turpentine than in air.

Prop. XII.—To find the conditions of equilibrium of an elastic sphere whose surface is exposed to normal and tangential forces, the tangential forces being proportional to the sine of the distance from a given point on the sphere.

Let the axis of z be the axis of spherical coordinates.

Let ξ, η, ζ be the displacements of any particle of the sphere in the directions of x, y, and z.

Let p_{xx}, p_{yy}, p_{zz} be the stresses normal to planes perpendicular to the three axes, and let p_{yz}, p_{zx}, p_{xy} be the stresses of distortion in the planes yz, zx, and xy.

Let μ be the coefficient of cubic elasticity, so that if

$$p_{xx}=p_{yy}=p_{zz}=p,$$

$$p=\mu\left(\frac{d\xi}{dx}+\frac{d\eta}{dy}+\frac{d\zeta}{dz}\right). \tag{80}$$

Let m be the coefficient of rigidity, so that

$$p_{xx}-p_{yy}=m\left(\frac{d\xi}{dx}-\frac{d\eta}{dy}\right), \text{ \&c.} \tag{81}$$

Then we have the following equations of elasticity in an isotropic medium,

$$p_{xx}=(\mu-\tfrac{1}{3}m)\left(\frac{d\xi}{dx}+\frac{d\eta}{dy}+\frac{d\zeta}{dz}\right)+m\frac{d\xi}{dx}; \tag{82}$$

with similar equations in y and z, and also

$$p_{yz}=\frac{m}{2}\left(\frac{d\eta}{dz}+\frac{d\zeta}{dy}\right), \text{ \&c.} \tag{83}$$

In the case of the sphere, let us assume the radius $= a$, and

$$\xi=exz, \quad \eta=exy, \quad \zeta=f(x^2+y^2)+gz^2+d. \tag{84}$$

Then

$$\left.\begin{aligned}
p_{xx}&=2(\mu-\tfrac{1}{3}m)(e+g)z+mez=p_{yy},\\
p_{zz}&=2(\mu-\tfrac{1}{3}m)(e+g)z+2mgz,\\
p_{yz}&=\frac{m}{2}(e+2f)y,\\
p_{zx}&=\frac{m}{2}(e+2f)z,\\
p_{xy}&=0.
\end{aligned}\right\} \tag{85}$$

The equation of internal equilibrium with respect to z is

$$\frac{d}{dx}p_{zx}+\frac{d}{dy}p_{yz}+\frac{d}{dz}p_{zz}=0, \tag{86}$$

which is satisfied in this case if

$$m(e+2f+2g)+2(\mu-\tfrac{1}{3}m)(e+g)=0. \tag{87}$$

The tangential stress on the surface of the sphere, whose radius is a at an angular distance θ from the axis in plane xz,

$$\mathrm{T}=(p_{xx}-p_{zz})\sin\theta\cos\theta+p_{xz}(\cos^2\theta-\sin^2\theta) \tag{88}$$

$$=2m(e+f-g)a\sin\theta\cos^2\theta-\frac{ma}{2}(e+2f)\sin\theta. \tag{89}$$

In order that T may be proportional to $\sin\theta$, the first term must vanish, and therefore

$$g=e+f, \tag{90}$$

$$\mathrm{T}=-\frac{ma}{2}(e+2f)\sin\theta. \tag{91}$$

The normal stress on the surface at any point is

$$\mathrm{N}=p_{xx}\sin^2\theta+p_{yy}\cos^2\theta+2p_{xz}\sin\theta\cos\theta$$

$$=2(\mu-\tfrac{1}{3}m)(e+g)a\cos\theta+2ma\cos\theta\big((e+f)\sin^2\theta+g\cos^2\theta\big); \quad (92)$$

or by (87) and (90),

$$\mathrm{N}=-ma(e+2f)\cos\theta. \quad . \quad . \quad . \quad . \quad . \quad (93)$$

The tangential displacement of any point is

$$t=\xi\cos\theta-\zeta\sin\theta=-(a^2f+d)\sin\theta. \quad . \quad . \quad . \quad . \quad (94)$$

The normal displacement is

$$n=\xi\sin\theta+\zeta\cos\theta=\big(a^2(e+f)+d\big)\cos\theta. \quad . \quad . \quad . \quad (95)$$

If we make

$$a^2(e+f)+d=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (96)$$

there will be no normal displacement, and the displacement will be entirely tangential, and we shall have

$$t=a^2e\sin\theta. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (97)$$

The whole work done by the superficial forces is

$$\mathrm{U}=\tfrac{1}{2}\Sigma(\mathrm{T}t)d\mathrm{S},$$

the summation being extended over the surface of the sphere.

The energy of elasticity in the substance of the sphere is

$$\mathrm{U}=\tfrac{1}{2}\Sigma\left(\frac{d\xi}{dx}p_{xx}+\frac{d\eta}{dy}p_{yy}+\frac{d\zeta}{dz}p_{zz}+\left(\frac{d\eta}{dz}+\frac{d\zeta}{dy}\right)p_{yz}+\left(\frac{d\zeta}{dx}+\frac{d\xi}{dz}\right)_{zx}\right.$$
$$\left.+\left(\frac{d\xi}{dy}+\frac{d\eta}{dx}\right)p_{xy}\right)d\mathrm{V},$$

the summation being extended to the whole contents of the sphere.

We find, as we ought, that these quantities have the same value, namely

$$\mathrm{U}=-\tfrac{2}{3}\pi a^5me(e+2f). \quad . \quad . \quad . \quad . \quad . \quad (98)$$

We may now suppose that the tangential action on the surface arises from a layer of particles in contact with it, the particles being acted on by their own mutual pressure, and acting on the surfaces of the two cells with which they are in contact.

We assume the axis of z to be in the direction of maximum variation of the pressure among the particles, and we have to determine the relation between an electromotive force R acting on the particles in that direction, and the electric displacement h which accompanies it.

Prop. XIII.—To find the relation between electromotive force and electric displacement when a uniform electromotive force R acts parallel to the axis of z.

Take any element δS of the surface, covered with a stratum whose density is ρ, and having its normal inclined θ to the axes of z; then the tangential force upon it will be

$$\rho R \delta S \sin\theta = 2T\delta S, \quad \ldots \ldots \quad (99)$$

T being, as before, the tangential force on each side of the surface. Putting $\rho = \frac{1}{2\pi}$ as in equation (34)*, we find

$$R = -2\pi m a(e+2f). \quad \ldots \ldots \quad (100)$$

The displacement of electricity due to the distortion of the sphere is

$$\Sigma\delta S \tfrac{1}{2}\rho t \sin\theta \text{ taken over the whole surface}; \quad . \quad (101)$$

and if h is the electric displacement per unit of volume, we shall have

$$\tfrac{4}{3}\pi a^3 h = \tfrac{2}{3} a^4 e, \quad \ldots \ldots \quad (102)$$

or

$$h = \frac{1}{2\pi} ae; \quad \ldots \ldots \quad (103)$$

so that

$$R = 4\pi^2 m \frac{e+2f}{e} h, \quad \ldots \ldots \quad (104)$$

or we may write

$$R = -4\pi E^2 h, \quad \ldots \ldots \quad (105)$$

provided we assume

$$E^2 = -\pi m \frac{e+2f}{e}. \quad \ldots \ldots \quad (106)$$

Finding e and f from (87) and (90), we get

$$E^2 = \pi m \frac{3}{1+\frac{5}{3}\frac{m}{\mu}}. \quad \ldots \ldots \quad (107)$$

The ratio of m to μ varies in different substances; but in a medium whose elasticity depends entirely upon forces acting between pairs of particles, this ratio is that of 6 to 5, and in this case

$$E^2 = \pi m. \quad \ldots \ldots \quad (108)$$

When the resistance to compression is infinitely greater than the resistance to distortion, as in a liquid rendered slightly elastic by gum or jelly,

$$E^2 = 3\pi m. \quad \ldots \ldots \quad (109)$$

The value of E^2 must lie between these limits. It is probable that the substance of our cells is of the former kind, and that we must use the first value of E^2, which is that belonging to

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* Phil. Mag. April 1861.

a hypothetically "perfect" solid*, in which

$$5m=6\mu, \qquad (110)$$

so that we must use equation (108).

Prop. XIV.—To correct the equations (9)† of electric currents for the effect due to the elasticity of the medium.

We have seen that electromotive force and electric displacement are connected by equation (105). Differentiating this equation with respect to t, we find

$$\frac{d\mathrm{R}}{dt}=-4\pi\mathrm{E}^2\frac{dh}{dt}, \qquad (111)$$

showing that when the electromotive force varies, the electric displacement also varies. But a variation of displacement is equivalent to a current, and this current must be taken into account in equations (9) and added to r. The three equations then become

$$\left.\begin{aligned} p&=\frac{1}{4\pi}\left(\frac{d\gamma}{dy}-\frac{d\beta}{dz}-\frac{1}{\mathrm{E}^2}\frac{d\mathrm{P}}{dt}\right), \\ q&=\frac{1}{4\pi}\left(\frac{d\alpha}{dz}-\frac{d\gamma}{dx}-\frac{1}{\mathrm{E}^2}\frac{d\mathrm{Q}}{dt}\right), \\ r&=\frac{1}{4\pi}\left(\frac{d\beta}{dx}-\frac{d\alpha}{dy}-\frac{1}{\mathrm{E}^2}\frac{d\mathrm{R}}{dt}\right), \end{aligned}\right\} \qquad (112)$$

where p, q, r are the electric currents in the directions of $x, y,$ and z; α, β, γ are the components of magnetic intensity; and P, Q, R are the electromotive forces. Now if e be the quantity of free electricity in unit of volume, then the equation of continuity will be

$$\frac{dp}{dx}+\frac{dq}{dy}+\frac{dr}{dz}+\frac{de}{dt}=0. \qquad (113)$$

Differentiating (112) with respect to $x, y,$ and z respectively, and substituting, we find

$$\frac{de}{dt}=\frac{1}{4\pi\mathrm{E}^2}\frac{d}{dt}\left(\frac{d\mathrm{P}}{dx}+\frac{d\mathrm{Q}}{dy}+\frac{d\mathrm{R}}{dz}\right); \qquad (114)$$

whence

$$e=\frac{1}{4\pi\mathrm{E}^2}\left(\frac{d\mathrm{P}}{dx}+\frac{d\mathrm{Q}}{dy}+\frac{d\mathrm{R}}{dz}\right), \qquad (115)$$

the constant being omitted, because $e=0$ when there are no electromotive forces.

Prop. XV.—To find the force acting between two electrified bodies.

The energy in the medium arising from the electric displace-

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[*] See Rankine "On Elasticity," Camb. and Dub. Math. Journ. 1851.

[†] Phil. Mag. March 1861.

ments is

$$U=-\Sigma\tfrac{1}{2}(Pf+Qg+Rh)\delta V, \quad . \quad . \quad . \quad (116)$$

where P, Q, R are the forces, and f, g, h the displacements. Now when there is no motion of the bodies or alteration of forces, it appears from equations (77)* that

$$P=-\frac{d\Psi}{dx}, \quad Q=-\frac{d\Psi}{dy}, \quad R=-\frac{d\Psi}{dz}; \quad . \quad (118)$$

and we know by (105) that

$$P=-4\pi E^2 f, \quad Q=-4\pi E^2 g, \quad R=-4\pi E^2 h; \quad . \quad (119)$$

whence

$$U=\frac{1}{8\pi E^2}\Sigma\left(\left(\frac{d\Psi}{dx}\right)^2+\left(\frac{d\Psi}{dy}\right)^2+\left(\frac{d\Psi}{dz}\right)^2\right)\delta V. \quad . \quad (120)$$

Integrating by parts throughout all space, and remembering that Ψ vanishes at an infinite distance,

$$U=-\frac{1}{8\pi E^2}\Sigma\Psi\left(\frac{d^2\Psi}{dx^2}+\frac{d^2\Psi}{dy^2}+\frac{d^2\Psi}{dz^2}\right)\delta V; \quad (121)$$

or by (115),

$$U=\tfrac{1}{2}\Sigma(\Psi e)\delta V. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (122)$$

Now let there be two electrified bodies, and let e_1 be the distribution of electricity in the first, and Ψ_1 the electric tension due to it, and let

$$e_1=\frac{1}{4\pi E^2}\left(\frac{d^2\Psi_1}{dx^2}+\frac{d^2\Psi_1}{dy^2}+\frac{d^2\Psi_1}{dz^2}\right). \quad . \quad . \quad (123)$$

Let e_2 be the distribution of electricity in the second body, and Ψ_2 the tension due to it; then the whole tension at any point will be $\Psi_1+\Psi_2$, and the expansion for U will become

$$U=\tfrac{1}{2}\Sigma(\Psi_1e_1+\Psi_2e_2+\Psi_1e_2+\Psi_2e_1)\delta V. \quad . \quad . \quad (124)$$

Let the body whose electricity is e_1 be moved in any way, the electricity moving along with the body, then since the distribution of tension Ψ_1 moves with the body, the value of Ψ_1e_1 remains the same.

Ψ_2e_2 also remains the same; and Green has shown (Essay on Electricity, p. 10) that $\Psi_1e_2=\Psi_2e_1$, so that the work done by moving the body against electric forces

$$W=\delta U=\delta\Sigma(\Psi_2e_1)\delta V. \quad . \quad . \quad . \quad . \quad . \quad (125)$$

And if e_1 is confined to a small body,

$$W=e_1\delta\Psi_2,$$

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* Phil. Mag. May 1861.

or

$$F dr = e_1 \frac{d\Psi_2}{dr} dr, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (126)$$

where F is the resistance and dr the motion.

If the body e_2 be small, then if r is the distance from e_2, equation (123) gives

$$\Psi_2 = E^2 \frac{e_2}{r};$$

whence

$$F = -E^2 \frac{e_1 e_2}{r^2}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (127)$$

or the force is a repulsion varying inversely as the square of the distance.

Now let η_1 and η_2 be the same quantities of electricity measured statically, then we know by definition of electrical quantity

$$F = -\frac{\eta_1 \eta_2}{r^2}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (128)$$

and this will be satisfied provided

$$\eta_1 = E e_1 \text{ and } \eta_2 = E e_2; \quad . \quad . \quad . \quad . \quad . \quad (129)$$

so that the quantity E previously determined in Prop. XIII. is the number by which the electrodynamic measure of any quantity of electricity must be multiplied to obtain its electrostatic measure.

That electric current which, circulating round a ring whose area is unity, produces the same effect on a distant magnet as a magnet would produce whose strength is unity and length unity placed perpendicularly to the plane of the ring, is a unit current; and E units of electricity, measured statically, traverse the section of this current in one second,—these units being such that any two of them, placed at unit of distance, repel each other with unit of force.

We may suppose either that E units of positive electricity move in the positive direction through the wire, or that E units of negative electricity move in the negative direction, or, thirdly, that $\frac{1}{2}$E units of positive electricity move in the positive direction, while $\frac{1}{2}$E units of negative electricity move in the negative direction at the same time.

The last is the supposition on which MM. Weber and Kohlrausch* proceed, who have found

$$\tfrac{1}{2}E = 155{,}370{,}000{,}000, \quad . \quad . \quad . \quad . \quad . \quad (130)$$

the unit of length being the millimetre, and that of time being one second, whence

$$E = 310{,}740{,}000{,}000. \quad . \quad . \quad . \quad . \quad . \quad (131)$$

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* *Abhandlungen der König. Sächsischen Gesellschaft*, vol. iii. (1857), p. 260.

Prop. XVI.—To find the rate of propagation of transverse vibrations through the elastic medium of which the cells are composed, on the supposition that its elasticity is due entirely to forces acting between pairs of particles.

By the ordinary method of investigation we know that

$$V=\sqrt{\frac{m}{\rho}}, \quad \ldots \ldots \quad (132)$$

where m is the coefficient of transverse elasticity, and ρ is the density. By referring to the equations of Part I., it will be seen that if ρ is the density of the matter of the vortices, and μ is the "coefficient of magnetic induction,"

$$\mu=\pi\rho\,; \quad \ldots \ldots \quad (133)$$

whence

$$\pi m=V^2\mu\,; \quad \ldots \ldots \quad (134)$$

and by (108),

$$E=V\sqrt{\mu}. \quad \ldots \ldots \quad (135)$$

In air or vacuum $\mu=1$, and therefore

$$\left.\begin{aligned} V&=E, \\ &=310{,}740{,}000{,}000 \text{ millimetres per second}, \\ &=193{,}088 \text{ miles per second.} \end{aligned}\right\} \quad . \quad (136)$$

The velocity of light in air, as determined by M. Fizeau*, is 70,843 leagues per second (25 leagues to a degree) which gives

$$\begin{aligned} V&=314{,}858{,}000{,}000 \text{ millimetres} \\ &=195{,}647 \text{ miles per second.} \end{aligned} \quad \ldots \quad (137)$$

The velocity of transverse undulations in our hypothetical medium, calculated from the electro-magnetic experiments of MM. Kohlrausch and Weber, agrees so exactly with the velocity of light calculated from the optical experiments of M. Fizeau, that we can scarcely avoid the inference that *light consists in the transverse undulations of the same medium which is the cause of electric and magnetic phenomena.*

Prop. XVII.—To find the electric capacity of a Leyden jar composed of any given dielectric placed between two conducting surfaces.

Let the electric tensions or potentials of the two surfaces be Ψ_1 and Ψ_2. Let S be the area of each surface, and θ the distance between them, and let e and $-e$ be the quantities of electricity

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* *Comptes Rendus*, vol. xxix. (1849), p. 90. In Galbraith and Haughton's 'Manual of Astronomy,' M. Fizeau's result is stated at 169,944 geographical miles of 1000 fathoms, which gives 193,118 statute miles; the value deduced from aberration is 192,000 miles.

on each surface; then the capacity

$$C=\frac{e}{\Psi_1-\Psi_2}. \quad \ldots \ldots \quad (138)$$

Within the dielectric we have the variation of Ψ perpendicular to the surface

$$=\frac{\Psi_1-\Psi_2}{\theta}.$$

Beyond either surface this variation is zero.

Hence by (115) applied at the surface, the electricity on unit of area is

$$\frac{\Psi_1-\Psi_2}{4\pi E^2\theta}; \quad \ldots \ldots \quad (139)$$

and we deduce the whole capacity of the apparatus,

$$C=\frac{S}{4\pi E^2\theta}; \quad \ldots \ldots \quad (140)$$

so that the quantity of electricity required to bring the one surface to a given tension varies directly as the surface, inversely as the thickness, and inversely as the square of E.

Now the coefficient of induction of dielectrics is deduced from the capacity of induction-apparatus formed of them; so that if D is that coefficient, D varies inversely as E^2, and is unity for air. Hence

$$D=\frac{V^2}{V_1{}^2\mu}, \quad \ldots \ldots \quad (141)$$

where V and V_1 are the velocities of light in air and in the medium. Now if i is the index of refraction, $\frac{V}{V_1}=i$, and

$$D=\frac{i^2}{\mu}; \quad \ldots \ldots \quad (142)$$

so that the inductive power of a dielectric varies directly as the square of the index of refraction, and inversely as the magnetic inductive power.

In dense media, however, the optical, electric, and magnetic phenomena may be modified in different degrees by the particles of gross matter; and their mode of arrangement may influence these phenomena differently in different directions. The axes of optical, electric, and magnetic properties will probably coincide; but on account of the unknown and probably complicated nature of the reactions of the heavy particles on the ætherial medium, it may be impossible to discover any general numerical relations between the optical, electric, and magnetic ratios of these axes.

It seems probable, however, that the value of E, for any given axis, depends upon the velocity of light whose vibrations are parallel to that axis, or whose plane of polarization is perpendicular to that axis.

In a uniaxal crystal, the axial value of E will depend on the velocity of the extraordinary ray, and the equatorial value will depend on that of the ordinary ray.

In "positive" crystals, the axial value of E will be the least and in negative the greatest.

The value of D_1, which varies inversely as E^2, will, *cæteris paribus*, be greatest for the axial direction in positive crystals, and for the equatorial direction in negative crystals, such as Iceland spar. If a spherical portion of a crystal, radius $=a$, be suspended in a field of electric force which would act on unit of electricity with force $=I$, and if D_1 and D_2 be the coefficients of dielectric induction along the two axes in the plane of rotation, then if θ be the inclination of the axis to the electric force, the moment tending to turn the sphere will be

$$\frac{3}{2}\,\frac{(D_1-D_2)}{(2D_1+1)(2D_2+1)}I^2a^3\sin 2\theta, \quad . \quad . \quad . \qquad (143)$$

and the axis of greatest dielectric induction (D_1) will tend to become parallel to the lines of electric force.

IV. *On the Direction of the Joints in the Faces of Oblique Arches.*

By G. B. Airy, *Esq., Astronomer Royal*[*].

MY attention was lately called to the following passage in Mr. Buck's 'Essay on Oblique Bridges,' 2nd edition, p. 7. "After having had several drawings of the faces of oblique arches made on a large scale and projected with great exactitude, we observed that the following remarkable property exists. If the lines, which are the chords of the small curves forming the joints in the face of the arch, be produced, they will all meet in one point O, below the axis of the cylinder; and this property was found to hold even when the obliquity is so great as to depress the point O out of the cylinder altogether." The author then determines the point O by geometrical calculations for the joints at the spring of the arch, and, as far as I can perceive, makes use of this empirical theorem for determining the directions of all the other face-joints.

The theorem is perfectly correct; and the discovery of it bears testimony to the accuracy with which the author's plans must have been drawn, in a process of rather difficult geometry, and to the care with which they have been examined. The theorem, moreover, is true in the utmost generality, as regards the extent

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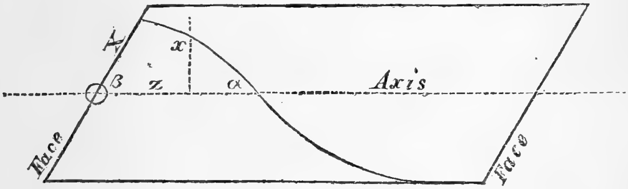
[*] Communicated by the Author.

of the arc of cylinder (applying even when the entire barrel or cylinder is built in spiral courses as for an oblique arch); and as regards the relation between the angle which the face-plane makes with the axis, and the angle which the direction of the spiral courses makes with the axis (no condition whatever being required for either of them). Only, as the joints are slightly curved, it is proper to suppose that the stones are not very deep, and that the geometrical direction used is a tangent to the lower part of the curve of each: the middle of each joint-curve might be used equally well, but the resulting point of convergence would be slightly altered.

The theorem may thus be investigated by the processes of analytical geometry.



Let the diagram represent the horizontal plan of the oblique arch, the curved line being the projection of the spiral in which one of the longitudinal joints meets the cylindrical intrados, or a concentric cylinder (as that which passes through the middle of the stones' depth). Let O be the origin of coordinates, z the ordinate parallel to the axis, of any point in the helical surface which forms the longitudinal joint; x the horizontal ordinate transversal to the axis, of the same point, x being not necessarily terminated in the curved line; y the vertical ordinate, its foot being in the horizontal plane passing through the axis of the cylinder. And let r be the distance of the same point from the axis of the cylinder; θ the inclination of r to the vertical. Also let α be the angle at which the spiral intersects the ridge-line of the cylinder, β the angle at which the face of the arch cuts the same line. Then

$$x = r \sin \theta, \; y = r \cos \theta, \text{ and } \frac{x}{y} = \tan \theta.$$

Now if H be the value of θ in the helical surface when $z=0$ (H having a different numerical value for every different helix, and being the characteristic of the particular longitudinal joint under consideration), θ will $= \mathrm{H} - n.z$; where n is a constant depending on the slope of the spiral, to be expressed more conveniently hereafter. It will be remarked here that the attribution of a constant value to H implies that, in any section of the helical surface transversal to the cylinder, $\frac{x}{y}$ is constant, or the section of the helical surface is a straight line directed to the axis. Hence the equation of the helical surface forming a longitudinal joint is $\frac{x}{y}=\tan\,(\mathrm{H}-n\,.\,z)$.

The equation of the plane of the face of the arch is $x=\tan\beta\,.\,z$.

The combination of these two equations gives the face-joint. The second enables us to eliminate z from the first, and we have $\frac{x}{y}=\tan\,(\mathrm{H}-n.\mathrm{cotan}\,\beta\,.\,x)$. If X be the horizontal coordinate measured in the plane of the face, we can, for joints necessarily in the plane of the face, make $x=\sin\beta\,.\,\mathrm{X}$; and then the equation of the face-joint, by ordinates upon the plane of the face, is

$$y=\mathrm{X}\,.\,\sin\beta\,.\,\mathrm{cotan}\,(\mathrm{H}-n\,.\,\cos\beta\,.\,\mathrm{X}).$$

This is not the equation to a straight line; and therefore the face-joint is curved.

We may, however, determine the direction of the tangent to the face-joint, either where it meets the intrados, or at the middle of the arch-stones' depth, in the following manner:—Let a be the radius of the cylindrical surface which forms the intrados, or of that which passes through the middle of the stones' depth (as the case may be). And let $a+\delta a$ be the radius of a concentric cylindrical surface, of diameter not differing much from the former. Let x, y, z be the ordinates of the point in the face-joint which corresponds to the former cylindrical surface; $x+\delta x, y+\delta y$, $z+\delta z$ those for the point in the same face-joint corresponding to the latter surface. Then (as above), $x=a\,.\,\sin\theta$, $y=a\,.\,\cos\theta$, $z=\frac{\mathrm{H}}{n}-\frac{\theta}{n}$, $x=\tan\beta\,.\,z$. To find from these the variations δx, δy, δz, it must be remarked that θ is to be varied as well as a, because the face-plane cuts the upper part of the helix at a place where the value of z, and consequently the value of θ, are different from those for the intersection with the lower part of the helix. Thus we find,

$$\delta x=\delta a.\sin\theta+a\cos\theta.\delta\theta;$$
$$\delta y=\delta a.\cos\theta-a\sin\theta.\delta\theta;$$
$$\delta z=-\frac{\delta\theta}{n};$$
$$\delta x=\tan\beta.\delta z.$$

From these the following values are easily found (putting $\delta a'$ for $\frac{\delta a}{na.\cos\theta+\tan\beta}$);

$$\delta x=\sin\theta.\tan\beta.\delta a';$$
$$\delta y=(na+\tan\beta.\cos\theta).\delta a';$$
$$\delta z=\sin\theta.\delta a'.$$

These may be considered as coordinates of the point in the face-joint for radius $a+\delta a$, the origin of such coordinates being the point in the face-joint for radius a.

Now take a new system of coordinates, y being the same as before, X horizontal in the face-plane, Z horizontal and perpendicular to the face-plane. Then, by the ordinary formulæ,

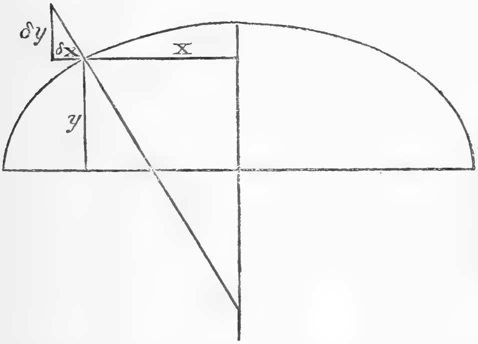
$$\delta Z=\delta z.\sin\beta-\delta x.\cos\beta,$$
$$\delta X=\delta z.\cos\beta+\delta x.\sin\beta,$$

we find on substitution of the values above,

$$\delta Z=0;$$
$$\delta X=(\sin\theta.\cos\beta+\sin\theta.\tan\beta.\sin\beta)\,\delta a'=\frac{\sin\theta}{\cos\beta}\,\delta a';$$
$$\delta y=(na+\tan\beta.\cos\theta)\,\delta a',\text{ as before.}$$

And it will be remembered that, on the face,

$$X=\frac{x}{\sin\beta}=\frac{a}{\sin\beta}\sin\theta;\ y=a.\cos\theta.$$



In the diagram, in which the semiellipse represents the section by the face-plane of the semicylinder whose radius is a, it is easily seen (by similar triangles) that the distance, from the place where X meets the vertical axis, to the place where the face-joint produced meets the vertical axis, is $\frac{X.\delta y}{\delta X}$; and therefore the distance from the centre of the ellipse to the place where the face-joint produced meets the vertical axis is $\frac{X.\delta y}{\delta X}-y$, or $\frac{a}{\sin\beta}.\sin\theta.\frac{\cos\beta}{\sin\theta}(na+\tan\beta.\cos\theta)-a\cos\theta$, or $na^2.\text{cotan}\,\beta$. As this expression is independent of θ, or of the position of the face-joint under consideration, it follows that all the face-joints produced meet in one point, whose distance below the centre of the ellipse is $na^2.\text{cotan}\,\beta$.

A more convenient form may thus be given to the expression. In the first diagram, consider the spiral in which the helix cuts the cylinder whose radius is a. Then $x=a.\sin\theta$, $y=a.\cos\theta$, $z=\frac{H}{n}-\frac{\theta}{n}$. At the crown, $\theta=0$, $x=0$, $z=\frac{H}{n}$. When θ has a small value, $\delta x=a\theta$, $\delta z=-\frac{\theta}{n}$, therefore $\frac{\delta x}{\delta z}=-an$. But $\frac{\delta x}{\delta z}$ for one continuous spiral, at the crown of the arch, is evidently in the diagram $=-\tan\alpha$. Therefore $-an=-\tan\alpha$. Hence the distance, below the centre of the ellipse, of the point at which all the face-joints produced will meet, is $a.\tan\alpha.\text{cotan}\,\beta$.

It is to be remarked that this investigation is in no way limited to that extent of arch which is conveniently adopted in practice, never exceeding a semicylinder; but applies geometrically to the entire cylinder. And if $\tan\alpha.\text{cotan}\,\beta$ exceeds 1, the joints nowhere tend to become horizontal, but are in certain parts of the circumference parallel to the tangent.

In practice, the angle α must not differ greatly from the complement of β; and, in order to divide the obliquity of bearing of surfaces between the crown and the spring of the arch, α must be a little less than that complement. The distance, therefore, below the centre of the ellipse, of the point at which all the face-joints produced will meet, is a little less than $a.(\text{cotan}\,\beta)^2$.

November 27, 1861.

V. *On the Relation of the Lateral Contraction to the Longitudinal Expansion in Rods of Spring Steel.* *By* G. KIRCHHOFF*.

[With a Plate.]

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, Heidelberg, October 17, 1861.

IN the Number of the Philosophical Magazine for June 1861, I find the translation of a paper by M. G. Wertheim, the chief subject matter of which consists in a polemic against a memoir of my own published in vol. cviii. of Poggendorff's *Annalen*. Allow me to express the wish that you will publish my original memoir in your Journal, so that your readers may be enabled to decide whether the strictures which M. Wertheim has made are founded on fact or not.

I am, Gentlemen,
Your obedient Servant,
G. KIRCHHOFF.

If a homogeneous cylinder, whose elasticity is the same in

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* Translated from Poggendorff's *Annalen*, vol. cviii. p. 369, by Dr. E. Atkinson.

different directions, is expanded in a longitudinal direction by a strain, its lateral dimensions undergo a contraction. From Poisson's theoretical considerations, the relation of the lateral contraction to the longitudinal expansion should always be $\frac{1}{4}$; Wertheim concluded from his experiments that it is $\frac{1}{3}$. According to a view which has been repeatedly expressed, it has neither one value nor the other, and differs in different substances. In most bodies, in which the same elasticity can be assumed in different directions, the experimental determination of this relation is hindered by the circumstance that, even with very slight changes of form, permanent expansion and elastic secondary effects take place in them to a considerable extent. This is the case with ignited metal wires and glass rods. With hard-drawn metal wires a permanent expansion and an elastic secondary action are much less perceptible; but in them the elasticity is certainly different in different directions. In the case of hardened steel rods, on the contrary, it may with probability be assumed that there is equal elasticity in different directions; and since, moreover, these more closely resemble an ideal elastic body than hard-drawn wires do, they appear especially fitted for determining the value of that relation. I have made such experiments on several round rods of spring steel, about 2·85 millims. in diameter, and about 300 millims. in length, and will here describe them.

In fig. 1, Plate I., let A^0 A' represent an elastic rod which is fixed horizontally at A^0; let A' B' be a horizontal arm fixed on A' at right angles to the longitudinal direction of the rod. If a weight P is suspended at B', it will produce both a flexion and a torsion. On the free end of the rod let a mirror C' be so fastened that its surface is almost horizontal; let a telescope be directed from above upon the mirror, and a scale consisting of two systems of lines cutting each other at right angles at equal distances, be so fixed horizontally that its image appears in the telescope. The flexion and the torsion may then be simultaneously observed. Assuming that the section of the bar is a circle, the coefficient of elasticity may be calculated from its radius, the weight P, the length A^0 A', and from the observed flexion; the coefficient of torsion may be calculated from the radius of the rod, the weight P, the length A^0 A', and the torsion observed. Assuming that the elasticity is equal in different directions, the desired relation of the lateral contraction to the longitudinal expansion may be readily calculated from these two coefficients. In determining this relation a knowledge of the radius of the rod is unnecessary, since it occurs in the same form in the expressions for the coefficients of elasticity and of torsion.

The rods which I subjected to these experiments were pieces of drawn wires; it cannot, therefore, be assumed with certainty that the section is a circle; but it may be assumed without appreciable error that the section is an ellipse, in which the square of the eccentricity may be neglected. By this assumption the influence of the deviation of the section from the circular shape may be eliminated. It is simply necessary to turn the bar about its axis by 90°, to fix the arm A′ B′ again horizontal on A′, and to observe for the second time the change of form which the rod experiences if the weight P′ is suspended at B′. The torsion must then be found to be as great as in the first position of the rod, but in general the flexion will be different. The torsion in either of the positions, and the arithmetical mean of the two flexions are as great as the torsion and the flexion would be if the section of the rod were a circle whose radius is the mean of the two semidiameters of the elliptical section.

The arm A′ B′ is not easily brought exactly at right angles to the axis of the rod; a slight obliquity influences the flexion, but not the torsion. This influence I have endeavoured to neutralize in the following manner:—The arm A′ B′ is half of a cross rod B′ D′; after the weight P has acted at B′, I suspend it at D′, and take the mean of the flexions observed in both cases. The torsion must be the same in both cases if the axis of the rod passes through the middle of B′ D′. If this condition is not fulfilled the torsions are different, but their mean is then as great as the torsion would be if the length of each arm were exactly equal to half of B′ D′.

In order to render myself independent of the supposition that the part of the rod at A^0 exactly retains its position, if the weight P is suspended to the end of its lever arm, I made the arrangement that the rod A^0 A′ is the half of a bar A′ A″, which supports at A″ a cross rod B″ D″ of the same dimensions as B′ D′, and a mirror C″, upon which a second telescope is directed. The rod is fixed at A^0 in a piece of thin metal plate; equal weights are suspended at B′ and B″, or at D′ and D″, and the image of the same scale is observed in both mirrors.

The apparatus which I have used is represented in perspective in fig. 2, Plate I. On the wall of the observing-room four boards, A, B, B, C, are fixed; on one of these, A, the scale is fixed, and on two others the two telescopes; the last supports the rod which is to be experimented upon. On the board A and at right angles to it, are two horizontal arms *a*, *a*; on each of these are fixed two small pieces of wood projecting inwards, through which the screws *α*, *α* are passed from below. The scale rests on these screws, and is made horizontal by means of a level. The scale is printed on paper, and stretched upon a glass plate.

Fig. 3, shows part of it. One of its axes is parallel to the wall, the other is at right angles thereto; I shall call the first the ξ axis, and the second the η axis.

The boards B, B, and C support the brackets *b*, *b*, and *c*, which project somewhat further than the arms *a*, *a*, and of which the first two support two telescopes β, β, magnifying about thirty times. The visual axes of these are placed vertically. To effect this, a cross consisting of two wires is stretched below each object-glass, and the telescope is so arranged that the image which a mercury horizon gives of the intersection of these wires coincides with the cross-hair of the telescope.

On the bracket *c* is suspended the rod whose changes of form are to be measured. Through the bracket, and near its front end, a vertical rectangular aperture is made, which is partially filled with a piece of wood; this is supported by the four lateral screws γ, γ, γ', γ', the points of which catch in cavities made in the wood. Of these four screws, the first two are in a horizontal, and the last two in a vertical plane. By their means the piece of wood can be shifted in the direction of the ξ axis, and rotated about two axes, one of which is almost vertical, and the other almost parallel to the η axis. The piece of wood is perforated vertically; through the perforation the tail of a small vice is passed from below, and, by means of a nut, fastened against the piece of wood, so that it only moves with considerable friction. In the jaws of the vice a small piece of steel plate is held, which has an aperture just as large as the section of the rod under investigation. Through this aperture the rod is pushed to its middle, and soldered here by a little tin. By means of these arrangements a position can be given to the rod (which is indicated by *d*, fig. 2, Plate I.) in which its axis is horizontal and parallel to the ξ axis, and the cross rods *e*, *e* fixed to it are as nearly horizontal as possible, even if they are not quite parallel to one another. For this purpose a level provided with hooks is so suspended to the rod *d*, that its middle is under the middle of the rod, and by turning the screws γ', γ' its bubble is brought to the centre. Thereupon the level is suspended to one of the cross rods *e*, *e*, and this made horizontal by turning the piece of metal which holds the rod *d*. If both cross rods are parallel to one another, the second must also be horizontal. It is ascertained whether this is the case by suspending the level to it. A small deflection need not be taken into account. If it occurs, the rod *d* is most conveniently arranged so that both cross rods deviate from the horizontal to the same extent in opposite directions. Finally, the motion which is necessary to arrange the axis of the bar *d* parallel to the ξ axis, can be produced partly by turning the vice in the piece of wood which supports this, and partly by the screws γ, γ. In order to ascertain whether the desired parallelism exists, I have adopted the following method:—In two points of the ξ axis of the scale are fastened the threads *f, f,* which terminate below in two loops. In these loops a rod *g,* of similar dimensions to those of the rod *d,* is placed. The length of the threads is so chosen that both rods are of about the same height, one behind the other. On the rod *g* is suspended a strip of mirror by means of hooks, which are like the hooks of a level, fig. 4, Plate I.; upon this mirror is directed a telescope, in front of whose object-glass a plummet is so suspended that the image of the plummet is covered by the vertical cross-hair of the telescope. Thereupon the same strip of mirror is suspended to the rod *d,* and this is so arranged that, when the position of the telescope remains unchanged, the image is covered as before.

On the ends of the rod *d* are screwed the supports of two silver mirrors *h, h,* the centres of which must be almost in the lines of vision of the two telescopes. To judge whether this is the case, there are two leads suspended to the intersection of the threads, which, as already mentioned, are underneath each of the two object-glasses. Each mirror can be moved within narrow limits towards its support by means of three pairs of small screws, and so arranged that suitable points of the scale are in the field of vision of the corresponding telescope.

Fig. 5 represents one of the two cross-rods. It is made of steel plate: in its middle is a hollow, the sides of which are knife-edges; when this is placed on the rod *d,* these edges fit into fine incisions made with the file on the bar *d,* and are soldered here by a small quantity of tin. On the ends of the cross bar, which are somewhat thickened, steel points *b, b* are screwed; they serve to support the rings *c, c,* on which fine wires are attached, terminating in hooks: on these are suspended the weights which are to bend and twist the bar *d.*

If it be allowable to consider the angle under which the rays reaching the telescope are reflected to be infinitely small, we may directly take as measures for the flexions and torsions of the two halves of the rods the changes of the coordinates of the points of the scales, the images of which are covered by the intersections of the two cross-hairs. But in the present experiments the finite magnitude of these angles must be allowed for. This correction renders some closer considerations necessary.

I establish a triaxial rectangular system of coordinates, two of whose axes shall be the ξ axis and the η axis of the scale: of these the first has the direction of the line $A' A''$, fig. 1, Plate I.; the second the direction of the line $A' D'$; the third, which I will call the ζ axis, shall be turned vertically backwards. The rod to be submitted to experiment I first consider to be made straight, which can be effected by bringing supports near its ends, and placing them so that a level suspended upon either of the halves of the rod shows it to be true. The axis of the rod is then parallel to the ξ axis. Starting from a variable point of the axis of the rod, I suppose three axes at right angles to each other, which I call x axis, y axis, z axis, which are firmly connected with the molecules of the rod, and in the above mentioned position of the latter are parallel to the axes ξ, η, ζ. If the bar undergoes a change of position and form, the former axes make with the latter angles whose cosine I will indicate by

$$\begin{array}{lll} \alpha_0, & \beta_0, & \gamma_0, \\ \alpha_1, & \beta_1, & \gamma_1, \\ \alpha_2, & \beta_2, & \gamma_2; \end{array}$$

so that the indices 0, 1, 2 respectively refer to the x axis, the y axis, and the z axis. Further, let ξ, η, ζ be the coordinates in reference to the ξ, η, ζ axes of the points from which the x, y, z axes proceed. The sign of these three coordinates and of the new cosine I will designate by the sign ^{0}, or $'$, or $''$ placed above, when they are to refer to the points of the axis of the rod A^0, A', A'', fig. 1, Plate I.

If the supports by which the bar is made straight are removed, it becomes curved in consequence of its own weight, of the weight of the mirrors, their supports and their cross-rods. In order not to make the considerations needlessly complicated, I shall assume that this curvature can be considered as being produced by equal weights which act at A' and A''; let this weight be G. The magnitudes of the equal weights which are to be suspended in B' and B'', or in D' and D'', I designate by P as before. Half of $A'A''$ I denote by s, and a quarter of the sums of $B'B''$ and $D'D''$ by l. In order to simplify the calculations somewhat, I assume that

$$A'A^0 = A''A^0,$$

and

$$B'A' = D'A' = B''A'' = D''A'';$$

but observe that the final result also holds good even if these equations are not fulfilled. The radius of the section of the bar which is assumed to be circular shall be ρ. The coefficients of elasticity I set, in accordance with designations which I have used in earlier papers on elasticity,

$$=2K\frac{1+3\theta}{1+2\theta};$$

the relation of the lateral contraction to the longitudinal expansion in the case in which the bar is expanded by a longitudinal

strain,
$$=\frac{\theta}{1+2\theta}.$$

For shortness' sake, I further make

$$\mathrm{L}=\frac{\pi}{2}\rho^4\mathrm{K},\quad \mathrm{N}=\frac{\pi}{2}\rho^4\mathrm{K}\frac{1+3\theta}{1+2\theta}.$$

The alterations of form of the bar are to be considered as infinitely small; we have then always

$$\alpha_0=1,\qquad \beta_1=1,\qquad {}_2=1,$$
$$\beta_2+\gamma_1=0,\quad \gamma_0+\alpha_2=0,\quad \alpha_1+\beta_0=0.$$

For the case in which the weights P do not act, we have*

$$\left.\begin{aligned}
a'_2&=a^0_2+\frac{\mathrm{G}s^2}{2\mathrm{N}},\\
\beta'_2&=\beta^0_2,\\
\zeta'&=\zeta^0+\alpha^0_2s+\frac{\mathrm{G}s^3}{3\mathrm{N}},\\
\alpha''_2&=\alpha^0_2-\frac{\mathrm{G}s^2}{2\mathrm{N}},\\
\beta''_2&=\beta^0_2,\\
\zeta''&=\zeta^0-\alpha^0_2s+\frac{\mathrm{G}s^3}{3\mathrm{N}};
\end{aligned}\right\}\quad \cdot\ \cdot\ \cdot\ \cdot\ (1)$$

from which there follows,

$$\alpha'_2-\alpha''_2=\frac{\mathrm{G}s^2}{\mathrm{N}},$$
$$\beta'_2-\beta''_2=0.$$

For the case in which the weights P are placed at B′ and B″, we have

$$\left.\begin{aligned}
\alpha'_2&=\alpha^0_2+\frac{(\mathrm{G}+\mathrm{P})s^2}{2\mathrm{N}}\\
\beta'_2&=\beta^0_2+\frac{\mathrm{P}ls}{\mathrm{L}},\\
\zeta'&=\zeta^0+\alpha^0_2s+\frac{(\mathrm{G}+\mathrm{P})s^3,}{3\mathrm{N}}\\
\alpha''_2&=\alpha^0_2-\frac{(\mathrm{G}+\mathrm{P})s^2}{2\mathrm{N}},\\
\beta''_2&=\beta^0_2-\frac{\mathrm{P}ls}{\mathrm{L}},\\
\zeta''&=\zeta^0-\alpha^0_2s+\frac{(\mathrm{G}+\mathrm{P})s^3}{3\mathrm{N}};
\end{aligned}\right\}\quad \cdot\ \cdot\ (2)$$

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* Compare my paper "On the Equilibrium and Motion of an infinitely thin Elastic Rod," Journal for Pure and Applied Mathematics, vol. lvi.

from which follow,

$$\alpha'_2-\alpha''_2=\frac{(G+P)s^2}{N},$$

$$\beta'_2-\beta''_2=\frac{2\,Pls}{L}.$$

I shall indicate, by prefixing a δ, the changes which the magnitudes in question experience by the weights P being suspended at B′ and B″; we have then

$$\delta\alpha'_2-\delta\alpha''_2=\frac{Ps^2}{N},$$

$$\delta\beta'_2-\delta\beta''_2=\frac{2Pls}{L},$$

from which is obtained

$$\frac{N}{L}=1+\frac{\theta}{1+2\theta}=\frac{\delta\beta'_2-\delta\beta''_2}{\delta\alpha'_2-\delta\alpha''_2}\,\frac{s}{2l}. \quad . \quad . \quad . \quad (3)$$

If the weights P are suspended in D′ and D″ instead of at B′ and B″, the same equations prevail, provided $-l$ is substituted for l.

It must now be shown how $\delta\alpha'_2$, $\delta\alpha''_2$, $\delta\beta'_2$, $\delta\beta''_2$ are found from the readings of the scale. For this purpose I shall take into account the directions of the normals to the mirrors directed downwards, and designate them by n' and n''. For the sake of shortness I will set

$$\cos(n'\xi)=\alpha', \quad \cos(n'\eta)=\beta', \quad \cos(n'\zeta)=\gamma',$$

$$\cos(n''\xi)=a'', \quad \cos(n''\eta)=\beta'', \quad \cos(n''\zeta)=\gamma''.$$

In fig. 6, let O ζ be the visual axis of the first telescope, Oξ and Oη be two lines drawn in the plane of the scale parallel to the ξ axis and the η axis; let the point ζ be the intersection of the visual line with the plane of the mirror, N be the intersection of the normal to the mirror drawn from the point ζ with the plane of the scale, and let S be the point of the scale the image of which lies in the visual line. The coordinates of the point ζ, in reference to the ξ, η, ζ axes, I designate by a', b', c', those of the point S by X′, Y′, o; those of the point N by A′, B′, o. We have then

$$A'-a'=-N\zeta\,.\,\alpha',$$

$$B'-b'=-N\zeta\,.\,\beta',$$

$$-c'=-N\zeta\,.\,\gamma';$$

hence

$$A'-a'=-\frac{\alpha'}{\gamma'}c',$$

$$B'-b'=-\frac{\beta'}{\gamma'}c'.$$

But since the line Nζ lies in the plane of the triangle OSζ, and bisects its angle at ζ, we have

$$X'-a'=(A'-a')\frac{2\gamma'^2}{2\gamma'^2-1},$$

$$Y'-b'=(B'-b')\frac{2\gamma'^2}{2\gamma'^2-1}.$$

From which there follows,

$$\left.\begin{aligned}X'-a'&=-\alpha'\frac{2\gamma'}{2\gamma'^2-1}c',\\ Y'-b'&=-\beta'\frac{2\gamma'}{2\gamma'^2-1}c'.\end{aligned}\right\}\quad\dots\dots\quad(4)$$

By a corresponding notation we obtain for the second mirror,

$$\left.\begin{aligned}X''-a''&=-\alpha''\frac{2\gamma''}{2\gamma''^2-1}c'',\\ Y''-b''&=-\beta''\frac{2\gamma''}{2\gamma''^2-1}c''.\end{aligned}\right\}\quad\dots\dots\quad(5)$$

If $(n'x)$, $(n'y)$, $(n'z)$ are the angles which n' the normal to the mirror forms with the axes of x, y, z proceeding from A$'$, we have

$$\alpha'=\cos(n'x)+\alpha'_1\cos(n'y)+\alpha'_2\cos(n'z),$$

$$\beta'=\beta'_0\cos(n'x)+\cos(n'y)+\beta'_2\cos(n'z).$$

α'_1 or $-\beta'_0$ is the angle by which the rod has twisted about a vertical axis from the position in which its axis is parallel to the ξ axis; this angle, if not $=0$, is certainly very small; and since $\cos(n'y)$ and $\cos(n'x)$ are also only small magnitudes, we may put

$$\alpha'=\cos(n'x)+\alpha'_2\cos(n'z),$$

$$\beta'=\cos(n'y)+\beta'_2\cos(n'z).$$

In order to designate the values which the magnitudes under consideration assume in the case in which the rod is straightened in the manner indicated above, I shall place a — over them. In that case

$$\cos n(n'x)=\overline{\alpha'},\quad \cos(n'y)=\overline{\beta'},\quad \cos(n'z)=\overline{\gamma'}.$$

In consequence of this, the last equations may be written

$$\left.\begin{aligned} \alpha'_2 &= \frac{\alpha' - \overline{\alpha'}}{\overline{\gamma'}}, \\ \beta'_2 &= \frac{-\overline{\beta'}}{\overline{\gamma'}}. \end{aligned}\right\} \quad \ldots\ldots \quad (6)$$

In like manner we get

$$\left.\begin{aligned} \alpha''_2 &= \frac{\alpha'' - \overline{\alpha''}}{\overline{\gamma''}}, \\ \beta''_2 &= \frac{\beta'' - \overline{\beta''}}{\overline{\gamma''}}. \end{aligned}\right\} \quad \ldots\ldots \quad (7)$$

With the aid of these equations expressions for c' and c'' can be developed. Let the equation of the plane of the first mirror in reference to the axes of x, y, and z drawn from the point A$'$ be

$$x \cos(n'x) + y \cos(n'y) + z \cos(n'z) - D' = o\,;$$

the equation of the same plane in reference to the axes of ξ, η, ζ is then

$$(\xi - \xi')\alpha' + (\eta - \eta')\beta' + (\zeta - \zeta')\gamma' - D' = o.$$

The point for which ξ is $= a'$, $\eta = b'$, $\zeta = c'$ belongs to this plane; hence

$$(a' - \xi')\alpha' + (b' - \eta')\beta' + (c' - \zeta')\gamma' - D' = o\,;$$

from which there follows

$$c' = \zeta' + \frac{D' + (\xi' - a')\alpha' + (\eta' - b')\beta}{\gamma'}.$$

In my apparatus D$'$ and $\eta' - b'$ were a few millimetres, $\xi' - a'$ a few centimetres, c' more than two metres; hence, considering the smallness of α' and β', we may write instead of this equation,

$$c' = \zeta' + D' + (\xi' - a')\alpha'\,;$$

and for α' it is permissible to substitute an approximate value, even if not very near. I set

$$\left.\begin{aligned} \overline{c'} &= C + h, \\ \overline{c''} &= C - h, \end{aligned}\right\} \quad \ldots\ldots \quad (8)$$

in which h is a very small magnitude as compared with C. In consequence of equation (4), I can make

$$\alpha' = -\frac{X' - a'}{2C} \quad \ldots\ldots \quad (9)$$

I then obtain

$$c' = \zeta' + D' - \frac{(\xi' - a')(X' - a')}{2C}\,;$$

from which is obtained

$$\bar{c}'=\bar{\zeta}'+\mathrm{D}'-\frac{(\xi'-a')(\bar{\mathrm{X}}'-a')}{2\mathrm{C}}.$$

Hence we have with reference to the equations (8),

$$c'=\mathrm{C}+h+\zeta'-\bar{\zeta}'-\frac{(\xi'-a')(\mathrm{X}'-\bar{\mathrm{X}}')}{2\mathrm{C}};$$

or, since $\bar{\zeta}'=\zeta^0$,

$$c'=\mathrm{C}+h+\zeta'-\zeta^0-\frac{(\xi'-a')(\mathrm{X}'-\bar{\mathrm{X}}')}{2\mathrm{C}}.$$

In the same way may be obtained

$$c''=\mathrm{C}-h+\zeta''-\zeta^0-\frac{(\xi''-a'')(\mathrm{X}''-\bar{\mathrm{X}}'')}{2\mathrm{C}}.$$

But from equations (1) and (2) we have

$$\zeta'-\zeta^0=\ \ \frac{5}{6}s\alpha'_2+\frac{1}{6}s\alpha''_2,$$

$$\zeta''-\zeta^0=-\frac{1}{6}s\alpha'_2-\frac{5}{6}s\alpha''_2;$$

from equations (6) and (9) it follows that approximately

$$\alpha'_2=-\frac{\mathrm{X}'-\bar{\mathrm{X}}'}{2\mathrm{C}},$$

and also

$$\alpha''_2=-\frac{\mathrm{X}''-\bar{\mathrm{X}}''}{2\mathrm{C}}.$$

Hence we obtain

$$c'=\mathrm{C}+h-\frac{1}{2}\mathrm{C}\left[\left(\frac{5}{6}s+\xi'-a'\right)(\mathrm{X}'-\bar{\mathrm{X}}')+\frac{1}{6}s(\mathrm{X}''-\bar{\mathrm{X}}'')\right],$$

$$c''=\mathrm{C}-h+\frac{1}{2}\mathrm{C}\left[\frac{1}{6}s(\mathrm{X}'-\bar{\mathrm{X}}')+\left(\frac{5}{6}s-(\xi''-a'')\right)(\mathrm{X}''-\bar{\mathrm{X}}'')\right].$$

Let these values of c' and c'' be substituted in equations (4) and (5), and put for γ' and γ'' the approximate values

$$\left.\begin{aligned}\gamma'&=1-\frac{(\mathrm{X}'-a')^2+(\mathrm{Y}'-b')^2}{8\mathrm{C}^2},\\ \gamma''&=1-\frac{(\mathrm{X}''-a'')^2+(\mathrm{Y}''-b'')^2}{8\mathrm{C}^2},\end{aligned}\right\}\quad .\ .\ .\ .\quad (10)$$

which are easily obtained from the equation (9), and from others formed in a similar manner. Neglecting small members of a higher order, we get then

$$\left.\begin{aligned}
\alpha' &= -\frac{X'-a'}{2C}\left\{1-\frac{h}{C}-\frac{3}{8C^2}\left[(X'-a')^2+(Y'-b')^2\right]\right.\\
&\qquad\left.+\frac{1}{8C^2}\left[\left(\frac{10}{3}s+4(\xi'-a')\right)(X'-\overline{X'})+\frac{2}{3}s(X''-\overline{X''})\right]\right\},\\
\beta' &= -\frac{Y'-b'}{2C}\left\{1-\frac{h}{C}-\frac{3}{8C^2}\left[(X'-a')^2+(Y'-b')^2\right]\right.\\
&\qquad\left.+\frac{1}{8C^2}\left[\left(\frac{10}{3}s+4(\xi'-a')\right)(X'-\overline{X'})+\frac{2}{3}s(X''-\overline{X''})\right]\right\},\\
\alpha'' &= -\frac{X''-a''}{2C}\left\{1+\frac{h}{C}-\frac{3}{8C^2}\left[(X''-a'')^2+(Y''-b'')^2\right]\right.\\
&\qquad\left.-\frac{1}{8C^2}\left[\left(\frac{2}{3}s(X'-\overline{X'})\right)+\left(\frac{10}{3}s-4(\xi''-a'')\right)(X''-\overline{X''})\right]\right\},\\
\beta'' &= -\frac{Y''-b''}{2C}\left\{1+\frac{h}{C}-\frac{3}{8C^2}\left[(X''-a'')^2+(Y''-b'')^2\right]\right.\\
&\qquad\left.-\frac{1}{8C^2}\left[\frac{2}{3}s(X'-\overline{X'})+\left(\frac{10}{3}s-4(\xi''-a'')\right)(X''-\overline{X''})\right]\right\}.
\end{aligned}\right\}\ .\ (11$$

By the aid of these expressions the values of α'_2, α''_2, β'_2, β''_2 may be formed from equations (6) and (7). I will not draw up these values themselves, but instead of them the values of four magnitudes which I denote by (X'), (Y'), (X''), (Y''), and which I define by the following equations:—

$$\left.\begin{aligned}
(X') &= a'-2C\left(\alpha'_2+\frac{\overline{\alpha'}}{\gamma'}\right),\\
(Y') &= b'-2C\left(\beta'_2+\frac{\overline{\beta'}}{\gamma'}\right),\\
(X'') &= a''-2C\left(\alpha''_2+\frac{\overline{\alpha''}}{\gamma''}\right)\\
(Y'') &= b''-2C\left(\beta''_2+\frac{\overline{\beta''}}{\gamma''}\right)
\end{aligned}\right\}\quad .\ .\ .\ .\quad (12)$$

On the one hand we get from this

$$\delta(X') = -2C\delta\alpha'_2,\quad \delta(Y') = -2C\delta\beta'_2,$$
$$\delta(X'') = -2C\delta\alpha''_2,\quad \delta(Y'') = -2C\delta\beta''_2\,;$$

and also taking into account the equation (3),

$$1+\frac{\theta}{1+2\theta}=\frac{\delta(Y'')-\delta(Y')}{\delta(X'')-\delta(X')}\frac{s}{2l}.\quad .\ .\ .\ .\quad (13)$$

On the other hand there is obtained from equations (12), taking into account equations (6) and (7),

$$(X') = a' - 2C\frac{\alpha'}{\gamma'},$$

$$(Y') = b' - 2C\frac{\beta'}{\gamma'},$$

$$(X'') = a'' - 2C\frac{\alpha''}{\gamma''},$$

$$(Y'') = b'' - 2C\frac{\beta''}{\gamma''}.$$

Taking now equations (11), and placing for $\overline{\gamma}'$ and $\overline{\gamma}''$ the approximate values,

$$\overline{\gamma}' = 1 - \frac{(\overline{X}' - a')^2 + (\overline{Y}' - b')^2}{8C^2},$$

$$\overline{\gamma}'' = 1 - \frac{(\overline{X}'' - a'')^2 + (\overline{Y}'' - b'')^2}{8C^2},$$

which are obtained from equations (10), and neglecting further small values of higher order, we obtain

$$\begin{aligned}(X') &= X' + (X' - a')\,F',\\ (Y') &= Y' + (Y' - b')\,F',\\ (X'') &= X'' + (X'' - a'')\,F'',\\ (Y'') &= Y'' + (Y'' - b'')\,F'',\end{aligned}$$

where

$$F' = -\frac{h}{C} + \frac{1}{8C^2}\Big[(\overline{X}' - a')^2 + (\overline{Y}' - b')^2 - 3\,[(X' - a')^2 + (Y' - b')^2] + \left(\frac{10}{3}s + 4(\xi' - a')\right)(X' - \overline{X}') + \frac{2}{3}s(X'' - \overline{X}'')\Big]$$

and

$$F'' = \frac{h}{C} + \frac{1}{8C^2}\Big[(\overline{X}'' - a'')^2 + (Y'' - b'')^2 - 3\,[(X'' - a'')^2 + (Y'' - b'')^2] - \frac{2}{3}s(X' - \overline{X}') - \left(\frac{10}{3}s - 4(\xi'' - a'')\right)(X'' - \overline{X}'')\Big].$$

The observations made are calculated according to these formulæ. Of the magnitudes occurring in them, X', Y', X'', Y'', $\overline{X}'$, $\overline{Y}'$, $\overline{X}''$, $\overline{Y}''$ were obtained directly from readings of the images of the scale; a', b', a'', b'' from the measured distances of the plummets *i*, *i* (fig. 2, Plate I.), and of two plummets formed of the threads *f*, *f*; $\xi' - a'$ and $\xi'' - a''$ were measured with a circle. There only remains for further discussion the manner in which the magnitudes C and h, or the magnitudes $\overline{C'}$, $\overline{C''}$, which are connected with them by the equations (8), were determined.

On the bracket which supported the rod submitted to experiment, a point was fixed, the depth of which below the scale was once for all determined on a large scale. In front of the elastic rod a cathetometer was adjusted; after the rod had been fixed and made straight in the manner previously described, the depth of a certain point of each reflecting surface below the above point was measured with the cathetometer; for after the height of the point had been read off on the scale of the cathetometer, its telescope was so arranged by turning around its vertical axis that its vertical thread covered one of the plummets i, and then it was lowered until the intersection of its threads appeared to fall on the front edge of the corresponding reflecting surface. The point upon which the telescope was then set, is the intersection of three planes, the equations of which have to be formed. One of these planes is the reflecting surface; it has the equation (if the mirror is the first)

$$(\xi-a')\overline{a'}+(\eta-b')\overline{\beta'}+(\zeta-\overline{c'})\gamma'=0.$$

A second plane is the vertical laid through the anterior edge of the mirror; let its equation be

$$\eta-r'=0.$$

The third plane is that which passes through the plummet i, and the axis of rotation of the cathetometer; if a''' and b''' are the ξ and η ordinates of this axis of rotation, the equation of this plane is

$$(\xi-a')(b'''-b')-(\eta-b')(a'''-a')=0.$$

If Z′ be the ζ ordinate of the point upon which the telescope of the cathetometer was set, we get from these three equations,

$$\overline{c'}=Z'+\frac{r'-b'}{\overline{\gamma'}}\left(\overline{\beta'}+\frac{a'''-a'}{b'''-b'}\overline{\alpha'}\right),$$

or approximately,

$$\overline{c'}=Z'-\frac{r'-b'}{2C}\left((\overline{Y'}-b')+\frac{a'''-a'}{b'''-b'}(\overline{X'}-a')\right).$$

By a similar notation we may obtain in the same manner,

$$\overline{c''}=Z''-\frac{r''-b''}{2C}\left((\overline{Y''}-b'')+\frac{a'''-a''}{b'''-b''}(\overline{X''}-a'')\right);$$

$\overline{c'}$ and $\overline{c''}$ are calculated from these equations by taking an approximate value for C.

I now pass on to a statement of the numerical results which the observations and measurements have given.

As far as the scale is concerned, the parts of each axis were not found exactly equal, yet the differences which they exhibited were so small that they may be here neglected. The differences of the mean value of one part of the ξ axis, and of the mean value of a part of the η axis, were, however, more considerable. From the measurements made the former is 1·7993 millim., the latter 1·8086 millim.

An approximate value for C is 2357 millims. In the experiments the particulars of which I here communicate, we had in millimetres,

$$a' = -24{\cdot}1, \quad b' = -147{\cdot}0, \quad a'' = 329{\cdot}6, \quad b'' = -152{\cdot}3,$$
$$a''' = 147{\cdot}5, \quad b''' = -1512, \quad 2l = 108{\cdot}85.$$

With a steel rod of about the dimensions given at the commencement, which I will indicate by No. 1,

$$s = 145{\cdot}04 \text{ millims.};$$

and in the first adjustment,

$$\mathrm{X}' = 143{\cdot}2, \quad \overline{\mathrm{Y}'} = 93{\cdot}3, \quad \overline{\mathrm{X}''} = 12{\cdot}4, \quad \overline{\mathrm{Y}''} = 98{\cdot}0,$$
$$\mathrm{Z}' = 2355{\cdot}2 \text{ millims.}, \quad \mathrm{Z}'' = 2355{\cdot}5 \text{ millims.},$$
$$\eta' - b' = -21, \quad \eta'' - b'' = -20,$$
$$\xi' - a' = 35, \quad \xi'' - a'' = -29.$$

Using weights of 100 gr., the following readings were obtained:—

	X'.	Y'.	X''.	Y''.
0 . . .	137·2	88·2	25·8	92·5
100 . . .	101·2	122·9	63·0	56·4
0 . . .	137·0	88·1	25·6	92·4
100 . . .	101·8	53·0	61·8	127·3
0 . . .	137·2	88·5	25·7	92·8
100 . . .	101·2	122·8	62·9	56·4
0 . . .	137·1	88·0	25·6	92·3
100 . . .	101·4	52·8	61·4	127·1
0 . . .	137·0	88·2	25·5	92·6

The readings were made first without the weights, then after these had been suspended at D' and D'' (fig. 1), then after removing them, then when they acted at B' and B'', then again after their removal, then after they had been again placed at D' and D'', and so on. I have observed the rod several times under similar conditions, first, in order to obtain greater accuracy than one observation would afford; and secondly, in order to notice whether if, after the weights had been removed, a perceptible part of the flexion or torsion produced by them remained.

If this was the case, it showed itself in a difference of the differences $X'-X''$ and $Y'-Y''$, on the observations in which the weights did not act. There was such a difference, but in all sets of observations it seldom exceeded 0·2 of a division of the scale, and can therefore be readily accounted for from errors of observation and accidental disturbances.

From the directly observed values of X', Y', X'', Y'', I have formed the following values by taking the mean between those which held for the same conditions:—

	X'.	Y'.	X''.	Y''.
0 . . .	137·10	88·20	25·64	92·52
100 . . .	101·20	122·85	62·95	56·40
100 . . .	101·60	52·90	61·60	127·20

From these are obtained

	(X').	(Y').	(X'').	(Y'').
0 . . .	136·00	86·96	26·91	91·08
100 . . .	100·20	121·07	63·45	55·81
100 . . .	101·20	52·43	62·67	125·42

From the first and second of these horizontal series it follows that,

$$\frac{\delta(X'')-\delta(X')}{2}=36{\cdot}17,\quad \frac{\delta(Y'')-\delta(Y')}{2}=-34{\cdot}69;$$

from the first and third,

$$\frac{\delta(X'')-\delta(X')}{2}=35{\cdot}28,\quad \frac{\delta(Y'')-\delta(Y')}{2}=34{\cdot}43.$$

Half the sum of the two values of $\frac{\delta(X'')-\delta(X')}{2}$ I will designate by B, half the difference of the two values of $\frac{\delta(Y'')-\delta(Y')}{2}$ by T; we have then

$$B=35{\cdot}72,\quad T=34{\cdot}56.$$

Using weights of 200 gr. there were obtained,—

	X'.	Y'.	X''.	Y''.
0 . . .	131·5	93·1	20·1	97·5
200 . . .	65·8	157·2	100·3	20·0
0 . . .	136·8	87·7	25·5	91·9
200 . . .	64·2	16·7	95·4	161·0
0 . . .	137·0	89·2	25·7	93·6
200 . . .	66·1	156·8	100·5	19·6
0 . . .	136·7	87·3	25·3	91·5
200 . . .	67·0	17·7	98·0	162·0
0 . . .	137·2	88·2	25·8	92·5

Hence in the mean,—

0	135·84	89·10	24·48	93·40
200	65·95	157·00	100·40	19·80
200	65·60	17·20	96·70	161·50

From which follows,—

	(X′).	(Y′).	(X″).	(Y″).
0	134·74	87·86	25·75	91·96
200	65·05	154·28	100·51	19·66
200	65·52	17·10	97·76	158·47

And further,

$$B=71{\cdot}42, \quad T=69{\cdot}00.$$

Calculating the values of B and T for 100 gr., by dividing the above values by 2, we find, in close approximation with the numbers previously found directly,

$$B=35{\cdot}71, \quad T=34{\cdot}50.$$

The bar was then turned 90 degrees about its axis, the cross-rods were again fastened horizontally to it, and then experiments made just in the same manner. There were obtained—

$$\overline{X'}=138{\cdot}2, \quad \overline{Y'}=100{\cdot}2, \quad \overline{X''}=23{\cdot}7, \quad \overline{Y''}=94{\cdot}0,$$

$$Z'=2355{\cdot}4 \text{ millims.}, \quad Z''=2355{\cdot}3 \text{ millims.},$$

$$r'-b'=-26, \quad r''-b''=-20,$$

$$\xi'-a'=32, \quad \xi''-a''=-32:—$$

	X′.	Y′.	X″.	Y″.
0	122·6	98·7	33·9	92·6
100	85·2	133·7	68·5	57·0
0	122·3	98·7	33·6	92·6
100	84·6	62·9	68·7	127·0
0	122·0	98·6	33·4	92·6
100	83·2	133·7	66·5	57·0
0	120·8	98·7	32·0	92·6
100	85·3	63·1	69·3	127·2
0	122·6	98·8	33·8	92·8

Hence in the mean,—

	X′.	Y′.	X″.	Y″.
0	122·06	98·70	33·34	92·64
100	84·20	133·70	67·50	57·00
100	84·95	63·00	69·00	127·10

	(X′).	(Y′).	(X″).	(Y″).
0	121·05	97·35	34·49	91·27
100	83·28	131·68	67·95	56·45
100	84·57	62·44	70·07	125·11

$$B=35{\cdot}82, \quad T=34{\cdot}48.$$

	X′.	Y′.	X″.	Y″.
0 . . .	122·5	99·6	33·6	93·5
200 . . .	49·1	170·0	103·9	22·3
0 . . .	121·3	99·5	32·5	93·3
200 . . .	41·2	27·7	97·5	161·9
0 . . .	124·5	99·6	35·8	93·5
200 . . .	50·0	169·6	104·7	21·9
0 . . .	122·7	99·0	34·0	92·8
200 . . .	48·6	27·6	105·0	161·8
0 . . .	122·6	98·7	33·9	92·6

Hence in the mean,—

0 . . .	122·72	99·28	33·96	93·14
200 . . .	49·55	169·80	104·30	22·10
200 . . .	44·90	27·65	101·25	161·85

	(X′).	(Y′).	(X″).	(Y″).
0 . . .	121·71	97·93	35·11	91·77
200 . . .	48·75	166·61	104·39	21·97
200 . . .	44·82	27·50	102·23	158·90

$$B=71{\cdot}56,\quad T=69{\cdot}01;$$

and for P=100 gr.,

$$B=35{\cdot}78,\quad T=34{\cdot}51.$$

In the following Table I give the values of B and T for P=100 gr., as they have subsequently been found; at the same time I will give the temperatures at which the experiments were made:—

B.	T.	
35·72	34·56	in one position; 21°·7 C.
35·71	34·50	
35·82	34·48	in another position; 22°·0 C.
35·78	34·51	

Hence in the mean,

$$B=35{\cdot}76,\quad T=34{\cdot}51.$$

The units which form the basis of these statements are, however, not the same, since, as mentioned above, the divisions of the ξ axis and of the η axis in the scale used were distinctly different from one another. Taking the mean values of the divisions of the scale as given above, we get

$$B=64{\cdot}34 \text{ millims.},\quad T=62{\cdot}41 \text{ millims.}$$

But from equation (13) we get

$$1+\frac{\theta}{1+2\theta}=\frac{T}{B}\,\frac{s}{2l}, \quad . \quad . \quad . \quad . \quad . \quad . \qquad (14)$$

from which is obtained for the steel rod No. 1,

$$\frac{\theta}{1+2\theta}=0\cdot293.$$

Two other steel rods of almost the same dimensions as the rod No. 1, were submitted to the same experiments. I content myself with adducing the following results:—

Steel rod No. 2.

B.	T.	
35·83	34·77	in one position; 12°·4 C.
35·82	34·78	
36·16	34·82	in another position; 16°·8 C.
36·14	34·84	

Mean .... B= 35·99, T=34·80,
= 64·76 millims., =62·94 millims.,
s=145·01 millims.

$$\frac{\theta}{1+2\theta}=0\cdot295.$$

Steel rod No. 3.

36·38	35·10	in one position; 22°·6 C.
36·33	35·10	
36·40	35·10	in another position; 22°·9 C.
36·35	35·11	

Mean .... B= 36·37, T=35·10,
= 65·43 millims., =63·48 millims.,
s=145·16 millims.

$$\frac{\theta}{1+2\theta}=0\cdot294.$$

Hence in the mean for the three steel rods, the relation of the lateral contraction to the longitudinal expansion is

0·294.

It would be interesting to ascertain whether with rods of a different section to that of those here investigated, the above relation would be as great. If that were the case, the assumption here made would be thereby confirmed, that a hardened steel rod may be considered homogeneous, and of the same elasticity in different directions. Objections may be raised against this assumption; in fact it may be assumed that in the hardening, in which heat flows from the axis towards the periphery, the elasticity in the direction of the axis is different to what it is in directions rectangular thereto, and that the molecules in the external layers have a different arrangement to those nearer the axis. If this is the case, it is probably also the case in different directions according to the thickness of the rod, and accordingly the latter relation will be different in thick from what it is in thin rods.

In conclusion I mention some experiments made with a hard-drawn brass rod of almost the same dimensions as those of the three steel rods. The experiments are of exactly the same kind as those made with each of the steel rods, excepting that weights of 50 gr. and 100 gr. were used instead of weights of 100 gr. and 200 gr. The following values of B and T were found for P=50 gr.:—

B.	T.	
35·55	37·14	in one position; 24°·1 C.
35·57	37·16	
35·94	37·07	in another position; 25°·0 C.
35·95	37·12	

Mean .... B=35·75 T=37·12

64·33 millims., =67·13 millims.

Here $s=144{\cdot}65$ millims. Hence using equation (14), we have

$$\frac{\theta}{1+2\theta}=0{\cdot}387.$$

This number has certainly not the same importance as that which I have thought myself justified in assigning to the corresponding numbers in the case of steel rods, for the elasticity of a drawn brass rod is certainly different in the direction of the axis to what it is in others.

Heidelberg, June 1859.

VI. *Notes on Mineralogy. By the* Rev. SAMUEL HAUGHTON, *M.A., F.R.S., Fellow of Trinity College, and Professor of Geology in the University of Dublin**.

No. IX. *On the Shower of Aëroliths that fell at Killeter, Co. Tyrone, on the 29th of April,* 1844.

ON the 29th of April, 1844, a shower of meteoric stones fell, in the sight of several people, at Killeter, near Castlederg, co. Tyrone: they broke into small fragments by the fall, one piece only being found entire; it was (according to the testimony of a resident) "about as large as a joint of a little finger." The stones were hot when found. The account given by three gentlemen, who, however, did not actually see the shower fall, was that they were at a distance of three or four miles, up the hills in the neighbourhood; it was a fine sunny afternoon (three or four

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* Communicated by the Author.

o'clock); they heard "music" towards Killeter, which they supposed to proceed from a strolling German band which they knew to be in the neighbourhood; they are under the impression that they heard the music several times in the course of the evening; they remember also to have noticed clouds in the direction of Killeter. On reaching Killeter the same evening, they were told of the wonderful shower of stones which had spread over several fields. I received the fragments of these stones from the Rev. Dr. M'Ivor, Ex-Fellow of Trinity College, Dublin, and Rector of Ardstraw: he writes to me that "it is now very difficult to get either a specimen of a stone, or any very distinct intelligence of them: even the very rumour of them has nearly died out, and you might ask intelligent middle-aged men about the neighbourhood who had never heard them mentioned." He adds that the people of that locality are very "uncurious," and that, if there were a veritable burning bush thereabouts, few would "turn aside to see."

The largest specimen given to me by Dr. M'Ivor weighed 22·23 grs. in air, and 16·32 grs. in water, showing that its specific gravity is 3·761. Both it and the smaller fragments presented the usual black crust and internal greyish-white crystalline structure and appearance, with specks of metallic lustre, occasioned by the iron and nickel alloy that was present. I analysed it in the usual manner, but, owing to an accident, I was unable to determine the composition of the earthy portion soluble in muriatic acid.

The following is the mineralogical composition of these Aëroliths:—

1. Hornblendic mineral (insoluble in acid)	34·18
2. Earthy mineral (soluble in acid)	30·42
3. Iron	25·14
4. Nickel	1·42
5. Sesquioxide of chrome	2·70
6. Cobalt	trace
7. Magnetic pyrites	6·14
	100·00

The earthy portion, insoluble in muriatic acid, had the following composition:—

		Atoms.	
Silica	55·01	1·22	
Alumina	5·35	0·10	
Protoxide of iron	12·18	0·34	1·66
Lime	3·41	0·12	
Magnesia	24·03	1·20	
	99·98		

Omitting the alumina, the preceding analysis gives the rational formula of the Hornblende family,

$$4RO, 3SiO^3,$$

and, taken as a whole, it agrees with the analysis of many Hornblendes. The variety of Hornblende with which it has the closest relation is Anthophyllite.

According to Mr. Greg's 'Catalogue of Meteoric Stones and Irons,' three other falls of aëroliths are recorded as having occurred in Ireland:—

1. A.D. 1779, at Pettiswood, Westmeath; 6 oz.
2. August, 1810, Mooresfort, Co. Tipperary; 7¾ lbs. Spec. grav. = 3·670.
3. September 10, 1813, Adare, Co. Limerick; 17 lbs. + 65 lbs. + 24 lbs.; moving E. to W. Spec. grav. = 3·64.
4. April 29, 1844, Killeter, Co. Tyrone; fragments of one stone. Spec. grav. = 3·761.

Of the meteorite that fell at Mooresfort, co. Tipperary, in 1810, the only analysis on record is one published by the late Professor Higgins, in the forty-seventh volume of the 'Proceedings of the Royal Dublin Society,' in whose museum the greater part of this stone, and a cast of the entire, are carefully preserved. My friend Mr. Robert H. Scott has undertaken to analyse a portion of it afresh.

Professor Higgins considered 35 per cent. of the stone to consist of metallic particles separable by the magnet. This would include the magnetic pyrites, iron, nickel, and chrome. In the Tyrone meteorite examined by me, the iron, nickel, chrome oxide, and magnetic pyrites amounted to 35·40 per cent., which is very nearly the same proportion.

Dr. Apjohn has published a detailed account of his analysis of the Adare meteorite in the eighteenth volume of the 'Transactions of the Royal Irish Academy,' from which it appears that the following is the mineralogical composition of that meteorite:—

1. Meteoric iron and nickel .	23·07
2. Magnetic pyrites . . .	4·38
3. Chrome iron	3·34
4. Earthy matrix	68·47
5. Alkalies and loss	0·74
	100·00

Its specific gravity varied from 3·621 to 4·230. The composition of 200 grs. of the matrix was found to be,—

		Atoms.	
Silica	78·19 grs.	1·717	
Magnesia	43·13 ,,	2·156	2·590
Protoxide of iron . .	15·62 ,,	0·434	
	136·94		

This analysis would make the earthy matrix, taken as a whole, have the composition of pyroxene,

$$3RO, 2SiO^3.$$

No. X. *Additional Notice of Hislopite and Hunterite.*

I published in the Philosophical Magazine of January 1859, an account of two new minerals, found by the Rev. Messrs. Hislop and Hunter near Nágpur, Central India. Being anxious to obtain additional information respecting the geological mode of occurrence of these minerals, I wrote to Mr. Hislop, who furnished me with information respecting them, from which I extract the following particulars.

Hislopite.—This mineral was found in a small stream which flowed down from a trap-hill at Takli. It was discovered by a servant of Major Wapshaw, an officer of the Madras army. Mr. Hislop believes its position *in situ* to have been in trap-rock, "probably in the thin stratum of freshwater tertiary which is imbedded in the volcanic rock, which has been dispersed in strings by the effusion, and which, generally speaking, contains a pretty equal proportion of calcareous and siliceous ingredients."

Mr. Hislop also forwarded to me a specimen of calc-spar, clouded like plasma with pale greenish streaks of a siliceous mineral, sent to him by Dr. Carter from Bombay. Its chemical examination gave me the following result:—

Carbonate of lime . . .	97·19
Green siliceous mineral . .	2·81
	100·00

The quantity of colouring mineral was too small for examination, and its per-centage much less than that of the Glauconite which gives its rich green colour to Hislopite, in which mineral 17·36 per cent. of Glauconite was found by me.

Hunterite.—This remarkable mineral was found *in situ* in a watercourse between Mr. Hislop's house and the city of Nágpur; "it was broken off a pegmatitic dyke, which, like many others, runs at right angles to the apparent strike of the gneiss which it has penetrated." Mr. Hislop regards this gneiss as metamorphic 'Mahadewa Sandstone' (of Oldham), which is probably of the age of the lowest tertiary or highest secondary beds; it was probably once completely covered by a considerable thickness of trap-rock, which still remains as an outlier in Sitábaldi Hill, about 300 yards from the watercourse in which the pegmatite dyke penetrates the gneiss. The trap-rocks of this hill are horizontally bedded, and interstratified with a freshwater stratum containing *Physa* and other shells.

It is certain, from the occurrence of such a mineral as Hunterite in the dyke penetrating the gneiss, that this dyke must be regarded as a fissure filled by the action of water holding mineral matter in solution under pressure and at a high temperature; and as the gneiss (10) itself contains Hunterite, it also must have been to some extent subjected to the same Neptuno-Plutonic agency.

No. XI. *On some Irish Dolomites of the Carboniferous age.*

Beds of dolomite limestone are found in many places in Ireland stratified conformably to the ordinary crystalline limestones of the carboniferous age. These dolomites are developed particularly in the lower and in the upper portions of the carboniferous limestone. The following analyses will give some idea of their composition.

No. 1. Dolomite, of a pale cream-colour, saccharoid; forming the uppermost bed of carboniferous limestone immediately underlying the coal-measure white sandstone of Belmore Mountain, co. Fermanagh. Two specimens, four miles apart, analysed, gave:—

	Per-centage.		Atoms.	
	(*a*)	(*b*)	(*a*)	(*b*)
Carbonate of lime . . .	61·20	62·48	1·224	1·249
Carbonate of magnesia .	37·80	36·30	0·900	0·864
Silica	0·20	0·28		
Peroxide of iron (probably present as carbonate) .	0·60	0·60		
Total . . .	99·80	99·66		

No. 2. Dolomite, of a rosy cream-colour, saccharoid, flaky, and crystalline; forming the uppermost bed of the carboniferous limestone immediately underlying the coal-measure shales of Raheendoran, Clogrennan Hill, co. Carlow —

	Per-centage.	Atoms.
Carbonate of lime	54·15	1·083
Carbonate of magnesia . .	43·01	1·024
Argil	2·84	
	100·00	

The occurrence of beds of dolomite, so pure as those just described, occupying precisely the same geological position, in localities so far apart as the counties Carlow and Fermanagh, suggests the possibility of a dolomite horizon, marking the upper limit of the carboniferous limestone of Ireland.

In the lower parts of the carboniferous limestone of Ireland, where the limestone abuts against the granite of the Leinster chain, dolomites are locally abundant, which differ from the dolomites of the upper limestone in containing a much larger proportion of argil.

I here give the analyses of two specimens from localities far asunder.

No. 3. Dolomite from Brown's Hill Quarries near Carlow, within a quarter of a mile of the junction of the limestone and granite; much used as a building-stone, but known to make bad lime. This dolomite is of a bluish-grey colour, is not crystalline, and contains numerous geodes filled with yellow clay, lined with crystals of Bitter-spar, and containing loose double-pyramidal crystals of quartz, many of which include cavities partially filled with fluid, enclosing small spherical bubbles, which are moveable on changing the position of the crystal. Spec. grav. =2·781.

	Per-centage.	Atoms.
Carbonate of lime	49·84	0·997
Carbonate of magnesia . .	39·36	0·937
Carbonate of iron	0·99	
Argil	8·60	
	98·79	

No. 4. Dolomite from Booterstown, co. Dublin; within a few yards of the granite which appears at the Black Rock station of the Dublin and Kingstown Railway. Dark grey, not crystalline.

	Per-centage.	Atoms.	
Carbonate of lime . .	47·21	0·944	
Carbonate of magnesia .	25·64	0·600	0·805
Carbonate of iron . .	11·89	0·205	
Argil	15·66		
	100·40		

VII. *On the Fourth Law of the Relations of the* Elastic Force, Density, *and* Temperature *in Gases; as sequel to a Paper on the same subject in the Philosophical Magazine for September* 1853. *By* Professor Potter, *A.M.*[*]

THERE is no need to be surprised that our knowledge of the relations of heat and dense matter has progressed slowly; for in examining these relations we are striving to learn the con-

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[*] Communicated by the Author.

nexion of the ponderable and imponderable elements of the universe. We are indeed striving to carry the domain of the inductive experimental philosophy into the properties of the subtile matters called, often, the imponderables, and searching for the causes of the development of mechanical force in the actions, reactions, affinities, incompatibilities, &c. of these subtile agents, which are struggling to pass from the state of instability which has been impressed upon them by an omnipotent power to a state of stable equilibrium, through the properties which they possess. The phenomena of the actions of these properties in their indefinitely varying states constitute the phenomena of nature; and in investigating them we are striving to attain the highest degree of knowledge within the reasoning powers which have been conferred upon us. We must therefore be content with the progress of knowledge which the inductive philosophy affords to us, and leave wild dreams and speculations to the day-dreamers and speculators, and receive as probable only such theories and hypotheses as have an experimental basis, and for the probability of which good reasons can be advanced.

From the time of the discoveries of Dr. Black in heat, about a century ago, the advance of our knowledge has been great, although apparently slow, and often deviating from a straight course. The properties now proposed to be discussed are amongst those which require peculiar watchfulness of our instruments, as changing properties are to be learnt, and evanescent results are to be noted.

It has long been known that, in popular language, *high-pressure steam blows cold.* Now this means that a jet of steam from a boiler in which it exists of high temperature and elastic force, on being allowed to escape through an aperture into the air, soon loses its high temperature and gives a feeling of cold to the hand on which it strikes. The sensible heat of the steam has diminished, because its capacity for caloric has increased by its expansion in volume and diminution of elastic force. It is now the question for consideration whether the change of capacity for caloric is instantaneous on the sudden escape of the steam from a high to a low pressure. We see that the change is not instantaneous, but is developed in time; for on bringing the hand or a thermometer nearer to the aperture from which the steam issues, the sensible heat increases, and the capacity in the jet of course diminishes, so that we must conclude that the change of the capacity for caloric is developed only in some time after the steam is relieved from the pressure balancing its elastic force in the boiler. To determine this time is a very important point in the theory of the mechanical force of steam, and its applications in the steam-engine. The corresponding properties of air are also in this respect important practically; for considerable expenses have been incurred, and may again be incurred, from misapprehensions of these properties.

That the properties of gases are analogous to those of steam, as related above, there can be no doubt; that is, on expanding from a state of more to a state of less condensation, the sensible heat is diminished, or, as Dr. Black would say, has become latent heat. The important points to be determined are the amount and the laws of the changes of temperature in respect to sudden changes of density and elastic force, as well as the interval of time in which such changes are completed, since they are evidently not instantaneous.

We are not without popular results which we may treat as preliminary experiments. Some years ago two young gentlemen, brothers, possessing talents, ingenuity, and perseverance, the one a scientific chemist, and the other a civil engineer, undertook, after a mild winter, the experiment of trying to make ice on the large scale artificially and economically, by passing air from a high state of condensation in a strong and large receiver to the atmospheric pressure, through water. To their disappointment, and mistrust of the theory, they found the water cooled only a few degrees of temperature when they expected the formation of ice. On the contrary, we have lately heard that in the railway tunnel which the Sardinian government is carrying through the Alps, by using condensed air-engines to work the boring machinery, that a degree of cold is produced which causes water to freeze, by the expansion of the air after escape from the engine,—the moderate condensation being performed at the entrance of the tunnel, and the condensed air carried in pipes to the air-engine at the workings. We may ask how are these results to be reconciled. Is it not a case analogous to the high-pressure steam-jet, where the change of temperature occupies a short but sensible interval of time? This seems to me the solution of the question.

To discuss experiments made with scientific views: we find that when MM. Gay-Lussac and Welter were trying experiments, at the suggestion of M. Laplace, on the sensible heat lost in the sudden rarefaction of air, they found it so great that the ratio of the specific heat under a constant pressure was to that under a constant volume as 1·3748 to 1; but when the air was allowed to escape from its state of condensation to atmospheric pressure through an aperture, there was no change of temperature due to the expansion*. This latter experiment was evidently a hasty one; for the scientific gentlemen attempting to form ice,

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* Herschel 'On Heat,' art. 121.

Mr. Joule[*], Professor W. Thomson, and myself[†] have all found a certain but small change of temperature; and I have also, in the paper to which the present is a sequel, investigated the law of the change, which, I believe, is the true law for the *instantaneous* result.

This law is as follows :—

Let v be the volume of a gas when the pressure is p and the density ρ,

v' be the volume of a gas when the pressure is p' and the density ρ'

after an expansion; and since the mass is the same, we have

$$v \,.\, \rho = v' \,.\, \rho'.$$

Also let δ be the rarefaction or negative condensation; and since the change of temperature is small, we have by Boyle's law,

$$p = \kappa\rho, \quad p' = \kappa\rho';$$

then

$$\delta = \frac{v'-v}{v} = \frac{\rho-\rho'}{\rho'}$$

$$= \frac{p-p'}{p'};$$

and it was found by the experiments that if ω_1 was the number of Fahrenheit's degrees through which the temperature fell for an expansion unity or for $\delta = 1$, and ω the degrees it fell for an expansion δ, then

$$\frac{\omega}{\omega_1} = \left(\frac{\delta}{1}\right)^3,$$

or

$$\omega = \omega_1 \,.\, \delta^3;$$

and the experiments gave the value of the constant,

$$\omega_1 = 0^\circ{\cdot}2077.$$

From this formula the ratio of the specific heats was deduced to be

$$\frac{\text{specific heat of air under a constant pressure}}{\text{specific heat of air with a constant volume}} = \frac{c}{c'}$$

$$= 1 + \frac{\delta^2}{2359(1+\alpha\theta)},$$

where $\alpha = \frac{1}{490}$, and $\theta =$ degrees of Fahrenheit's scale above the freezing temperature.

In the paper to which the present is a sequel, I stated that I

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[*] Phil. Trans. for 1853, and Phil. Mag. for September 1853, p. 230.

[†] Phil. Mag. for September 1853, p. 161.

had other experiments in view on the same subject, and soon after I procured one of M. Breguet's exceedingly sensitive metallic helix thermometers, by means of which I hoped to ascertain directly the temperature of an expanding jet of air entering the exhausted receiver of an air-pump, but was disappointed in obtaining anything more than confirmatory results, and should not by means of it have ascertained the law. This arose from the unsteadiness and vibrations of the helix and index-needle when a jet passed through them, even when an addition had been made to the instrument to steady the index-needle at the expense of a small loss of sensibility. This addition consisted of a small brass vertical pillar screwed into the pedestal under the centre of the helix, with a small cylindrical hole down a part of its axis, which was directly over a small polished agate cup on a part of the pillar which had been filed away so that it was directly under the cylindrical hole. The vertical needle of the helix which carries the horizontal index-needle being raised and passed down the fine cylindrical hole, its sharp point rested on the agate cup. A very small loss of sensitiveness was found, but great additional steadiness, from this arrangement. The helix in the experiments was surrounded with a cylinder of gilt paper rather larger than itself, which was attached air-tight to the part of the instrument where the upper end of the helix is screwed fast. This part turns round in a step to adjust the index-needle to any point in the horizontal circle of degrees, and has a hole down its axis. This hole was used in the experiments to pass a jet of air through. A long fine brass tube, small enough to go into the hole just named, passed through a stuffing-box in a brass plate covering the opening in the top of a large glass receiver of an air-pump within which Breguet's thermometer was placed. The fine brass tube had its lower end prepared with an aperture which could be opened and shut by a plug at the end of a brass wire passing down it, which was somewhat longer than the tube. The tube being smaller than the hole in the moveable part of the thermometer, was packed tight in it at its lower end with lint. A light cup of paper was, after some experiments, also placed to rest on the index-needle, to turn with it and envelope the lower end of the vertical cylinder of gilt paper without touching it. This was intended to retain the expanded air of the jet around the helix. When the receiver of the air-pump was partially exhausted, and the plug raised by means of the long wire passing down the brass tube, the external air rushed into the receiver through the cylinder of gilt paper, and communicated its temperature to the helix; the temperature was thus shown by the degree on the scale to which the index-needle pointed. It will be easily conceived that the helix, of which the thickness (containing silver, gold, and platinum) is less than $\frac{1}{600}$th of an inch*, was considerably agitated by the jet, and the index-needle set into vibration, so that tenths of degrees could not be read off with quickness and certainty.

In January 1854 I used a method of experiment similar to that with Marcet's boiler, described in the paper of 1853, namely, the exterior thermometer having been compared with Breguet's within the reciver, this latter fell on the pump being worked; and when it had risen gradually to some particular degree, the jet was allowed to pass for about one second, and the effect upon the index-needle noted. When the barometer-gauge of the air-pump stood about half the height of the barometer, we had $\delta=\frac{p-p'}{p'}=1$ nearly; and if Breguet's thermometer stood 4°, 3°, $2\frac{1}{2}$°, $1\frac{4}{10}$°, or 1° degree below the exterior thermometer, it rose instantly on the jet passing through the helix; but when the difference was only $\frac{2}{10}$° it fell. These are in accordance with the former results, which gave $\frac{1}{4}$° as the temperature of such a jet; but the actual readings of the index, contrary to what I had hoped for, were irregular and uncertain.

Experiments were then tried of rarefactions about five and three; or if the barometer stood at 30 inches, the jet was passed through the helix when the barometer-gauge stood at about 25 inches and about 23 inches, for then $\delta=\frac{p-p'}{p'}$ became

$$\delta=\frac{30-5}{5}=5, \text{ and } \delta=\frac{30-7}{7}=3\tfrac{2}{7}.$$

The first motion of the needle was always in the direction due to cold; but the very great agitation of the helix and index-needle from the violence of the jet, prevented a reading being obtained before the temperature approached that of the external air. Accordingly this method of experiment only confirmed the results of the paper of 1853, without adding anything to them.

The next method of experiment which I adopted was with a Newman's air-pump, and the Breguet's thermometer placed under a glass receiver upon the plate. Newman's air-pump having a single barrel of large dimensions, a considerable exhaustion of a moderate-sized receiver is obtained by the first stroke of the piston; and this can be easily performed, and the communication cut off by the stopcock in one second of time. The object was to find the effect of such rapid exhaustion on the thermometer, and then by using receivers of different sizes giving different degrees of exhaustion for the same single stroke of the pump, to find the law for the corresponding changes of

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* $\frac{1}{25}$th of a millimetre.

temperature. A considerable degree of cold was produced by even a small amount of expansion; but the time required was eight or nine seconds before the thermometer reached its minimum. This slowness in a great measure no doubt arises from the quiescence of the remaining air in the receiver, which only then acts slowly by radiation and convection on the helix of the thermometer. This slowness renders the results of little value, beyond showing that a considerable degree of cold is produced by a moderate rarefaction after a short interval of time.

Many experiments gave nearly the same results; and averages of five successive good experiments were as follows:—The temperature in the receiver in degrees of Breguet's thermometer 54°·42: after one stroke of the piston of the pump, in eight seconds the index arrived at the minimum 47°·82; after eight seconds more it stood at 49°·46; in eight more at 51°·04; in eight more at 52°·33; in eight more at 53°·16; in eight more at 53°·72; in eight more at 54°·02; after some time more at 54°·32. The differences in the successive intervals of eight seconds after the completion of the exhaustion were thus 6°·60, 1°·64, 1°·58, 1°·29, 0°·83, 0°·56, 0°·30, 0°·30. If we suppose the heat lost in the first interval of eight seconds equal to that in the second, we have 6°·60 + 1°·64 = 8°·24 for the maximum of cold on Breguet's scale with the circle divided into 360 parts. The barometer-gauge rose 1·9 to 2 inches, and therefore the expansion δ was as follows:

$$\delta = \frac{p - p'}{p'} = \frac{30 - 28}{28} = \tfrac{1}{14}\text{th}.$$

Hence 1 degree of cold on the scale was produced by a rarefaction $\frac{1}{14 \times 8\cdot4} = \frac{1}{117\cdot6}$; which, supposing with M. Prony that these degrees are each $\frac{7}{10}$ths of a degree Centigrade when the circle is divided into 100 parts, gives $\frac{7}{10} \times \frac{100}{360} = \frac{7}{36}$ of a degree Centigrade as the value of each degree of the thermometer used. While Poisson supposed 1 degree Centigrade of cold to be produced by a rarefaction $\frac{1}{116}$, we have only found by this method one-fifth of that quantity.

Experiments were also undertaken with dry air. The receiver being exhausted, it was allowed to refill slowly with air passing through a V-tube of glass containing pumice-stone moistened with strong sulphuric acid. The results were not greatly different from those obtained with the ordinary air of the room, which was the experimental lecture-room of University College, and was heated by a stove. With the dry air the temperature was 57°·6 in the receiver, the barometer 29·95 inches; after one stroke of the piston completed in one second, in nine seconds the thermometer attained its minimum, on the average of six good consecutive experiments, of 6°·73 below the original temperature, and in the succeeding eight seconds rose, on the average, 1°·25. The rarefaction was sensibly the same, $\frac{1}{14}$th, as with common air.

The two methods above detailed for finding the temperature *directly* being objectionable, I laid aside Breguet's thermometer, and have only resumed its use lately from having found a method in which it reaches its maximum of temperature for condensations in one second, as will be found further on in the paper.

On the want of success by the direct methods, in the beginning of 1854 I recurred to the indirect method of observing the barometer-gauge of the Newman's air-pump in the case of sudden rarefactions, which is in principle the same as the method of MM. Desormes and Clement, and of MM. Gay-Lussac and Welter, also of Mr. Meikle, and as adopted by M. Poisson in his *Traité de Mécanique*, vol. ii. p. 643; that is, by finding the temperatures through the pressures, and considering them connected by the law of Amontons. In this manner the objection to using a light fluid having a long space to move through as a barometer-gauge was avoided, so that the effect of the momentum acquired in moving through a long space, but opposed by the capillary attraction of watery fluids for glass, was not incurred; and by noting the extremes of the first two oscillations of the mercury in the gauge, the mean might be taken as the correct result; but still the attraction of aggregation of the mercury and its adhesion to the glass are defects, preventing the mercury ascending so high as it might have done.

Assuming the accuracy of Amontons's law, let p, ρ, t° be the pressure, density, and degrees of temperature above the freezing-point of water, κ and α known constants, then

$$p=\kappa\rho(1+\alpha t^\circ);$$

similarly, for another state of the same gas we have

$$p'=\kappa\rho'(1+\alpha t'^\circ)$$

and

$$\frac{p'}{p}=\frac{\rho'(1+\alpha t'^\circ)}{\rho(1+\alpha t^\circ)};$$

and in the experiments ρ and ρ' are so nearly equal, that we may take $\frac{\rho'}{\rho}=1$, since the volume of gas changes only by the small space of the barometer-gauge through which the mercury moves.

Taking $\alpha=\frac{1}{494}$, as given by M. Rudberg's experiments, for Fahrenheit's degrees of temperature, we have

$$1-\frac{p'}{p}=1-\frac{1+\alpha t'^{\circ}}{1+\alpha t^{\circ}}=\frac{\alpha(t^{\circ}-t'^{\circ})}{1+\alpha t^{\circ}}$$

and

$$t^{\circ}-t'^{\circ}=\frac{1+\alpha t^{\circ}}{\alpha}\left(1-\frac{p'}{p}\right).$$

Applying this formula to an experiment made on the 25th of January, 1854, when the barometer stood at 30·30 inches and the thermometer at 58°·5, we have as follows:—By a stroke of the piston performed in one second with a large receiver and Newman's air-pump, the mercury in the barometer-gauge rose, on the average of seven experiments which differed only slightly, to 2·64 inches, and falling gradually during some minutes rested at 2·01 inches; then

$$\begin{aligned} t^{\circ} &= 58^{\circ}{\cdot}5-32^{\circ} = 26^{\circ}{\cdot}5 \\ p &= 30{\cdot}30-2{\cdot}01=28{\cdot}29 \text{ inches of mercury} \\ p' &= 30{\cdot}30-2{\cdot}64=27{\cdot}66 \quad " \quad " \end{aligned}$$

and

$$\begin{aligned} t^{\circ}-t'^{\circ} &= 494\left(1+\frac{26{\cdot}5}{494}\right)\left(1-\frac{27{\cdot}66}{28{\cdot}29}\right) \\ &= 520{\cdot}5\times\frac{{\cdot}63}{28{\cdot}29} \\ &= 11^{\circ}{\cdot}59. \end{aligned}$$

Now Poisson's assumption was that the temperature 116° δ Centigrade, or 209° δ nearly Fahrenheit, would be lost by an expansion δ; and in the above experiments we should have

$$209^{\circ}\times\frac{2{\cdot}01}{28{\cdot}29}=14^{\circ}{\cdot}85,$$

which differs a little from the experiment, in which we have reason to expect a loss of heat.

In other experiments with the same receiver, the air dried by passing through a V-tube with pumice moistened with sulphuric acid, the results gave

$$t^{\circ}-t'^{\circ}=13^{\circ}{\cdot}5,$$

and Poisson's formula 14°·8; which are closer than the former.

When smaller receivers were used and more rarefaction produced by the single stroke of the piston, the results did not so well accord. In one set of experiments

$$t^{\circ}-t'^{\circ}=29^{\circ}{\cdot}4$$

was found, whilst the formula gave 54°·3.

With the smallest receiver, the result

$$t^\circ - t'^\circ = 43^\circ{\cdot}2\,;$$

and by Poisson's formula the result is 53°·7.

In another experiment with the smallest receiver the result was

$$t^\circ - t'^\circ = 50^\circ{\cdot}5,$$

for which case Poisson's formula gave 92°·4.

Although the results at the smaller rarefactions were not very different from Poisson's rule, yet those at greater rarefactions did not appear to accord with it. In this state of the experiments I laid the results aside until lately, when I wished again to try if any better method of using Breguet's thermometer could be found, and more decisive results obtained.

On placing Breguet's thermometer in the large receiver of the air-pump, its capacity being about 650 cubic inches, with an opening of 2½ inches diameter at the top, to be closed by a circular plate of glass smeared with grease, I found that, having exhausted the receiver to a given degree, then when the plate of glass was slid away suddenly (say, in one tenth of a second) the agitation of the air in the receiver caused the thermometer to arrive at its maximum in one second of time, and therefore probably indicated the true result very nearly. It was thus desirable to make the experiments with all possible accuracy, and carefully allow for the capillary depression of the mercury in a new barometer-gauge Experiments were also made by using a separating diaphragm of tissue paper in a bag-form at the upper part of the receiver, and held up by an elastic ring of wire pressing it to the glass. The diaphragm yielded to the re-entering air, but kept it separate from the larger part of the rest. With the small condensations the thermometer resumed its sluggishness, arriving at its maximum only in four or five seconds; but as the condensation increased, it acted more rapidly, and at the greatest condensation arrived at its maximum nearly as quickly as without the diaphragm, and with nearly the same indication of heat. I consequently conclude that the phenomenon of quickness was not due to the re-entering air, otherwise than as producing agitation and a rapid effect upon the helix of the thermometer. Other experiments were tried with the bell-glass of the thermometer left over the helix, but raised on pieces of card, and others, again, with the bell-glass raised out of its groove and moved sideways, leaving considerable space for the air to pass. These gave the maxima results smaller than when the helix was uncovered, but they occurred in one second of time.

A preliminary set of experiments, taken on the 26th of September 1861, the barometer 29·61 inches, with the air-pump and large receiver, and a new barometer-gauge before it was measured to allow for the capillary depression, gave results, converted to Fahrenheit's degrees, as follows:—

Height of mercury in the gauge above the level............	in. 1	the air re-entering suddenly, the temperature rises	4°·0
,, ,,	2	,, ,,	7·5
,, ,,	3	,, ,,	12·0
,, ,,	4	,, ,,	15·1

These are nearly in arithmetic progression for condensations in arithmetic progression, according to Poisson's law. On the 27th of September, with the capillary depression of the gauge carefully allowed for, the following were obtained, the barometer standing at 29·88 inches, and the thermometer at 60°:—

Height of mercury in the gauge.	Number of trials.	Increase of temperature in Fahrenheit's degrees.	Approximate condensations.
inches. 6	7	22°·2	$\frac{1}{5}$th
5	3	18·2	$\frac{1}{6}$th
4	3	13·9	$\frac{1}{7\cdot5}$th
3	3	11·4	$\frac{1}{10}$th
2	3	7·4	$\frac{1}{15}$th
1	3	3·7	$\frac{1}{30}$th

The experiments differed only slightly amongst themselves, and, with the exception of those where the height of the gauge was 4 inches, they form a very certain arithmetic progression in accordance with M. Poisson's rule; but the absolute increase of temperature is very little more than one-half what his expression 209° δ requires; for $\delta=\frac{p'-p}{p'}=\frac{1}{30}$ gives 6°·96 Fahrenheit, whilst the observed increase of temperature was only 3°·7, occurring in one second of time after the condensation had taken place.

The law of the increase of temperature being as the condensation, is the important point shown in these experiments, and which the other methods did not show; whilst the time in which it occurred may have been much less than one second, as exhibited by the thermometer.

VIII. *On the Solar Spectrum, and the Spectra of the Chemical Elements.* *By* Professor H. E. Roscoe.

To the Editors of the Philosophical Magazine and Journal.

Owens College, Manchester,
December 21, 1861.

Gentlemen,

The following extract from Professor Kirchhoff's interesting memoir "On the Solar Spectrum, and the Spectra of the Chemical Elements," just published, with magnificent maps of the lines, in the 'Transactions of the Berlin Academy,' and about to appear in English, may interest your readers as helping to explain the appearance of the blue band in the spectrum of intensely ignited lithium vapour, first noticed by Dr. Tyndall, and referred to in your Number of last month by Dr. Frankland.

"The position of the bright lines (or, to speak more precisely, the maxima of light in the spectrum of an incandescent vapour) is independent of the temperature, of the presence of other substances, and of all other conditions except the chemical composition of the vapour. The truth of this assertion has been well tested by experiments made by Bunsen and myself with special regard to this point, and it has been confirmed by many observations which I have had occasion to make with the extremely sensitive instrument above described*. Nevertheless the spectrum of the same vapour may, under different circumstances, appear to be very different. Even the alteration of the mass of the incandescent vapour is sufficient to give another character to its spectrum. If the thickness of the column of vapour whose light is being examined be increased, the luminous intensities of all the lines increase, but in different ratios. In accordance with a theorem which will be considered in the next section, the intensity of the bright lines increases more slowly than that of the less visible lines. The impression which a line produces on the eye depends upon its breadth as well as upon its brightness. Hence it may happen that one line, being less bright although broader than a second, is less visible than the latter when the thickness of incandescent gas is small, but becomes more distinctly seen than the latter when the thickness of the vapour is increased. Indeed if the luminosity of the whole spectrum be so lowered that the most striking of the lines only are seen, it may happen that the spectrum appears to be totally changed when the mass of the vapour is altered. Change of temperature seems to produce an effect similar to this alteration in the mass of the incandescent vapour. If the temperature be raised, *no deviation of the maxima of light is observed*; but the intensities of the lines increase so differently, that those which are most plainly seen at a high temperature are not the most visible at a

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* With the magnificent instrument here referred to, Kirchhoff was able to separate the two "D" lines by a width of 4 millimetres.—H. E. R.

low temperature. This influence on the mass of the temperature of the incandescent gas explains perfectly why, in the spectra of many metals, those lines which are the most prominent when the metal is placed in the colourless gas-flame, are not the most distinct when the spectrum of the induction-spark from the metal is examined. This is most clearly seen in the case of the calcium spectrum. I have found that if a wet string or a narrow tube filled with water be placed in the circuit of the Leyden jar which gives the spark, and if the electrodes be moistened with a solution of chloride of calcium, a spectrum is obtained which coincides exactly with that seen when a chloride-of-calcium bead is placed in the colourless gas-flame. Those lines appear absent which are the most distinct when an entire metallic circuit is employed. If the narrow column of water be replaced by a column of larger sectional area and of shorter length, a spectrum is produced in which both those lines which are seen in the flame and those obtained by the intense spark are equally plainly visible. In this experiment we see the mode in which the calcium spectrum, as given in the flame, may be converted into that produced by the bright electric spark."

I may likewise add that, in lately examining the spectrum of lithium obtained by the induction-spark from a Ruhmkorff's coil with one of Steinheil's prisms, Professor Clifton and I observed the appearance of two blue lines, one of which (probably the line noticed by Dr. Tyndall) we found to be coincident with the common blue strontium line δ, whilst the other coincided with a second blue strontium line, which became first apparent in the spark-spectrum of this metal. Whether the lines thus produced in the spectra of lithium and strontium will prove to be coincident when examined with a larger number of prisms and with a higher magnifying power we are unable at present finally to decide; but by employing three of Steinheil's prisms, each having a refracting angle of 60°, there appeared to us a slight difference in refrangibility between the first blue lithium line and the line Sr δ,—this difference, however, not being so large as that between the two sodium lines. We hope to be able before long to give a definite answer to this important and interesting question. The explanation of the coincidence by possible presence of strontia in the lithia, is disposed of by the fact that when the blue lines are most intense no trace of the orange or red strontium lines α, β, and γ can be observed. The lithium-salt which I used was the sulphate, being a portion of some *pure* salt sent me by Professor Bunsen; the strontium-salts employed were the chloride and nitrate, and with both of these the same coincidence was observed.

I remain, Gentlemen,

Yours truly,

Henry E. Roscoe.

IX. *On the inapplicability of the new term "Dyas" to the "Permian" Group of Rocks, as proposed by* Dr. Geinitz. *By* Sir Roderick Impey Murchison, *F.R.S., D.C.L., LL.D. &c., Director-General of the Geological Survey of Britain*[*].

IN the year 1859 M. Marcou proposed to substitute the word "Dyas" for "Permian," and summed up his views by saying that he regarded "the New Red Sandstone, comprising the Dyas and Trias, as a great geologic period, equal in time and space to the Palæozoic epoch of the Graywacke (Silurian and Devonian), the Carboniferous (Mountain-limestone and Coal), the Mesozoic (Jurassic and Cretaceous), the Tertiary (Eocene, Miocene, and Pliocene), and the recent deposits (Quaternary and later)" ! ![†].

As that author, who had not been in Russia, criticized the labours and inductions of my associates de Verneuil and von Keyserling, and myself, in having proposed the word "Permian" for tracts in which he surmised that we had commingled with our Permian deposits much red rock of the age of the Trias, I briefly defended the views I had further sustained by personal examination of the rocks of Permian age in various other countries of Europe[‡].

It was, indeed, evident that M. Marcou's proposed union of the so-called Dyas and Trias in one natural group could not for a moment be maintained, since there is no conclusion on which geologists and palæontologists are more agreed, than that the series composed of Roth-liegende, Kupfer-Schiefer, Zechstein, &c., forms the uppermost Palæozoic group, and is entirely distinct in all its fossils, animal and vegetable, from the overlying Trias, which forms the true base of the Mesozoic or Secondary rocks.

Owing to such a manifest confusion respecting the true palæontological value of the proposed "Dyas," we should probably never have heard more of the word, had not my distinguished friend, Dr. Geinitz of Dresden, recently issued the first volume of his valuable palæontological work, entitled 'Dyas, oder die Zechstein-Formation und das Rothliegende'[§]. In borrowing the term "Dyas" from Marcou, Dr. Geinitz shows, however, that that author had been entirely mistaken in grouping the deposits so named with the Trias or the Lower Secondary rocks, and necessarily agrees with me in considering the group to be of Palæozoic age.

As there is no one of my younger cotemporaries for whom I have a greater respect as a man of science, or more regard as a friend, than Dr. Geinitz, it is painful, in vindicating the propriety and usefulness of the word "Permian," to be under the necessity of pointing out the misuse and inapplicability of the word "Dyas."

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[*] Communicated by the Author.

[†] See "Dyas et Trias de Marcou," Bibliothèque Universelle de Genève, 1859.

[‡] See 'American Journal of Science and Arts,' 2nd ser. vol. xxviii. p. 256,—the work of M. Marcou having attracted more attention in America than in England.

[§] Leipzig, 1861.

The term "Permian" was proposed twenty years ago for the adoption of geologists, without any reference whatever to the lithological or mineral divisions of the group; for I well knew that a certain order of mineral succession of this group prevailed in one tract, which could not be followed out in another. After surveys, during the summers of 1840 and 1841, of extensive regions in Russia in Europe, in which fossil shells of the age of the Zechstein of Germany, and the Magnesian Limestone of England, were found to occur in several courses of limestone, interpolated in *one great series* of red sandstones, marls, pebble-beds, copper-ores, gypsum, &c., and seeing that these varied strata occupied an infinitely larger superficial area than their equivalents in Germany and other parts of Europe, I suggested to my associates, when we were at Moscow in October 1851, that we should employ the term "Permian" as derived from the vast Government of that name, over which and several adjacent Governments we had traced these deposits.

In a letter addressed to the late venerable Dr. Fischer von Waldheim, then the leading naturalist of Moscow, I therefore proposed the term "Permian" [*], to represent by one unambiguous geographical term a varied mineral group, which neither in Germany nor elsewhere had then received one collective name [†] adopted by geologists, albeit it was characterized by one typical group only of animal and vegetable remains. As the subdivisions of this group in Germany consisted, in ascending order, of Roth-liegende, with the subordinate strata of Weiss-liegende, Kupfer-Schiefer, and Lower and Upper Zechstein, and in England of Lower Red Sandstone and Magnesian Limestone, with other accompanying sands, marls, &c., so well described by Sedgwick [‡], the name of "Permian"—purposely designed to comprehend these various strata—was readily adopted, and has since been generally used. Even Geinitz himself, as well as his associate, Gutbier, published a work under the name of the 'Permische System in Sachsen' [§]. Naumann has also used the term in reference to the group in other parts of Saxony; whilst Göppert has clearly shown that the rich Permian Flora is peculiar and characteristic of this supra-carboniferous deposit. In England, France, and

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[*] See Leonhard's 'Jahrbuch' of 1842, p. 92; and the Philosophical Magazine, vol. xix. p. 418, "Sketch of some of the Principal Results of a Geological Survey of Russia."

[†] It is true that the term Pénéen was formerly proposed by my eminent friend, M. d'Omalius d'Halloy; but as that name, meaning *sterile*, was taken from an insulated mass of conglomerate near Malmédy in Belgium, in which nothing organic was ever discovered, it was manifest that it could not be continued in use as applied to a group which was rich in animal and vegetable productions.

[‡] Trans. Geol. Soc. London, New Series, vol. iii. p. 37.

[§] I may here note that the great Damuda formation of Bengal, with its fossil flora and animal remains, including Saurians and Labyrinthodonts, described by Professor Huxley, has recently been referred (at least provisionally) to the Permian age, by Dr. Oldham, the Superintendent of the Geological Survey of India. In fact, Dr. Oldham actually cites the plant *Tæniopteris*, of the "*Permian beds of Geinitz and Gutbier in Saxony*," in justification of his opinion. See 'Memoirs of the Geological Survey of India,' vol. iii. p. 204.

America no other term in reference to this group has been used for the last fifteen years.

The chief reason assigned by Geinitz for the substitution of the word "Dyas" is, that in parts of Germany the group is divided into two essential parts only—the Roth-liegende below, and the Zechstein above, the latter being separated abruptly from all overlying deposits.

Now, not doubting that this arrangement suits certain localities, I affirm that it is entirely inapplicable to many other tracts. For, in other regions besides Russia, the series of sands, pebbles, marls, gypseous, cupriferous, and calcareous deposits form but one great series. In short, the Permian deposits are for ever varying. Thus in one district they constitute a *Monas* only, in others a *Dyas*, in a third a *Trias*, and in a fourth a *Tetras* *.

In this way many of the natural sections of the North of Germany differ essentially from those of Saxony; whilst those of Silesia differ still more from each other in their mineral subdivisions, as explained in 'Siluria,' 2nd edition, particularly at p. 342. Near the northern extremity of the Thüringerwald, for example, and especially in the environs of Eisenach, an enormous thickness of the Roth-liegende, in itself exhibiting at least two great and distinct parts, is surmounted by the Zechstein, thus being even so far tripartite, whilst the Zechstein is seen to pass upwards to the east of the town, by nodular limestones, into greenish and red sandy marl and shale, the "Lower Bunter Schiefer" of the German geologists. The same ascending order is seen around the copper-mining tract near Reichelsdorf, as well as in numerous sections on the banks of the Fulda, between Rotheburg and Altmorschen, where the Zechstein crops out as a calcareous band in the middle of escarpments of red, white, and green sandstone †.

But in showing that in many parts of Germany, as well as in England, the Zechstein has a natural, conformable, and unbroken cover of red rock, I never proposed to abstract from the Trias any portion of the Bunter Sandstein or true base of the group, as related to the Muschelkalk by natural connexion or by fossils. I simply classed as Permian a peculiar thin red band (Bunter Schiefer), into which I have in many localities traced an upward passage from the Zechstein, and in which no triassic shell or plant has ever been detected.

On my own part, I long ago expressed my dislike to the term Trias; for, in common with many practical geologists who had surveyed various countries where that group abounds, I knew that in

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* See 'Siluria,' 2nd edit., 1859, and 'Russia in Europe and the Ural Mountains,' 1845.

† On two occasions (1853–4) Professor Morris accompanied me, and traced with me these relations of the strata; subsequently, when Mr. Rupert Jones (1857) was my companion, we saw other sections clearly exhibiting this upward transition which I have described. Since then, Professor Ramsay, when at Eisenach, convinced himself of the accuracy of the fact that the Zechstein passes up conformably into an overlying red cover. My note-books contain many additional evidences, which I have not thought it necessary to repeat.

numerous tracts the deposits of this age are frequently not divisible into three parts. In Central Germany, where the Muschelkalk forms the central band of the group, with its subjacent Bunter Sandstein and the overlying Keuper, the name was indeed well used by Alberti, who first proposed it; but when the same group is followed to the west, the lower of the three divisions, even in Germany, is seen to expand into two bands, which are laid down as separate deposits on geological maps of Ludwig and other authors. In these countries, therefore, the Trias of Alberti's tract has already become a Tetras. In Britain it parts entirely with its central or calcareous band, the Muschelkalk, and is no longer a Trias; but, consisting simply of Bunter Sandstein below, and Keuper above, it is therefore a Dyas; though here again the Geological Surveyors have divided the group into four and even into five parts, as the group is laid down upon the map—No. 62, 'Geographical Survey of Great Britain.'

The order of succession in the Permian group all along the western side of the Pennine chain or geographical axis of England proves the impossibility of applying to it the word "Dyas;" for over wide areas in Shropshire and Staffordshire it is one great red arenaceous series, with a few subordinate courses of calcareous conglomerate. Following it to the north, Mr. Binney has demonstrated that the fossils of the Zechstein show themselves in the heart of red marls which occupy on the whole a superior part of such a red series; and in tracing these rocks northwards he has demonstrated that there are, besides, two great underlying masses, first of conglomerates and breccias, and next of soft red sandstones, the latter attaining, as he believes, a thickness of not less than 2000 feet. Here then the Permian may be considered a Trias. Prof. Harkness, in a memoir he is preparing, estimates the thickness of these Lower Sandstones and Conglomerates to the N.E. of West Ormside, in Cumberland, at 4000 to 5000 feet, and shows that they are surmounted by marl-slates bearing plants, thin-bedded red sandstone, grey shale, and sandstone and limestone, the latter—the representative of the Magnesian Limestone—being covered by red argillaceous shale*. Now in all these cases the Permian is a series divisible into three or more parts. But when we follow the same group into Scotland, it there parts with its calcareous feature, and, becoming one red sandstone of vast thickness, is again a *Monas*.

I have entered into this explanation because my friend, Dr. Geinitz, has seized upon one illustration in my work 'Siluria' which shows that in certain tracts, where the Zechstein or Magnesian Limestone is subordinate to an enveloping series of sandstones, the Permian of my classification is *there* as much a tripartite Palæozoic group as the Trias of Central Germany is a triple formation of Mesozoic age. Unless, therefore, the data to which my associates and

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* The red clay or argillaceous shale which covers the limestone is surmounted at Hilton, in Cumberland, by five hundred feet of red sandstone, which, though perfectly conformable to the subjacent Permian rocks, he considers to belong to the Bunter Sandstein of the Trias. Here, then, as in Germany, the limestone may have a red cover, and yet the Bunter Sandstein be intact.

self have appealed, in the work on 'Russia and the Ural Mountains,' and which I have further developed in Memoirs read before the Geological Society and in my two editions of 'Siluria,' be shown to be inaccurate, I hold to the opinion that there are tracts in which the Zechstein is simply a fossiliferous zone in a great sandstone series, to which no division by numerals can be logically applied. Even if I do not appeal to the natural evidences in England, Russia, and parts of Germany, but refer to those tracts where the Zechstein or Magnesian Limestone has no natural red cover, I may well ask, does not the word "Permian," in the sense in which it was originally adopted, serve for every tract wherein the uppermost palæozoic fossil animals and plants are found, whether the strata of which the group is composed form, as in Russia and Silesia, one great series of alternations of plant-bearing sandstones and marls in parts containing bands of fossiliferous limestone, or whether, as in other tracts, the Zechstein stands alone (as near Saalfeld), or in others, again, where the group is tripartite, and even quadripartite? Quite irrespective, however, of the question of whether there are or are not localities in Germany where the Zechstein passes upwards into a red rock, which forms no true part of the Bunter Sandstein of the Trias, we have only to look to the environs of Dresden, on the one hand, and to Lower Silesia on the other, to see the inapplicability of the word "Dyas" to this group.

Near the capital of Saxony, Dr. Geinitz himself pointed out to me that the Roth-liegende is there divided into two very dissimilar parts; and these, if added to the limestone which is there interpolated, or to the true Zechstein of other places, constitute a Trias. Again, Beyrich, in his Map of Lower Silesia [*], has divided the vast Roth-liegende of those mountains into Lower and Upper, the two embracing *eight* subdivisions according to that author.

In repeating, then, that the word "Permian" was not originally proposed with the view of affixing to this natural group any number of component parts, but simply as a convenient short term to define the Uppermost Palæozoic group, I refer all geologists to the very words I used in the year 1841, when the name was first suggested. In speaking of the structure of Russia, I thus wrote:—"The Carboniferous system is surmounted to the east of the Volga by a vast series of beds of marls, schists, limestones, sandstones, and conglomerates, to which I propose to give the name of 'Permian System,' because, although this series represents *as a whole* the Lower New Red Sandstone (Rothe-todte-liegende) and the Magnesian Limestone or Zechstein, yet it cannot be classed exactly, whether by the succession of the strata or their contents, with either of the German or British subdivisions of this age [†]."

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After pointing to the Governments of Russia over which such Permian rocks ranged, I added:—"Of the fossils of this system, some undescribed species of *Producti* might seem to connect the Permian with the Carboniferous era; and other shells, together with

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[*] See also 'Siluria,' 2nd edit. p. 343.

[†] Phil. Mag. xix. p. 419.

fishes and saurians, link it more closely to the period of the Zechstein, whilst its peculiar plants appear to constitute a Flora of a type intermediate between the epochs of the New Red Sandstone or Trias and the Coal-measures. Hence it is that I have ventured to consider this series as worthy of being regarded as a system *."

In subsequent years, having personally examined this group in the typical tracts of Germany as well as of Britain, I felt more than ever assured that, from the great local variations of mineral succession of the group, the word "Permian," which might apply to any number of mineral subdivisions, was the most comprehensive and best term which could be used, the more so as it was in harmony with the principle on which the term Silurian had been adopted.

Apart from the question of the substitution of the new word "Dyas" for the older name "Permian," I take this opportunity of expressing my regret that some German geologists are returning to the use of the term "Grauwacke Formation," as if years of hard labour had not been successfully bestowed in elaborating and establishing the different Palæozoic groups, all of which, even including the Lower Carboniferous deposits, were formerly confusedly grouped under the one lithological term of the "Grauwacke Formation."

Respecting as I do the labours of the German geologists who have distinguished themselves in describing the order of the strata and the fossil contents of the group under consideration, I claim no other merit on this point for my colleagues de Verneuil and von Keyserling, and myself, than that of having propounded twenty years ago the name of "Permian" to embrace in one natural series those subformations for which no collective name had been adopted. Independently, therefore, of the reasons above given, which show the inapplicability of the word "Dyas," I trust that, in accordance with those rules of priority which guide naturalists, the word "Permian" will be maintained in geological classification.

London: Belgrave Square.
Nov. 30, 1861.

X. *Notices respecting New Books.*

Euclid's Elements of Geometry, designed for the use of the higher Forms in Public Schools and Students in the Universities. By Robert Potts, *M.A., Trinity College, Cambridge. Corrected and Improved edition.* London: John W. Parker, Son, and Bourn.

MR. POTTS' first octavo edition of Euclid appeared in 1845, and since then has been gradually gaining ground in the estimation of our best teachers as one of the most unexceptionable books of its class at present within the reach of the students in our schools and universities. The work is too well known to require description,

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* In my last edition of 'Siluria' I have spoken of the Permian as the Uppermost Palæozoic *group*, but have not deemed it a system by comparison with the vast deposits of Carboniferous, Devonian, and Silurian age.

and little need be added to the notice already given in this Magazine (see vol. xxxii. S. 3. p. 69), beyond the assurance that the new edition possesses all the best and most characteristic features of the old one, *minus* many of its imperfections. The conscientious care with which the verbal defects of Simson's text have been emended cannot be too highly praised, and it must be a great satisfaction to Mr. Potts to know that he has done much towards the cultivation, in our schools, of a more correct taste, as far as the purity of geometrical reasoning is concerned. In fact, if we were to judge the work merely from its author's own point of view, that is to say, as a careful reproduction of, and judicious commentary upon the 'Elements of Euclid,' we should have little to say except in praise of the result as now offered to the public. It is only when we take different, and higher ground, when, in short, we compare this treatise on the science of geometry with the purely ideal one which English students do not, but ought to possess, that we find room for much criticism.

We have no desire to enter here into the *question épineuse* as to the absolute merits of Euclid's 'Elements;' as a classical work it is second to none in point of interest, and it will ever continue to be studied by *men* of culture. The opinion is gaining ground, however, that our national admiration of Euclid has been carried too far—that it has too long deprived our *schools* of the advantages to be gained from an elementary treatise on geometry which, although based upon the old one, shall be superior to it in point of method and accuracy, of purely English origin, and in every way worthy of the present state of the science. Even in our universities, which are proverbially and, on the whole, wisely conservative, symptoms of a more vigorous and healthy criticism—too long discouraged by an inordinate notion of Euclid's perfection—manifest themselves more and more frequently. Mr. Potts' notes to the several books of Euclid might be cited in support of our assertion, whilst the frequent use in them of such words as *seems* and *appears*, in place of the more decisive and emphatic verb *is*, curiously enough indicates the state of transition to which we have referred. Numerous instances might be given; let one suffice. In his notes to the 3rd book, Mr. Potts modestly informs us that the 9th proposition "*appears* to follow as a corollary from the 7th," whereas it *is*, as he well knows, a purely *logical* consequence of the latter; for if it be impossible to draw more than two equal lines from any *non-central* point to the circumference of a circle, any competent *logician*, even if he were ignorant of the very nature of a circle, would be able to conclude that the point must be a *central* one from which three equal lines can be drawn to the circumference.

Having mentioned these notes, it is but just to add that in their present improved form they constitute a very valuable feature of the work, and on the whole are both judicious and accurate. Whilst admitting, however, that the opposite excess would have been intolerable, we cannot but think that the notes in question would have been of far greater value had they been less purely explanatory, and more thoroughly critical. Instead of "exemplifying" by geometrical figures such axioms as "if equals be added to equals the wholes are equal," it would surely have been more profitable to have enumerated the several axioms which Euclid tacitly assumes. Instead of showing, in a very questionable manner too, how the 11th and 12th propositions of the third book might have been proved directly had they been placed after the 18th proposition, would it not have been better to have proposed a rearrangement of the whole book—to have given a sketch, in fact, of a better treatment of the many beautiful properties of the circle?

The collection of geometrical exercises given by Mr. Potts is another characteristic feature of his work, and has also been improved. In order to give to *exercises* their full value, however, and to prevent them from degenerating into mere riddles, they should be made subordinate to, and illustrative of geometrical *methods*; and this, it must be admitted, Mr. Potts has not been able to do fully, since, for other and good reasons, he has preferred selecting his exercises from college and university examination-papers. Comparing his selections with others, however, we cannot but agree with the Reviewer whom Mr. Potts himself quotes in his preface. With respect to the first of the exercises "on tangencies," we will merely caution the student against accepting the author's analysis, either as a model for imitation or as a specimen of Mr. Potts' ability; it is unusually defective. The very enunciation of the problem is objectionable, disfigured as it is by the introduction of the perfectly irrelevant datum "of a line given in position."

We do not care to dwell longer upon imperfections which, if not trivial, are certainly far outweighed in importance by the many excellent features of the book. We will merely repeat, then, that although we trust the work, considered as an *introduction* to the science of geometry, will some day be superseded, we are convinced that as a careful English reproduction of Euclid's Elements, illustrated by the notes of an able and judicious teacher, and enriched by a large collection of very useful exercises, it will long maintain its ground.

XI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxii.]

April 11, 1861.—Major-General Sabine, R.A., Treasurer and Vice-President, in the Chair

THE following communication was read:—

"On the Motion of a Plate of Metal on an Inclined Plane, when dilated and contracted; and on the Descent of Glaciers." By the Rev. Henry Mosely, M.A., Canon of Bristol, F.R.S., Inst. Sc. Paris Corresp.

The case in which the upper edge of such a plate (supposed rectangular) is fixed is first discussed; and then that in which the lower edge is fixed. Each of these cases is considered subject to the condition of friction; first, when the plate is dilated, and secondly, when it is contracted. Two other principal conditions arise in the discussion; one being that in which a part only, and the other that in which the whole of the plate dilates and contracts.

In the former the dilatation or contraction is represented by

$$\frac{E\lambda^2 t^2 \cos\phi}{2\mu(1\pm\lambda t)\sin(\phi\pm\iota)},$$

or by

$$\frac{E\lambda^2 t^2 \cos\phi}{2\mu(1\mp\lambda t)\sin(\phi\mp\iota)},$$

according as the plate is fixed at the top or the bottom.

In the latter it is represented under the same conditions by

$$a\left\{\lambda t-\frac{\mu\sin(\phi\pm\iota)}{2E\cos\phi}a\right\}$$

or by

$$a\left\{\lambda t-\frac{\mu\sin(\phi\mp\iota)}{2E\cos\phi}a\right\}.$$

In which formulæ—

a represents the length of the plate.

μ its weight in lbs. per foot of its length.

ι the inclination of the plane.

ϕ the limiting angle of resistance (the angle of friction) between the surface of the plane and of the plate.

E the modulus of elasticity of the plate.

λ the dilatation or contraction per foot of the length for each variation of 1° of Fahrenheit.

$\pm t^\circ$ the rise or fall of the temperature in degrees of Fahrenheit, by which the dilatation or contraction of the plate is supposed to be caused.

In the case in which no part of the plate is fixed, a horizontal line may be taken in it above which it *dilates* upwards, and below it downwards. The position of this line is determined by the consideration that, if the plate be imagined to be cut through along that line, the thrust necessary to push the part above upwards must be equal to that necessary to push the part below downwards.

In like manner a horizontal line may be found above which the plate *contracts* downwards and below it, upwards.

The former neutral line is nearer the top than the bottom, the other nearer the bottom than the top. The one is at the same distance from the top as the other is from the bottom. This distance is represented by the formula

$$\tfrac{1}{2}a\frac{\sin(\phi-\iota)}{\sin\phi\cos\iota}.$$

When the plate is dilated, it is the longer portion which dilates *downwards*; and when it is contracted, it is the shorter portion which contracts *upwards*. The lower end of the plate descends therefore by a given increase of temperature more than it ascends by an equal fall; and on the whole the plate descends.

If we suppose the temperature first to be increased by $t_1{}^\circ$, and then diminished by $t_2{}^\circ$; then—

1st. In the case in which a portion only of the plate dilates, the descent is represented by

$$\frac{E\lambda^2 \cos\phi}{2\mu}\left\{\frac{t_1{}^2}{(1+\lambda t_1)\sin(\phi-\iota)}-\frac{t_2{}^2}{(1-\lambda t_2)\sin(\phi+\iota)}\right\}.$$

2ndly. In the case in which the whole plate dilates and the whole contracts, the descent is

$$\tfrac{1}{2}a\left\{(t_1-t_2)\lambda+(t_1+t_2)\frac{\tan\iota}{\tan\phi}-\frac{\mu a\sin(\phi+\iota)\sin(\phi-\iota)\tan\iota}{E\sin^2\phi\cos\iota}\right\}.$$

The first case passes into the second.

If E be very great as compared with μa, the second term in the above formula may be neglected. It then corresponds with the formula given by the author in a former communication to the Society.

To verify the fact of the descent of a plate of metal under the conditions supposed, a deal board 9 feet long and 5 inches broad, was fixed at an inclination of 18½° against the wall of a house having a southern aspect, and a sheet of lead was placed upon it one-eighth of an inch thick and weighing 28 lbs., and having its edges turned over the edges of the board so as not to bind upon it. Near the lower extremity a vernier was constructed, by which the position of the lead on the board could be determined to the 100th of an inch. Its position was observed daily between 7 and 8 in the morning and 6 and 7 in the evening, from the 16th of February to the 28th of June, 1858.

A Table is given showing the descent for every day of that period, from 7 A.M. to 6 P.M. and from 6 P.M. to 7 A.M.

In the months when there was no sunlight from 6 P.M. to 7 A.M., there was no descent in that interval. The descents from 7 A.M. to 6 P.M. were very different on different days. Sometimes they amounted to a quarter of an inch in the day, and sometimes were not appreciable. The greatest descents were on sunny days, and especially when with a warm sun there was a cold wind. The least were on days of continual rain. The average daily descents were, in inches,—

February.	March.	April.	May.	June.
·10000	·13806	·16133	·21500	·21888

These descents were not due to the extreme temperatures of the periods in which they took place, but to the aggregate of the variations up and down during each interval. The difference of the highest and lowest temperatures in any interval may have been small, and yet the changes of temperature up and down may have been many, and their aggregate great. It is upon this aggregate that the descent depends.

The dilatation of ice was measured in the years 1845, 1846, at the Observatory of Pultowa, by Schumacher, Pohr, and Moritz; and the particulars of their experiments were communicated to the Academy of St. Petersburgh, by W. Struve, in 1848, and published in its Memoirs (Sciences Mathém. et Phys., sér. 6. t. iv.). By exposing water to the action of the frost in a mould, Schumacher obtained a block of ice, which, after reducing it with the plane, measured 6 ft. 3 ins. in length and 6 ins. by $6\frac{1}{2}$ inches in section; and he caused three thermometers to be frozen into it with their stems projecting above its surface. This block of ice he carried out from a room, where it had been preserved at a uniform temperature of $-2°$ R. during the day, into the open air at night, and slung it in a horizontal position from a beam supported by tressles. As its temperature fell he measured the distance between two steel points frozen into it near its two ends, by a measuring rod of dry wood (well-clothed), the distances on which were referred to a standard measure on the wall of a room of the Observatory which retained nearly a constant temperature of $-2°$ R. His measurements had reference to observed temperatures of the ice varying from $-2°{\cdot}3$ R. to $-22°$ R. After applying the requisite corrections, it resulted from them that the coefficient of expansion of ice is for 1° R.

$$\cdot 00006466,$$

which is *nearly* twice as great as the coefficient of dilatation of lead, and *more* than twice as great as that of any other solid.

We do not know the modulus of elasticity of ice, or the pressure under which it disintegrates.

If it were as elastic as slate and did not resist crushing more than hard brick, a block of it placed with its ends between two immoveable obstacles, would crumble when its temperature was raised one degree of Fahrenheit. It is its great dilatability which gives to ice this tendency to disintegrate, when, not being free to dilate, its temperature is raised*, even so slightly as this.

If the block of ice experimented on by Schumacher had been placed upon a plank inclined at the same angle as that used in the experiment with the lead was, and if its under side had been coated with lead-foil so as to give it the same friction on the plank as the lead had, then, under the same variations of temperature as the lead experienced, it could not but have descended as the lead did, but twice as fast, because its dilatability is twice as great.

We may conceive such a block of ice to be made up of thin plates parallel to its upper surface, such as plates of glass would be, if glass were as dilatable as ice and as friable, and if it possessed that property of passing from a disintegrated into a solid state, which in ice is called regelation. If we put the adherence of these plates to one another in the place of friction, and conceive the variations of external temperature (or the effects of solar radiation) to reach them in succession, each one being dilated or contracted independently of the rest, then each would descend by a motion proper to itself, and also by reason of the descents of those subjacent to it. The extremities of the plates would under these circumstances overlap, and the descent of each, proper to itself, would be increased by the overlappings of those beneath it.

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* Agassiz describes a disintegration of the transparent ice of the blue bands of glaciers when laid bare, which appears to be due to its expansion.—Bulletin Un. de Genève, vol. xliv. p. 142.

Each plate would under these circumstances descend faster than the one beneath ; and supposing the adherence of the lowest plate to the board to be the same as that of the plates to one another, then, of any number of blocks similarly placed and subject to the like variation of temperatures, the thickest or deepest would descend, at its surface, the fastest ; and if there were a block of different depths in different parts, the deepest parts would descend the fastest. The differential motion thus set up would not be appreciable in a block of ice of different thicknesses in different parts if its dimensions were no larger than the block experimented on by Schumacher, but in a glacier it would be appreciable.

To bring Schumacher's block to the proportions of a glacier, it must be converted into a slab twelve feet long, twenty inches wide, and two inches thick. It would then represent on a scale of the 1500th part, a glacier 2500 feet wide, 250 feet deep, and 18,000 feet long, which are something like the dimensions of the Mer de Glace from 2300 feet below Montanvert to the Tacul. If we suppose it to be placed at the same inclination of 18½° at which the lead was, its under surface being coated with lead so as to have the same friction on board as the lead had, then it may be calculated that if it had experienced the same variations of temperature as the lead did, its average daily descent, measured in inches, would have been

February.	March.	April.	May.	June.
·26666	·36816	·43022	·57334	·58368

If, now, we conceive its inclination to change from 18½° to that of the Mer de Glace, which is about 5°, and its dimensions to become actually those of that glacier, then, supposing the glacier to experience the same elevations and depressions of temperature as the lead did, its average daily descents in inches would be

February.	March.	April.	May.	June.
104·56	144·06	168·74	224·87	228·92

which rates of motion are probably twelve times greater than the actual rates of motion of the glacier ; showing that variations of the temperature of the glacier twelve times less than those of the lead, would be sufficient to produce its actual descent ; or that it would descend as it actually does, if the resistances opposed to its descent were twelve times greater than the resistances opposed to the descent of the lead—If its descent were resisted by a friction, for instance, having twelve times the coefficient of that of the lead on the board, or such as would cause it to rest without slipping on an incline having twelve times the tangent of the inclination of the board ; or if the variations of temperature were less and the resistance greater in any proportion which would retard the descent twelve times as much. So that we may suppose in the case of the glacier a far greater resistance in proportion than that sustained by the lead upon the board, and variations of temperature far less, without passing the limits within which a probability is created by the experiment that the descent of the glacier is due to the same cause as that of the lead.

In the act of descending on the board, the slab of ice of which we have spoken could not but be thrown into a state of extension in some parts and of compression in another. The conditions of the descent being in other respects given, the amount of this extension or compression might be at any point determined. If at any point the extension exceeded the tenacity of the ice, the slab would there separate across its length; and if at any point the compression exceeded the resistance to crushing, it would there crush.

Supposing it to be thinner at the sides than in the middle, the surface-motion of the middle would be faster than that of the sides, and from this differential motion would result cracks oblique to the axis of the slab, the explanation of which, as they exist in glaciers, is one of the most successful attempts yet made at the solution of the mechanical problem of glacier-motion. These conditions of the descent of the slab, when referred to a glacier, explain the formation of transverse and lateral crevasses, and the fact of a glacier crushing itself through a gorge.

The Mer de Glace moves faster by day than by night*. Its mean daily motion is twice as great during the six summer as during the six winter months †. It moves fastest in the hottest months, and in those months varies its motion the most, because in them the variations of temperature are the greatest. It moves most slowly in the coldest months, and in those varies its motion the least, because in those months the variations of temperature are the least. These differences are more remarkable at lower stations on a glacier than at higher, "because the lower are exposed to more violent alternations of heat and cold than the higher: this (says Forbes) we shall find to be general."

It moves fastest on the hottest days. "This I apprehend (says Forbes) to be clearly made out from my experiments, that thawing weather and a wet state of the ice conduce to its advancement, and that cold, whether sudden or prolonged, checks its progress ‡." "The striking variations in September, especially at the lower stations, which were frequently observed, prove the connexion of temperature with velocity to a demonstration §."

It is, however, impossible to do justice to the positive character of the evidence on which this conclusion has been founded by Professor Forbes without reference to those diagrams, by means of which he has compared the mean rates of the daily motions of glaciers and the corresponding mean temperatures. This comparison is founded on observations made by himself and Aug. Balmat, as to the motion of the "Mer de Glace," at fourteen different stations in three different years, and on observations on the mean temperature of the atmosphere made at the same times at the Great St. Bernard and at Geneva. It results from it that no change in the mean temperature

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* Forbes, 'Occasional Papers,' p. 12.

† Ibid. p. 129. Tyndall, 'Glaciers of the Alps,' p. 294.

‡ Forbes, 'Travels in the Alps,' p. 148.

§ Ibid.

of the atmosphere is unaccompanied by a corresponding change in the mean motion of the glacier.

The glacier moves with different velocities at different depths, the surface-motion being faster (probably two or three times) than that of the deepest part. The motions at different depths cannot but be related to one another: so that as the influence of variations of temperature is felt on the *surface*, it cannot but be felt throughout the glacier.

If every change of solar heat is associated with a corresponding change of glacier-motion, it seems to follow that the two are either dependent upon some common cause, or that the one set of changes is caused by the other; and the former of these conclusions being inadmissible, we are forced on the latter. It is not necessary to show how it is that changes of external temperature penetrate glaciers. Of the power of the sun upon them there are, however, evidences in the ablation of surface constantly going on and in the preservation of the ice which is covered by the stones of a moraine, which sometimes forms an icy ridge from 50 to 80 feet high, and some hundred feet in width.

"The sun's rays," says Tyndall*, "striking upon the unprotected surface of the glacier, *enter the ice to a considerable depth*; and the consequence is that the ice near the surface of the glacier is always disintegrated, being cut up into minute fissures and cavities filled with water and air, which, for reasons already assigned, cause the glacier when it is clean to appear white and opaque. The ice under the moraines, on the contrary, is usually dark and transparent. I have sometimes seen it as black as pitch, the blackness being a proof of its great transparency, which prevents the reflexion of light from its interior. The ice under the moraines cannot be assailed in its depths by the solar heat, because this heat becomes obscure before it reaches the ice, and as such it lacks the power of penetrating the substance. It is also communicated in great part by way of contact instead of by radiation. A thin film at the surface of the moraine ice engages all the heat that acts upon it, its deeper portions remaining transparent and intact."

It matters not to the argument how little below freezing the temperature of a glacier may be. So long as the ice *exists in a solid state* and is capable of being penetrated by the solar heat, it cannot but dilate and contract. Its central portions, lying folded in ice 100 feet thick above and below, may well, however, be conceived to retain some of the cold of the region from which they have descended. The observations of Agassiz on the temperature of the Aar Glacier are not to be relied upon, because the access of damp external air to the borings in which they were made, and of water percolating the disintegrated ice of the surface, was not effectually stopped. The included thermometers could not but under these circumstances show zero, although the temperature of the surrounding ice was below it. For the water freezing on the walls of the boring, the latent heat thereby given out, would raise the temperature of the air about the bulb to the freezing-point, and this water being continually renewed, the quicksilver would always be kept at that point.

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* Glaciers of the Alps, p. 294.

That glacier-ice possesses no such properties of viscidity or compressibility as would cause it to descend by its weight along such slopes as those on which some glaciers descend may be shown thus. Let the Mer de Glace be conceived to be cut up by vertical sections at right angles to one another, into blocks, whose bases are large enough to prevent them toppling over; and let these blocks be imagined to be separated from one another. Then (supposing it not to slip) each block would stand in its place without the support of the neighbouring blocks; for its vertical sides would be walls of ice needing no external support, like the ice-wall of the Glacier du Géant, 141 feet high near the Tacul, described by Tyndall*; or that of the Mer de Glace near the Augle, pictured by Forbes†. Needing no external support when thus placed asunder, they could need none when brought again together; nor could they, by the fact of their being so brought together, be made to exert any mutual pressure, or have any more or other tendency to move than each block had separately. If this reasoning be true, there is no physical property of ice, whether it be called viscosity or plasticity, which would cause it to descend by its weight alone on any surface along which it would not slide. It is plastic no doubt—Tyndall has proved that by the Hydraulic Press,—but not as to any pressure created in a glacier by the weight of the glacier. If it were, or if it were semifluid, then under those enormous pressures which it is supposed to sustain, it would bulge out at the ice-wall of the Tacul, and mould itself to the sides of its channel; for it is the character of a compressible substance, not less than of a semifluid, to yield not only in the direction in which pressure is applied to it, but in every other.

Nor if it were sufficiently a fluid to flow by its weight alone, however slowly, down slopes of 3° or 5°, could it descend otherwise than as a torrent down slopes, such as that of the Silberberg Glacier, of 40°, on which its descent is nevertheless several times slower. The phenomena of these secondary glaciers offer themselves as a test of rival theories of glacier-motion. They lie on slopes so steep that it is scarcely possible to conceive the ice, if solid, to be loosened from the face of the rock, and not to descend in fragments; or if viscous, not to become a torrent.

XII. *Intelligence and Miscellaneous Articles.*

ON THE CONDUCTIBILITY OF SALINE SOLUTIONS.

BY M. MARIE-DAVY.

1. I CALL the *calculated density* of a solution the number obtained by adding to unity the weight of salt dissolved in a gramme of water. The relation of the calculated to the true density gives the measure of the degree of expansion which water experiences in consequence of salt held in solution.

2. I call the *corrected conductibility* of a solution the product obtained by multiplying its true conductibility by the ratio of its cal-

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* Glaciers of the Alps, p. 289.

† Travels in the Alps, p. 76.

culated to its true density. The corrected conductibility expresses thus the conductibility which the solution would have on the double hypothesis,—1st, that the dissolved salt would not produce any increase in volume of the solvent, water; 2nd, that the conductibility of water increases proportionally to its density, the temperature remaining constant, and that the same is true of the salt.

3. I call the *calculated conductibility* the sum obtained by adding to the proper conductibility of water a number proportional to the quantity of salt dissolved in a gramme of water.

4. If the double hypothesis of § 2 is true, the calculated and corrected conductibilities ought to coincide; and reciprocally, if coincidence takes place, the double hypothesis is admissible.

5. In operating on nine solutions of sulphate of copper in distilled water, the true density of which varied from 1·018 to 1·177, the agreement obtains as far as 1 to 2 per cent., except for the first, which has a very feeble conducting power, and therefore difficult to determine, and for the last, which is saturated. In the two last cases the deviation is less than a tenth, and is in the same direction in both cases.

6. It may be admitted as a first approximation,—1st, that the conductibility of a solution of sulphate of copper is equal to the sum of the conductibilities of water and the salt; 2nd, that the latter are proportional to the respective densities of the two substances, the temperature remaining the same.

7. The latter law supposes that the constitution of each of the molecules, water and sulphate of copper, has not been modified by the fact of solution; but when we consider how a mere difference of a few degrees in temperature modifies the conductibility of a liquid, we are surprised at the small limits between the corrected and the calculated conductibilities of solutions of sulphate of copper.

8. In a solution of sulphate of copper the water and the salt act separately as conductors; each is traversed by its own peculiar derived current; each consequently is decomposed by the current.

9. The copper reduced by electricity in a solution of sulphate of copper has a double origin: one part proceeds from the direct reduction effected by the current, the other is a secondary product of the action of nascent hydrogen on the copper salt. The relation of the weights of these two deposits varies with the concentration, and probably with the temperature of the liquid.

10. The presence of a free acid in a solution adds a third conductor to the two others, and diminishes, so far, the proportion of copper directly reduced.

11. As the properties of the deposit vary with its origin, they will probably vary also with the concentration and degree of neutrality of the solution.

12. The work absorbed by the reduced metal, depends not only on the nature of the metal, and of the acid with which it was combined, but also on its molecular state; the electromotive force of a Daniell's battery varies with the concentration and neutrality of the solution of sulphate of copper. This variation, however, is comprised within very narrow limits.—*Comptes Rendus*, Oct. 21, 1861.

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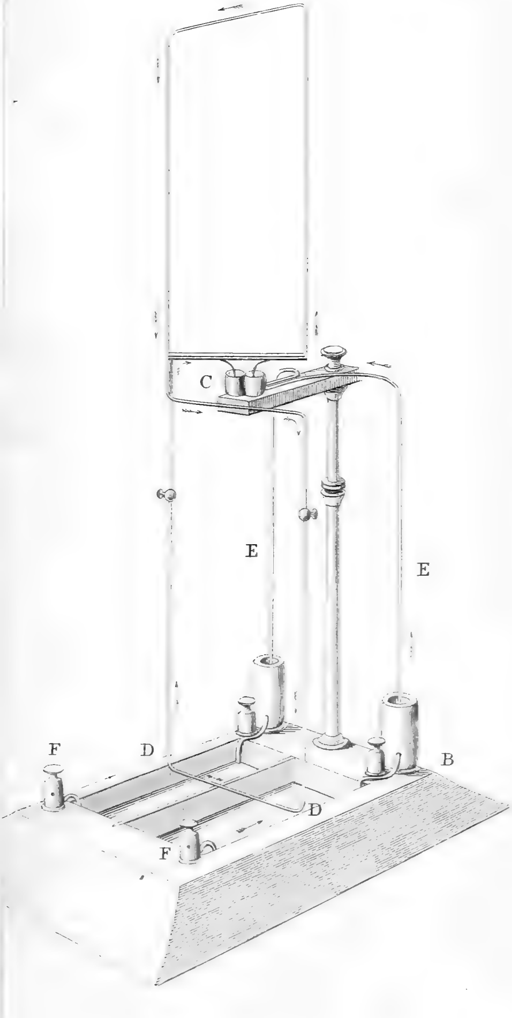

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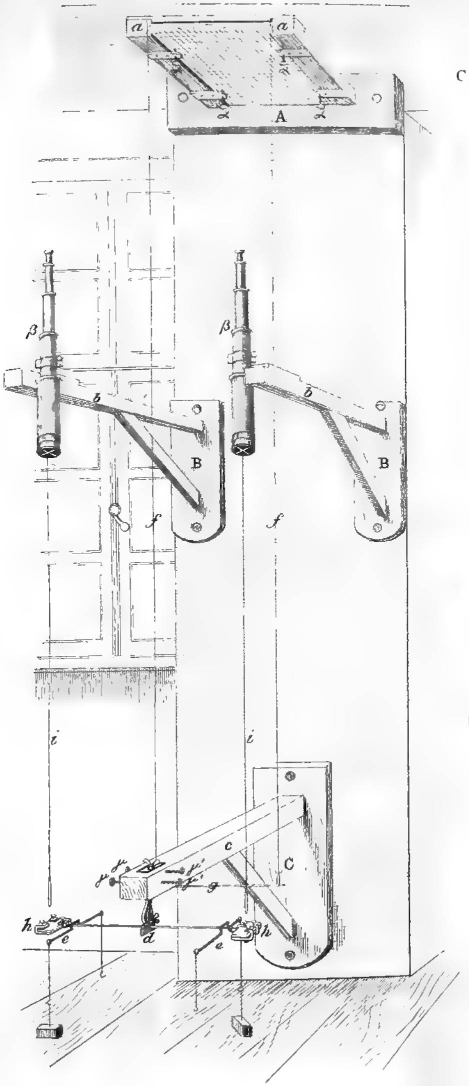

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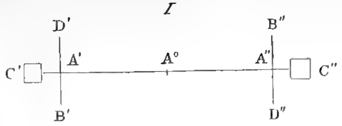

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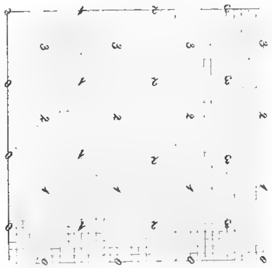

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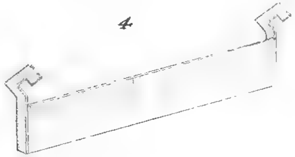

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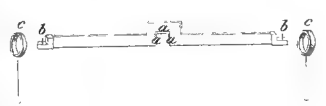

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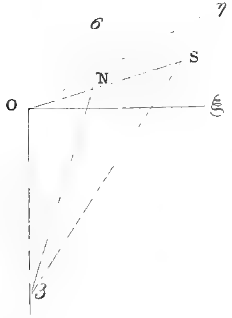

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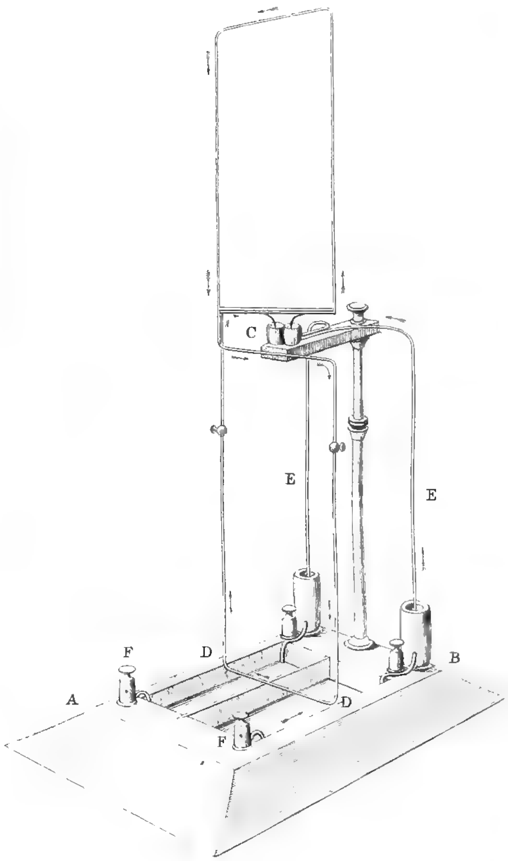

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THE
LONDON, EDINBURGH AND DUBLIN
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[FOURTH SERIES.]

FEBRUARY 1862.

XIII. *On the Cause of Vesicular Structure in Copper.*

By W. J. RUSSELL, *Ph.D., and* A. MATTHIESSEN, *F.R.S.**

ALL specimens of commercial copper, when carefully examined, are found to be more or less vesicular in their structure: in some cases the vesicles are so small as to require a magnifying glass to see them, but in other specimens they are developed on a much larger scale. Copper and silver are, we believe, the only metals which may readily be made to assume a vesicular structure. In the case of silver, it is well known that the fused metal absorbs oxygen, which being again given out on cooling, produces the vesicular structure. It appeared therefore possible that the vesicular structure in copper might also be caused by the absorption of some gas. Our experiments have, however, proved this not to be the case, but have led us to the same conclusion with regard to the cause of this porous structure as that arrived at by Mr. Dick†. As, however, our experiments on some points have been rather more extended than his, and remove, we believe, all doubt from the subject, a short account of them may not be without interest.

The amount of gas contained in these cavities found in copper was evidently too small to allow of its being collected, we were therefore obliged to carry on the investigation synthetically. It seemed, however, probable, from the inside of the cavities always appearing untarnished, that they did not contain any oxygen. In order, then, first to ascertain whether the vesicular structure was owing to any mere absorption of a gas by the melted metal, we passed each of the ordinarily occurring gases, carefully purified

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* Communicated by the Authors.

† Phil. Mag. June 1856.

and dried, through fused copper for five minutes. In all our experiments electrotype copper was used, as being the purest, and each experiment was made with about 150 grammes of the metal. The gases experimented with were hydrogen, oxygen, nitrogen, air, carbonic acid, and carbonic oxide. With each gas the experiment was made under three different circumstances:—1st, the gas was passed through when the melted copper was without flux or covering; 2nd, when it was under a flux of salt; and 3rd, when covered with charcoal. We need not describe separately each experiment; it will be sufficient to state the general result obtained from this series of experiments, which is, that the vesicular structure in the copper is only developed when either oxygen or air is passed through the melted metal under charcoal. Under these circumstances, not only did the metal become very porous, but when solid the surface was found to have risen or vegetated, often to a very considerable extent. In some cases even small particles of the melted copper, on the cooling of the mass, were projected out of the crucible. A similar phenomenon is known to copper-smelters, and called by them copper rain. In order to obtain satisfactory results, a considerable amount of care is necessary in performing these experiments; for if the surface of the melted copper towards the end of the experiment be exposed, by the burning off of the charcoal or any other accidental circumstance, only for a very short time to the air, that alone is sufficient to induce the vesicular structure in the metal. Again, we found that the same phenomenon was produced when any of the fuel fell into the melted copper. Having satisfactorily proved that air and oxygen were the only gases which caused the copper to become vesicular, and in fact these only when carbon was present, we naturally concluded that the vesicular structure could not be owing to any mere absorption or chemical combination of the melted copper and gas, but was probably due to the formation of carbonic oxide arising from the reduction of the suboxide of copper by means of the charcoal. The following experiments show, we think, that this supposition is correct:—

Copper was fused alone in air: immediately on removing it from the furnace, powdered charcoal was thrown on the surface; considerable vegetation ensued, and on being fractured it was found to be very vesicular.

Another specimen of copper was fused, as before, in the air; but now, on removing it from the furnace, instead of throwing charcoal on the surface of the melted metal, a jet of coal-gas was allowed to play upon it. The result was precisely similar to that obtained with the charcoal, the carbon from the coal-gas reducing the suboxide.

In another experiment of the same kind we varied the conditions by throwing on the charcoal before the fused copper had been removed from the furnace; a vegetation in bulk nearly equal to half that of the copper used took place. In order to give a more definite idea of the great extent to which the metal becomes porous, we have taken the specific gravity of several of these specimens of copper in which we have developed the vesicular structure. The specific gravity of the copper operated on in the last experiment was found to be only 5·683, whereas that of pure copper is 8·952. Mr. Dick, in the paper before alluded to, describes some very interesting experiments which he made by casting copper which had been fused under charcoal in an atmosphere of coal-gas, thus not allowing the melted metal to come in contact with the air; the result was that he then obtained a perfectly solid casting; but if, instead of using coal-gas, he made the casting in air, using even metal out of the same crucible in both experiments, he in the latter case always obtained a casting which was very vesicular. Our experiments entirely confirm these results of Mr. Dick. The specific gravity of the casting thus obtained shows in a striking manner the great alteration in the density of the copper. A specimen of the electrotype copper which we used, simply fused and allowed to cool under charcoal, gave us a specific gravity of 8·952. The casting made in coal-gas had a specific gravity of 8·929; another made in the same way had a specific gravity of 8·919; whereas a third specimen, also made with metal out of the same crucible, but cast in air, had a specific gravity of 6·193. To render our experiments still more conclusive, we now fused copper, allowing the air free access to it, and cast one portion of the metal in moulds containing air, and the other in moulds filled with coal-gas. Two specimens of the copper cast in air had respectively the specific gravities 8·618 and 8·665, while the specimens which had been cast in coal-gas had a specific gravity of only 6·926 and 6·438.

The cause, then, of the vesicular structure in copper appears to be due to the reduction of the suboxide of copper by the carbon mechanically drawn down into the mass of the metal by the currents continually formed from the cooling of the surface. The carbonic oxide thus produced throughout the whole mass of the copper is given off as long as the surface remains fused; but as soon as it solidifies, the crust is lifted up, and breaking, produces vegetation. The carbonic oxide formed at the time of solidification not being able to escape, remains dispersed through the metal, giving it the vesicular structure. With lampblack (carbon in a finer state of division) instead of powdered charcoal, the action appears to be still more intense; and on throwing some of it on melted copper, the evolution of gas may be easily seen. That the vesicular structure is not owing to any especial affinity of melted copper for carbonic oxide, is shown by the fact that when copper is fused under charcoal or a flux of salt, and carbonic oxide passed through it, the metal, on cooling, is found to be entirely devoid of all porous structure, as proved by its specific gravity, which we found to be 8·943.

That carbon can exercise an influence of the kind attributed to it in the foregoing experiments, is shown by its action on melted silver; for if silver be fused under a layer of charcoal, and oxygen gas passed through it for any length of time, still no spitting will take place on the cooling of the metal. Again, when silver is fused in air, if charcoal be thrown on the melted surface no spitting occurs, a fact well known to assayers: sand or any other body of that kind does not exercise a similar influence on the silver*.

We next tried the action of sulphur on suboxidized copper, and found that it also produced the vesicular structure, and even caused the copper to vegetate to a very considerable extent. In fact, when sulphur is thrown on copper which has been melted with access of air, results are obtained similar to those which carbon produces under the same circumstances.

Two specimens of copper which had been rendered vesicular by the action of sulphur, were found to have respectively the specific gravities of 6·6 and 5·1. It is a somewhat curious fact, that the phenomenon of copper rain is caused to a much greater extent by the action of sulphur than it is by carbon. The sulphur, of course, acts on the suboxide of copper in the same kind of way as the carbon; and the vesicular structure and copper rain are in this case owing to the evolution of sulphurous acid.

We also tried the action of iodine and of phosphorus on suboxidized copper, but they did not produce any appearance of vesicular structure.

The foregoing experiments were carried out partly in Professor Percy's, and partly in Professor Williamson's laboratory.

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* While engaged on this subject, we also made a series of experiments upon silver, to ascertain whether any other gas than oxygen was absorbed by it. The melted silver was treated in precisely the same way as the copper, and the gases oxygen, hydrogen, air, nitrogen, carbonic acid, and carbonic oxide passed through it. The spitting of the silver we found to be caused only by oxygen or air, and, further, that, as mentioned above, this was entirely prevented by the presence of charcoal. In Gmelin's 'Chemistry' (vol. vi. p. 139) it is stated that, when silver is fused under nitrate of potash, the spitting takes place: this statement we believe to be incorrect; for we always found that when silver was carefully fused, so that the air did not come in contact with it, under a layer of either nitrate or chlorate of potash, no spitting took place. As both these salts are decomposed below the melting-point of silver, this result is what might be expected.

XIV. *On Physical Lines of Force. By* J. C. Maxwell, *F.R.S., Professor of Natural Philosophy in King's College, London*[*].

Part IV.—*The Theory of Molecular Vortices applied to the Action of Magnetism on Polarized Light.*

THE connexion between the distribution of lines of magnetic force and that of electric currents may be completely expressed by saying that the work done on a unit of imaginary magnetic matter, when carried round any closed curve, is proportional to the quantity of electricity which passes through the closed curve. The mathematical form of this law may be expressed as in equations (9)[†], which I here repeat, where α, β, γ are the rectangular components of magnetic intensity, and p, q, r are the rectangular components of steady electric currents,

$$\left.\begin{aligned} p&=\frac{1}{4\pi}\left(\frac{d\gamma}{dy}-\frac{d\beta}{dz}\right), \\ q&=\frac{1}{4\pi}\left(\frac{d}{dz}-\frac{d\gamma}{dx}\right), \\ r&=\frac{1}{4\pi}\left(\frac{d\beta}{dx}-\frac{d\alpha}{dy}\right). \end{aligned}\right\} \quad . \quad . \quad (9)$$

The same mathematical connexion is found between other sets of phenomena in physical science.

(1) If α, β, γ represent displacements, velocities, or forces, then p, q, r will be rotatory displacements, velocities of rotation, or moments of couples producing rotation, in the elementary portions of the mass.

(2) If α, β, γ represent rotatory displacements in a uniform and continuous substance, then p, q, r represent the *relative* linear displacement of a particle with respect to those in its immediate neighbourhood. See a paper by Prof. W. Thomson "On a Mechanical Representation of Electric, Magnetic, and Galvanic Forces," Camb. and Dublin Math. Journ. Jan. 1847.

(3) If α, β, γ represent the rotatory velocities of vortices whose centres are fixed, then p, q, r represent the velocities with which loose particles placed between them would be carried along. See the second part of this paper (Phil. Mag. April 1861).

It appears from all these instances that the connexion between magnetism and electricity has the same mathematical form as that between certain pairs of phenomena, of which one has a *linear* and the other a *rotatory* character. Professor Challis[‡]

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[*] Communicated by the Author.

[†] Phil. Mag. March 1861.

[‡] Phil. Mag. December 1860, January and February 1861.

conceives magnetism to consist in currents of a fluid whose direction corresponds with that of the lines of magnetic force; and electric currents, on this theory, are accompanied by, if not dependent on, a rotatory motion of the fluid about the axes of the current. Professor Helmholtz* has investigated the motion of an incompressible fluid, and has conceived lines drawn so as to correspond at every point with the instantaneous axis of rotation of the fluid there. He has pointed out that the lines of fluid motion are arranged according to the same laws with respect to the lines of rotation, as those by which the lines of magnetic force are arranged with respect to electric currents. On the other hand, in this paper I have regarded magnetism as a phenomenon of rotation, and electric currents as consisting of the actual translation of particles, thus assuming the inverse of the relation between the two sets of phenomena.

Now it seems natural to suppose that all the direct effects of any cause which is itself of a longitudinal character, must be themselves longitudinal, and that the direct effects of a rotatory cause must be themselves rotatory. A motion of translation along an axis cannot produce a rotation about that axis unless it meets with some special mechanism, like that of a screw, which connects a motion in a given direction along the axis with a rotation in a given direction round it; and a motion of rotation, though it may produce tension along the axis, cannot of itself produce a current in one direction along the axis rather than the other.

Electric currents are known to produce effects of transference in the direction of the current. They transfer the electrical state from one body to another, and they transfer the elements of electrolytes in opposite directions, but they do not† cause the plane of polarization of light to rotate when the light traverses the axis of the current.

On the other hand, the magnetic state is not characterized by any strictly longitudinal phenomenon. The north and south poles differ only in their names, and these names might be exchanged without altering the statement of any magnetic phenomenon; whereas the positive and negative poles of a battery are completely distinguished by the different elements of water which are evolved there. The magnetic state, however, is characterized by a well-marked rotatory phenomenon discovered by Faraday‡—the rotation of the plane of polarized light when transmitted along the lines of magnetic force.

When a transparent diamagnetic substance has a ray of plane-polarized light passed through it, and if lines of magnetic force

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* Crelle, *Journal*, vol. lv. (1858) p. 25.

† Faraday, 'Experimental Researches,' 951–954, and 2216–2220.

‡ Ibid., Series XIX.

are then produced in the substance by the action of a magnet or of an electric current, the plane of polarization of the transmitted light is found to be changed, and to be turned through an angle depending on the intensity of the magnetizing force within the substance.

The direction of this rotation in diamagnetic substances is the same as that in which positive electricity must circulate round the substance in order to produce the actual magnetizing force within it; or if we suppose the horizontal part of terrestrial magnetism to be the magnetizing force acting on the substance, the plane of polarization would be turned in the direction of the earth's true rotation, that is, from west upwards to east.

In paramagnetic substances, M. Verdet* has found that the plane of polarization is turned in the opposite direction, that is, in the direction in which negative electricity would flow if the magnetization were effected by a helix surrounding the substance.

In both cases the absolute direction of the rotation is the same, whether the light passes from north to south or from south to north,—a fact which distinguishes this phenomenon from the rotation produced by quartz, turpentine, &c., in which the absolute direction of rotation is reversed when that of the light is reversed. The rotation in the latter case, whether related to an axis, as in quartz, or not so related, as in fluids, indicates a relation between the direction of the ray and the direction of rotation, which is similar in its formal expression to that between the longitudinal and rotatory motions of a right-handed or a left-handed screw; and it indicates some property of the substance the mathematical form of which exhibits right-handed or left-handed relations, such as are known to appear in the external forms of crystals having these properties. In the magnetic rotation no such relation appears, but the direction of rotation is directly connected with that of the magnetic lines, in a way which seems to indicate that magnetism is really a phenomenon of rotation.

The transference of electrolytes in fixed directions by the electric current, and the rotation of polarized light in fixed directions by magnetic force, are the facts the consideration of which has induced me to regard magnetism as a phenomenon of rotation, and electric currents as phenomena of translation, instead of following out the analogy pointed out by Helmholtz, or adopting the theory propounded by Professor Challis.

The theory that electric currents are linear, and magnetic forces rotatory phenomena, agrees so far with that of Ampère and Weber; and the hypothesis that the magnetic rotations exist wherever magnetic force extends, that the centrifugal force of these rotations accounts for magnetic attractions, and that the inertia of

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* *Comptes Rendus*, vol. xliii. p. 529; vol. xliv. p. 1209.

the vortices accounts for induced currents, is supported by the opinion of Professor W. Thomson*. In fact the whole theory of molecular vortices developed in this paper has been suggested to me by observing the direction in which those investigators who study the action of media are looking for the explanation of electro-magnetic phenomena.

Professor Thomson has pointed out that the cause of the magnetic action on light must be a real rotation going on in the magnetic field. A *right-handed* circularly polarized ray of light is found to travel with a different velocity according as it passes from north to south, or from south to north, along a line of magnetic force. Now, whatever theory we adopt about the direction of vibrations in plane-polarized light, the geometrical arrangement of the parts of the medium during the passage of a right-handed circularly polarized ray is exactly the same whether the ray is moving north or south. The only difference is, that the particles describe their circles in opposite directions. Since, therefore, the *configuration* is the same in the two cases, the forces acting between particles must be the same in both, and the motions due to these forces must be equal in velocity if the medium was originally at rest; but if the medium be in a state of rotation, either as a whole or in molecular vortices, the circular vibrations of light may differ in velocity according as their direction is similar or contrary to that of the vortices.

We have now to investigate whether the hypothesis developed in this paper—that magnetic force is due to the centrifugal force of small vortices, and that these vortices consist of the same matter the vibrations of which constitute light—leads to any conclusions as to the effect of magnetism on polarized light. We suppose transverse vibrations to be transmitted through a magnetized medium. How will the propagation of these vibrations be affected by the circumstance that portions of that medium are in a state of rotation?

In the following investigation, I have found that the only effect which the rotation of the vortices will have on the light will be to make the plane of polarization rotate in the *same* direction as the vortices, through an angle proportional—

(A) to the thickness of the substance,
(B) to the resolved part of the magnetic force parallel to the ray,
(C) to the index of refraction of the ray,
(D) inversely to the square of the wave-length in air,
(E) to the *mean radius* of the vortices,
(F) to the capacity for magnetic induction.

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* See Nichol's *Cyclopædia*, art. "Magnetism, Dynamical Relations of," edition 1860; Proceedings of Royal Society, June 1856 and June 1861; and Phil. Mag. 1857.

A and B have been fully investigated by M. Verdet[*], who has shown that the rotation is strictly proportional to the thickness and to the magnetizing force, and that, when the ray is inclined to the magnetizing force, the rotation is as the cosine of that inclination. D has been supposed to give the true relation between the rotation of different rays; but it is probable that C must be taken into account in an accurate statement of the phenomena. The rotation varies, not exactly inversely as the square of the wave-length, but a little faster; so that for the highly refrangible rays the rotation is greater than that given by this law, but more nearly as the index of refraction divided by the square of the wave-length.

The relation (E) between the amount of rotation and the size of the vortices shows that different substances may differ in rotating power independently of any observable difference in other respects. We know nothing of the absolute size of the vortices; and on our hypothesis the optical phenomena are probably the only data for determining their relative size in different substances.

On our theory, the direction of the rotation of the plane of polarization depends on that of the mean moment of momenta, or *angular momentum*, of the molecular vortices; and since M. Verdet has discovered that magnetic substances have an effect on light opposite to that of diamagnetic substances, it follows that the molecular rotation must be opposite in the two classes of substances.

We can no longer, therefore, consider diamagnetic bodies as being those whose coefficient of magnetic induction is less than that of space empty of gross matter. We must admit the diamagnetic state to be the *opposite* of the paramagnetic; and that the vortices, or at least the influential majority of them, in diamagnetic substances, revolve in the direction in which positive electricity revolves in the magnetizing bobbin, while in paramagnetic substances they revolve in the opposite direction.

This result agrees so far with that part of the theory of M. Weber[†] which refers to the paramagnetic and diamagnetic conditions. M. Weber supposes the electricity in paramagnetic bodies to revolve the same way as the surrounding helix, while in diamagnetic bodies it revolves the opposite way. Now if we regard negative or resinous electricity as a substance the absence of which constitutes positive or vitreous electricity, the results will be those actually observed. This will be true independently of any other hypothesis than that of M. Weber about magnetism

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[*] *Annales de Chimie et de Physique*, sér. 3. vol. xli. p. 370; vol. xliii. p. 37.

[†] Taylor's 'Scientific Memoirs,' vol. v. p. 477.

and diamagnetism, and does not require us to admit either M. Weber's theory of the mutual action of electric particles in motion, or our theory of cells and cell-walls.

I am inclined to believe that iron differs from other substances in the manner of its action as well as in the intensity of its magnetism; and I think its behaviour may be explained on our hypothesis of molecular vortices, by supposing that the particles of the *iron itself* are set in rotation by the tangential action of the vortices, in an opposite direction to their own. These large heavy particles would thus be revolving exactly as we have supposed the infinitely small particles constituting electricity to revolve, but without being free like them to change their place and form currents.

The whole *energy* of rotation of the magnetized field would thus be greatly increased, as we know it to be; but the *angular momentum* of the iron particles would be opposite to that of the ætherial cells and immensely greater, so that the total angular momentum of the substance will be in the direction of rotation of the iron, or the reverse of that of the vortices. Since, however, the angular momentum depends on the absolute size of the revolving portions of the substance, it may depend on the state of aggregation or chemical arrangement of the elements, as well as on the ultimate nature of the components of the substance. Other phenomena in nature seem to lead to the conclusion that all substances are made up of a number of parts, finite in size, the particles composing these parts being themselves capable of internal motion.

Prop. XVIII.—To find the angular momentum of a vortex.

The angular momentum of any material system about an axis is the sum of the products of the mass, dm, of each particle multiplied by twice the area it describes about that axis in unit of time; or if A is the angular momentum about the axis of x,

$$\mathrm{A}=\Sigma dm\left(y\frac{dz}{dt}-z\frac{dy}{dt}\right).$$

As we do not know the distribution of density within the vortex, we shall determine the relation between the angular momentum and the energy of the vortex which was found in Prop. VI.

Since the time of revolution is the same throughout the vortex, the mean angular velocity ω will be uniform and $=\frac{\alpha}{r}$, where α is the velocity at the circumference, and r the radius. Then

$$\mathrm{A}=\Sigma dmr^2\omega,$$

and the energy

$$E=\tfrac{1}{2}\Sigma dmr^2\omega^2=\tfrac{1}{2}A\omega,$$

$$=\frac{1}{8\pi}\mu\alpha^2V \text{ by Prop. VI.*},$$

whence

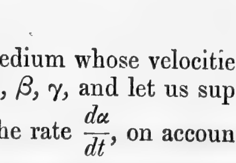
$$A=\frac{1}{4\pi}\mu r\alpha V \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (144)$$

for the axis of x, with similar expressions for the other axes, V being the volume, and r the radius of the vortex.

Prop. XIX.—To determine the conditions of undulatory motion in a medium containing vortices, the vibrations being perpendicular to the direction of propagation.

Let the waves be plane-waves propagated in the direction of z, and let the axis of x and y be taken in the directions of greatest and least elasticity in the plane xy. Let x and y represent the displacement parallel to these axes, which will be the same throughout the same wave-surface, and therefore we shall have x and y functions of z and t only.

Let X be the tangential stress on unit of area parallel to xy, tending to move the part next the origin in the direction of x.



Let Y be the corresponding tangential stress in the direction of y.

Let k_1 and k_2 be the coefficients of elasticity with respect to these two kinds of tangential stress; then, if the medium is at rest,

$$X=k_1\frac{dx}{dz}, \quad Y=k_2\frac{dy}{dz}.$$

Now let us suppose vortices in the medium whose velocities are represented as usual by the symbols α, β, γ, and let us suppose that the value of α is increasing at the rate $\frac{d\alpha}{dt}$, on account of the action of the tangential stresses alone, there being no electromotive force in the field. The angular momentum in the stratum whose area is unity, and thickness dz, is therefore increasing at the rate $\frac{1}{4\pi}\mu r\frac{d\alpha}{dt}dz$; and if the part of the force Y which produces this effect is Y', then the moment of Y' is $-Y'dz$, so that $Y'=-\frac{1}{4\pi}\mu r\frac{d\alpha}{dt}$.

The complete value of Y when the vortices are in a state of

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* Phil. Mag. April 1861.

varied motion is

$$Y=k_2\frac{dy}{dz}-\frac{1}{4\pi}\mu r\frac{d\alpha}{dt}.$$

Similarly,

$$\left.\begin{aligned}Y&=k_2\frac{dy}{dz}-\frac{1}{4\pi}\mu r\frac{d\alpha}{dt}.\\X&=k_1\frac{dx}{dz}+\frac{1}{4\pi}\mu r\frac{d\beta}{dt}.\end{aligned}\right\}\quad\ldots\ldots\quad(145)$$

The whole force acting upon a stratum whose thickness is dz and area unity, is $\frac{dX}{dz}dz$ in the direction of x, and $\frac{dY}{dz}dz$ in direction of y. The mass of the stratum is ρdz, so that we have as the equations of motion,

$$\left.\begin{aligned}\rho\frac{d^2x}{dt^2}&=\frac{dX}{dz}=k_1\frac{d^2x}{dz^2}+\frac{d}{dz}\frac{1}{4\pi}\mu r\frac{d\beta}{dt},\\\rho\frac{d^2y}{dt^2}&=\frac{dY}{dz}=k_2\frac{d^2y}{dz^2}-\frac{d}{dz}\frac{1}{4\pi}\mu r\frac{d\alpha}{dt}.\end{aligned}\right\}\quad.\quad(146)$$

Now the changes of velocity $\frac{d\alpha}{dt}$ and $\frac{d\beta}{dt}$ are produced by the motion of the medium containing the vortices, which distorts and twists every element of its mass; so that we must refer to Prop. X.* to determine these quantities in terms of the motion. We find there at equation (68),

$$\delta\alpha=\alpha\frac{d}{dx}\delta x+\beta\frac{d}{dy}\delta x+\gamma\frac{d}{dz}\delta x\ .\ .\ .\ (68).$$

Since δx and δy are functions of z and t only, we may write this equation

and in like manner,

$$\left.\begin{aligned}\frac{d\alpha}{dt}&=\gamma\frac{d^2x}{dz\,dt};\\\frac{d\beta}{dt}&=\gamma\frac{d^2y}{dz\,dt};\end{aligned}\right\}\quad\ldots\ldots\quad(147)$$

so that if we now put $k_1=a^2\rho$, $k_2=b^2\rho$, and $\frac{1}{4\pi}\frac{\mu r}{\rho}\gamma=c^2$, we may write the equations of motion

$$\left.\begin{aligned}\frac{d^2x}{dt^2}&=a^2\frac{d^2x}{dz^2}+c^2\frac{d^3y}{dz^2\,dt},\\\frac{d^2y}{dt^2}&=b^2\frac{d^2y}{dz^2}-c^2\frac{d^3x}{dz^2\,dt}.\end{aligned}\right\}\quad\ldots\ldots\quad(148)$$

These equations may be satisfied by the values

$$\left.\begin{aligned}x&=A\cos(nt-mz+\alpha),\\y&=B\sin(nt-mz+\alpha),\end{aligned}\right\}\quad\ldots\ldots\quad(149)$$

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* Phil. Mag. May 1861.

provided

$$\left.\begin{array}{l}(n^2-m^2a^2)\mathrm{A}=m^2nc^2\mathrm{B},\\ (n^2-m^2b^2)\mathrm{B}=m^2nc^2\mathrm{A}.\end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad (150)$$

and

Multiplying the last two equations together, we find

$$(n^2-m^2a^2)(n^2-m^2b^2)=m^4n^2c^4, \quad . \quad . \quad . \quad (151)$$

an equation quadratic with respect to m^2, the solution of which is

$$m^2=\frac{2n^2}{a^2+b^2\mp\sqrt{(a^2-b^2)^2+4n^2c^4}}. \quad . \quad . \quad (152)$$

These values of m^2 being put in the equations (150) will each give a ratio of A and B,

$$\frac{\mathrm{A}}{\mathrm{B}}=\frac{a^2-b^2\mp\sqrt{(a^2-b^2)^2+4n^2c^4}}{2nc^2},$$

which being substituted in equations (149), will satisfy the original equations (148). The most general undulation of such a medium is therefore compounded of two elliptic undulations of different eccentricities travelling with different velocities and rotating in opposite directions. The results may be more easily explained in the case in which $a=b$; then

$$m^2=\frac{n^2}{a^2\mp nc^2} \text{ and } \mathrm{A}=\mp\mathrm{B}. \quad . \quad . \quad (153)$$

Let us suppose that the value of A is unity for both vibrations, then we shall have

$$\left.\begin{array}{l}x=\quad\cos\left(nt-\dfrac{nz}{\sqrt{a^2-nc^2}}\right)+\cos\left(nt-\dfrac{nz}{\sqrt{a^2+nc^2}}\right),\\ y=-\sin\left(nt-\dfrac{nz}{a^2-nc^2}\right)+\sin\left(nt-\dfrac{nz}{\sqrt{a^2+nc^2}}\right).\end{array}\right\} (154)$$

The first terms of x and y represent a circular vibration in the negative direction, and the second term a circular vibration in the positive direction, the positive having the greatest velocity of propagation. Combining the terms, we may write

$$\left.\begin{array}{l}x=2\cos(nt-pz)\cos qz,\\ y=2\cos(nt-pz)\sin qz,\end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad (155)$$

where

$$\left.\begin{array}{l}p=\dfrac{n}{2\sqrt{a^2-nc^2}}+\dfrac{n}{2\sqrt{a^2+nc^2}},\\ q=\dfrac{n}{2\sqrt{a^2-nc^2}}-\dfrac{n}{2\sqrt{a^2+nc^2}}.\end{array}\right\} \quad . \quad . \quad (156)$$

and

These are the equations of an undulation consisting of a plane vibration whose periodic time is $\frac{2\pi}{n}$, and wave-length $\frac{2\pi}{p}=\lambda$, propagated in the direction of z with a velocity $\frac{n}{p}=v$, while the plane of the vibration revolves about the axis of z in the positive direction so as to complete a revolution when $z=\frac{2\pi}{q}$.

Now let us suppose c^2 small, then we may write

$$p=\frac{n}{a} \text{ and } q=\frac{n^2c^2}{2a^3}; \quad . \quad . \quad . \quad . \quad . \quad . \qquad (157)$$

and remembering that $c^2=\frac{1}{4\pi}\frac{r}{\rho}\mu\gamma$, we find

$$q=\frac{\pi}{2}\frac{r}{\rho}\frac{\mu\gamma}{\lambda^2 v}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (158)$$

Here r is the radius of the vortices, an unknown quantity. ρ is the density of the luminiferous medium in the body, which is also unknown; but if we adopt the theory of Fresnel, and make s the density in space devoid of gross matter, then

$$\rho=si^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (159)$$

where i is the index of refraction.

On the theory of MacCullagh and Neumann,

$$\rho=s \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (160)$$

in all bodies.

μ is the coefficient of magnetic induction, which is unity in empty space or in air.

γ is the velocity of the vortices at their circumference estimated in the ordinary units. Its value is unknown, but it is proportional to the intensity of the magnetic force.

Let Z be the magnetic intensity of the field, measured as in the case of terrestrial magnetism, then the intrinsic energy in air per unit of volume is

$$Z^2=\frac{1}{8\pi}, \quad \frac{\pi s\gamma^2}{8\pi},$$

where s is the density of the magnetic medium in air, which we have reason to believe the same as that of the luminiferous medium. We therefore put

$$\gamma=\frac{1}{\sqrt{\pi s}}Z. \quad . \quad . \quad . \quad . \quad . \quad . \qquad (161)$$

λ is the wave-length of the undulation in the substance. Now if Λ be the wave-length for the same ray in air, and i the index of refraction of that ray in the body,

$$\lambda=\frac{\Lambda}{i}. \quad \ldots \ldots \quad (162)$$

Also v, the velocity of light in the substance, is related to V, the velocity of light in air, by the equation

$$v=\frac{V}{i}. \quad \ldots \ldots \quad (163)$$

Hence if z be the thickness of the substance through which the ray passes, the angle through which the plane of polarization will be turned will be in degrees,

$$\theta=\frac{180^\circ}{\pi}qz; \quad \ldots \ldots \quad (164)$$

or, by what we have now calculated,

$$\theta=90^\circ\frac{1}{\sqrt{\pi}}.\frac{r}{s^{\frac{3}{2}}}\frac{\mu i Z z}{\Lambda^2 V}. \quad \ldots \quad (165)$$

In this expression all the quantities are known by experiment except r, the radius of the vortices in the body, and s, the density of the luminiferous medium in air.

The experiments of M. Verdet* supply all that is wanted except the determination of Z in absolute measure; and this would also be known for all his experiments, if the value of the galvanometer deflection for a semirotation of the testing bobbin in a known magnetic field, such as that due to terrestrial magnetism at Paris, were once for all determined.

XV. *On the Composition, Structure, and Formation of Beekite.*

By Arthur H. Church, *B.A. Oxon, F.C.S.*†

[With a Plate.]

THERE occurs in the triassic red conglomerate of Torbay and its neighbourhood, an interesting siliceous substance (generally considered to be a variety of hornstone), which offers a problem not only to the geologist and palæontologist, but also to the chemist. The Beekite is, in fact, not a mineral merely, but a fossil which has been more or less completely mineralized, the mineralization having, however, been effected in a way not very easy to understand. In the present paper, after having quoted some authorities in order to show the geological character and position of Beekites, I shall endeavour to throw some light,

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* *Annales de Chimie et de Physique*, sér. 3. vol. xli. p. 370.

† Communicated by the Author.

by means of evidence deduced from experiments and observations, on the chemical and physical relations of these bodies.

In addition to the localities near Torbay, it has been stated that Beekites occur near Lidcot in Somersetshire; also in the north of Scotland; while foreign localities for them have likewise been mentioned. Their appearance varies so much that it is scarcely possible to give such a description of their general form as shall include all the varieties; yet figs. 1–4 and 8–13 in Plate III. may perhaps indicate some of their chief characteristics. We shall have to recur to several features shown in these figures further on. A specimen of Beekite from Vallecas near Madrid, in the British Museum, presents a very close resemblance to some of the more common Torbay forms, but at the same time is remarkable for unusual translucency.

"The Beekite is not exactly a fossil, but an incrustation of chalcedony upon a nucleus of coral, and occasionally, but rarely, upon fragments of limestone. The chalcedony is deposited in concentric circles around minute tubercles. These are very sharply defined in the Beekites that are freshly dug out of the cliff above high-water mark; but if picked up on the beach, or taken from the cliff where tide-washed, they are smoother and have lost much of their peculiar character. . . . . Their form is irregular; most commonly they are more or less round. They take their shape from the fragments of coral upon which the chalcedony has been deposited, and which having become more or less decomposed and disintegrated, the chalcedony forms a kind of shell or case enclosing its remains. The coral within is found in various stages of decomposition,—in some specimens filling the interior, in others nearly so, allowing of so much movement that when shaken the contents may be heard to rattle; in others the coral is so completely broken down that only a powder, consisting of the carbonate of lime and some brown particles of organic matter, remains. The interior of the siliceous shell has often the markings of the original coral."

In these remarks, which I quote from a letter by Mr. Kesteven*, we have little more than a repetition of parts of Mr. Pengelly's paper on Beekites read before the British Association in 1856†. The description given of these fossils, and the theories which have been started to account for their present state, differ but little.

Beekites, unlike the pebbles of the conglomerate in which they occur, do not exhibit signs of having been water-worn; it is allowed consequently that their organic bases must have been silicified *in situ*. So say Messrs. Pengelly and Kesteven. But

* Athenæum, August 27, 1859.

† British Association, 1856, p. 74.

by what agency was this change effected? Mr. Kesteven extends the suggestion made in the Report of the Commission of the French Academy of Sciences on Water-glass, concerning the origin of flints, agates, petrified woods, &c., and applies it in the case of Beekites. In these instances a slow decomposition of an alkaline silicate by means of carbonic acid is supposed. Mr. Kesteven thus describes his theory:—"Fragments of coral, broken by the waves, and deposited with the beach now constituting rocks of red conglomerate, would retain a certain portion of chlorides, while their decomposition would liberate the carbonic acid which would separate the alkaline constituent of siliceous springs, and cause the deposition of silica upon the nucleus of coral. That a similar siliceous deposition is not found upon the surrounding deposits is satisfactorily explained by the non-liberation of carbonic acid from the pebbles, into the composition of which its elements did not enter. This view is strengthened by the fact of the non-silicification of the nucleus itself, the silicate being arrested on its surface by the escape of carbonic acid. Furthermore, where chalcedony presenting the Beekite characters has been found upon stone, it has been limestone, from which it is possible carbonic acid may have been disengaged at the time of deposition. The characters of chalcedony, as presented in Beekites, moreover, approach very closely to those of the siliceous incrustations of the Geyser springs in Iceland."

Mr. Pengelly's view is as follows. He says, "It seems probable that after the formation of the triassic conglomerate, some of the calcareous pebbles in it underwent decomposition, that water holding chalcedony in solution, and passing through the rock, deposited the chalcedony on the nucleus: the nucleus in some cases continued to decompose, by which it was wholly or partially detached from its envelope, and not unfrequently reduced to dust. Suppose the decomposition to have commenced at various points or centres on the surface of the pebble, the chalcedony deposited at these points would form central tubercles; let the decaying process extend from and around these centres, the chalcedony deposited around these centres would form a ring,"—and so on.

Mr. Pengelly states in the paper just referred to, that "the interior of the Beekite is calcareous," and that "the nucleus appears to be always a fossil, and is either a sponge, a coral, a shell, or a group of shells. . . . . The organic structure is frequently preserved on the inner or concave surface of the enveloping crust, even when the nucleus is reduced to powder. Occasionally organic traces are discernible on the exterior surface of the chalcedony; but such cases are not frequent. Some of the nuclei are slightly siliceous, but in no case more so than ordinary limestones are."

A correspondent at Exeter first directed my attention to these singular fossils, and not only gave me a large number of specimens, but much valuable information and many useful hints. The following interesting remarks are extracted from a letter lately received from this gentleman: to some of his remarks I shall have occasion to refer when detailing my own theory and experiments.

"The chief locality for them in this county is Torbay: I have heard that they are found at the Ness near Shaldon, and I have a few from North Tawton. Beekites proper are confined to the New Red Sandstone, although a structure very near akin to it is found in the Mountain Limestone (I have one specimen); the same may be said of the Lias and the Greensand, but there the rings (concentric ridges) are not so large. I have often thought whether this peculiar arrangement is not due to the displacement of carbonate of lime by silex which takes place in fossilization, as the shells must originally have been carbonate of lime. The experiment I tried with hydrofluoric acid was on mammillated or bubble chalcedony, to see if there were any connexion between this structure and the Beekite. I thought the latter was the former with the tops of the mammillations either dissolved or rubbed off; the appearance of the chalcedony after the application of the acid rather favoured that supposition. I have seen Beekites with the mammillated structure on a part of them, and the Beekite structure on the other, passing gradually from the one to the other. The mammillated structure I speak of is found abundantly in the interior of the flints at Haldon, and perhaps in the Chalk also. I shall send you the Beekite which effervesces; you will find that as the carbonate of lime is dissolved, the rings more and more appear. I shall also send a few specimens of shells from the Greensand of Haldon. With your glass you will see that they are entirely made up of rings, such as are shown in the Beekites on a larger scale."

Both Mr. Pengelly and Mr. Kesteven speak of the fragments of coral and of limestone, to which they refer as the basis of Beekite, as undergoing decomposition spontaneously; and it seems that most other writers who have attempted an explanation of the phenomena presented by these singular fossils, have made the same assumption. We meet, for instance, with such explanations as the following:—"It seems probable that, after the formation of the conglomerate beds, many of the calcareous pebbles in them continued to decompose at the surface, and thus allowed water, holding siliceous matter in solution, to pass through the rock; and that, in passing, it deposited the chalcedony on the diminished pebble, which in most cases continued to decompose." Of what nature is the decomposition of limestone which is here spoken of? and how did it originate? or did it occur at all? The whole problem before us resolves itself into two parts, which it will be better to examine separately. We have to account for—

I. The chemical composition of Beekite; and
II. Its physical appearance.

I. The chemical constituents of Beekite vary less both in nature and proportion than the outward appearance of different specimens would lead one to infer. I have been unable to find more than one recorded analysis; it is given by Mr. Kesteven in the letter to which I have already referred, and is as follows. A Beekite weighing 1040 grains contained, according to his statement—

	grains.	per cent.
Carbonate of lime	470	45·20
Chalcedony	540	51·93
Sesquioxide of iron and alumina	5	·46
Carbonaceous matter, residue of animal matter of coral . .	25	2·41
	1040	100·00

These results, which I presume are to be regarded as approximative only, must not be taken as indicative of the general composition of Beekite; for I have examined qualitatively or quantitatively nearly twenty specimens, and in only two cases have I found more than 3 per cent. of carbonate of lime: even where the appearance of the mass most forcibly recalled the original coral, the proportion was no larger. In the two cases where carbonate of lime occurred in considerable quantity, the Beekite was singularly compact. Parts of one specimen did not show the characteristic concentric ridges of chalcedony until the carbonate of lime in which they had been imbedded was dissolved away by means of an acid; while the other specimen, which is given in Pl. III. figs. 10, 11, 12, although showing, when cut and polished (fig. 10), or when in thin section (figs. 11, 12), a distinct coral throughout, was enveloped in a siliceous coat, but possessed a calcareous nucleus. In this case the portions of the coral towards the exterior of the mass had been replaced by silica, so far as the cell-walls of the polypidoms were concerned, the cells being partly filled with carbonate of lime, the interior portions remaining calcareous throughout. Occasionally a Beekite is found containing a purely inorganic nucleus of limestone, but in a large number of specimens there is actually no carbonate of lime whatever, all the lime present being in the form of silicate.

In consequence of their hardness, and the necessity of fusing them with carbonate of soda, Beekites, previous to analysis, should be disintegrated by igniting them and throwing them into cold water. The fragments are to be ground carefully, boiled with concentrated nitric acid (this solution being added to the original liquid for further examination), and the residue fused with carbonate of soda and treated in the usual way. It would be tedious to describe the various analytical processes found necessary in all their details; the following Table contains the main results. The spectroscope was used with success for the detection of minute traces of several elements present in too small a proportion to be recognizable by reagents.

Specimens.	I.	II.	III.	IV.	V.	VI.	VII.
Silica	90·707	93·037	93·115	92·7	92·707	91·96	92·119
Lime	·44	2·26	2·76	3·031	2·7	1·35	3·03
Alumina & phosphate of alumina	·075	trace	·002	·06	·012	trace	trace
Sesquioxide of iron	5·09	1·01	·097	·07	·78	2·94	2·16
Magnesia	·002	·014	·007	trace	trace	trace	·021
Soda	·03	·075	·019	·017	trace	trace	trace
Potassa	trace	trace	trace	trace	trace	trace	trace
Lithia	...	...	...	...	...	...	trace
Water	·7	1·072	1·56	·96	1·76	2·1	·17
Carbonic acid and organic matter	2·27	1·53	2·44	2·51	1·97	1·64	2·29
Chlorine (probably combined with the sodium)	·04	·091	trace	trace	trace	trace	trace
Loss	·656	·911	...	·652	·071	·01	·21
	100	100	100	100	100	100	100

Specimen II. of the above synopsis contained also a minute trace of iodine, while lithia was detected in another specimen not further analysed. The compact specimen of Beekite figured in Pl. III. fig. 10, and to which I have already alluded, had lost after removal of the carbonate of lime by an acid, about $\frac{1}{5}$th of its weight. The carbonate of lime was situated almost altogether in the central portion of the specimen; the exterior did not show any signs of its presence when touched with an acid.

In the preceding Table I have not attempted to combine the various bases and acids together. The greater part of the lime existed, I believe, as silicate, and the iron and traces of other bases were probably in a similar state. In a few instances a portion of carbonate of lime was present, and occasionally a combination of lime with some organic substance. The loss partly represents the sulphuric acid and other undetermined constituents of the Beekites.

I have not included in the Table of Analyses the examination of several Beekites in which no lime whatever was found. Their composition scarcely differed from that of flint, except in the larger per-centage of iron which they contained.

The chemical causes concerned in the formation of Beekite appear to me of peculiar interest. It is reasonable to believe that the removal of carbonate of lime from a shell or a coral, and its replacement by silica, has been effected by the agency of water holding carbonic acid and silica in solution together. Such solutions occur frequently in nature, not only in springs like those in the neighbourhood of the Iceland Geysers, but in several places in Great Britain,—silica being, indeed, an almost invariable constituent of common waters. We may argue, then, that such a solution filtering through the débris of organic forms, not only removes their carbonate of lime by virtue of the solvent power of its carbonic acid, but deposits some silica instead. This supposition has been tested and confirmed by experiment. A fragment of a recent coral was fitted into the neck of a funnel, and a 1 per cent. solution of silica (prepared from silicate of potash by Graham's dialytic method), containing a little carbonic acid gas, was allowed to drop slowly on the coral and filter through: after a time the liquid ceased to pass. The filtrate contained *no silica*, but *much carbonate of lime*. The fragment of coral had lost nearly all its lime, but had retained its structure in great measure; it was, however, covered with a thick film of gelatinous silica, and was very soft. In such a reaction as this, it is not unlikely that a small portion of lime would be retained as silicate. Where the process of silicification has gone on to its completion, we have a tolerably exact reproduction in silica of the original organism, the result being, in the case of corals, sponges, &c., a light hard porous mass, occasionally hollow, of chalcedony. But where the process has been arrested by the stoppage of the flow of the siliceous solution, the central portions of the Beekite have retained nearly their original composition as well as structure; and in some rare instances it would seem that a subsequent deposition of carbonate of lime, in the spaces not occupied by silica, had taken place.

II. Of the physical aspect of Beekite, the quotations made in the earlier part of this paper, taken in connexion with the illustrations in Plate III., will give some idea. The mammillations and concentric ridges (figs. 1–4, 9 & 13), though frequent, are not invariably present. Besides the organic structure (shown more or less distinctly in figs. 2, 9, & 10–12), Beekites often display one or more lines and furrows on their surface, quite independent of the mammillations. These lines indicate for the most part certain planes of cleavage or fracture, as shown in the upper and lower sides of fig. 2 and the light side of fig. 3.

These lines are referable rather to the effects of pressure than to an organic origin,—the frequent angularity of the specimens pointing in the same direction.

In a fragment of a silicified Pecten from the Greensand of Haldon, Devon, the elevated ridges, characteristic of the shell, are preserved intact, notwithstanding the complete displacement of the carbonate of lime by silica, and the assumption by the silica of that arrangement in concentric rings characteristic of most Beekites. In the present specimen of shell, some of the systems of rings are partly situate in a furrow of the exterior surface, bend upwards, and follow the curve of a ridge, and then dip into another furrow: the inner aspect of the shell being level, shows no corresponding contortions of the systems of concentric ridges. In like manner, in the Beekite proper, silica *displaces* rather than *increases* the original substance,—for the organic structure is generally traceable in every part of the tubercles and ridges, even to their summits,—being in fact more conspicuous on the exterior surface of most of the hollow specimens than on their interior.

The tendency to deposition in a circular form, though not peculiar to this substance, is seen in many other varieties of silica—stalagmitic quartz or quartz-sinter, for instance, such as that from the Geysers of Iceland, and the hot springs of Luzon in the Philippine Isles. A specimen from the latter place is represented in figs. 5 & 6. Eyed agate (fig. 7) shows the same arrangement. Float-stone from Menil Montant, near Paris, when examined with a lens, often displays minute beads and concentric ridges; while menilite, or liver-opal from the same locality, presents an appearance very closely resembling that of the Beekite No. 9.

The annexed letter from Dr. Gladstone, commenting as it does on both the physical and chemical questions under discussion, may not inappropriately conclude the present paper:—

"My dear Mr. Church,—Your Beekites seem to me very interesting, not only in a chemical and geological, but also in a physical point of view, as affording a remarkable illustration of the tendency of certain bodies to assume a globular form during deposition. I say 'deposition,' because it is evident that the Beekites are not fossils owing their mammillated appearance to the organic form which has been silicified, but are rather deposits on some substratum, which may be a coralline, a sponge, a shell, or even a piece of stone. Yet this deposition seems always accompanied by the replacement of part, or the whole, of the carbonate of lime by silica.

"Many of the concentric globular deposits in your specimens bear a striking resemblance to the globular deposits of carbonate of lime to which Mr. Rainey has recently drawn attention as occurring in animal tissues during the formation of shell*, bone, &c., and which he has artificially produced by the slow formation of carbonate of lime in the presence of gum or albumen. In all probability the physical forces concerned in the building up of the calcareous and the siliceous globes are the same. In many of the specimens of Beekite, the concentric masses look as though they had been more perfect at one time, but the outer portion had been disintegrated before the mass had become thoroughly hard: at least they suggest that idea to me; and if true, that forms another link of analogy between them and the phenomena described by Rainey.

"It seems at first sight difficult to understand how, if the silicification of, say a coral, begins on the outside all round, it can advance to the interiŏr, and how the carbonate of lime within can be removed; but it must be remembered that the silica, when first deposited, was in the gelatinous condition, and permeable to salts in solution, as Prof. Graham has shown in the case of other of those substances which he designates 'colloids.' It is only gradually that such a gelatinous globular mass would pass into the rigid flint which we now handle. In the mean time, too, it would be subject to all those changes of form, or that tendency to cleavage, which pressure might superinduce.

"It may be worthy of experiment to see whether the pellicle formed by the gradual gelatinization of silica in solution ever assumes a form at all corresponding with that of the Beekites.

"I remain, yours ever truly,
"J. H. Gladstone."

"28 Pembridge Gardens,
Dec. 14, 1861."

XVI. *On the Specific Heat of some Simple Bodies.*
By M. V. Regnault[†].

[With a Plate.]

I PURPOSE bringing together in the present paper the experiments made in the last few years on the determination of the specific heat of some simple bodies which I have hitherto not been able to obtain in sufficient quantity or of adequate purity. The methods which I have used, differ little from those which served for my former investigations (*Annales de Chimie et de Physique*, 2nd series, vol. lxxiii. p. 20); I have, however, advan-

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[*] The structure of pearls, when compared with these globular bodies, leads one to believe in their original identity, only the former are detached.—A. H. C.

[†] Translated from the *Annales de Chimie et de Physique*, September 1861, by Dr. E. Atkinson.

tageously modified the bath which I employed for raising the body to a temperature near 100°, inasmuch as I have substituted for the charcoal fire by which the water in a boiler was kept boiling, a gas-lamp which effects the same purpose, and does not require the presence of the operator.

The substance submitted to experiment is usually placed in a brass wire-gauze basket M, provided on the inside with a cylinder of brass-foil, in which is fitted the bulb of the thermometer T which marks the temperature of the substance. This basket is suspended by a silk thread, which traverses a hollow metallic stopper R. The thermometer T is inserted in a stopper to such a depth that the division 100 degrees only projects above the bath by about a centimetre.

The hot bath consists of three concentric envelopes. The internal cylinder A, in which is arranged the basket and the thermometer, is soldered hermetically both at the top and the bottom to the external envelope. Its upper orifice is closed by the stopper R; its lower orifice by a slide *m*, so long as the basket with its contents is being heated. Between the external envelope C and the cylinder A, there is an intermediate envelope fixed to the superior lid, and reaching below to the conical part of the external envelope. The bath is supported by a wooden stand D D P P′, which acts besides as a screen in preventing the radiation of heat on the calorimeter H, when this latter occupies the place indicated in the figure (Pl. II. fig. 1), at the instant of immersing therein the heated basket.

The boiler V communicates with the bath by a tube *a b*, which traverses the intermediate envelope, and by which steam reaches the annular space B B round the internal cylinder A. This steam escapes by the apertures *o o* on the side opposite that by which it enters. The steam descends by the external annular space C C, and the condensed water re-enters the boiler by the tube *c d*. The steam which has retained its gaseous state passes from the bath by the tube *e f* into the larger tube G, terminated above by a narrower tube *h i*, which is surrounded by cold water continually renewed. The steam is completely condensed in this refrigerator, and returns to the boiler by the tube *k p*: its temperature on so doing is very near 100°, for at G it traverses the steam which is constantly coming from the bath.

Ebullition is produced in the boiler V by means of a gas-lamp W; it goes on continuously without necessitating the presence of the operator; the same quantity of water serves for an indefinite period, for it always returns without loss to the boiler. This new arrangement renders the operation very simple, and allows the operator to proceed with other work.

If the water in the boiler V is replaced by other volatile liquids, stationary temperatures, very different from 100° may be obtained in the bath. With bisulphide of carbon the temperature is 46°; with chloroform 60°; with alcohol 78°; with oil of turpentine it is 157°, &c. It is even easy to obtain in the bath a stationary temperature perfectly fixed beforehand; for this purpose it is sufficient to place in the boiler V a liquid whose temperature under the ordinary atmospheric pressure is very little different from that desired in the bath, and then to boil this liquid under a pressure either greater or less than that of the atmosphere, so that the thermometer exactly indicates the desired temperature. In this case the tube $h\,i$ is connected by means of a leaden pipe, with an air-reservoir, the pressure of which may be varied at pleasure by a suction or forcing-pump.

I shall not revert to the method of conducting these experiments; it has already been sufficiently described in my previous memoirs.

When it is desired to determine the specific heat of a body which liquefies or even softens much at temperatures slightly above that of the surrounding air, it cannot be heated in the bath, and recourse must be had to the inverse method, which consists in cooling the body in a cooling mixture, and determining the fall of temperature which it produces by its immersion in the calorimeter. I have described (*Ann. de Chim. et de Phys.* vol. xlvi. p. 270*) the apparatus I used for this purpose. This apparatus I have replaced by another, easier of manipulation, and by which the temperature can be better regulated. The cold is produced by the evaporation of a very volatile liquid in a continuous current of air, which can be regulated at will: fig. 2, Plate II. represents its vertical section. In a central tube A, like that in fig. 1, and provided at both ends with the same stoppers, is placed the basket M containing the substance whose specific heat is to be determined. It is surrounded by a second tube B of the form shown in the figure, and which is hermetically soldered above and below to the tube A. A third tube, C, surrounds the two others, and forms a protection to the tube B against immediate contact with the surrounding air, and prevents the deposition of dew.

On the upper circular base of the tube B are two tubulures; on one of which is soldered the trifurcate tube $b\,a\,d\,c$; through the other a bent tube $e\,f\,g$ passes, provided with a stopcock r, and which descends to the bottom of the tube B. The whole system is placed on a support provided with screws, like that in fig. 1.

The volatile liquid, ether or bisulphide of carbon, is poured into the tube B through the orifice d, which is then closed. In order that the level of the liquid may not sink below $h\,i$ in con-

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* Phil. Mag. vol. xii. p. 498.

sequence of too much evaporation during a lengthened experiment, the tube B is somewhat larger towards the upper part.

A current of air is driven through the tube *efg*, either by means of an aspirator adapted to the tubulure *c*, or by a force-pump, or by a pair of bellows applied directly to the tube *efg*. As I have at my disposal large reservoirs in which a force-pump, moved by a machine, compresses the air to several atmospheres in a very short tine, the operation is very simple; a large reservoir of compressed air communicates with the tube *efg*. By regulating the stopcock *r*, a more or less rapid current of air traverses the ether of the tube B, and escapes saturated with vapour through the tubulure *ac*: the temperature rapidly sinks in consequence of evaporation. When the ether is near the desired temperature, which is seen by a thermometer whose bulb dips in the ether, the stopcock is turned so as to stop the cooling, and with a little practice the temperature may be maintained stationary as long as is desired. The thermometer T, whose bulb is in the basket M, is necessarily behind that which is immersed in the ether; but the two thermometers gradually approximate when the current of air is suitably regulated. The basket M is only immersed in the calorimeter when equilibrium is almost established.

The temperatures obtained by this apparatus are not so low as those obtained by means of freezing-mixtures of ice and crystallized chloride of calcium: thus, when the external temperature is +20°, it is difficult to keep the ether stationary at a lower temperature than −12° C.; under the same circumstances the temperature only sinks with bisulphide of carbon to −8°: but, from the readiness with which low temperatures are kept stationary for a long time, more accuracy is obtained in the determinations.

By a precisely similar arrangement, the temperature of a liquid can be gradually lowered and kept stationary at any desired point. I have often used it to determine the point at which a liquid solidifies when this solidification takes place between −15° and +10°. The inside tube A is closed at the bottom (fig. 3); the liquid is placed in it along with a thermometer which indicates the temperature and serves as an agitator. By means of a current of air, the temperature of the ether is lowered gradually and as slowly as required; the thermometer immersed in the liquid sinks in a like degree to the point at which solidification begins; its temperature then becomes stationary. To invert the process, the current of air is diminished or completely stopped. The temperature of the ether then rises; the liquid should be continually agitated with the thermometer, and it should be observed whether, when it commences to rise, the solidified part has entirely resumed the liquid state.

If for special experiments it is desired to obtain temperatures lower than −40°, some liquid ammonia, which is prepared easily and in abundance by an apparatus which I have described (*Mémoires de l'Académie des Sciences*, vol. xxvi.), should be placed on the annular space. The air-current passing through the liquid ammonia with sufficient velocity, lowers its temperature to about −80°*. By regulating the current, the temperature may be kept stationary at any point between −40° and −80°; the great latent heat of evaporation of ammonia renders it very easy to do this.

Magnesium.

The specific heat of magnesium has not hitherto been determined. I used for this experiment a beautiful specimen of magnesium, lent by M. Rousseau. The metal was prepared by decomposing chloride of magnesium by sodium at a high temperature; a single regulus was formed with a considerable depression in the centre. The regulus was wrapped in several pieces of lead-foil to preserve it from contact with the air. Magnesium can be kept for a long time in dry air without undergoing any perceptible alteration.

M	$91^{gr}\cdot870$	$92^{gr}\cdot22$
Lead . . .	$49^{gr}\cdot410$	$29^{gr}\cdot69$
T	97°·63	98°·28
θ'	23°·81	22°·99
$\Delta\theta'$	3°·8289	3°·9203
A	$466^{gr}\cdot69$	$466^{gr}\cdot69$
C	$0^{gr}\cdot2466$	$0^{gr}\cdot2533$

Mean of C . . . . . $0^{gr}\cdot2499$.

This specific heat, multiplied by 150, the number now usually taken as the atomic weight of magnesium, gives the product 37·49. Magnesium is consequently included in the law of specific heats of simple bodies, and its specific heat confirms the accuracy of the formula, MgO, given to magnesia.

Lithium.

In the memoir which I published (*Ann. de Chim. et de Phys.* 3rd series, vol. xlvi. p. 276), I gave the specific heat of chloride of lithium, and, reasoning on this datum, I endeavoured to show that the atomic weight which has been adopted for lithium, 80·37, ought to be halved, that is, reduced to 40·18. The formula of lithia would then be Li^2O, like those of potash and soda, which, according to my experiments, should be written K^2O and Na^2O. But for a convincing proof in the case of lithia, it was

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* MM. Loir and Drion have recently announced that by the evaporation of ammonia *in vacuo* a cold of −90° is obtained (Phil. Mag. vol. xxi. p. 495).

desirable to determine the specific heat of metallic lithium. M. Debray provided me with an opportunity, by placing at my disposal a small quantity of lithium which he had carefully prepared by Bunsen's method. It consisted of a large globule weighing about a gramme, and of 12 grains of the size of a small pea. I endeavoured in vain to fuse these small grains into a single globule; but the layer of oxide with which they were coated prevented them from completely welding. I preferred to operate solely with the large globule, which in the cold could be readily worked by the hammer, and presented a very lustrous surface.

To preserve the metal from contact with the air during the experiment, it was hermetically enclosed in a leaden box, represented in fig. 4. The globule of lithium was first formed into a cylinder by pressure in a lapidary's steel mortar; while at the same time a hollow leaden cylinder *a b c d* had been made, in which the plunger of the mortar fitted, as well as a leaden piston P. The cylinder of lithium L having been placed in the hollow cylinder of lead, the piston P was inserted above it, and briskly struck with a hammer in order to enclose the lithium completely.

The weight of the lithium being known, as well as that of the leaden vessel, the calorific capacity of the whole arrangement was determined by experiment; and as that of the lead is known, it is easy to calculate the specific heat of lithium. For the sake of greater accuracy, before placing the lithium in the leaden vessel, several experiments were made with this vessel provided with its piston: these determinations gave exactly the calorific capacity found by calculation for the weight of lead in the vessel. The following are the results of three experiments made with the lead and lithium together:—

M	0$^{\text{gr}}$·945	0$^{\text{gr}}$·945	0$^{\text{gr}}$·945
Lead . . .	109$^{\text{gr}}$·985	109$^{\text{gr}}$·985	109$^{\text{gr}}$·985
T	99°·27	99°·57	99°·77
θ'	26°·56	26°·89	26°·83
$\Delta\theta'$ . . .	2°·0856	2°·0555	2°·0588
A	151$^{\text{gr}}$·55	151$^{\text{gr}}$·55	151$^{\text{gr}}$·55
C	0·9412	0·9405	0·9407

Mean . . . . . 0·9408.

The calorific capacity of lithium is very considerable, being almost equal to that of water. Taking its atomic weight at 80·37, the number assigned by chemists to lithium, the product of its specific heat into its atomic weight would be 75·61. But assuming that the atomic weight is 40·18, which gives for lithia the formula Li^2O, we obtain the product 37·80, and lithium then completely satisfies the law of the specific heats of simple bodies.

METALS WHICH ACCOMPANY PLATINUM.

I have had frequent occasion to determine the specific heat of some of the metals which accompany platinum (*Ann. de Chim. et de Phys.* 2nd series, vol. lxxiii.; 3rd series, vol. xlvi.), but I had doubts as to the purity of several of the specimens used. M. Chapuis lent me, in June 1857, several specimens of rhodium, osmium, and iridium, which he had prepared with the greatest care by the methods described by him in the following Note:—

1. The osmium was obtained by roasting osmium-iridium. Osmic acid, condensed in a solution of caustic potash, was converted into osmite of potash by alcohol, and then washed for a long time with a solution of chloride of ammonium. The double chloride of osmium and ammonium was heated in a current of hydrogen, and the spongy osmium thus formed united by compression into a single ingot.

2. The iridium was obtained by heating with nitre the residues of platinum-mineral previously freed from osmium. The product, washed with water, was treated with aqua regia, and the liquors precipitated with chloride of ammonium. The double salt was calcined in a platinum crucible, the temperature being slowly raised. The metallic mass was again washed with a solution of sal-ammoniac, and then heated in the muffle; a considerable quantity of osmium was given off. After these operations it was found that the iridium still contained perceptible quantities of platinum, palladium, and gold. It was again treated with weak aqua regia; and as the presence of foreign metals was still apprehended, it was again fused with nitre, and the oxide thus obtained treated with aqua regia. The evaporated liquors gave well-defined crystals of the double chloride of iridium and potassium: the double chloride was heated in a crucible with carbonate of soda, and the iridium thus obtained was compressed into a single cylinder by percussion.

3. The rhodium was extracted from the residues of the preparation of iridium. These residues, exhausted by the treatment with aqua regia, were mixed with fused salt and then heated to redness in a current of dry chlorine. The substance was then treated with boiling water, and the solution gave, on evaporation, beautiful octahedral crystals of the double chloride of rhodium and sodium. These crystals were redissolved in hot water, and sal-ammoniac added; on cooling, needle-shaped crystals of the ammoniacal double chloride were deposited. This substance, heated in a muffle, gave metallic rhodium. As some doubt still remained of its purity, it was again treated with aqua regia, the metallic residue again fused with common salt, and the mixture heated in a current of dry chlorine. The substance was redis-

solved in boiling water; and the solution gave, by boiling, the crystallized chloride of rhodium and sodium. From this salt, treated as before, metallic rhodium was obtained, which was compacted by percussion.

Osmium.

The osmium consisted of a single cylinder strongly compacted by hammering. I only made one determination, the results of which are as follows:—

M	204^{gr}·59
p	1^{gr}·5048
T	97°·77
θ'	18°·71
$\Delta\theta'$	1°·3338
A	466·69

The specific heat of osmium, as deduced from this experiment, is C=0·03113. This number scarcely differs from the number 0·03063, which I obtained for osmium prepared by M. Fremy (*Ann. de Chim. et de Phys.* 3rd series, vol. xlvi. p. 162). The product of the equivalent of osmium, 1244·2, into its specific heat, 0·03063, is 38·11; osmium therefore completely follows the law of specific heats of simple bodies, and the formula of osmic acid is Os O^5, as is generally admitted.

Rhodium.

The rhodium on which I experimented in 1855 was prepared by M. Chapuis, but contained small quantities of one of the platinum-metals with high equivalents. The specific heat of rhodium was accordingly found somewhat too low, 0·05408; for multiplied by its equivalent 652·1, it gives the product 35·26. The new specimen proceeds also from M. Chapuis, who applied himself to purify it as completely as possible. The metal formed a single cylinder strongly compacted by hammering. The following are the results of the experiments:—

M	200^{gr}·31	200^{gr}·31
p	1^{gr}·5048	1^{gr}·5048
T	98°·15	97°·50
θ'	19°·64	19°·97
$\Delta\theta'$	2°·1138	2°·0917
A	466^{gr}·69	466^{gr}·69
C	0·5522	0·5532

Mean . . . 0·05527.

The product of this specific heat by the equivalent 652·1 is 36·04. The product is somewhat too low, owing probably to the presence of a little iridium.

I made some determinations with another specimen of rhodium, also lent by M. Chapuis; it was a medal, remarkable for its sharpness, and had been struck at the Paris Mint. The medal was suspended during the experiment by a very fine silk thread, the influence of which might be neglected:—

M . . .	47^{gr}·107	47^{gr}·107
T . . .	96°·77	97°·75
θ' . . .	11°·07	9°·80
$\Delta\theta'$ . . .	1°·5498	1°·6791
A . . .	149^{gr}·576	149^{gr}·73
C . . .	0·05742	0·05865

Mean . . . 0·05803

The product of the specific heat 0·05803 by the equivalent 652·1 is 37·84.

Iridium.

I have made several determinations of the specific heat of iridium at different times on specimens which I have succeeded in procuring. In 1839 I worked upon a very large piece of consolidated iridium which had been lent by M. Meyendorff. I found (*Ann. de Chim. et de Phys.* 2nd series, vol. lxxiii. p. 53) for the specific heat 0·03683; this number, multiplied by the equivalent 1233·2, gives 45·43. In 1855 (*Ann. de Chim. et de Phys.* 3rd series, vol. xlvi. p. 263) M. Chapuis lent me another quantity of iridium which formed half a disc, strongly compacted by percussion; I found for this specimen the number 0·0363, which is little different from that obtained with M. Meyendorff's specimen. Nevertheless, as the product 45 of the specific heat by the atomic weight is very high, I always considered the metal impure, and sought opportunities of making fresh determinations. In 1857 M. Chapuis offered me a new specimen, also consolidated by percussion, and which he considered purer than that of 1855; this specimen gave me the following results:—

M . . .	300^{gr}·55	300^{gr}·55
p . . .	1^{gr}·5048	1^{gr}·5048
T . . .	97°·63	97°·50
θ' . . .	20°·71	21°·34
$\Delta\theta'$ . . .	2°·3284	2°·2923
A . . .	466^{gr}·69	466^{gr}·69
C . . .	0·04200	0·04173

Mean . . . 0·04186.

This specific heat is still higher than that which I found for the preceding specimens, and, multiplied by the atomic weight, gives the number 51·62.

This experiment clearly proves that iridium is always altered by considerable proportions of one or more metals with low equivalents which accompany platinum, that is, palladium, rhodium, and ruthenium. Palladium can scarcely be there; for it is so easily attacked by aqua regia that it must go into the first mother-liquors.

Lastly, M. Debray recently offered me fused iridium; he considered it to be quite free from platinum, osmium, and iridium, and only suspected the presence of a little ruthenium. This specimen formed a single cylinder with almost plane bases. The fusion, if it had not brought about perfect liquidity, had at all events produced a very complete agglomeration. The metal was suspended in the apparatus by a silk thread:—

M . . .	262gr·78	262gr·78
T . . .	98°·47	99°·27
θ' . . .	17°·61	17°·18
$\Delta\theta'$ . . .	1°·7437	1°·8001
A . . .	395gr·58	395gr·58
C . . .	0·03246	0·03273

Mean . . . 0·03259.

This specific heat is much less than that which I found for other specimens; multiplied by the atomic weight 1233·2, it gives the product 40·19, which is within the limits which I have found for simple bodies. I think nevertheless that the specific heat of perfectly pure iridium is below 0·0325, and that the fused iridium of M. Debray still contained a little ruthenium, which considerably increased the specific heat.

Manganese.

I formerly determined the specific heat of a carburetted manganese (*Ann. de Chim. et de Phys.* vol. lxxiii. p. 57) which had been prepared by heating carbonate of manganese in a crucible lined with charcoal to a strong furnace-heat. The metal was very hard and brittle; it contained more carbon and silicon than cast iron; hence its specific heat was found to be higher than that which belongs to pure manganese.

Since then I have had occasion to analyse some other specimens of manganese, but none of them was pure metal. M. Rousseau lent me an ingot which had been obtained by reducing the natural binoxide in a lined crucible. The metal was very brittle, and of a crystalline texture; it contained much carbon and silicon. I only made a single determination of its specific heat.

M	130^{gr}·15
p	2^{gr}·027
T	97°·50
θ'	19°·98
$\Delta\theta'$	3°·2159
A	466·69
C	0·1332

The product of the specific heat by the atomic weight 344·7 is 45·91, a number which is far too high.

M. Dumas lent me another specimen of manganese prepared by M. Brunner by the new method which this skilful chemist has described (*Comptes Rendus,* vol. xliv. p. 630). The metal formed two small ingots, which could be flattened under the hammer, and took a beautiful polish with the file. I nevertheless detected the presence in it of a small quantity of silicon:—

M . . .	54^{gr}·980	54^{gr}·980
p . . .	0^{gr}·0515	0^{gr}·0515
T . . .	97°·40	96°·91
θ' . . .	13°·10	14°·25
$\Delta\theta'$ . . .	1°·2082	1°·2043
A . . .	466^{gr}·69	466^{gr}·69
C . . .	0·1207	0·1227

Mean . . . 0·1217.

The product of the specific heat 0·1217 by the atomic weight 325·0 is 39·55, which is comprised within the specified limits.

It may therefore be concluded that pure manganese is as ductile as iron, and that its specific heat should be about 0·114.

Nickel.

In my first memoir "On the Specific Heat of Simple Bodies," I gave the specific heat of a specimen of nickel which had been prepared by calcining at a strong furnace-heat oxalate of nickel contained in a closed porcelain crucible, this being enclosed in a crucible lined with charcoal. This specific heat is 0·10863; if multiplied by the atomic weight 350·0, which M. Dumas has deduced from his last researches, the product 38·02 is obtained, which agrees very well with the law of the specific heat of simple bodies. This proves the correctness of the formula assigned to nickel compounds.

Since then I have had occasion to determine the specific heat on some specimens of nickel which had been prepared by different methods. M. Rousseau lent me nickel obtained by calcining at furnace-heat a mixture of oxide of nickel and sal-ammoniac enclosed in an earthen crucible. The metal formed two ingots, readily scratched by the file, and which were flattened under the hammer. This nickel gave the following results :—

M . . .	311^{gr}·00	311^{gr}·00
T . . .	96°·77	97°·88
θ' . . .	14°·87	18°·81
$\Delta\theta'$ . . .	6°·2413	6°·1315
A . . .	434^{gr}·98	434^{gr}·98
C . . .	0·10659	0·10845

Mean . . . 0·10752.

The product of the specific heat by the atomic weight 350 is 37·62.

The second specimen was lent me by M. Dumas, who had great confidence in its purity. It consisted of three almost spherical bullets, and could be readily filed :—

M .	229^{gr}·88	229^{gr}·88	229^{gr}·88	229^{gr}·88	229^{gr}·88
p .	1^{gr}·5015	1^{gr}·5015	1^{gr}·5015	1^{gr}·5015	1^{gr}·5015
T .	97°·55	97°·85	97°·45	97°·20	97°·45
θ' .	11°·32	14°·54	10°·53	10°·69	11°·12
$\Delta\theta'$	5°·4547	5°·3615	5°·5610	5°·5210	5°·5287
A .	422^{gr}·30	422^{gr}·30	422^{gr}·30	422^{gr}·30	422^{gr}·30
C .	0·10970	0·11169	0·11100	0·11071	0·11112

Mean . . . 0·1108.

The product of this specific heat by the atomic weight is 38·78, a number perceptibly higher than that found for the other specimens of nickel.

Cobalt.

In my first memoir I found 0·1071 for the specific heat of cobalt. The metal had been prepared by heating at a strong furnace-heat oxalate of cobalt pressed in a porcelain crucible, which was itself enclosed in a crucible lined with charcoal. I recently examined two ingots of the same metal which had been prepared by M. Rousseau, by calcining at furnace-heat a mixture of oxide of cobalt and sal-ammoniac. This metal was malleable, and readily scratched by the file.

M	113^{gr}·905	113^{gr}·905
p	1^{gr}·5015	1^{gr}·5015
T	97°·55	97°·09
θ'	12°·27	12°·43
$\Delta\theta'$	2°·5535	2°·5260
A	434^{gr}·98	434^{gr}·98
C	0·10113	0·10075

Mean . . . . 0·10094.

This specific heat is much lower than that which I found for metal prepared with the oxalate, and the product of the specific heat by the atomic weight 350 is 35·33. I think that the metal contained some foreign body.

M. Dumas placed at my disposal another quantity of cobalt which he considered to be pure. One portion of it consisted of small bars; the other formed three almost spherical bullets. The metal was malleable enough to be flattened in the cold by the hammer, and the file readily cut it; but the small ingots grasped in a vice were broken when struck transversely by a hammer.

The metal in bullets gave the following results:—

M .	225$^{\text{gr}}$·17	225$^{\text{gr}}$·17	225$^{\text{gr}}$·17	225$^{\text{gr}}$·17
p .	1$^{\text{gr}}$·5015	1$^{\text{gr}}$·5015	1$^{\text{gr}}$·5015	1$^{\text{gr}}$·5015
T .	97°·90	97°·90	97°·35	97°·45
θ' .	8°·39	9°·57	10°·56	10°·44
$\Delta\theta'$.	5°·3267	5°·3034	5°·2338	5°·2920
A .	422$^{\text{gr}}$·30	422$^{\text{gr}}$·30	422$^{\text{gr}}$·30	422$^{\text{gr}}$·30
C .	0·10494	0·10593	0·10653	0·10740

Mean . . . . 0·10620.

The product by the atomic weight 350 is 37·17.

The cobalt in bars gave—

M	164$^{\text{gr}}$·65	164$^{\text{gr}}$·65
p	1$^{\text{gr}}$·5015	1$^{\text{gr}}$·5015
T	97°·55	97°·45
θ'	8°·14	8°·08
$\Delta\theta'$	4°·0732	4°·0127
A	422$^{\text{gr}}$·30	422$^{\text{gr}}$·30
C	0·10772	0·10682

Mean . . . . 0·10727.

This specific heat scarcely differs from that found above.

The following are the numbers which I have successively obtained for the specific heats of cobalt and of nickel, excluding some numbers which refer to metals evidently impure:—

	Cobalt.	Nickel.
In 1843	0·1070	0·1086
1859	0·1062	0·1075
1859	0·1073	0·1108

The specific heat of cobalt is hence in all cases a little less than that of nickel; and it might be concluded that the atomic weight of cobalt should be higher than that of nickel. The recent experiments of M. Dumas have given the same atomic weights for these two metals. M. Schneider, who has made some new determinations (Phil. Mag. vol. xv. p. 115, and vol. xviii. p. 273), gives cobalt a higher atomic weight. My determinations of the specific heats favour the latter conclusion; but the differences in question are so small that it is impossible to decide the question by a determination of the specific heats, until the experiments can be made with absolutely pure metals.

Tungsten.

The specific heat of tungsten was given in my first memoir. The metal was obtained by calcining, at furnace-heat in a lined crucible, oxide of tungsten previously reduced by hydrogen. It was to be feared, however, that the metal had by this process absorbed carbon and silicon. I found, in fact, the specific heat to be 0·03636, which, multiplied by the atomic weight 1150, now usually admitted for tungsten, gives 41·81, a number distinctly too high for the law of specific heats.

M. Rousseau placed at my disposal a large quantity of tungsten. He had prepared it by reducing tungstic acid at a high and steady long-sustained temperature by means of hydrogen. This tungsten is crystalline, but pulverulent. To determine its specific heat, it was pressed in a circular brass vessel weighing 35^{gr}·60, and the calorific equivalent of which was 3^{gr}·3428. The following are the results of the two experiments which have been made upon this substance:—

M	420^{gr}·80	425^{gr}·05
p	3^{gr}·3428	3^{gr}·3428
T	98°·15	98°·16
θ'	12°·32	12°·09
$\Delta\theta'$	3°·5512	3°·5625
A	422^{gr}·30	422^{gr}·30
C	0·03358	0·03326

Mean . . . . 0·03342.

This specific heat, multiplied by 1150 0, the atomic weight of tungsten according to M. Dumas's last experiments, gives the product 38·43, which is quite within the limits of the law of specific heats.

Silicon.

Chemists are at present quite uncertain as to the formula which should be assigned to silicic acid, and therefore as to the true equivalent of silicon. The majority write the formula SiO^3, and the equivalent is then 266·7; others write it SiO^2, which puts the equivalent at 177·8; lastly, the formula SiO has been proposed, which makes the equivalent 88·9.

It has hitherto been possible to form only a few definite compounds of silicon; and the chemical and crystallographical analogies which have been sought to be established between these compounds and other similar compounds whose formulæ are definitely known, are by no means certain. Hence it becomes interesting to determine the specific heat of silicon; and during the last few years I have neglected no opportunity of doing so.

I worked on two varieties of silicon; crystallized silicon, prepared by M. St.-Claire Deville's method, and the same silicon melted at a very high temperature.

First specimen of crystallized silicon lent by M. Deville; it was in small lustrous crystals, from which the uncrystallized part had been separated as completely as possible:—

M . . .	44$^{\text{gr}}$·11	41$^{\text{gr}}$·06	38$^{\text{gr}}$·74
p . . .	2$^{\text{gr}}$·027	2$^{\text{gr}}$·027	2$^{\text{gr}}$·027
T . . .	97°·83	97°·70	97°·83
θ' . . .	21°·02	19°·34	19°·31
$\Delta\theta'$ . . .	1°·5356	1°·5032	1°·4359
A . . .	466$^{\text{gr}}$·69	466$^{\text{gr}}$·69	466$^{\text{gr}}$·69
C . . .	0·1655	0·1686	0·1679

Mean . . . . 0·1673.

Second specimen of crystallized silicon lent by M. Rousseau; it had been prepared by M. St.-Claire Deville's method, and the best-defined and most lustrous crystals were selected:—

M . . .	33$^{\text{gr}}$·38	34$^{\text{gr}}$·08
p . . .	2$^{\text{gr}}$·027	2$^{\text{gr}}$·027
T . . .	97°·50	97°·50
θ' . . .	14°·95	15°·65
$\Delta\theta'$ . . .	1°·4991	1°·5161
A . . .	434$^{\text{gr}}$·58	434$^{\text{gr}}$·58
C . . .	0·1757	0·1767

Mean . . . . 0·1762.

Third specimen of crystallized silicon from another preparation, which M. Rousseau made by the same method:—

M . . .	44$^{\text{gr}}$·53	42$^{\text{gr}}$·515	43$^{\text{gr}}$·815
p . . .	2$^{\text{gr}}$·027	2$^{\text{gr}}$·027	2$^{\text{gr}}$·027
T . . .	99°·67	99°·77	99°·82
θ . . .	12°·85	13°·70	11°·37
$\Delta\theta'$ . . .	1°·9714	1°·9275	2°·0017
A . . .	422$^{\text{gr}}$·96	422$^{\text{gr}}$·96	422$^{\text{gr}}$·96
C . . .	0·1712	0·1751	0·1722

Mean . . . . 0·1742.

Lastly, Captain Caron lent me a large quantity of crystallized silicon, remarkable for its lustre and the distinctness of its crystals. I give in a note* the method of its preparation as described by Captain Caron.

M	87^{gr}·62	90^{gr}·12	93^{gr}·07
p	1^{gr}·6648	1^{gr}·6648	1^{gr}·6648
T	99°·40	99°·81	99°·20
θ'	22°·24	21°·06	19°·95
$\Delta\theta'$	2°·8663	3°·0303	3°·1135
A	464^{gr}·48	464^{gr}·48	464^{gr}·48
C	0·17811	0·17986	0·17818

Mean . . . . . 0·1787.

I have therefore obtained for the specific heat of crystallized silicon—

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* A mixture is made of—

Dried silicofluoride of potassium	300
Granulated zinc	400
Sodium in small pieces	80

These proportions are not absolutely necessary, but they seem to give the best yield of silicon. The mixture thus made is projected into a crucible, which, along with its cover, is red-hot. The reaction is brisk, although when the cover is not sufficiently hot it is often necessary to press the mixture with a clay pipe. When the whole is liquid, the crucible is removed and allowed to cool. It is necessary to execute the operation as rapidly as possible, otherwise the crucible is liable to be perforated, and part of the zinc and the silicon lost.

The cooled crucible is broken to extract the ingot of zinc, which will have settled down well if the operation has been successful: the crystallized silicon is almost entirely at the upper part of the zinc. The pieces of the crucible and scoriæ adhering to the regulus are removed, and the latter is melted at as low a temperature as possible, so that the zinc is liquid while the silicon is solid. The zinc is run out and granulated, and can be used for another operation: crystals of silicon remain in the crucible surrounded by a little zinc. This residue is treated with concentrated hydrochloric acid, which removes zinc and iron, and crystallized silicon is left still containing a little lead (if the zinc was not quite pure), and always a little protoxide of silicon. The lead is removed by boiling with strong nitric acid and washing, and the protoxide of silicon, as well as any of the mass of the crucible, are removed by treatment with hydrofluosilicic acid. The pure silicon which remains is washed with water and dried.

To melt this silicon, it is mixed with silicofluoride of potassium and placed in a double crucible, having been previously covered with a thick layer of coarsely powdered glass. It is next heated to the melting-point of iron for some time, and is then immersed while hot in cold water in order to render the glass more friable. The crucible is then carefully broken, and the globule of silicon is found surrounded by glass, which is easily removed either by a hammer or by means of a sharp-pointed steel. To purify it completely, it must be boiled for some time with concentrated hydrofluoric acid, which completely removes any slag, provided it is not in the centre of the regulus.

The only acid which attacks melted or crystallized silicon is nitrofluoric acid.

First specimen . . .	0·1673
Second specimen . . .	0·1762
Third specimen . . .	0·1742
Fourth specimen . . .	0·1787
Mean . . .	0·1774

Fused Silicon.

M. St.-Claire Deville lent me a specimen of silicon which had been fused in a strong furnace. It constituted a single bar, all the parts of which were firmly welded together. There were, however, some cavities, one of which was filled with a vitreous matter very different from the rest of the mass, and which it was impossible to detach.

M . . .	$28^{\mathrm{gr}}\cdot228$	$28^{\mathrm{gr}}\cdot228$	$28^{\mathrm{gr}}\cdot228$
p . . .	$2^{\mathrm{gr}}\cdot027$	$2^{\mathrm{gr}}\cdot027$	$2^{\mathrm{gr}}\cdot027$
T . . .	97°·63	97°·63	97°·50
θ' . . .	20°·63	20°·68	21°·26
$\Delta\theta'$ . . .	1°·0647	1°·0479	1°·0555
A . . .	$466^{\mathrm{gr}}\cdot69$	$466^{\mathrm{gr}}\cdot69$	$466^{\mathrm{gr}}\cdot69$
C . . .	0·1568	0·1533	0·1571

Mean . . . . 0·1557

A second specimen of fused silicon was lent to me by M. Caron; it consisted of a single globule, apparently quite homogeneous, with a facette which was a perfect mirror.

M	$11^{\mathrm{gr}}\cdot620$	$11^{\mathrm{gr}}\cdot620$
T	99°·42	99°·52
θ'	10°·53	12°·27
$\Delta\theta'$	1°·4382	1°·4436
A	$112^{\mathrm{gr}}\cdot40$	$112^{\mathrm{gr}}\cdot40$
C	0·1611	0·1648

Mean . . . . 0·1630

M. Caron lent me recently a larger quantity of fused silicon forming several reguli, and in which no foreign matter could be detected. I obtained—

M	$81^{\mathrm{gr}}\cdot67$	$81^{\mathrm{gr}}\cdot67$
p	$1^{\mathrm{gr}}\cdot3287$	$1^{\mathrm{gr}}\cdot3287$
T	99°·61	99°·55
θ'	22°·29	20°·99
$\Delta\theta'$	2°·6069	2°·6257
A	$464^{\mathrm{gr}}\cdot48$	$464^{\mathrm{gr}}\cdot48$
C	0·1755	0·1738

Mean . . . . 0·1747

At my request M. Caron was good enough to melt this into a single regulus. I then obtained the following results:—

M	$78^{gr}\cdot700$	$78^{gr}\cdot700$
T	99°·49	99°·49
θ'	22°·01	22°·70
$\Delta\theta'$	2°·2739	2°·2986
A	$464^{gr}\cdot48$	$464^{gr}\cdot48$
C	0·1732	0·1767

Mean . . . . 0·1750

The experiments made with the silicon melted by M. Caron are evidently the most reliable, because they were made with a sufficient quantity of substance. They lead to the result that the specific heat of silicon is 0·1750, which is virtually the same as that of crystallized silicon (p. 119).

The mean specific heat, 0·176, multiplied

by the atomic weight, 266·7, gives the product 46·92
„ „ 177·8 „ 31·29
„ „ 88·9 „ 15·64

None of these results is comprised within the limits of variation which we have found in the case of other bodies. We must assume either that silicic acid has none of the formulæ which chemists have hitherto assigned to it, or that crystallized silicon is not a simple body, but contains another element which has escaped the sagacity of observers, or, lastly, that silicon forms an exception to the law of the specific heats of simple bodies. It is difficult to admit that this anomaly arises from a numerical error in the determination of the equivalent of silicon; for this equivalent has been determined by the synthesis of silicic acid, and by the analysis of chloride of silicon.

In order that silicon should obey the law of the specific heats of simple bodies with the value which we have found for its specific heat, it would be necessary to write the formula of silicic acid, $Si^2 O^5$; it would then resemble that of nitric, phosphoric, and arsenic acids. The atomic weight of silicon would be 222·3, and the product of this atomic weight by its specific heat would become 39·12, which agrees with the analogous product which the other simple bodies give.

If we are only guided by apparent analogies, it is not difficult to find resemblances between silicon, phosphorus, and arsenic. Thus:—

Silicic acid being a polybasic acid, could form, like phosphoric and arsenic acids three kinds of salts,—monobasic, bibasic, and tribasic; it would thus be easier to explain the great number of silicates with multiple bases which nature offers us in beautiful well-defined crystals. The existence of the natural hydrosilicates would then be readily understood, &c.

Silicic acid forms, with alcohol and wood-spirit, three silicic ethers like phosphoric acid, while the monobasic acids only form one compound ether.

M. Wöhler has recently described a siliciuretted hydrogen which is spontaneously inflammable in the air, and presents the greatest analogy with phosphuretted hydrogen.

The protoxide of silicon discovered by the same chemist would correspond to phosphorous acid; chloride of silicon to chloride of phosphorus, &c.

But it is difficult to refer to this formula for silicic acid ($Si^2 O^5$) the numerical results which analysis assigns to many compounds of silicon. I confine myself at present to calling the attention of mineralogists to this subject. Moreover it is possible that silicon presents in its calorific capacity anomalies like those which I have met with for carbon in its different conditions. The specific heat of crystallized or melted silicon would not be that which appertains to silicon in its compounds.

Boron.

The atomic weight of boron is as uncertain as that of silicon; and chemists can give no definite reason for fixing the formula of boracic acid. The formula BoO^3 is usually adopted, and then borax is regarded as a neutral borate of soda. In the hope of elucidating this question, a great interest attached to the determination of the specific heat of boron, and I have successively made experiments upon all the specimens of boron which I could procure.

M. H. St.-Claire Deville distinguishes three varieties of boron:—1, amorphous boron; 2, graphitoidal boron; and 3, crystallized boron.

I worked with all three varieties, and proceed to detail the results which I have obtained.

1. *Amorphous Boron.*

The amorphous boron was prepared in M. St.-Claire Deville's laboratory. I washed it repeatedly with distilled water, and then dried it under the receiver of an air-pump. In order to determine with some accuracy the specific heat of this pulverulent substance, I compacted it in the form of cylindrical discs, by compressing it with the blows of a hammer in a lapidary's mortar. These discs were placed for six days *in vacuo* over sulphuric acid, and were then placed in a basket of brass wire. To prevent the boron from absorbing oxygen by being raised to the elevated temperature of the bath, the inverse method was adopted, that is, the boron was cooled in the apparatus which I have described.

Several experiments made in this way have given very discordant results. The reason is, that at a low temperature boron absorbs and condenses a large quantity of air, which it disengages often with a brisk effervescence the moment the basket is immersed in the calorimeter. To avoid this source of error it would have been necessary to press the amorphous boron in a brass cylinder, which was then hermetically closed. I tried in fact to make the experiment in this way; but boron in this pure state of division is such a bad conductor of heat, that the water of the calorimeter only assumes its maximum temperature after a lengthened immersion; and that renders the determination very uncertain.

I then decided to heat the boron in the water-bath. The pulverulent boron was strongly pressed in a very thin brass cylinder, which was closed with a circular leaden cover to protect the substance completely from the action of the air. The brass vessel was suspended in the bath of fig. 1, the reservoir of the thermometer occupying the central space of the vessel. I soon found that the thermometer rose much more rapidly than when the vessel contained another substance; after some time it even exceeded the temperature of 100°. I thought at first that this arose from the boron undergoing at this temperature one of those allotropic modifications which M. Deville has mentioned; but I have since found that this increase of temperature simply arises from a brisk absorption of oxygen by the boron, which takes place at about 100°, and changes it into boracic acid. This is soon seen by digesting in the water of the calorimeter the boron which filled the basket. This water, filtered, gave on evaporation a notable quantity of boracic acid, which certainly arose from an oxidation which the boron had experienced in the bath, spite of the precautions which had been taken to isolate the surrounding air; for the boron had been repeatedly washed with distilled water, and then dried *in vacuo* over sulphuric acid before being compressed in the circular vessel.

This second method of experimenting presents therefore an important source of error, and confidence cannot be placed in its results. I nevertheless transcribe the elements of the three experiments made in this manner:—

	I.	II.	III.
M . . .	$20^{gr}\cdot14$	$19^{gr}\cdot22$	$18^{gr}\cdot88$
p . . .	$1^{gr}\cdot1451$	$1^{gr}\cdot1451$	$1^{gr}\cdot1451$
T . . .	$104^{\circ}\cdot10$	$99^{\circ}\cdot70$	$101^{\circ}\cdot55$
θ' . . .	$10^{\circ}\cdot41$	$14^{\circ}\cdot19$	$15^{\circ}\cdot14$
$\Delta\theta'$ . . .	$2^{\circ}\cdot0771$	$1^{\circ}\cdot5934$	$1^{\circ}\cdot6315$
A . . .	$422^{gr}\cdot30$	$422^{gr}\cdot30$	$422^{gr}\cdot30$

In each of these experiments the quantity of boracic acid formed was determined: in the first 0·56 grm. was found; so that at the moment of immersion the brass vessel contained $19^{gr}\cdot95$ of boron, and 0·56 of boracic acid. In like manner in Experiment II. there were—

Boron	$19^{gr}\cdot10$
Boracic acid	0·35

Lastly, in the third experiment there were—

Boron	$18^{gr}\cdot72$
Boracic acid	0·49

The specific heat of anhydrous boracic acid is 0·2374 (*Ann. de Chim. et de Phys.* 3rd series, vol. i. p. 148). If there were no other sources either of the disengagement or absorption of heat, the perturbation produced by the presence of the boracic acid could be calculated. But the boracic acid is probably formed in the anhydrous state in the bath; in the water of the calorimeter it changes into hydrated acid, which dissolves more or less completely in this water during the time which elapses between immersion and the observation of the stationary temperature. These are new sources of error which I was not able to take into account.

Simply making the correction due to a replacement of part of the boron by boracic acid, the specific heat of boron is found to be—

I.	0·4053
II.	0·3483
III.	0·3598

These values agree very little with each other, and I think they cannot even be regarded as approximate.

I then tried to study amorphous boron in the isomeric modification which it experiences, according to M. Deville, when heated to 200° in an oil-bath in a current of hydrogen. I placed in a glass flask discs of amorphous boron which had been prepared by percussion. The flask having been placed in an oil-bath, a current of hydrogen was passed through, and the temperature of the oil-bath gradually raised to 200°, where it was maintained for about half an hour. In this experiment I did not observe either incandescence, or any visible phenomenon of molecular modification. The oil-bath having been removed, the current of hydrogen was continued for two hours. The substance had not changed in appearance; but when I was transferring the discs from the flask into a porcelain capsule, they took fire one after the other, and burned with a lively incandescence. A funnel placed immediately over the ignited boron was immediately covered with abundant drops of water.

It appears from this that amorphous boron, in becoming cooled in hydrogen gas, had condensed in its pores a large quantity of this gas. The gas took fire in air just as is the case when hydrogen is directed on spongy platinum.

2. *Graphitoidal Boron.*

This was prepared by M. Debray, by accurately following the method described by M. Deville. It was in small lustrous laminæ, closely resembling those of graphite.

M	13^{gr}·86	13^{gr}·87	13^{gr}·56
p	6^{gr}·593	6^{gr}·593	6^{gr}·593
T	99°·32	99°·72	99°·27
θ'	16°·64	18°·09	17°·41
$\Delta\theta'$ . . .	1°·9117	1°·8814	1°·9272
A	422^{gr}·96	422^{gr}·96	422^{gr}·96
C	0·2299	0·2275	0·2481

Mean . . . . 0·2352

The results obtained by these experiments differ appreciably from each other. This arises from the fact that, in order to retain this pulverulent boron in the brass basket, it was necessary to line the latter with lead-foil; and in order that the basket should sink rapidly in the water of the calorimeter, it had to be loaded with a disc of lead. The calorimetric value of p is thus twice that of M; and the latter, which is the unknown, necessarily bears the sum of the errors of each experiment.

3. *Crystallized Boron.*

Crystallized boron is prepared, according to M. St.-Claire Deville, by heating aluminium in a porcelain tube in a current of chloride of boron.

I worked with three specimens prepared by this method.

The first specimen was lent me by M. Deville: the quantity was not great; and it was necessary to take special precaution to obtain accurate results. The following are the elements of the three experiments which I made:—

M	7^{gr}·330	7^{gr}·280	7^{gr}·202
p	0^{gr}·2826	0^{gr}·2413	0^{gr}·2413
T	97°·60	97°·73	97°·67
θ'	10°·52	9°·35	10°·26
$\Delta\theta'$ . . .	1°·4436	1°·3750	1°·3350
A	134^{gr}·566	134^{gr}·846	141^{gr}·076
C	0·2657	0·2552	0·2652

Mean . . . . 0·2622

Second specimen prepared by M. Rousseau. It was treated by hydrofluoric acid; M. Rousseau feared nevertheless the presence of a little aluminium.

M	11^{gr}·38	11^{gr}·18
p	1^{gr}·332	1^{gr}·332
T	98°·25	97°·95
θ'	13°·54	10°·32
$\Delta\theta'$	0°·7878	0°·7928
A	422^{gr}·30	422^{gr}·30
C	0·2280	0·2226

Mean . . . . . . 0·2253.

The specific heat of the second specimen is perceptibly less than that of the first; and that proves clearly that even by the same method products of different nature may be obtained.

The third specimen was prepared by M. Debray in M. Deville's laboratory, and by the same method. It consisted of very lustrous crystalline laminæ, to some of which an amorphous substance adhered, from which it was very difficult to free them.

M	21^{gr}·00	16^{gr}·690
p	1^{gr}·513	3^{gr}·3472
T	99°·92	99°·52
θ'	10°·82	16°·85
$\Delta\theta'$	1°·4615	1°·5938
A	422^{gr}·96	395^{gr}·58
C	0·2584	0·2564

Mean . . . . . . 0·2574

This value differs little from that which I found for the first specimen.

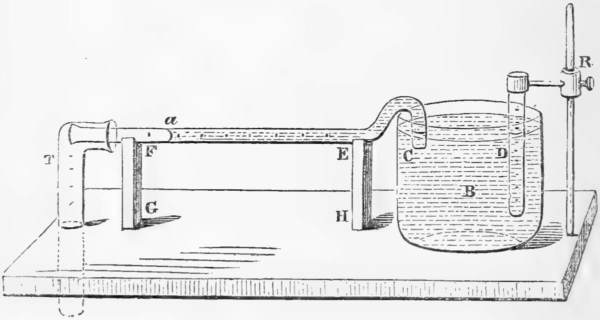
In conclusion, I have but little confidence in the results obtained for crystallized boron, and I shall assume that its specific heat is 0·250, which is about the mean of the numbers furnished by the three specimens. If we write the formula of boracic acid BoO^3, the equivalent of boron becomes 136·1, and the product of the specific heat, 0·250, by the equivalent is 34·1. Thus, from the specific heat of crystallized boron, the most probable formula of boracic acid is BoO^3. But it would be imprudent to draw any such conclusion from this fact; for crystallized boron might well contain another simple body, which would materially modify its specific heat.

XVII. *Experimental Researches on the Laws of Evaporation and Absorption, with a Description of a new Evaporameter and Absorbometer.* *By* THOMAS TATE, *Esq.**

Evaporameter.

THIS instrument enables us readily to determine with considerable precision the amount of evaporation which takes place from a given surface of water at different states of the atmosphere. The most direct and probably the most accurate method of determining this consists in exposing a known weight of water, placed in a large shallow pan, to the action of the air; and then, after the lapse of a certain time, by weighing the pan with the residue of water, the weight of the water which has been evaporated becomes known. This method, however, is not only troublesome, but supplies no correction for the rain or moisture that may have fallen during the period of exposure. The instrument which I have constructed not only takes the rain-fall into account, but is also simple in its use, and sufficiently accurate in its indications. It consists of a large glass bell, such as are used for aquariums, B, fig. 1, nearly filled with water, placed upon a stout deal board G H; a glass tube, C E F, divided into linear inches, about 18 inches long, and $\frac{2}{10}$ths of an inch internal diameter, supported on the pillars E H and F G in a *slightly inclined position*, the extremity F having a rise of about $\frac{1}{50}$ in 1, and having its bent extremity E C dipping into the water contained in the glass bell; a displacement-gauge D, graduated into cubic inches and parts of cubic inches, supported in a vertical position by the sliding ring R, with its clamp-screw, so that it may be readily raised out of or depressed in the water contained in the glass bell; a wide graduated tube T, placed as shown in the diagram, to catch the overflow of water from the

Fig. 1.



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* Communicated by the Author.

tube E F. The pillar F G admits of being raised or depressed, so as to give any desired inclination to the tube E F. A mark is made on the surface of the glass bell at D, a little below the ordinary level of the water, with which the graduations on the displacement-tube are at each observation compared; also a special mark is made on one of the graduations of the tube E F at *a*, towards its outer extremity, with which the water in the tube at each observation is made to coincide by raising or depressing the displacement-gauge. Now as the water in the tube always stands at a point depending on the height of the water in the glass bell, any slight depression of the water in the bell, produced by evaporation or by any other means, will cause the water in the tube to move through a comparatively large space; thus if the tube has a rise of $\frac{1}{10}$ in 5, then a fall of $\frac{1}{50}$th of an inch in the bell will cause the water in the tube to fall through the space of an inch; that is, the scale of reading in this case will be increased fifty times; moreover, if the displacement-gauge be depressed until the water in the tube is brought again to the position *a*, the reading of this gauge will give the number of cubic inches of water evaporated.

To adjust the inclination of the tube E F so as to give any proposed scale of reading.—Let it be required, for example, to give the tube such an inclination that one cubic inch depression of the gauge shall cause the water in the tube to move through the space, say, of half an inch. The zero-mark on the displacement-gauge being made to coincide with the mark D on the glass bell, water is then added until it reaches a certain mark on the tube; the gauge is then depressed until the mark of one cubic inch coincides with the mark on the bell, and the space through which the water in the tube has moved will show whether the inclination of the tube is too much or too little, and then the moveable pillar G F is lowered or raised accordingly; and this is repeated until the proper inclination is found. In order to obtain the true position of the water in the tube, it is only necessary alternately to raise and depress the displacement-gauge so as to cause the water to oscillate in the tube until it is found to settle itself exactly at the same mark when the displacement-gauge is brought to its desired position. This remark also applies to all the ordinary observations made with the instrument. This adjustment of inclination being once made, does not require to be repeated, unless the place of the instrument has to be changed, or unless some other scale of reading has to be adopted.

The instrument is ordinarily used in the following manner:—At the commencement of the observations the displacement-gauge is at zero, whilst the water in the tube stands at the mark *a*;

after the lapse of any proposed time, the evaporation from the surface of the water in the glass bell will have caused the water in the tube to have moved towards E; and this distance converted into cubic inches, by a relation of scale previously ascertained, will for short intervals very nearly give the amount of evaporation; but this will in all cases be more accurately and directly found by depressing the displacement-gauge until the liquid in the tube is brought back to the mark *a*; for the reading of the mark on the gauge, coincident with the mark on the glass bell, at once gives the number of cubic inches of water evaporated in the interval of time, care being taken to give a slight oscillation to the water in the tube, as already explained, before the reading is taken. After a series of observations it will be found that the displacement-gauge has nearly reached its lowest point of depression; in this case fresh water must be added, and the adjustment made as already described. If rain has fallen during the interval of observation, the amount of this rain, as determined by an ordinary rain-gauge, reduced (if necessary) to the surface of the water of the glass bell, must be added to the amount of evaporation indicated by the evaporameter, and then the excess of this quantity over that which has flowed into the tube T will give the true amount of evaporation. Should the water in the tube extend beyond the mark *a*, the gauge must in this case be depressed so as to throw a portion of water into the tube T, and then the process may be conducted as above described. The amount of evaporation thus determined is that which is due to the surface of the water in the glass bell; but by proportion the evaporation due to any other surface may be readily found. The water-surface of the instrument which I have constructed is equal to about 80 square inches, and the inclination of the tube is about 1 in 50: when the inclination is much less than this, the motion of the water in the tube becomes somewhat fitful and irregular.

With the view of testing the reliability of the instrument, I have compared the results derived from it with those derived from the balance, and have found that they nearly agree with each other. Thus the evaporation during twenty-four hours was found by the instrument to be 2·2 cubic inches, whilst the evaporation during the same time, as indicated by the balance, was found to be 540 gr. or 2·14 cubic inches.

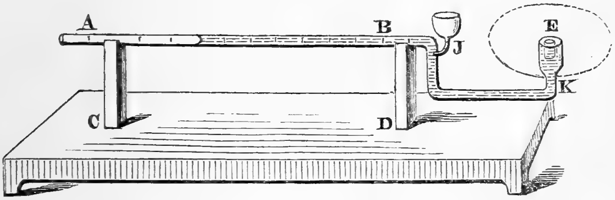
No doubt the variation of the force of liquid cohesion due to change of temperature will form a source of error in the readings of this instrument; but this variation, slight even at extreme atmospheric temperatures, must be exceedingly small for any change of temperature which can take place between two consecutive observations.

A more delicate instrument, adapted for special purposes, will be hereafter described.

Absorbometer.

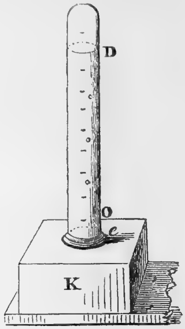
This instrument is used for determining the volumes of liquid absorbed during successive intervals of time, and generally for finding the rate at which liquids are transmitted through the pores of different absorbents. It consists of a graduated glass tube A B, about $\frac{2}{10}$ths of an inch in the bore and 20 inches long, sustained in a horizontal position by the supports A C and B D, and having one of its extremities bent after the manner shown in the diagram (fig. 2); an enlarged tube E, on which is placed the absorbent to be examined, polished at its top, which is on a level with the axis of A B, cemented or otherwise attached to the top of the bent tube K; and a capillary tube J, having its upper end on a level with the upper side of the tube A B, with a funnel-shaped top for supplying the tube with liquid as may be required. The enlarged tube E may be replaced by tubes of different diameters and lengths. The whole tube having been filled with liquid, the finger is applied to the extremity A, and a few drops of water are added, so as to cause the water to cover the top of the tube E; the absorbent is then laid on the tube E, the finger being at the same time withdrawn from the extremity A: as the water is being absorbed by the absorbent, the column of water in the horizontal tube A B moves towards B; and the rate of this motion being observed by means of a watch with a pointer indicating seconds, gives the rate at which the water diffuses itself through the pores of the absorbent; and so on to other cases. The following form of the apparatus was employed when the amount of water diffused or transmitted, as the case might be, was unusually large:—

Fig. 2.



A graduated tube, O D, fig. 3, about 9 inches long and $\frac{3}{4}$ of an inch diameter, closed at the top and having a smooth welt at the bottom,—a small perforation, O, about $\frac{1}{20}$th of an inch diameter, having been made in the tube at a short distance from its lower extremity, for admitting the external air into the tube as the water is being absorbed by the absorbent *e* K, on which the tube stands, thereby maintaining the water at a constant pressure on the absorbent. The instrument is adjusted in the following manner:—The tube is held in a vertical position, with its mouth upwards, and filled with water; the flat surface of the absorbent is then placed on the orifice of the tube, and the whole is inverted and placed upon the edge of the table. In the case of flexible absorbents, such as calico or cloth, the material is first covered with a piece of polished slate, and then the instrument is inverted as above described.

Fig. 3.



A more delicate Evaporameter, answering the purpose of a Hygrometer.

This instrument is a modification of the apparatus represented in diagram 2. The tube A B K E being filled with filtered rain-water, a damp piece of calico, about 8 inches square, is placed upon the top of the tube E, and a small weight is placed over it to keep it in position. In order to stretch this calico and to keep its surface horizontal, tinned bent wires are passed through its corners and inserted in wooden pegs fixed to the board C D; the instrument thus adjusted may remain without any further interference until it is found requisite to clean the calico by washing. The calico is thus always kept in a damp condition by absorption; and as the moisture is being evaporated from its surface, the water in the horizontal tube A B moves towards B with a velocity proportional to the rate of evaporation; this rate of evaporation, therefore, is indicated by the space passed over by the extremity of the water column in the tube A B in successive equal intervals of time; and further, as the section of this tube is known, the number of cubic inches of water evaporated in any proposed time becomes known. When the liquid has reached the extremity B of the scale, fresh water is added to the tube by the funnel J. The delicacy of the indications of the instrument is such, that at a mean temperature, and at an average state of dryness of the atmosphere, the water in the tube A B will move at the rate of about 2 inches per hour. Having determined the rate of evaporation from the surface of water as compared with that which takes place from the surface of damp calico, the indications of the instrument may be readily reduced so as to give the rate of evaporation from the surface of still water. By means of this instrument the following laws of evaporation have been established :—

1. Other things being the same, the rate of evaporation is nearly proportional to the difference of the temperatures indicated by the wet- and dry-bulb thermometers.

2. Other things being the same, the augmentation of evaporation, due to air in motion, is nearly proportional to the velocity of the wind.

3. Other things being the same, the evaporation is nearly inversely proportional to the pressure of the atmosphere.

By means of Apjohn's formula, or by Glaisher's multipliers and a special constant, this instrument therefore may be used as a hygrometer, giving indications vastly more delicate, if not more reliable, than those of the wet- and dry-bulb thermometer commonly employed. It is believed that the proposed instrument would supply a desideratum in meteorological observations, inasmuch as it would give with the utmost delicacy the evaporating capacity of the atmosphere, or, what amounts to the same thing, the comparative dryness and salubrity of the atmosphere extending over given intervals of time; whereas other hygrometers only give the state of the atmosphere at the particular time of observation. When used as a hygrometer, the instrument should be placed in a situation where there are no currents of air.

Results of Experiments relating to Absorption.

The following experimental researches form a continuation of those given by me in the Philosophical Magazine for 1860–61.

1. When water is diffused from a central point through the pores of an absorbent (with a certain proviso), nearly equal volumes of water are absorbed in equal times.

Dry calico, in four folds, was laid on the tube of the absorbometer represented in fig. 2. At first the absorption was very rapid, but after the lapse of a few minutes it became nearly uniform; thus the number of minutes corresponding to each successive interval of 2 inches on the tube were 3·16, 3·3, 3·5, 3·6, 3·8, 3·8.

Plaster of Paris being placed on the tube, gave the following results: the numbers of seconds corresponding to successive intervals of 4 inches on the tube were 55, 60, 62, 64.

For a very porous sandstone, the numbers of seconds corresponding to successive intervals of 4 inches on the tube were 70, 75, 80, 80.

For a sandstone of greater density and closer in its pores, the numbers of minutes corresponding to the absorption of successive half cubic inches of water were 36, 39, 41. Similar results were obtained for fine sand, wood, and other porous substances.

A sector of unsized paper was placed on the absorbent tube, first with its surface in a horizontal position, and second with its surface bent vertically downwards. In the first case, the numbers of minutes corresponding to successive equal quantities of absorption were 20, 20, 21, 21·5, 22, 23, 23·2, 23·4; whereas in the second case the times for the same amount of absorption were 18, 18, 19, 19, 20, 20·5, 21, 21.

In like manner a sector of calico, in four folds, was placed on the absorbent tube: in the horizontal position one-half of a cubic inch of water was absorbed in 95 minutes, whereas in the vertical position the same volume of water was absorbed in 87 minutes.

This experiment shows that the rate of absorption is slightly affected by the gravity of the liquid absorbed. When water is diffused from a central point in the surface of the absorbent (as in Exp. 5, Phil. Mag. vol. xx. p. 500), the rate of diffusion (within certain limits of range) in an upward direction is the same, or practically the same, as it is in the horizontal direction. But it appears from the foregoing experiment that, when the upward and downward currents are divided, the effect of the gravity of the liquid becomes appreciable. Within short distances of the central point of diffusion, the force arising from the gravity of the liquid absorbed is exceedingly small as compared with the force of absorption; but as the former is an accumulative force, whilst the latter is a constant force acting against a continually increasing resistance, the effects of the gravity of the liquid at length become appreciable. The law of absorption, therefore, given in the article above referred to, must only be accepted with these limitations.

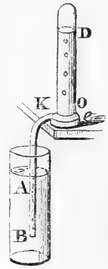
2. The rate of absorption (in the case of substances composed of loose material) is not much affected by a reduction of the pressure of the liquid on the absorbent.

Thus an experiment was made with a calico absorbent under different pressures: viz., first, when the pressures on both sides of the absorbent were the same, that is, equal to that of the atmosphere; and second, when the pressure of the liquid on the under side of the absorbent was less than the atmospheric pressure on the upper side by the pressure of a column of water 7 inches high. For equal volumes of water absorbed, the times in the two cases were 13·5 minutes and 16 minutes respectively.

3. The rate of absorption increases with the diameter of the liquid circle in contact with the surface of the absorbent; also, in the case of sheets of paper and textile fabrics, it increases with the number of the sheets, or with the thickness of the material placed in contact with the liquid.

4. Under constant pressure, the rate at which a SIPHON-FILTER transmits water is nearly the same for all lengths of the filter, provided the length is not less than a certain minimum corresponding to a maximum of discharge.

Fig. 4.



Let K A B (fig. 4) be a siphon-filter of calico, transmitting water from the tube of the second absorbometer which has been described; A B a glass cylinder nearly filled with water, in which the lower end of the filter is inserted. By lowering or elevating the cylinder, any proposed length may be given to the filter, and the times required for discharging a cubic inch of water from the tube O D at the different lengths of the filter can be accurately ascertained. Thus it was found that the times requisite for the discharge of one cubic inch of water were as follows:—

viz.	25	minutes	with a filter	3	inches long
	20·5	„	„	6	„
	20	„	„	8	„
	20	„	„	13	„
	20·3	„	„	25	„

Here the minimum length corresponding to maximum discharge exceeds 3 inches; but for lengths of filter exceeding this, the rate of discharge is very nearly constant. The filter in this experiment was fine cloth 1½ inch in width.

5. When a portion of the filter of the last experiment is placed horizontally, the rate of discharge (with a certain proviso) varies inversely as the length of this horizontal portion.

The results of experiment were correctly represented by the formula

$$v=\frac{38{\cdot}2}{\mathrm{L}+5},$$

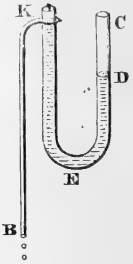
where L is put for the length of the horizontal portion in inches, and v is the corresponding weight of the discharge of water in grains per minute.

Here a constant is added to the actual length of the horizontal portion of the filter, this constant being the distance due to the initial velocity; when $\mathrm{L}=0$, the initial velocity is 7·6. When L was 12 inches, the rate of discharge per minute was found to be 2·25, which by the formula would be 2·24; when L was 6 inches, v was found to be 3·53, which by the formula would be 3·47; when $\mathrm{L}=3{\cdot}6$ inches, v was found to be 4·49, which by the formula would be 4·44; and so on.

6. When a siphon-filter transmits water under a reduced pres-

sure, the decrements of the rate of transmission vary directly as the column of liquid equivalent to the reduction of pressure.

Fig. 5.



Let K E C (fig. 5) represent a U-tube filled with water; K B a siphon-filter, saturated with moisture, placed on the orifice K in contact with the water, a small weight being laid upon the end of the filter to keep it in position: then the water being discharged by the siphon-filter causes the liquid to descend on the side C D of the tube, so that the reduction of pressure on the filter at any instant is measured by the column of descent C D. The rate of discharge is generally expressed by the formula

$$v=a-bh,$$

where v represents the velocity of discharge per minute, corresponding to h the descent of the column C D. When $h=0$, $v=a$, which is the initial velocity; and bh is the decrement of velocity due to the descent h, which is proportional to h.

The formula closely expressing the results of experiment was found to be

$$v=\cdot35-\cdot05h;$$

so that for $h=1, 2, 3, 4, 5, 6$, the corresponding values of v are ·3, ·25, ·2, ·15, ·1, and ·05 respectively; but by experiment these velocities were found to be ·3, ·25, ·19, ·14, ·1, and ·055 respectively.

When the extremity B of the filter is inserted in a large vessel of water, and the length K B of the filter is less than the depth C D of the column, the current of the water is reversed; that is, the water is transmitted through the pores of the filter into the tube, and the water D will rise in the tube. When the absorbent is horizontal, the following law of transmission obtains:—

7. When an absorbent transmits water into a closed vessel or tube containing water under a reduced pressure, the rate of transmission varies directly as the column of liquid measuring the reduced pressure; that is, the rate of transmission, the length of the absorbent being constant, is expressed by the formula

$$v=bh;$$

where v is put for the rate at which the water is transmitted per minute at the corresponding depth h of the liquid measuring the reduction of pressure, and b a constant depending on the size and nature of the absorbent, and the relative dimensions of the essential parts of the apparatus.

The formula expressing the results of experiment was found to be

$$v = \cdot 075h;$$

so that for $h=1, 2, 3, 4, 5, 6, 7$, the values of v are ·075, ·15, ·225, ·3, ·375, ·45, ·525 respectively; but by experiment these velocities were found to be respectively ·078, ·15, ·22, ·3, ·36, ·43, ·52.

[To be continued.]

XVIII. *On Transcendental and Algebraic Solution.—Supplementary Paper.* By JAMES COCKLE, *M.A., F.R.A.S., F.C.P.S. &c.*[*]

IT is not, for the purposes of my paper in the last May Number, necessary to deal with more than one root of $fx=0$. Assume

$$-\frac{dx}{da}=p+qx+rx^2+\ldots+tx^{n-1};$$

form the equation

$$fx=(p+qx+rx^2+\ldots+tx^{n-1})\mathrm{F}x,$$

and put it under the form[†]

$$\mathrm{P}+\mathrm{Q}x+\mathrm{R}x^2+\ldots+\mathrm{T}x^{n-1}=0$$

by eliminating x^n, x^{n+1}, x^{n+2}, &c. Then the n linear equations

$$\mathrm{P}=0,\quad \mathrm{Q}=0,\quad \mathrm{R}=0,\ldots \mathrm{T}=0$$

will determine the n quantities $p, q, r, \ldots t$. Thus, for the cubic

$$x^3-3x+2a=0, \quad \ldots\ldots\ldots \quad (1)$$

we have

$$fx=2,\quad \mathrm{F}x=3(1-x^2),$$

$$-\frac{dx}{da}=p+qx+rx^2,\quad \mathrm{P}=3(p+2aq)-2,$$

$$\mathrm{Q}=6(ar-q),\quad \mathrm{R}=-3(p+2r),$$

whence[‡], clearing of fractions, &c.,

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[*] Communicated by the Author.

[†] The deduced form

$$\mathrm{P}+\mathrm{Q}x+\mathrm{R}x^2+\ldots+\mathrm{T}x^{n-1}$$

of a rational function of x is the remainder after a division by fx. Hence it is attainable by division and readily (more particularly where the coefficients are all numerical) by Horner's synthetic division. When x^n, x^{n+1}, &c. are eliminated by substitution, the higher powers should be eliminated first, and in order of magnitude.

[‡] For

$$p=\frac{2}{3(1-a^2)},\quad q=\frac{-a}{3(1-a^2)},\quad r=\frac{-1}{3(1-a^2)}.$$

$$3(a^2-1)\frac{dx}{da}=x^2+ax-2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(a)}$$

Differentiating this, and transposing,

$$3(a^2-1)\frac{d^2x}{da^2}=(2x-5a)\frac{dx}{da}+x.$$

Multiplying this result into $3(a^2-1)$, and reducing by means of (a), we find

$$\begin{aligned}3^2(a^2-1)^2\frac{d^2x}{da^2}&=(2x-5a)(x^2+ax-2)+3(a^2-1)x\\ &=-3ax^2-(2a^2+1)x+6a; \quad . \quad . \quad . \qquad \text{(b)}\end{aligned}$$

whence, putting $\frac{1}{3}=m\sqrt{-1}$,

$$(1-a^2)\frac{d^2x}{da^2}-a\frac{dx}{da}-m^2x=0, \quad . \quad . \quad . \quad . \qquad (2)$$

the differential resolvent to which, in the Number for November 1860, we were led by an entirely different process*. But (2) is equivalent to the symbolical equation

$$\left(\sqrt{1-a^2}\,.\,\frac{d}{da}\pm m\right)\left(\sqrt{1-a^2}\,.\,\frac{d}{da}\mp m\right)x=0,$$

whence†

$$\begin{aligned}x&=\left(\sqrt{1-a^2}\,.\,\frac{d}{da}\mp m\right)^{-1}\left(\sqrt{1-a^2}\,.\,\frac{d}{da}\pm m\right)^{-1}0\\ &=\left(\sqrt{1-a^2}\,.\,\frac{d}{da}\mp m\right)^{-1}e^{\mp m\sin^{-1}a}\\ &=\mp\frac{1}{2m}e^{\mp m\sin^{-1}a}.\end{aligned}$$

Hence, introducing arbitrary constants and substituting for m, the general expression for x is‡

$$x=\mathrm{K}_1e^{\frac{\sin^{-1}a}{3\sqrt{-1}}}+\mathrm{K}_2e^{-\frac{\sin^{-1}a}{3\sqrt{-1}}}=\mathrm{A}\sin\left(\frac{\sin^{-1}a}{3}+\mathrm{B}\right). \quad . \qquad (3)$$

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[*] The process given in the May Number leads to

$$\frac{dx}{da}=\frac{2}{3(1-x^2)}=\frac{x}{3(a-x)}=\frac{x(a-x_2)(a-x_3)}{3(a-x)(a-x_2)(a-x_3)}=\frac{a^2x+ax^2-2a}{3(a^3-a)},$$

which reduces itself to the result (a) of the text.

[†] I have found it often convenient to represent by $(a][b)$ the product of a and b treated as ordinary algebraical quantities. Thus we see at once that

$$\left(\sqrt{1-a^2}\,.\,\frac{d}{da}\pm m\right]\left[\sqrt{1-a^2}\,.\,\frac{d}{da}\mp m\right)=(1-a^2)\frac{d^2}{da^2}-m^2,$$

and the accuracy of the symbolical decompositions given in the text are manifest.

[‡] If the first coefficient of a linear differential equation of the second

Next, to determine these arbitrary constants. Multiply (1) into 4, and in the product substitute for x its value given by (3). Then, eliminating the cube of the sine by the known formula of trigonometry, the result is

$$3(A^3-4A)\sin\left(\frac{\sin^{-1}a}{3}+B\right)$$
$$-A^3\sin(\sin^{-1}a+3B)+8a=0,$$

which is satisfied if

$$A^3-4A=0,\quad A^3=8,\quad \cos 3B=1,\quad \sin 3B=0;$$

that is, r being an integer, if

$$A=2,\quad 3B=2r\pi.$$

Hence
$$x=2\sin\left(\frac{\sin^{-1}a+2r\pi}{3}\right).$$

This discussion embraces the "irreducible case;" but if a be greater than 1 we must employ logarithmic in place of trigonometric forms. Putting (2) under the form

$$(a^2-1)\frac{d^2x}{da^2}+a\frac{dx}{da}-m^2x=0, \quad . \quad . \quad . \quad . \quad (4)$$

where $m=\frac{1}{3}$, we find that (4) is* equivalent to the symbolical equation

$$\left(\sqrt{a^2-1}\,.\frac{d}{da}\pm m\right)\left(\sqrt{a^2-1}\,.\frac{d}{da}\mp m\right)x=0,$$

whence†

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order be divided by the last, and the square root of any multiple of the quotient be integrated, the form of the integral occasionally suggests a convenient transformation. Thus, for (2) let

$$t=\int\frac{da}{\sqrt{1-a^2}}=\sin^{-1}a;$$

then, a being determinable as a function of t (for $a=\sin t$), if t be made the independent variable, we see *à priori* that the first and last coefficients of the transformed equation will be constant. In the present case, indeed, all three are constant.

[*] In this case

$$t=\int\frac{da}{\sqrt{a^2-1}}=C\log(a\pm\sqrt{a^2-1});$$

and, as before, a is determinable as a function of t, and all the coefficients of the transformed equation are constants, the middle one vanishing.

[†] In the equation

$$\left(\psi x\,.\frac{d}{dx}+\chi x\right)^{-1}\rho x=W,$$

W is always determinable as a function of x. For

$$\psi x\,.\frac{dW}{dx}+\chi x\,.\,W=\rho x$$

is a linear differential equation.

$$x=\left(\sqrt{a^2-1}\,.\,\frac{d}{da}\mp m\right)^{-1}\left(\sqrt{a^2-1}\,.\,\frac{d}{da}\pm m\right)^{-1}0$$

$$=\left(\sqrt{a^2-1}\,.\,\frac{d}{da}\mp m\right)^{-1}(a\mp\sqrt{a^2-1})^m$$

$$=\mp\frac{1}{2m}\cdot\frac{1}{(a\pm\sqrt{a^2-1})^m}.$$

Hence, introducing arbitrary constants, effecting an obvious reduction, and substituting for m, the general expression for x is

$$x=\mathrm{C}_1\sqrt[3]{a\pm\sqrt{a^2-1}}+\mathrm{C}_2\frac{1}{\sqrt[3]{a\pm\sqrt{a^2-1}}}$$

$$=\mathrm{C}_1\sqrt[3]{a\pm\sqrt{a^2-1}}+\mathrm{C}_2\sqrt[3]{a\mp\sqrt{a^2-1}}.$$

By substitution in (1) we are led to

$$0=(\mathrm{C}_1{}^3+\mathrm{C}_2{}^3+2)a+$$
$$3(\mathrm{C}_1{}^2\,\mathrm{C}_2-\mathrm{C}_1)\sqrt[3]{a^2\pm\sqrt{a^2-1}}+3(\mathrm{C}_1\mathrm{C}_2{}^2-\mathrm{C}_2)\sqrt[3]{a\mp\sqrt{a^2-1}},$$

the dexter of which will vanish, independently of a, if

$$\mathrm{C}_1{}^3+\mathrm{C}_2{}^3+2=0,\ \text{and}\ \mathrm{C}_1\mathrm{C}_2=1.$$

Hence the values of the arbitrary constants may be written

$$\mathrm{C}_1=(-1)^{\frac{1}{3}},\quad \mathrm{C}_2=(-1)^{\frac{2}{3}},$$

and we may put

$$x=(-1)^{\frac{1}{3}}\mathrm{R}_1+(-1)^{\frac{2}{3}}\mathrm{R}_2.$$

These agree with known results. Further, assuming for the solution of (2), or rather of (4), the series

$$\Sigma\alpha_r a^r,$$

r being taken from zero to infinity, the form of those differential equations shows that the above breaks up into two independent series, and that we may assume

$$x=\mathrm{D}\Sigma\alpha_{2r}a^{2r}+\mathrm{E}\Sigma\beta_{2r+1}a^{2r+1},$$

D and E being arbitrary constants, and α and β being determined by the conditions

$$\frac{\alpha_{2r+2}}{\alpha_{2r}}=\frac{(6r-1)(6r+1)}{(6r+3)(6r+6)},$$

$$\frac{\beta_{2r+3}}{\beta_{2r+1}}=\frac{(6r+2)(6r+4)}{(6r+6)(6r+9)},$$

in the sinister of which α_0 and α_1 may, in consequence of the arbitrary nature of the multipliers D and E, each be taken as unity. Now, when $a=0$, then

$$x=\mathrm{D}=0,\ \text{or}\ \sqrt{3},\ \text{or}\ -\sqrt{3};$$

and, ω denoting an unreal cube root of unity, we may write

$$D_m = (\omega^m - \omega^{2m}) \sqrt{-1}.$$

Again, determining E by the condition that the whole series must change sign when a changes sign, and that at the last of the set of real values we have

$$x = -2, \text{ or } 1, \text{ or } 1$$

in a succession corresponding to that given above, we find

$$E_m = \omega^m + \omega^{2m}.$$

Consequently the relation

$$x_m = (\omega^m - \omega^{2m}) \,.\, \sqrt{-1} \,.\, \Sigma\alpha_{2r}\, a^{2r} + (\omega^m + \omega^{2m}) \Sigma\beta_{2r+1}\, a^{2r+1}$$

will, when m is replaced by 0, 1, and 2 successively, give the three values of x.

The foregoing is a complete illustration of the process in its application to cubics. Quadratics lead to a linear differential equation. In the case of the higher equations, series may be obtained corresponding to that above given, even though radicals corresponding to R_1 and R_2 have no existence. The increasing complexity of the process as we pass the fifth degree may perhaps be met by the following modification of it. Let there be given

$$x^n + lx^{n-1} + mx^{n-2} + \ldots + r = 0.$$

Change this equation into

$$x^n + l\lambda(a)x^{n-1} + m\mu(a)x^{n-2} + \ldots + r\rho(a) = 0,$$

where $\lambda, \mu, \ldots \rho$ are functional symbols which, a being replaced by unity, or by c, satisfy the respective sets of conditions

$$\lambda(1) = 1, \quad \mu(1) = 1, \ldots \rho(1) = 1,$$

or

$$\lambda(c) = 1, \quad \mu(c) = 1, \ldots \rho(c) = 1,$$

but which are in other respects arbitrary. Then if, treating a as the independent variable, and $l, m, \ldots r$ as constants, and applying the foregoing process, we can, by means of the arbitrary constitution of $\lambda(a), \mu(a), \ldots \rho(a)$, obtain the $n-1$ particular integrals of the differential resolvent, the n values of x must be sought by writing 1, or c, in place of a in those integrals, introducing arbitrary constants, and pursuing a path already traced in the case of cubics. This modification of a process which I glanced at in the August (1860) Number, would render it unnecessary to deal with more than one parameter.

4 Pump Court, Temple, London,
November 5, 1861.

XIX. *Remarks on* Ampère's *Experiment on the Repulsion of a Rectilinear Electrical Current on itself. By* Professor VAN BREDA *of Haarlem, in a Letter to* JAMES D. FORBES, *D.C.L., F.R.S., V.P.R.S.E., Principal of the United College, St. Andrews**.

[Plate I. fig. 7.]

To James D. Forbes, Esq., F.R.S.L. & E.

SIR,

IN the February Number of the Philosophical Magazine for this year, you have published the description of some experiments tending to prove that, contrary to Ampère's theory, the contiguous parts of an electric current attract instead of repel each other. And in the April Number of the same Journal, Mr. Croll of Glasgow endeavours to show that the experiment by which Ampère believed he had proved the reality of this repulsion might be explained in an entirely different manner. The result of experiments, partly old and partly new, which we have made, leads us to differ from you as to the import and significance of the results which you have obtained; and from Mr. Croll, as to the validity of his explanation. We shall describe these experiments and communicate the reflections which they have suggested, requesting you to publish them in the same Journal if they seem to you to be worth the attention of physicists.

We shall commence with Mr. Croll's article. We shall not discuss it from the theoretical point of view, nor ask the author how, if he admits that two currents, one of which is directed towards their point of intersection, and the other from it, repel each other when they make any given angle with each other, it is possible to deny this repulsion in the single case in which this angle is equal to two right angles, that is, when the two currents form part of the same rectilinear current. Such discussions appear to us only of relative value; they can only serve to decide a question when this cannot be decided experimentally. We have resorted therefore to experiment to determine the value of Mr. Croll's experiment.

At a height of about 8 inches above a wooden trough A B (fig. 7, Plate I), divided into two compartments for Ampère's

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* Communicated by Principal Forbes. The paper by Mr. Forbes, referred to in this communication, appeared in the Philosophical Magazine for February last. An interesting experiment by Professor Tait of Edinburgh, in which the Ampèrian repulsion was distinctly proved in a homogeneous conductor composed of mercury alone, is printed in this Magazine for April 1861, but has probably escaped the notice of Professor van Breda. We are authorized to state that Professor Tait's experiment had already removed all doubt from Principal Forbes's mind as to the reality and energy of the Ampèrian repulsion.—ED.

experiments, two small mercury cups C were placed upon a suitable support. In these cups were placed the two pointed ends of a conductor of copper wire about a millimetre in diameter; they were bent in such a manner as to neutralize the action of the earth's magnetism, and to present a horizontal part D D at right angles to the partition between the two compartments. By altering the height of the support, the distance between this horizontal part of the conductor and the surface of the mercury in the compartments could be changed. Two fixed conductors, E E, connect one end of each of these compartments with one of the mercury cups. By connecting the two other ends of these compartments by means of the binding-screws F F with the poles of a battery, the current passed through the mercury from one end of the compartment to the other, then by one of the fixed conductors through the moveable conductor, and then returned to the other pole of the battery through the second fixed conductor, and the mercury in the second compartment. When, first, the two cups were fixed at such a height that the part D D of the moveable conductor was only about a centimetre distant from the mercury in the trough (that is, about half as distant as the same part of one of Ampère's floating conductors), and when the current was closed, there was no perceptible motion in the conductor. At this distance, therefore, the crossed currents were not strong enough to displace visibly a part of a conductor which a breath could deviate from its direction; and yet an Ampère's floater placed in the mercury in the same trough after the moveable conductor had been removed, was briskly repelled by the current of the same battery, which consisted of six Bunsen's elements united in a series of three double elements.

You see the result of this experiment is not favourable to Mr. Croll's point of view. To make it conclusive, it was necessary to determine the mechanical force necessary to move each of the two conductors, and to show that it was less for our moveable conductor than for Ampère's floater. We intended to make it, but in repeating the experiment with this floater a very simple expedient presented itself, which, had the idea occurred to us sooner, would have rendered superfluous all the apparatus described above. It is to place this conductor, so to speak, in the contrary direction in the mercury, that is to say, so that the parts *a b* and *d c* (see the illustration to Mr. Croll's paper, page 248, vol. xxi. of the Philosophical Magazine), instead of being directed from the curvature *b c* towards the points P and N where this current enters, should be directed towards the other end of the trough. Now, reasoning on Mr. Croll's hypothesis, the floater ought still to move from the points P and N when the current is closed; for there has been no change in its direction either in the mercury or in the part *b c* of the floater. But the very opposite takes place; the floater moves briskly towards the points P and N, just as is required by Ampère's experiment. Here, it appears to us, is a result which scarcely agrees with the explanation of the motion of this floater by the action of crossed currents. How, then, is it to be explained, if not by Ampère's theory itself? If there is any doubt on this point, we call to remembrance the experiment which we published three years ago in the French journal *Cosmos*. Permit us to describe it:—

"One of us succeeded, twelve years ago, in showing directly the repulsion of the parts of the same current, by employing a dozen iron bullets 8 or 9 millims. in diameter, suspended like the ivory balls in experiments on the impact of bodies, touching each other, and so that their centres were in the same right line. By means of conductors suitably arranged and moveable in mercury, the two terminal bullets could be connected with the two poles of a battery; there was besides on each a light thin rod, by which their motion could be read off on a graduated scale. When the current from ten Grove's elements was passed through the bullets, the terminal ones were seen to diverge about a millimetre, and between each pair of bullets small sparks were continually seen to pass. When the battery was not too strongly charged, and the action of the current not continued too long, so as to prevent a permanent connexion in consequence of a fusion of the bullets at the point of contact, the bullets, so soon as contact was broken, were seen to approach each other and resume their primitive position, which proved that the effect was not due to heat, and gave a confirmation of Ampère's law at once simple and direct.

"But," you will perhaps say, "you have appealed to facts, and my experiments furnish facts which demonstrate an attraction instead of a repulsion of the contiguous parts of the same conductor." But these experiments, Sir, are they conclusive? You will permit us to doubt it. To justify this doubt, we commence by clearly distinguishing two circumstances which are confounded in your experiments, and which nevertheless are in our eyes essentially different. While a conductor remains charged with electricity, however strongly, it does not become heated; there is no special action on the magnetic needle, no chemical action is produced in its interior. It is only when and whilst the electrical condition of its particles changes, and whilst a current traverses it, as we usually say, that all these effects are produced. These instances, and many others, would prove to demonstration that there is a fundamental difference between the phenomena produced by statical and dynamical electricity, if a thesis so generally admitted required proof. But whenever in your experiments you have observed a true attraction at a distance, the two parts of your conductor were necessarily disjoined; that is to say, there could be no current other than that which is propagated by the sparks; and you will agree with us that every spark indicates a marked difference in the electrical condition of the two surfaces between which it springs, a difference which ought necessarily to produce an attraction between the two. The current transmitted by these sparks is always too feeble, in too small quantity, as would formerly have been said, to produce any appreciable mechanical effect. But in making contact between the two portions of your conductor, there was certainly a current; an attraction at a distance could not be perceived, but instead of it you have seen an adhesion which lasted some time *after the current had been broken.* Could not this persistence be an indication that this adhesion is not a *direct* effect of the current (the rupture of which ought to stop it instantaneously), but is a secondary effect? We think so. In our opinion the explanation of the phenomenon is as follows.

As soon as the current is closed, the moveable current *is repelled.* It only, however, moves to a slight distance, which cannot be otherwise, seeing that the force of repulsion, not very great itself, must overcome that of torsion while only acting during an excessively short time, and on a relatively considerable mass. It would go further than it does, if at the same moment that it quits the conductor an induction spark, a small voltaic arc, did not instantaneously pass between the two. This arc, as is always the case when it springs between two fusible electrodes placed at a very small distance, is almost entirely composed of a kind of melted metal. It is broken after a very short existence; but the rupture of the circuit brings about a new induction with its arc, which also quickly breaks, and so on. You have often heard these sparks: this is the *fizzing noise* of which you speak. The interposition of an induction-coil in the circuit, which, as you found, promotes the success of your experiments, does so only by giving rise to stronger sparks. Without that its effect would be absolutely inexplicable, for its presence can only weaken the continuous current. And further, if you still possess the conductors which you have used in your experiments, be good enough to examine, through a lens, the ends by which they touched; you will readily see the traces, the relics, so to speak, of the melted wire of which we have spoken. This wire prevents the two conductors from removing too far from each other, just as a very small drop of liquid adhering to their surfaces would, if introduced between the two. After the rupture of the circuit, it solidifies instantaneouslyand keeps them soldered to each other. The unequal contraction of the two parts of the conductor, of different form and often of two different metals, sometimes causes a spontaneous rupture of this junction after a longer or shorter time, as you have observed.

There is fortunately one means of preventing this welding; and that is by using a metal, mercury, which is liquid to begin with, and then you can observe the Ampèrian repulsion. In the same article of *Cosmos*, of which some lines have been cited, we have described an experiment which has much analogy with your own. We described it thus:—

"On one of the arms of a small and delicate balance is suspended a copper-wire conductor about 3 millims. in diameter, and bent in the form of an inverted U. The other arm is loaded with weights which almost counterbalance the conductor; that being done, the two ends of the latter are respectively immersed in two mercury cups, placed at such a height that they are not immersed to a greater depth than 2 millimetres at most. As soon as the circuit is closed so that a pretty strong current passes from the mercury to the conductor, and from the other end of the conductor to the mercury in the other cup, the conductor is briskly repelled out of the mercury; the current is broken, and the conductor falls again to be again repelled, and so on indefinitely*."

The current for this experiment ought to proceed from four Grove's elements, each presenting an active surface of 12 square inches, and connected so as to form a series of two elements of double the size. By using a weaker current, the repulsion is not strong enough to throw the conductor entirely out of the mercury; it merely rises a little as soon as the current begins to pass. We have lately improved the apparatus by fixing at the other end of the balance a small glass disc, on which there is a scale divided into tenths of a millimetre. This can be observed by means of a small fixed microscope magnifying about twenty times, and with cross-wires on its ocular. In addition to this, platinum wires are soldered to each of the ends of the bent conductor; they are about a millimetre in diameter, and are covered with glass so as only to leave the two ends free. The results obtained by means of the apparatus thus modified have appeared interesting to us, because they have confirmed us in our point of view, which, in the explanation of all the motions cited, as well as in that of the phenomena observed with hollow spheres by Mr. Gore, and in Mr. Page's experiment with the Trevelyan instrument, only attributes a secondary part to the heat developed by the current; it refers some exclusively, and others principally, to the Ampèrian, just as you have hitherto done. In the case of Mr.

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* We have since learnt that this experiment had been previously made by Mr. Faraday.

Gore's sphere, which touches each of the two rails in almost a mathematical point, the heat developed at the point of contact may in fact be one of the causes of motion: if we were unaware of the repulsion which ought to be developed on this point independently of the heat, the cause of the phenomenon would have to be attributed to the latter. In the case of the vibrating blocks of Trevelyan this explanation becomes less probable, because the weight of the upper block ought to render the two surfaces in contact much greater. In fact if their surfaces are increased *n* times, the heat developed, and the resultant sudden expansion, ought to be n^2 times less. If now one of the two is a liquid metal, like mercury, which assumes exactly the extent of surface of the solid conductor immersed in it, and which ought therefore to touch this in all points, the expansion produced by the heat developed in these points of contact ought to be very small. We nevertheless see that the repulsion takes place; and its effect is readily distinguished from that produced by the heat which causes the mercury and all the vertical parts of the conductor to expand, by the fact that it ceases instantaneously with the rupture of the circuit, whereas heat requires a tolerably long time to become dissipated. In observing, through the microscope, the apparatus just described, as soon as the two cups are connected with four or six Bunsen's elements arranged in a series of elements of double surface, as indicated for the experiments with the floater, we see the disc suddenly sink, and then the moveable conductor rise about a tenth of a millimetre. This motion is immediately succeeded by another in the same direction, but far slower, due to the expansion by heat. When the circuit is broken, either after the lapse of a few seconds or after ten minutes, the conductor is seen at the same moment to fall again, exactly as it had been raised at the commencement, and then sink with great slowness in consequence of cooling.

This, Sir, is what we had to communicate to you. If our experiments still do not seem to you entirely conclusive, you will oblige us by saying so before the publication of this communication, so that we may, if possible, be able to modify them or amplify them according to your desires.

Receive, Sir, the assurances of our very distinguished consideration*.

J. G. S. van Breda.

Teylerian Laboratory, Haarlem,
October 1861.

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* Though this letter is throughout in the plural number, it bears the signature of M. van Breda alone.—Edit.

XX. *Remarks on* M. Hermite's *Argument relating to the Algebraical Resolution of Equations of the Fifth Degree.* *By* G. B. JERRARD*.

1. I WAS not aware of what M. Hermite had written with respect to the impossibility of effecting generally the algebraical resolution of equations of the fifth degree, until I saw an abstract of his argument in a paper by Mr. Cockle which appeared in the last Number of the 'Quarterly Journal of Pure and Applied Mathematics.' The abstract to which I refer is this:—

"M. Hermite's argument may help to settle a still vexed question. It is as follows:

"Let us assume that between the roots of the sextic *réduite* of the general quintic there exist relations which render that sextic an Abelian. These relations would, in effect, lead to the conclusion that the *réduite* is resolvible algebraically by quadratic and cubic radicals; and without having recourse to the demonstration of Abel, we may at once convince ourselves that it would follow that the equation of the fifth degree is resolvible by radicals of the same knd. Let us call x_0, x_1, x_2, x_3, x_4 the roots of this equation, and put

$$u = x_0x_1 + x_1x_2 + x_2x_3 + x_3x_4 + x_4x_0,$$

$$v = x_0x_2 + x_2x_4 + x_4x_1 + x_1x_3 + x_3x_0,$$

the quantities $u+v$ and uv will be, the one rational, and the other a root of an equation of the sixth degree resolvible algebraically by hypothesis. Then u and v and their various values will be expressed by means of quadratic and cubic radicals. The same conclusion will hold with respect to the more general functions

$$u_a = (x_0x_1)^a + (x_1x_2)^a + (x_2x_3)^a + (x_3x_4)^a + (x_4x_0)^a,$$

$$v_a = (x_0x_2)^a + (x_2x_4)^a + (x_4x_1)^a + (x_1x_3)^a + (x_3x_0)^a,$$

whatever be the integral exponent a. It follows that

$$x_0x_1,\ x_1x_2,\ x_2x_3,\ x_3x_4,\ x_4x_0,$$

for example, will satisfy an equation of the fifth degree, the coefficients of which will only involve radicals of the kind in question. But *with two values* of u_a and v_a it will be possible to form two equations of the fifth degree having *a common root*, for example x_0x_1, the others being different. Hence we may deduce x_0x_1, and consequently the similar function x_0+x_1, in terms of cubic and quadratic radicals; consequently also x_0 and x_1, so that the equation of the fifth degree would be resolvible without quintic radicals.

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* Communicated by the Author.

"Such a conclusion is of course inadmissible. M. Hermite's argument is given, with developments, in his "Considérations sur la Résolution Algébrique de l'équation du 5 degré." See pp. 326–336 of vol. i. of the *Nouvelles Annales de Mathématiques* (par MM. Terquem et Gerono), 1842."

I propose to examine the argument in question.

2. Denoting the function

$$x_0+\iota^n x_1+\iota^{2n}x_2+\iota^{3n}x_3+\iota^{4n}x_4 \text{ by } f(\iota^n),$$

and consequently*

$$x_0+\iota^{4n}x_1+\iota^{3n}x_2+\iota^{2n}x_3+\iota^{n}x_4 \text{ by } f(\iota^{4n}),$$

we see that the 5×5 terms which compose the product

$$f(\iota^n)\,f(\iota^{4n})$$

will collapse into

$$\begin{aligned}&\Sigma(x_h)^2,\\ &+(\iota^n+\iota^{4n})\Sigma(x_h\,x_{1+h}),\\ &+(\iota^{2n}+\iota^{3n})\Sigma(x_h\,x_{2+h}),\end{aligned}$$

Σ indicating, in each case, the sum of all the terms which arise on putting h successively equal to 0, 1, 2, 3, 4, and x_5, x_6 being such as to take the places of x_0, x_1 respectively in a cycle of the roots arranged in the order: x_0, x_1, x_2, x_3, x_4.

3. Here we at once recognize M. Hermite's functions u and v; the former of which has for its general term $x_h\,x_{1+h}$, the latter $x_h\,x_{2+h}$. Thus u and v are linked together in the equation

$$f(\iota^n)\,f(\iota^{4n})=\mathfrak{S}2+(\iota^n+\iota^{4n})u+(\iota^{2n}+\iota^{3n})v, \quad . \quad . \quad (e)$$

as will be seen on writing, in accordance with the notation of my 'Essay on the Resolution of Equations†,' $\mathfrak{S}2$ for $\Sigma(x_h)^2$.

4. We can, too, without the aid of this equation, prove the truth of what he says respecting the rationality of $u+v$.

For

$$u+v=\Sigma(x_h\,x_{1+h})+\Sigma(x_h\,x_{2+h});$$

and since any one of the ten terms which enter into the two functions characterized by Σ differs from all the rest in both groups, it is clear that all the $\dfrac{5\times 4}{1\times 2}$ distinct combinations of the assigned form are exactly comprised in the expression for $u+v$. Hence $u+v$, being a symmetrical function of the roots of the equation in x, must admit of being expressed in rational terms of the coefficients A_1, A_2, .. A_5.

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* Observing that

$$\iota^{2(4n)}=\iota^{5n+3n}, \quad \iota^{3(4n)}=\iota^{10n+2n}, \quad \iota^{4(4n)}=\iota^{15n+n}.$$

† Published by Taylor and Francis, Red Lion Court, Fleet Street, London.

But it remains to consider uv.

5. From the equation (e) there spring at once

$$f(\iota)\,f(\iota^4)=\mathfrak{S}2+(\iota+\iota^4)u+(\iota^2+\iota^3)v,$$
$$f(\iota^2)f(\iota^3)=\mathfrak{S}2+(\iota^2+\iota^3)u+(\iota+\iota^4)v.$$

Now the product of the two first members of these two equations, or the function

$$f(\iota)\,f(\iota^2)\,f(\iota^3)\,f(\iota^4),$$

must manifestly be symmetrical with respect to u and v. It is, in effect, expressible by

$$\begin{aligned}&(\mathfrak{S}2)^2\\ &+(\iota+\iota^2+\iota^3+\iota^4)\mathfrak{S}2(u+v)\\ &+(\iota+\iota^4)(\iota^2+\iota^3)(u^2+v^2)\\ &+\{(\iota+\iota^4)^2+(\iota^2+\iota^3)^2\}uv.\end{aligned}$$

And if from this expression we eliminate u^2+v^2 by means of the identical equation

$$u^2+v^2=(u+v)^2-2uv,$$

we shall immediately obtain

$$f(\iota)\,f(\iota^2)\,f(\iota^3)\,f(\iota^4)=\mathrm{L}+\lambda uv\,; \quad . \quad . \quad . \quad . \quad (\epsilon_1)$$

in which L is rational and even integral relatively to $\mathrm{A}_1, \mathrm{A}_2, \ldots \mathrm{A}_5$ (arts. 4, 5), and λ is a numerical constant.

6. According, then, to M. Hermite's theory,

$$f(\iota)\,f(\iota^2)\,f(\iota^3)\,f(\iota^4)$$

ought—on the hypothesis that the equation in x admits generally of a finite algebraical solution—to involve no radical higher than a cubic. His conclusion, however, appears to me to be quite untenable. It follows, indeed, from what has been demonstrated in my 'Essay,' that the fifth power of the function in question,

$$[f(\iota)]^5\;[f(\iota^2)]^5\;[f(\iota^3)]^5\;[f(\iota^4)]^5,$$

depends directly on an Abelian equation, and therefore involves in its solution quadratic and cubic radicals only. But although $f(\iota)\,f(\iota^2)\,f(\iota^3)\,f(\iota^4)$ is, as well as its fifth power, six-valued, we cannot, with the aid of Lagrange's theory of homogeneous functions, establish a rational communication between the two functions

$$f(\iota)\,f(\iota^2)\,f(\iota^3)\,f(\iota^4), \qquad [f(\iota)]^5\;[f(\iota^2)]^5\;[f(\iota^3)]^5\;[f(\iota^4)]^5,$$

as I pointed out to Mr. Cayley in the Philosophical Magazine for May 1861.

7. Setting aside, therefore, the objection raised from the theory of Lagrange, we may now see clearly the way in which quintic radicals enter into the expression for uv.

For since, in accordance with what has been already stated, we are permitted to assume

$$[f(\iota)]^5\,[f(\iota^2)]^5\,[f(\iota^3)]^5\,[f(\iota^4)]^5=\mathrm{K}, \quad . \quad . \quad . \quad . \qquad (\epsilon_2)$$

K being a root of an Abelian equation, (ϵ_1) will take the form

$$\sqrt[5]{\mathrm{K}}=\mathrm{L}+\lambda uv. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (\epsilon_1, \epsilon_2)$$

8. We might, indeed, without considering at all the ground of M. Hermite's argument, have inferred, from the very enunciation of the result at which he had arrived, that an error must somewhere have crept into his processes. How, in effect, can we reconcile such a result with the possibility of solving binomial equations of the fifth degree, not to speak of any other class of solvable equations of that degree into the expressions for whose roots irreducible radicals of the form $\sqrt[5]{z}$ enter? But I thought that it was due to a mathematician so eminent to trace his error to its source. Trusting implicitly in the theory of Lagrange, M. Hermite was led, as we have seen, to suppose that in the equation (ϵ_1, ϵ_2) the function designated by $\sqrt[5]{\mathrm{K}}$ would be generally expressible in rational terms of K. What a confirmation is here of the truth of the solution given in my 'Essay'!

December 1861.

XXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 79.]

April 18, 1861.—Major-General Sabine, R.A., Treasurer and Vice-President, in the Chair.

THE following communication was read:—

"On the Effect produced on the Deviation of the Compass by the Length and Arrangement of the Compass Needles; and on a New Mode of correcting the Quadrantal Deviation." By Archibald Smith, Esq., M.A., F.R.S.; and Frederick John Evans, Esq., R.N.

When the length of the compass needle may be neglected compared with the distance of the iron which acts on a ship's compass, the deviation is accurately expressed by the formula

$$\sin\delta=\mathrm{A}\cos\delta+\mathrm{B}\sin\zeta'+\mathrm{C}\cos\zeta'+\mathrm{D}\sin(\zeta+\zeta')+\mathrm{E}\cos(\zeta+\zeta');$$

in which ζ is the azimuth of the ship's head measured *eastward* from the *correct magnetic north*;

ζ' is the same azimuth, but measured from the direction of the *disturbed needle*;

$\delta=\zeta-\zeta'$ is the *easterly* deviation of the needle;

A, D, E are coefficients depending on the distribution of the soft iron of the ship.

B and C are coefficients depending partly on the distribution of the hard and soft iron of the ship, and partly on the magnetic dip and horizontal force at the place.

In all ships which have been examined, A and E are so small that they may be neglected; and, if the deviation be of such an amount that we may take δ for $\sin\delta$,

$$\delta = \mathrm{B}\sin\zeta' + \mathrm{C}\cos\zeta' + \mathrm{D}\sin(\zeta+\zeta').$$

The first two terms represent the "Semicircular" deviation, the last term the "Quadrantal" deviation.

The "semicircular" deviation is, on the plan proposed by the Astronomer Royal, and extensively practised in the mercantile marine, corrected by magnets; the "quadrantal" by masses of soft iron placed on each side and at the same level as the compass; and when the distance of the correction is sufficiently great, this correction may be considered as perfect for the time and place at which it is made; but when this is not the case, errors are introduced, which it is the object of the paper to consider.

Mr. Evans observed that the standard compass of the 'Great Eastern,' which had been corrected on Mr. Airy's plan by Mr. Gray of Liverpool, had errors of between 5° and 6° on some points. It occurred to him that this error was caused by the length of the needle and the proximity of the correctors, and to test this he made experiments on the deviation produced on needles of different length by magnets and soft iron with the following results.

With 3-inch single needles deflected by magnets, the deviations were nearly "semicircular;" but with 6-inch needles, and still more strongly with 12-inch needles, a "sextantal" error of very considerable magnitude was introduced.

With soft iron correctors deflecting a $7\frac{1}{2}$-inch single needle, in addition to the "quadrantal" deviation, a considerable "octantal" error was introduced.

When the same experiments were made with an Admiralty standard compass card, constructed as usual with four parallel needles, the extremities of which are 15° and 45° on each side of the extremities of the diameter to which they are parallel, *there was no appreciable sextantal or octantal deviation.* And on investigating the subject mathematically, it appeared that *this arrangement of needles, or the simpler arrangement of two needles each* 30° *on each side of the diameter, produces a complete compensation and correction of these errors.*

The formulæ are the following:—The diviation produced in a single needle of length $2a$ by a magnetic particle M at the same level and at a distance b, is

$$\frac{\mathrm{M}}{b^2}\cdot\frac{1}{\mathrm{H}}\left\{\left(1+\frac{3}{8}\frac{a^2}{b^2}\right)\sin\zeta' + \frac{15}{8}\frac{a^2}{b^2}\sin 3\,\zeta'\right\},$$

giving a sextantal deviation bearing to the semicircular proportion of

$$\frac{15}{8}\frac{a^2}{b^2} : 1+\frac{3}{8}\frac{a^2}{b^2}.$$

If the compass has two needles the ends of each α° from the ends of a diameter, the deviation is

$$\frac{\mathrm{M}}{b^2}\cdot\frac{1}{\mathrm{H}}\left\{\left(1+\frac{3}{8}\frac{a^2}{b^2}\right)\sin\zeta'+\frac{15}{8}\cdot\frac{a^2}{b^2}\frac{\cos 3\,\alpha}{\cos\alpha}\cdot\sin 3\,\zeta'\right\}.$$

So that if $\alpha=30°$, or if the two needles be each 30° on each side of the diameter which is parallel to them, the sextantal term disappears.

If we have four needles the ends of each pair $\alpha°$ and $\alpha'°$ from the ends of a diameter, the sextantal term has a factor,

$$\cos 3\,\frac{\alpha+\alpha'}{2}.$$

Showing that if, as in the Admiralty compass, the needles of each pair are placed at equal distances on each side of the lines of 30°, the sextantal deviation will be reduced to zero.

A similar investigation shows that the same arrangement of needles reduces to zero the octantal error introduced by the too great proximity of the soft iron ; and further, the error introduced by the magnetism of the needle inducing magnetism in the soft iron in its vicinity.

The conclusion of the authors is that by the use of the Admiralty standard compass, or of a compass with two needles each 30° from the diameter parallel to them, the correcting magnets and soft iron correctors may be placed much nearer the compass than can safely be done with a single needle compass card, and the large deviations found in iron ships far more accurately corrected.

Correction of the Quadrantal Deviation.—It has long been known that two compasses placed as in the common double binnacle, produce in each other a *negative* quadrantal deviation. The discussion by Mr. Evans of the deviations of all the iron-built ships in the Royal Navy (Phil. Trans. 1860, p. 337), showed that the quadrantal deviation in such ships is always *positive* ; and as there is great difficulty and inconvenience in the usual mode of correcting large quadrantal deviation by soft iron, it occurred to Mr. Evans that the correction might be made by the reciprocal action of two compasses placed at the distance of 18 to 24 inches from each other, as in the common double binnacle. The precautions to be used are that the two compasses must be of equal power, and as the correction is inversely as the earth's horizontal force at the place, if great accuracy is required there ought to be the means of adjusting the distance of the two compasses, and the method will probably be found inapplicable in very high magnetic latitudes.

The deflection so produced by one compound compass on a like compass is

$$-\frac{3\mathrm{M}}{b^2}\frac{1}{\mathrm{H}}\,a\cos\alpha\left\{\left(1+\frac{5}{3}\frac{a^2}{b^2}\cos^2\alpha\right)\sin 2\,\zeta'-\frac{35}{b}\frac{a^2}{b^2}\frac{\cos 3\,\alpha}{\cos\alpha}\sin 4\,\zeta'\right\}.$$

In this arrangement therefore an octantal error is introduced which may be avoided by the use of two Admiralty standard cards, or cards with two needles each 30° from the diameter which is parallel to them.

April 25.—Major-General Sabine, Treas. and V.P., in the Chair.

The following communication was read:—

"On the Distribution of Aqueous Vapour in the Upper Parts of the Atmosphere." By Lieut.-Col. Richard Strachey, F.R.S.

The experiments of chemists having shown that any gas will flow into a space occupied by another gas, and diffuse itself there as though the space were a vacuum and the second gas not there, it was suggested by Dalton that the atmosphere might be considered to be a combination of as many distinct atmospheres as it has gaseous components, and that the actions of each of these might be treated of separately, and irrespective of the others. Meteorologists, pursuing this idea, have proposed to separate the pressure of the aqueous vapour from the whole barometric pressure of the atmosphere, and thence to infer the pressure of the permanently elastic portion, or as it has been called, the *Gaseous Pressure,* or the *Pressure of the Dry Air.*

It is my object to inquire how far the facts of the matter will support Dalton's suggestion of the possible independent existence of an atmosphere of aqueous vapour, and whether we can in truth eliminate the pressure of this vapour by subtracting the observed tension from the total barometric pressure, in the manner that has commonly been done of late years.

And first as to the hypothesis of an atmosphere of aqueous vapour pressing only upon itself. If there be such an atmosphere, the general laws of pressure of elastic fluids will apply to it, as they do to the mixed atmosphere. But in consequence of the small specific gravity of the vapour, the rate of the diminution of pressure in the upper strata of the vapour atmosphere would be much slower than in the mixed atmosphere; and irrespective of any variation in the law of the decrease of temperature, the height to which we should have to ascend in the vapour atmosphere to produce a given diminution of pressure, would be to the corresponding height in the mixed atmosphere inversely as the specific gravities of the atmospheres, that is, as 1 to ·625 or as 8 to 5. Thus by ascending about 19,000 feet in the atmosphere, the barometric pressure is found to be reduced one-half; and consequently it would be necessary to ascend to about $\frac{8}{5}$ of 19,000 feet, or upwards of 30,000 feet, to produce a corresponding diminution of pressure in a vapour atmosphere, or to reduce the tension, say from 1 inch to $\frac{1}{2}$ an inch.

Now let us compare this result with the observed facts. This is done in the annexed Table I., in which the ratio of the tensions of the vapour at heights extending to 20,000 feet with the surface tension, as actually observed, is set down in juxtaposition with the ratios that should hold good in an independent vapour atmosphere. We here see that in reality the tension is reduced to one-half of what it is at the earth's surface by an ascent of about 8000 feet, instead of 30,000 feet, as the hypothesis of the independent vapour atmosphere would require.

TABLE I.—*Comparison of Tensions of Dalton's Hypothesis with those actually observed.*

Height above the earth's surface in feet.	Barometric pressure of the entire atmosphere.	Calculated tensions in atmosphere of aqueous vapour alone, that at the earth's surface being 1·0.	Ratio of tensions to surface tension actually observed.		
			By Dr. Hooker on the Himalaya.	By Mr. Welsh in four balloon ascents.	At Dodabetta & Mahabaleshwar (fide Col. Sykes).
	in	in.			
0	30·0	1·00	1·00	1·00	
2,000	28·0	·96	·82	·88	
4,000	26·1	·92	·68	·77	·67
6,000	24·3	·88	·62	·58	
8,000	22·6	·84	·52	·45	·47
10,000	21·0	·80	·42	·35	
12,000	19·5	·77	·35	·30	
14,000	18·0	·73	·29	·19	
16,000	16·6	·70	·25	·18	
18,000	15·3	·67	·20	·16	
20,000	14·1	·64	·16	·12	

The facts, as indicated by the long series of observations, of which the Table contains an abstract, are altogether in accordance with the results of my own observations; but I have thought it better to rest my conclusions on the facts observed by others. Further, the discrepancy between the observations and the hypothesis is so great, and so constant with reference to all the localities, the Himalaya, England, and the mountains of the south of India, while the observations are themselves so thoroughly consistent, that the conclusion is inevitable that the hypothesis is untenable.

A similar conclusion as to the entire incompatibility of the hypothesis of a separate vapour atmosphere with the facts, may be drawn quite independently of any observation of tensions, from a mere consideration of the known laws of the diminution of temperature as we ascend. An argument, something to this effect, will be found in Bessel's paper on Barometric Heights *; but its form being too mathematical to be generally intelligible, I shall endeavour to place the matter in a rather more popular point of view.

Let us suppose, then, that we are at a place at the sea-level where the temperature of the air is 80°, the tension of vapour being ·80, which would make the dew-point 72°·5—a case that must be of constant occurrence. If, now, we rose gradually above the earth's surface, the temperature of the air would be reduced at the known rate of about 3° for 1000 feet; while the tensions of vapour, and the corresponding dew-points, calculated upon the hypothesis of an atmosphere of vapour pressing upon itself, would be as follows:—

	ft.		
At	0 .. Tension ·80 ..	Dew-point 72°·5 ..	Air 80°·0
	1000 .. „ ·78 ..	„ 71·8 ..	„ 77·0
	2000 .. „ ·77 ..	„ 71·2 ..	„ 74·0
	3000 .. „ ·75 ..	„ 70·5 ..	„ 71·0
	4000 .. „ ·74 ..	„ 69·9 ..	„ 68·0

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* Astronomische Nachrichten, Nos. 356, 357; and Taylor's Scientific Memoirs, vol. ii. p. 517.

Hence, up to about 3000 feet, the temperature of the air would be found to be higher than the dew-point, and the supposed tensions might of course exist. But the temperature of the air, it will be seen, diminishes much more rapidly as we ascend than that of the dew-point; and the former will therefore soon fall below the latter. Thus at 4000 feet, the air being at 68°, the theory demands vapour, with a dew-point of 69°·9, which is impossible; for any vapour, in excess of that corresponding to the air temperature 68°, would be instantly precipitated. In like manner it might be shown that, under all conceivable conditions of heat or cold, and of damp or dryness at the surface of the earth, we could always ascend to a height where the diminution of temperature would render the progression of the tensions according to the presumed law impossible. We may therefore conclude generally, that the known diminution of temperature in the atmosphere is incompatible with the existence of so large a quantity of vapour in the upper strata as the theory in question demands; and, consequently, that the tensions observed at the surface are neither dependent on, nor balanced by, the pressure of the vapour in the higher parts of the atmosphere (in the way in which the entire barometric pressure depends on the weight of the whole superincumbent column of air), for this would be insufficient to produce them. To render an independent vapour atmosphere possible would, indeed, require a fall of temperature in the air of about 1° for 1500 feet, or less than a quarter of that which really takes place.

It will also follow that, as the tension of vapour at any point exceeds the sum of all the pressures of the vapour above it, it must in part be due to the reaction of the air particles, which must therefore press upon those of vapour, contrary to the supposition with which we started. This is, in fact, equivalent to saying that the air offers a resistance to the diffusion of vapour, instead of having no effect whatever in obstructing it; and thus from an erroneous assumption, based upon experiments made on very small quantities of air in confined vessels, arises the fallacy of the theory I have been considering.

I am aware of no systematic observations relative to the actual distribution of vapour in the atmosphere, excepting those made by Dr. Joseph Hooker, and published in his Himalayan Journals*. He found in his journeys in Sikim, which extended to heights of 18,000 feet and upwards, that the quantity of vapour was dependent rather on the temperature of the air than on anything else, and that it was, in fact, simply a certain proportion of the maximum quantity that can exist in accordance with the conditions of temperature at any altitude, the relative quantity being pretty nearly constant throughout the whole column. These conclusions of Dr. Hooker are altogether corroborated by my own observations. In the annexed Tables I have further illustrated this. In Table II. I have shown for a considerable range of temperature at the earth's surface, the proportion of vapour that would be found at various heights in the atmosphere, as compared to that at the surface (which is in each case assumed to be represented by 1·0), supposing the air to be everywhere

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* Himalayan Journals, vol. ii. p. 422.

saturated with moisture, and the reduction of temperature for ascent to be 3° for 1000 feet. To these calculated ratios are added those actually observed by Dr. Hooker in the Eastern Himalaya, and by Mr. Welsh in his balloon ascents*, as already given in a preceding page.

TABLE II.

Proportion of Vapour at various Altitudes.

Height in feet.	Calculated, the temperature of air at the surface being				Observed,	
	80°.	60°.	40°.	20°.	By Dr. Hooker, in Sikim—Therm. at sea-level being 70° to 90°.	By Mr. Welsh, in a Balloon—Therm. at surface being 50° to 70°.
0	1·00	1·00	1·00	1·00	1·00	1·00
2,000	·82	·81	·79	·77	·82	·88
4,000	·67	·65	·62	·58	·68	·77
6,000	·54	·52	·48	·41	·62	·58
8,000	·44	·41	·36	·34	·52	·45
10,000	·35	·32	·28	·26	·42	·35
12,000	·28	·25	·21	·19	·35	·30
14,000	·22	·19	·16	·14	·29	·19
16,000	·18	·15	·12	·10	·25	·18
18,000	·14	·12	·09	...	·20	·16
20,000	·11	·08	·07	...	·16	·12

In Table III. the results of Dr. Hooker's and my own observations are given in more detail.

The accordance between the calculated and observed quantities of vapour shown by these figures is so close, that we can have no hesitation in admitting that Dr. Hooker's conclusion will, in all probability, be found to afford the general solution of this problem. The relative quantity of vapour in the case of his observations having been rather greater in the higher than in the lower strata of the atmosphere, is a circumstance which cannot be held to affect the general truth of our conclusions, as will be perfectly accounted for by supposing that the diminution of temperature with height was less rapid in reality, than 3° for 1000 feet, on which my calculation is based. It is further worthy of notice, that the calculated proportion of vapour at various altitudes varies but little even with considerable change of the surface temperature, though there is a manifest tendency for the upper strata to contain a rather higher per-centage with a high than with a low temperature; a result likewise in accordance with fact, so far as we are able to judge from the comparison of Dr. Hooker's Indian observations with those made in England by Mr. Welsh.

The precise determination of the entire pressure of the vapour thus shown by observation to be suspended in the atmosphere is a matter of some difficulty; but an approximation may be made to it as follows:—

Let us suppose the weight of the vapour to be measured, as is often

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* Phil. Trans. 1853, p. 311.

TABLE III.

Ratio of Tensions observed at various Altitudes on the Himalaya to the Tension at the Sea-level.[1]

Heights.	Jan.		Feb.		March.		April.		May.		June.		July.		August.		Sept.		Oct.		Nov.		Dec.		Mean.		General mean by observa-tions.	Theoretical ratio by calcula-tion.
	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.	A.	B.		
Sea-level	1·00	1·00	1·00	1·00	1·00	1·00	1·00	1·00	1·00	1·00	1·00	1·00	1·00	1·00	1·00	1·00	1·00	1·00	1·00	1·00	1·00	1·00	1·00	1·00	1·00	1·00	1·00	1·00
1 to 2,000 feet......	·95	·83	..	..	·70	..	·70	·90	·69	..	..	..	..	..	..	..	..	·82	..	..	·89	..	·85	..	·80	·85	·81	·86
3,000 ,, ...	..	·89	..	·85	..	..	..	·71	·64	..	..	·89	..	..	..	..	..	·86	..	·73	·78	..	·92	..	·78	·82	·81	·78
4,000 ,, ...	·78	..	..	..	..	..	..	·83	·62	..	..	..	..	..	..	..	..	..	·58	..	·67	·81	·71	..	·67	·82	·71	·71
5,000 ,, ...	·70	..	..	..	·48	..	..	·78	·49	..	..	·75	..	·72	..	..	..	..	·55	·70	·76	·67	·67	..	·61	·72	·66	·64
6,000 ,, ...	·58	·58	..	·54	..	·69	..	·71	·53	·67	..	·61	..	·65	·68	·62	..	·60	..	·60	·75	·58	·63	·74	·63	·63	·63	·57
7,000 ,, ...	·58	..	·58	..	..	..	..	·68	·54	..	..	·65	..	·60	·58	·59	..	·60	·48	·60	..	..	·58	..	·56	·62	·59	·52
8,000 ,, ...	·55	..	·46	..	·45	..	·43	·63	·45	..	·50	·53	·57	..	·56	..	·53	..	·48	·46	·60	..	·48	..	·51	·54	·51	·47
9,000 ,, ...	·48	..	..	·36	..	·61	·37	..	·46	..	·45	·47	·46	..	·52	..	..	..	·40	·40	·49	..	..	..	·45	·46	·46	·42
10,000 ,, ...	..	..	..	..	..	..	..	..	..	..	·34	·49	..	..	..	..	..	..	..	·36	·42	..	·48	..	·41	·43	·42	·37
11,000 ,, ...	..	..	..	..	..	..	..	..	·37	..	·36	·36	·38	·40	..	..	..	..	·36	·47	·42	..	·35	..	·37	·41	·39	·34
12,000 ,, ...	..	..	..	..	..	..	..	..	..	..	..	·34	·39	·32	·39	·33	·38	·34	·28	·29	·36	..	·38	..	·36	·36	·35	·30
13,000 ,, ...	..	..	..	..	..	..	..	..	..	..	·37	·36	..	·29	..	·32	..	..	·31	..	·27	..	..	..	·32	·32	·32	·27
14,000 ,, ...	..	..	..	..	..	..	..	..	..	..	·33	..	..	·23	·31	..	..	..	..	..	·20	..	·23	..	·27	·23	·26	·24
15,000 ,, ...	..	..	..	..	..	..	..	..	..	..	..	..	..	·16	..	·21	..	·23	..	·17	..	..	..	..	..	·19	·19	·21
16,000 ,, ...	..	..	..	..	..	..	..	..	..	..	..	..	..	·16	·29	·13	·29	·13	·26	..	..	..	·23	..	·27	·14	·21	·19
17,000 ,, ...	..	..	..	..	..	..	..	..	..	..	..	..	..	·14	·26	·18	·17	..	·16	..	·09	..	·23	..	·18	·16	·18	·17
18,000 ,, ...	..	..	..	..	..	..	..	..	..	..	..	..	..	·15	..	..	·21	..	..	..	..	..	..	..	·21	·15	·18	·15
19,000 ,, ...	..	..	..	..	..	..	..	..	..	..	..	..	..	·14	..	·15	·22	..	·13	..	..	..	..	..	·18	·15	·16	·13

NOTES.

A. Ratios from observations by Dr. Hooker in Sikim, referred to Calcutta.

B. Ratios from observations by Lieut.-Col. Strachey in Kumaon, referred to Fatehgarh.

The Theoretical ratios are calculated as explained; the temperature at the surface being taken as 80°, and the decrement 3° in 1000 feet.

done in the case of the entire atmosphere, by the height of a column of the density observed at the surface. The height of a homogeneous atmosphere of vapour, equivalent to an independent vapour atmosphere, on Dalton's hypothesis would obviously be $\frac{8}{5}$ of the height of the homogeneous air atmosphere, that is $\frac{8}{5}$ of 26,250 feet, or about 42,000 feet.

But the vapour actually existing is much less than this. Taking the results of Dr. Hooker's observations, and considering the density at the surface to be unity, the mean density of the whole vapour below 20,000 feet will readily be calculated to be about ·47; so that the whole of the vapour up to this height would be equivalent to a homogeneous column of 9460 feet of density 1·0. Now it may be assumed approximately that the quantity of vapour above 20,000 feet will bear the same relation to the entire quantity, as holds good between the densities at that height and at the surface; and as we see from the Table that the density at 20,000 feet is $\frac{16}{100}$ of what it is at the surface, we may infer that this is the proportion of the vapour above that altitude, the remainder, or $\frac{84}{100}$, being below it. Consequently the whole quantity of vapour, according to Dr. Hooker's observations, would be equivalent to a homogeneous column of $\frac{100}{84} \times 9460$, or 11,260 feet. Using the balloon observations, the height would be rather less than this, viz. 10,050 feet, so that we may infer that the actual pressure of the vapour in the atmosphere is to that represented by the tension at the surface of the earth, as 10,500 to 42,000, or as about one to four; and this ratio would also subsist between the actual pressures and observed tensions at all elevations.

The problem might otherwise be solved, by comparing the diminution of density as we ascend, according to Dalton's hypothesis, and the observations, as shown by the series of figures in Table I. This diminution, it will be seen, takes place in all the series, approximately in a geometrical ratio, so that the density is reduced nearly in an equal proportion for each 2000 feet of ascent, namely, from 1·00 to ·96, that is by $\frac{4}{100}$, on Dalton's hypothesis; from 1·00 to ·84, that is by $\frac{16}{100}$, according to Dr. Hooker; and from 1·00 to ·82, that is by $\frac{18}{100}$, according to Mr. Welsh. Now it follows, from an obvious mathematical law, that the entire quantities of vapour in these different cases are inversely proportional to the constant reduction of density; so that the quantity on Dalton's hypothesis, which is that represented by the observed tension at the surface, is to the quantity according to Dr. Hooker, as sixteen to four, and to the quantity according to Mr. Welsh, as eighteen to four, a result nearly identical with the former. The subtraction of the observed tension of vapour from the total barometrical pressure, in the hope of obtaining the simple gaseous pressure, must consequently be denounced as an absurdity; and the barometrical pressure thus corrected, as it is called, has no true meaning whatever.

In conclusion, I would remark that the consideration of the small quantity of vapour that is disseminated in the upper parts of the atmosphere, shows us that inequalities of level on the earth's surface, which are insignificant when viewed in relation to the dimensions of the globe, become objects of the greatest importance in connexion with the atmosphere which surrounds it. Three-fourths of the whole mass of the air is within range of the influence of the highest mountains; one-half of the air and nearly nine-tenths of the vapour are concentrated within about 19,000 feet of the sea-level, a height which hardly exceeds the mean level of the crest of the Himalaya; while one-fourth of the air and one-half of the vapour are found below a height of 8500 feet. Thus, mountains even of moderate magnitude may produce important changes in very large masses of the atmosphere, as regards their movements, their temperature, and their hygrometric state; and especially in those strata that contain the great bulk of the watery vapour, and that have the greatest effect therefore in determining the character of climate.

XXII. *Intelligence and Miscellaneous Articles.*

PHYSICAL CONSIDERATIONS REGARDING THE POSSIBLE AGE OF THE SUN'S HEAT. BY PROFESSOR W. THOMSON*.

THE author prefaced his remarks by drawing attention to some principles previously established. It is a principle of irreversible action in nature, that, "although mechanical energy is indestructible, there is a universal tendency to its dissipation, which produces gradual augmentation and diffusion of heat, cessation of motion, and exhaustion of potential energy, through the material universe." The result of this would be a state of universal rest and death, if the universe were finite and left to obey existing laws. But as no limit is known to the extent of matter, science points rather to an endless progress through an endless space, of action involving the transformation of potential energy through palpable motion into heat, than to a single finite mechanism, running down like a clock and stopping for ever. It is also impossible to conceive either the beginning or the continuance of life without a creating and overruling power. The author's object was to lay before the Section an application of these general views to the discovery of probable limits to the periods of time *past* and *future*, during which the sun can be reckoned on as a source of heat and light. The subject was divided under two heads: 1, on the secular cooling of the sun; 2, on the origin and total amount of the sun's heat.

In the first part it is shown that the sun is probably an incandescent liquid mass radiating away heat without any appreciable compensation by the influx of meteoric matter. The rate at which heat is radiated from the sun has been measured by Herschel and Pouillet independently; and, according to their results, the author estimates that if the mean specific heat of the sun were the same as that of liquid water, his temperature would be lowered by 1°·4 Centigrade annually. In considering what the sun's specific heat may actually be, the author first remarks that there are excellent reasons for believing that his substance is very much like the earth's. For the last eight or nine years, Stokes's principles of solar and stellar chemistry have been taught in the public lectures on natural philo-

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* Communicated by the author, having been read at the Meeting of the British Association at Manchester, September 1861.

vol. xxxi. Abstr. A, p. 27.

sophy in the University of Glasgow; and it has been shown as a first result, that there *certainly is sodium in the sun's atmosphere.* The recent application of these principles in the splendid researches of Bunsen and Kirchhoff (who made an independent discovery of Stokes's theory), has demonstrated with equal certainty that there are iron and manganese, and several of our other known metals in the sun. The specific heat of each of these substances is less than the specific heat of water, which indeed exceeds that of every other known terrestrial solid or liquid. It might therefore at first sight seem probable that the mean specific heat of the sun's whole substance is less, and very certain that it cannot be much greater, than that of water. But thermodynamic reasons, explained in the paper, lead to a very different conclusion, and make it probable that, on account of the enormous pressure which the sun's interior bears, his specific heat is more than ten times, although not more than 10,000 times, that of liquid water. Hence it is probable that the sun cools by as much as 14° C. in some time more than 100 years, but less than 100,000 years.

As to the sun's actual temperature at the present time, it is remarked that at his surface it cannot, as we have many reasons for believing, be incomparably higher than temperatures attainable artificially at the earth's surface. Among other reasons, it may be mentioned that he radiates heat from every square foot of his surface at only about 7000 horse-power. Coal burning at the rate of a little less than a pound per two seconds would generate the same amount; and it is estimated (Rankine, 'Prime Movers,' p. 285, edit. 1859) that in the furnaces of locomotive engines, coal burns at from 1 lb. in 30 seconds to 1 lb. in 90 seconds per square foot of grate-bars. Hence heat is radiated from the sun at a rate not more than from fifteen to forty-five times as high as that at which heat is generated on the grate-bars of a locomotive furnace, per equal areas.

The interior temperature of the sun is probably far higher than that at the surface, because conduction can play no sensible part in the transference of heat between the inner and outer portions of his mass, and there must be an approximate *convective* equilibrium of heat throughout the whole; that is to say, the temperatures at different distances from the centre must be approximately those which any portion of the substance, if carried from the centre to the surface, would acquire by expansion without loss or gain of heat.

Part II. *On the Origin and Total Amount of the Sun's Heat.*

The sun being, for reasons referred to above, assumed to be an incandescent liquid now losing heat, the question naturally occurs, how did this heat originate? It is certain that it cannot have existed in the sun through an infinity of past time, because as long as it has so existed it must have been suffering dissipation; and the finiteness of the sun precludes the supposition of an infinite primitive store of heat in his body. The sun must therefore either have been created an active source of heat at some time of not immeasurable antiquity by an overruling decree; or the heat which he has already radiated away, and that which he still possesses, must have been acquired by some natural process following permanently established laws. Without pronouncing the former supposition to be essentially incredible, the author assumes that it may be safely said to be in the highest degree improbable, if, as he believes to be the case, we can show the latter to be not contradictory to known physical laws.

The author then reviews the meteoric theory of solar heat, and shows that, in the form in which it was advocated by Helmholz[*], it is adequate, and it is the only theory consistent with natural laws which is adequate to account for the present condition of the sun, and for radiation continued at a very slowly decreasing rate during many millions of years past and future. *But neither this nor any other natural theory can account for solar radiation continuing at anything like the present rate for many hundred millions of years.* The paper concludes as follows :—"It seems therefore, on the whole, most probable that the sun has not illuminated the earth for 100,000,000 years, and almost certain that he has not done so for 500,000,000 years. As for the future, we may say with equal certainty that inhabitants of the earth cannot continue to enjoy the light and heat essential to their life for many million years longer, unless new sources, now unknown to us, are prepared in the great storehouse of Creation."

DESCRIPTION OF A NEW MINERAL FROM THE URAL.

BY M. RODOSZKOVSKI.

In 1857 I discovered at Nijni-Jagurt a variety of concretionary silicate of zinc, the existence of which, as far as I am aware, was not previously known in the Ural Mountains.

It is in concretionary crusts. The surface is covered with small roughnesses, which, seen under a lens, present the appearance of tolerably lustrous indistinct crystals, which are analogous to zeolite. The colour of these is a light blue, with a tinge of green.

The hardness is 5, the specific gravity 2·707. It is soluble without effervescence in acids, gives off water when calcined; it is infusible before the blowpipe, but becomes opake when submitted to the action of the flame; it dissolves in borax, forming an insoluble glass.

The composition of this silicate of zinc, from my analyses, is—

		Oxygen.	
Silica	26·0	13·507	3
Oxide of calcium	1·55	0·43	3
Oxide of zinc	66·9	13·133	
Water	4·7	4·177	1
Oxide of copper	traces		
Protoxide of iron			

and is represented by the formula

$$3\,\text{Zn}\,\text{Si}+\text{Aq}.$$

This variety of silicate of zinc greatly resembles a variety of concretionary carbonate of zinc which I saw at London in the British Museum under the name *Smithsonite*; but as its composition, its form, and its colour differ from those of ordinary silicate of zinc, I name it *Wagite* in honour of M. Waga, the venerable naturalist of Warsaw.—*Comptes Rendus*, December 9, 1861.

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[*] Popular Lecture delivered at Königsberg on the occasion of the Kant commemoration, February 1854.

Fig. 7.

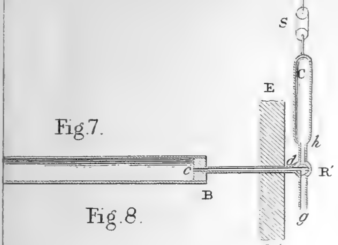
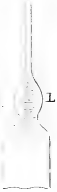


Fig. 8.

Fig. 16.



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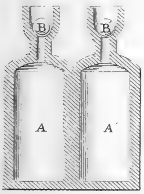

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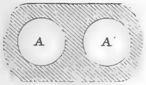

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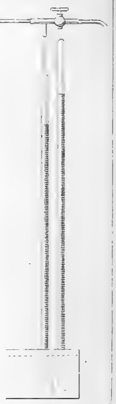

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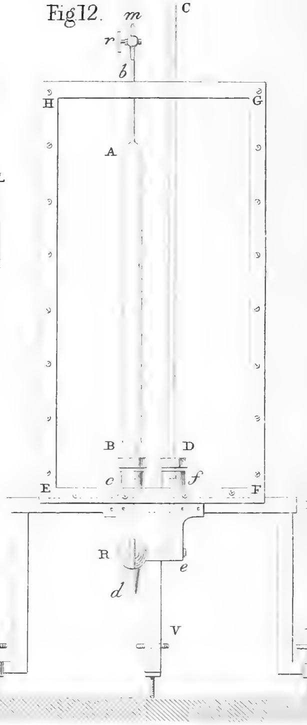

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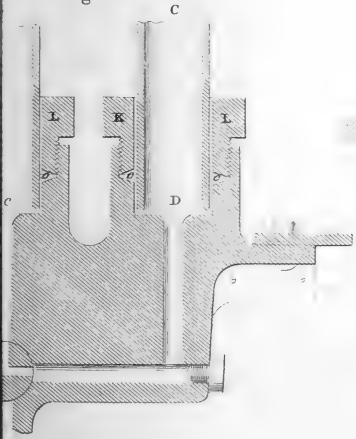

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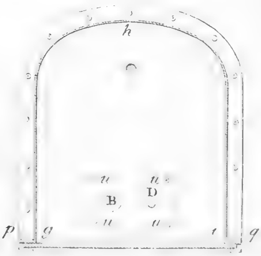

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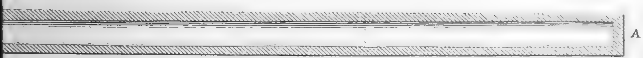

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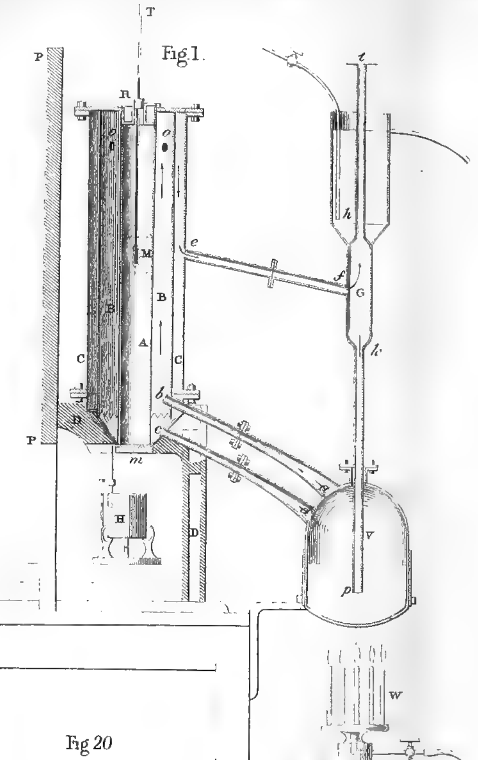

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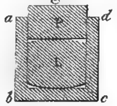

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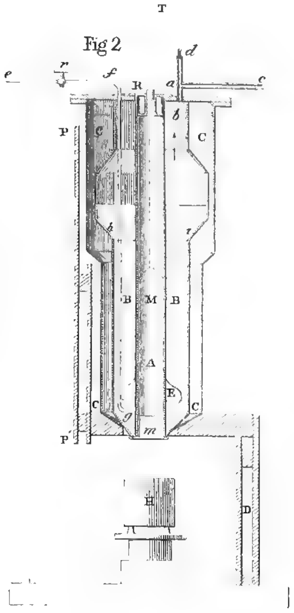

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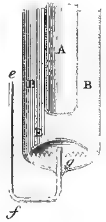

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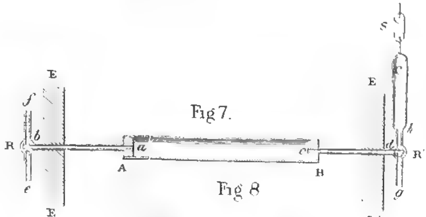

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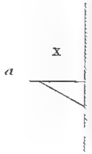

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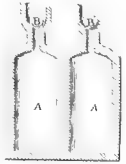

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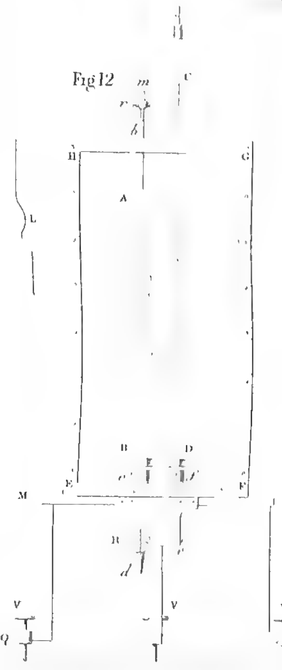

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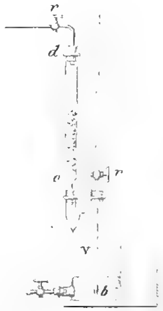

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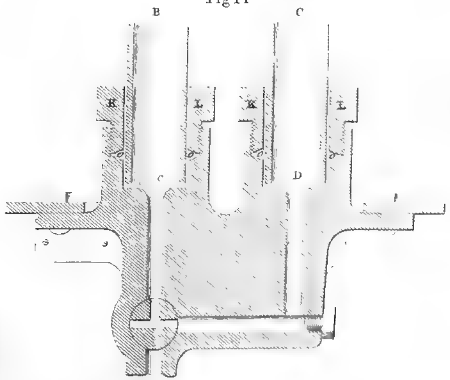

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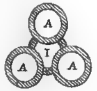

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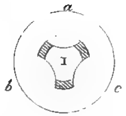

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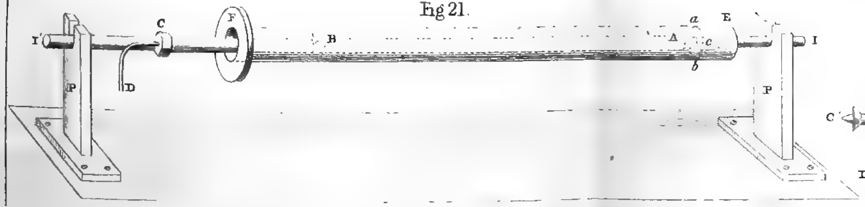

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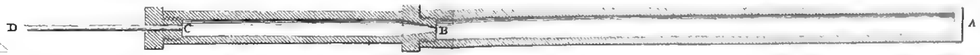
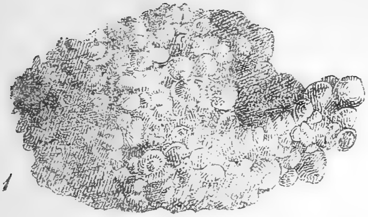


Phil. Mag. Ser. 4. Vol. 23. Pl. III.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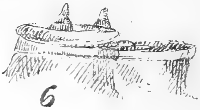
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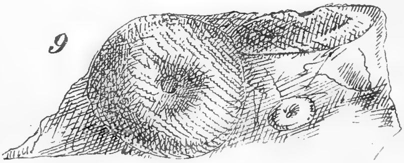
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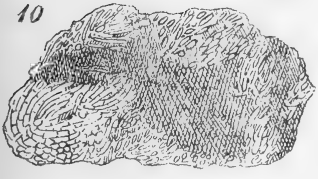
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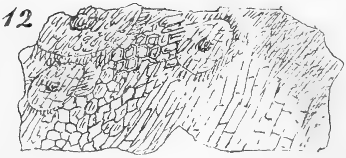
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THE
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[FOURTH SERIES.]

MARCH 1862.

XXIII. *On the Form and Distribution of the Land-tracts during the Secondary and Tertiary periods respectively; and on the effects upon Animal Life which great changes in Geographical Configuration have probably produced. By* SEARLES V. WOOD, *Jun.**

SECTION 1. Introductory.—SECTION 2. The General Geographical Configuration of the Secondary Period.—SECTION 3. The Changes in the Geographical Configuration which resulted from Post-cretaceous Volcanic Action.—SECTION 4. The Effect produced by the Post-cretaceous Geographical Changes upon the Secondary Fauna.—SECTION 5. The Preservation, at the present day, of isolated Remnants of the Secondary Continents, and of the Secondary Fauna inhabiting them.—SECTION 6. Summary and Conclusion.

SECTION 1.—*Introductory.*

THE attempt to restore in description the outline of the lands and seas of a past geological period, although but in their broadest features, and from that restoration to draw conclusions as to results emanating from changes in the distribution of the continental tracts in succeeding periods, will probably in the present state of our knowledge be, by many at least, deprecated as illusory. The consideration, however, of a few leading principles to be observed in making such an attempt will, I trust, tend to remove from the minds of some such an impression, at least sufficiently so to induce a fair consideration of the views here put forward.

It is obvious that if any tract, large or small, be submerged or elevated by subterranean action, the relative levels of all parts of the tract would, if that tract were raised or depressed by a force exerted equally on every portion, remain the same, however frequently the elevation or depression occurred. Such an elevation or depression is, it is true, dynamically impossible, as all these

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* Communicated by the Author.

elevations appear to have emanated from foci of force, where the volcanic action was the most exerted, or at least where it found the least resistance, and produced the greatest dislocations; in a word, the upheaval (however prolonged) of mountain chains, has converted large tracts falling within their influence into dry land. Now if we can in imagination remove the inequalities produced by any volcanic upheaval, and by so doing restore the surface as it existed before such upheaval took place, it is clear that we should remove the chief difficulty in arriving at a correct view of the relative configurations of the land and water during the antecedent period.

Again, consider how during every successive geological epoch since the close of the palæozoic period, but more particularly during the Jurassic and cretaceous epochs, the sea over the south-east of England and north of France has returned to the basin occupied by it during the immediately preceding geological period, where no anticlinal has interfered to change the relative levels of the surface: thus we see the outcrop of the jurassic and cretaceous formations, and even of the older tertiaries over this district, forming a series of concentric rings, the newer formation lying within the older*. If we can so plainly perceive this where powerful dislocations have taken place subsequent to the older tertiaries, which have not only interfered with the old and gradually narrowing sea-basin of the secondary and older tertiary seas, but reversed the very inclination of the surface, so that the land, from which came the sediment that supplied the clays and limestones of those ancient formations, now falls away to the west under the deepening water of the British Channel towards the Atlantic, how much more plainly ought we to perceive it in those parts of the world where the strata have remained over great areas undisturbed by anticlinals since they were deposited, as in Russia, North America, &c., places in which, if the tracts were now sufficiently depressed, the ocean would again wash almost the same coast-line which it did in the secondary periods. Even in England, which is a geological microcosm, and where a more regular succession of strata exists than in any other known tract of equal size, there is by no means that overlay of successive deposits to the extent that apparently exists, since not only does the whole jurassic series thin out as it recedes from those ancient lands the drainage of which formed the sources whence was derived the sediment of its deposits†, but the basins which the

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* It is intended only to be said that this is the result of the geological changes since the commencement of the secondary period, broadly considered, as it is well understood that numerous local interruptions occurred in this order of events, causing local absence of some of the subordinate divisions of the several secondary groups.

† See Hull, Quart. Journ. Geol. Soc. vol. xvi. p. 63.

Jurassic and lower cretaceous deposits apparently fill are found to be traversed by anticlinals of anterior origin*, which formed either peninsulas or islands in the Jurassic and Neocomian seas that occupied the South-east of England, and parts of France and Belgium, during the formation of those deposits.

Further, we find, in the case of deposits since the palæozoic period, that almost all of them have been formed in the neighbourhood of land which has supplied the material for their composition. There are exceptions, such as the cretaceous and the nummulitic series of Europe and Asia; but even these fall far short of what we should conceive to be the bed of a great ocean, such as the Pacific, were it the case that deposits took place in it of a thickness sufficient to ensure their preservation on upheaval. It has been remarked by Mr. Darwin (Origin of Species, pp. 300 and 343) that seas have been seas, and continents have been continents, for periods far greater, geologically speaking, than we have been apt to assign for their existence.

In applying these principles to elucidate the broader features of the geographical configuration at any geological period, we have to bear in mind another and even more important fact, viz. the permanence through vast periods of the general direction of the lines of volcanic eruption over a whole hemisphere: I shall at a later stage of this paper enter into some detail upon this subject, and therefore only refer here to the fact of this permanence. Consider the chain of the Andes forming a line of volcanic eruption more or less active through near 60 degrees of the earth's circumference, and prolonged for an equal distance by the chain of the Rocky Mountains, and the almost continuous volcanic band extending from the Azores in a south-easterly direction to the centre of the Pacific, and we see that the development of volcanic eruptions has been exhibited with a permanence and persistency of direction over immense areas, and may therefore well assume that the influence of this persistence upon the geographical configuration of the period during which it prevailed must have been, perhaps beyond all other things, important and enduring. Into the causes of this persistency of direction during long periods I do not pretend here to enter, further than to remark upon the insufficiency of the adventitious action of percolated water upon the metallic bases to account for it. The fact so often mooted, of the contiguity of all active volcanoes to the sea or to great inland waters, is not only explicable on othe grounds, but is, I venture to suggest, the necessary concomitant of any elevatory action acting spasmodically like that of volcanoes.

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* See Prestwich, Quart. Journ. Geol. Soc. vol. xii. p. 10; also vol. xiv p. 250; and Degousée and Laurent, Quart. Journ. Geol. Soc. vol. xii. p. 252.

If we admit that every elevation takes place at the expense of material removed from subterraneous places to the surface, the void thus caused must, even if we conceive a cavernous structure, be supplied sooner or later by other material subsiding into the cavity, so that in such case we may assume that every volcanic elevation is accompanied by a depression coequal in amount (although perhaps not in area), and also contiguous. Did mountain chains come into existence by one great catastrophe, instead of their being formed (as the evidence shows) by a multitude of minor and spasmodic volcanic outbursts, this contiguity would not so necessarily accompany the volcanic elevations; but the smallness of the effect produced by each volcanic elevation when compared with the sum of their action, as seen in mountain chains, shows that, upon the principle stated above, the depressions are contiguous. Thus, as it seems to me, every volcanic outburst has a tendency, by the contiguous depressions that it causes, to bring the drainage into its neighbourhood. This drainage is generally the ocean; but, as in the Caspian, it may be only waters having their origin from the surrounding land collected into the depressed area. And hence is it that great waters are not only contiguous to volcanoes at the present day, but that in all geological periods volcanic outbursts are associated with marine formations.

SECTION 2.—*The General Geographical Configuration of the Secondary Period.*

The volcanic forces which prevailed during the later part of the palæozoic period, at least during the carboniferous age, appear to have had a general direction from east to west. The convulsions which broke up the palæozoic deposits, and formed the mountain systems which governed the geographical configuration of the secondary period, have obliterated these features to a greater extent than have the tertiary upheavals obliterated those of secondary age; enough, however, remains to show this east and west direction in several well-marked and extensive anticlinals over the northern hemisphere which originated during the carboniferous period: witness the anticlinals of Nova Scotia, of South Scotland, of North Devon, of the Ardennes, of some of the Sierras of Spain, of Corbières in the Pyrenees*. The close of this period, however, appears to have been accompanied (or

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* As to Nova Scotia, see Dawson, Proc. Geol. Soc. vol. iv. pp. 184, 269; Quart. Journ. Geol. Soc. vol. i. pp. 26, 322; vol. iv. p. 50; vol. vi. p. 349; vol. viii. p. 398; vol. x. p. 42. As to the Ardennes, see Austen, Quart. Journ. Geol. Soc. vol. xi. p. 533. As to North Devon, Scotland, and Spain, see Murchison's 'Siluria,' London, 1854. As to Corbières, see D'Archiac, *Bull. Soc. Géol. d. France*, vol. xiv. p. 507. In addition to which

probably was caused) by an entire change in this alignement: the volcanic bands which brought into existence the extensive mountain systems which are formed out of the palæozoic strata, broken up and thrown into parallel ridges of immense extent, obliterating almost entirely the alignement which the palæozoic strata had previously possessed, appear to have burst forth, not merely in one hemisphere, but over the whole world as far as hitherto examined, in a direction more or less from north to south, and to have maintained this direction during the whole secondary period. These old volcanic bands have left their evidences in several great systems which have been examined by competent geologists, and, there is reason to believe, in other mountain chains of similar direction not yet examined. The well-marked and examined systems consist, in the northern hemisphere, of the Alleghanies*, the Oural†, and of the system of Portugal‡ prolonged into the North of England; and in the southern hemisphere, of the great system of Eastern Australia§; of like origin with which appears to be the palæozoic and schistose system of New Zealand; and lastly, the grand systems of the Rocky Mountains|| and of the Andes¶.

There seems reason also for inferring that the north and south ridges of Central and Southern Africa, crossed by the late travellers in that region (Burton, Speke, and Livingstone), of whose

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it may be added that, according to M. Abich, *Bull.* vol. xii. p. 116, a great east and west axis, presumably of carboniferous date (being formed of Devonian rock), traverses European Russia from the meridian of Smolensk to that of the Oural. M. Tchihatchef also describes similar axes running through Galatia and Paphlagonia (*Bull.* vol. viii. p. 312), and through the Antitaurus (*Bull.* vol. xi. p. 402).

* See Rogers, "Physical Structure of the Appalachian Chain," in 'Reports of Survey of Massachussetts,' p. 522 (Boston, 1838). See also Report on Geol. Explor. Pennsylvania, 1836, 1838, 1839, 1840, 1841. Report on Geol. Survey of Virginia, 1840, 1841.

† Murchison, Proc. Geol. Soc. vol. iii. pp. 398, 717. Also 'Siluria,' pp. 294 to 300, and p. 333.

‡ Sharpe, Quart. Journ. Geol. Soc. vol. vi. p. 135.

§ See Strzelecki's 'Australia,' Lond. 1845. See also Odernheimer, in Quart. Journ. Geol. Soc. vol. xi. p. 399. Clarke in same, p. 408. Selwyn, in vol. x. p. 299; vol. xiv. p. 533 (wherein the north and south strike of the palæozoic rocks and their unconformability to the secondary coal-bearing strata reposing on them is shown); vol. xvi. p. 147. Rosales, in vol. xv. p. 497 (showing the palæozoic strike below the drift).

|| See Hector, Journ. of Geograph. Soc. 1860; Edinb. New. Phil. Journ. vol. xi. p. 169; Quart. Journ. Geol. Soc. vol. xvii. p. 388. Shumard, Trans. Acad. St. Louis, vol. i. No. 3. p. 341.

¶ See Forbes, Quart. Journ. Geol. Soc. vol. xvii. pp. 38, 48. See also Darwin's 'South America' (1846), pp. 237–248, who at page 247 proceeds thus:—"Hence it would appear that the Cordillera has been probably, with some quiescent periods, a source of volcanic matter from an epoch anterior to our cretaceo-oolitic formation to the present day."

structure, however, we know nothing beyond the direction of their strike, will, when examined, present similar evidences of an origin at the close of the palæozoic period.

The way in which the secondary formations occur upon the flanks of these known systems, stretching from them in successive outcrops, indicate that throughout the secondary period the tracts falling within the influence of these volcanic bands were, with some interruption, undergoing a steady continuous elevation. Thus, to commence on the east with the Oural Mountains, we see that the elevatory action of that chain commenced after the close of the carboniferous period, but was in full action during the last age of the palæozoic period, the Permian, the deposits of which spread over large tracts, and that this action, prolonged into the secondary period, elevated the Permian deposits into ridges subordinate to the original ridge of the Oural but parallel with it, while the Jurassic deposits were formed in the same but diminished basin as that occupied by the Permian Sea, these now lying within the Permian deposits in a concentric form, precisely as we see the secondary deposits of England and France forming successive concentric rings of outcrop diminishing in the direct ratio of their age. The system of England and Portugal, although not so marked as the uninterrupted chain of the Oural, is yet distinctly apparent from a consideration of the manner in which the secondary deposits in those countries are assembled. The chief part of the Portuguese system appears now to have disappeared under the Atlantic; but the Jurassic and subcretaceous deposits which, fenced on the east by the schistose region of Eastern Portugal and Western Spain, occupy the littoral region of Central Portugal, have been shown by the late Mr. Sharpe* to have a regular outcrop along a line of strike from N. by W. to S. by E., in which the earliest-deposited Jurassic formations were elevated at intervals into ridges having this direction until the cretaceous age; while in England we find this line of the Portuguese strike traversing the island, and becoming conspicuous in the midland and northern counties of England, the volcanic outbursts appearing in the trappean beds of Skye, which there alternate with oolitic deposits. The elevatory effect of this band upon the formations of the great secondary gulf of England and Northern France I have before alluded to, in the concentric outcrop of the formations deposited in that gulf, which, like the Oural region and the secondary tract of Portugal, exhibit a gradual and successive elevation and desiccation of the sea-bottom during the whole period, at least until the cretaceous epoch.

Passing westward, we find under the Atlantic, within the

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* Quart. Journ. Geol. Soc. vol. vi. p. 135.

distance of 400 miles from the Irish shore, ridges of considerable elevation apparently parallel with the line of Portugal and England, terminating with an abrupt declivity of upwards of 7000 feet, which would appear to be the western escarpment of the Anglo-Portuguese system. Crossing the immense valley which is occupied by the Atlantic between this point and the American shore, we find a magnificent development of parallel secondary movements in the Appalachian chain and the deposits flanking it. The elaborate surveys, by the State surveyors, of the Atlantic border of the Appalachian chain, and of that chain itself, enable us to speak with precision of the phenomena attending the development of the secondary formations of that region. The parallelism of the ridges into which the palæozoic deposits have been thrown in the Alleghanies is perhaps even more marked and persistent than in the Oural chain; and the persistence of outline of the shore of the secondary oceans, exhibited by the successive outcrop of the secondary formations along the littoral border of the Alleghanies, is almost uninterrupted, and so nearly coincides with the present Atlantic shore, that if the whole region were now to be depressed to the level it occupied during any age of the secondary period, the sea would again wash a coast-line agreeing in its main features with the outcrop of the formation of that age. We see represented here the same features that occur in England and France, viz. the return of sea after sea, from the Jurassic down to and including the older tertiaries, to the same, though in most cases shrunken, bed as that occupied by its immediate predecessor. This is most conspicuous in Virginia and the States to the south of it, the outcrop of the tertiary and secondary strata successively disappearing under the Atlantic as it advances northward. Here also we see that successive desiccation to which I have adverted in the case of Russia and England and Portugal; but the succession is less regular in the Alleghany region, the newer secondaries more generally overlaying the older, and exhibiting a greater alternation of level than is the case in Europe; indeed so considerable has this alternation been, that the equivalents of the middle secondaries of Europe have not been well made out, being mostly either absent or else hidden by the overlay of the newer secondary (cretaceous) deposits.

The investigations and explorations of Marcou, Shumard, Swallow, Heyden, Meek, and many other American geologists and explorers of the formations on the western flank of the Appalachian chain warrant an inference that the secondary sea, particularly during the cretaceous periods, swept round the southern termination of the chain and filled the area now occupied by Texas, Kansas, and the Indian territory, extending thence northwards along the eastern side of the Rocky Mountains to the Polar Sea; the Alleghany region forming a great peninsula pointing southwards, and joined to a continental tract occupied by what is now the palæozoic region of the northern and north-western states of America and of Canada, and the crystalline region of the Hudson Bay territories, but separated from the Rocky-Mountain region by this secondary sea. Here, again, the same changes of level during the secondary period which the Atlantic flank of the Appalachian region presents seem repeated, the newer secondary (cretaceous) formations so overlapping the middle and older secondary that they are mostly found reposing here on the palæozoic,—the middle and older secondary formations being either absent or so far obscured that, in the present state of their knowledge of this region, the American geologists are at issue whether any formations really referable to the middle and older secondary periods have yet been found west of the Alleghanies*.

The extensive region of the Rocky Mountains, which includes within it the whole elevated tract between the Mississippi and Saskatchewan valleys on the east, and the Pacific on the west, was until recently almost an unknown region; but the report of Dr. Hector †, who, under Palliser's expedition, made a rapid survey of part of this chain, shows that, like the Alleghanies, the core of the chain consists of mural precipices of highly inclined palæozoic formations, flanked with secondary deposits lying quite unconformably on them, and stretching away from the chain with a very easy dip, the development of the cretaceous formation being such as apparently to have filled the whole valley lying between the Rocky Mountains on the west, and the palæozoic system of the United States and Canada, and the hypersthene system of the Lake and Hudson's Bay region on the east, from the coast of Texas on the south, to the Polar Sea on the north. Here again, therefore, is exhibited a yet more marked continental alignement from north to south during the secondary periods.

There remain to be noticed the great systems of the southern hemisphere, and first that of Australia. Considerable progress has been made by the surveyor of one of the eastern colonies of

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* See Marcou, 'Geology of North America' (Zurich, 1858); his views are, however, repudiated by the American geologists. See Dana, in Silliman's Journal, vol. xxvi. p. 323. Heyden and Meek in same, vol. xxvii. pp. 35, 219. This overlay of the cretaceous deposits in many parts of the northern hemisphere, together with their great extent there, appears to indicate that extensive subsidences in this hemisphere preceded that general change in the geographical alignement which in the third section I propose considering.

† See Note ‖, *ante*, p. 165.

Australia in the examination of their geological features. The similarity of these features over a large tract, coupled with the exhibition of similar features wherever exploring vessels have examined the eastern sea border, favours the inference that the whole of Eastern Australia is one geological system composed of disrupted palæozoic formations, and having a strike throughout from north to south. The resemblance of this structure to that of the Appalachian and Rocky-Mountain chains is striking. Here, as there, the Devonian and carboniferous deposits have been broken up into numerous parallel ridges from north to south, showing the origin of the system to be subsequent to the carboniferous period, but prior to the deposit of the coal-bearing strata of this continent, which, like those of India, appear to be of secondary age, these coal-bearing strata resting in Australia unconformably on the true carboniferous and older palæozoic strata*.

The schistose system of New Zealand seems evidently due to the same elevatory action as that which formed the Australian system, since the coast-line of that island is almost identical with the opposite shore of Australia in a somewhat lower latitude.

Lastly, we have the grandest mountain system of the world—the Andes—exhibiting similar features to the other systems above discussed. This chain exhibits the greatest constancy of direction of any, and in its extent it is unrivalled. The reports and sections published of this chain, the latest of which is the elaborate memoir of Dr. Forbes, show that this system, although still in the height of its activity, had its origin as far back as the oolitic period†, if indeed it do not eventually prove, as there is reason to believe, to have been brought into existence, like the Oural and the other systems to which I have adverted, at the close of the carboniferous epoch. The activity of this volcanic chain during the secondary period is shown by the formations of that period being interstratified with porphyries and other volcanic rocks; and the direction of the volcanic band is shown to have been, during the period, coincident with that of the present chain, by the circumstance that these secondary deposits, so interstratified with volcanic rocks of contemporaneous date, lie in a band from north to south between the palæozoic formations of the central or higher region of the Cordillera and the Pacific, forming a subordinate division of the chain of lower elevation, and comprising within it the greater part of the existing volcanoes of the Cordillera. It is worthy of remark also, as showing the identity of this system with that of the Rocky Mountains, that Dr. Forbes in the one, and Dr. Hector in the

* See Selwyn, Quart. Journ. Geol. Soc. vol. xiv. p. 533.

† See Darwin's 'South America,' p. 247.

other, have observed a remarkable absence, or at least rarity, of volcanic rocks penetrating the palæozoic portion or core of the chains, these rocks being developed in the lateral region where the secondary formations were deposited,—illustrating, I venture to think, the hypothesis mentioned in Section 1, that the contiguity of the sea (as shown here by the deposits) to the volcanic foci is due to the depressions caused by the volcanic action, its absence from those parts of the chains deficient in volcanic rocks being due to the steadiness of level there permitted by the absence, during the period, of volcanic disturbances. These two great systems of the Andes and the Rocky Mountains, although originating early in the secondary period, have preserved their alignement and activity until the present time; for although the Andes be the only one of them in which the volcanic force is still entirely active, yet the Rocky Mountains themselves, as well as the Cascade Mountains and the other Pacific-coast ranges (which are but the lateral and subordinate chains of the great Rocky-Mountain system), exhibit evidences of very recent volcanic activity*.

We thus see over half the northern hemisphere, and again in important parts of the southern, well-marked evidences of the continental development which prevailed during the secondary period, sufficient, I think, to justify an inference that during that period, when the chief part of the present Europeo-Asiatic continent and of Northern Africa was sea, the continents had an alignement from north to south as well marked as is the Europeo-Asiatic continent of the present day in the opposite direction (the great development of which from east to west being due, as I shall presently attempt to show, to the east and west development of tertiary volcanic bands), all the examples of great systems of secondary origin yet studied, with the exception of the Jura and of part of the Pyrenees (both of which originated very late in the secondary period), having this north and south direction. This inference will be greatly strengthened if further explorations should show that the north and south ridges of Central and Southern Africa, to which the configuration of that continent south of the Niger is due, and the north and south ridges of Madagascar are of contemporaneous origin with those of Australia, the Alleghanies, and the Oural, and contributed to the geographical configuration of the secondary period. In short, it may be asserted that the present configuration of our continents is due to the engrafting, as it were, upon secondary continents or their remains, of post-cretaceous land, elevated by mountain chains running from west to east which have come into existence since the close of the secondary period,—in some

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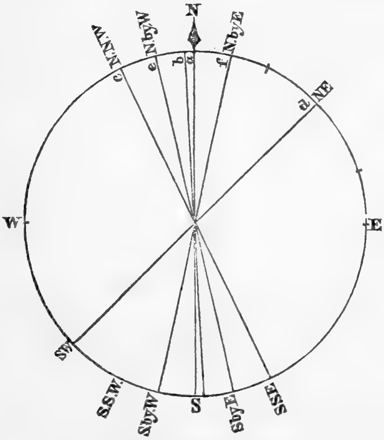
* See Geology of California and Oregon, by J. S. Newberry (Washington, 1857). Bauerman, Quart. Journ. Geol. Soc. vol. xvi. p. 198.

cases, as in that of the Oural, incorporating them into the body of the new continent, but in others leaving them in the form of peninsulas extending north and south, as in the case of South America, or of insulated tracts, as in the case of Australia.

The following are the (known) important axes which governed the geographical configuration of the secondary period; the letters refer to the diagram below. Axes such as the Jura, which came into existence very late in the secondary period, and whose influence upon this configuration was but subordinate, or as the principal axis of the Pyrenees, which, although of secondary origin, preceded only the newer cretaceous epoch, and whose influence is rather to be considered among those to be discussed in the 3rd section as governing the post-cretaceous configuration, are omitted in this list and diagram.

		Direction.
a.	Chain of Oural	N. to S.
b.	Chain of Andes	N. to S.
c.	Chain of Rocky Mountains . .	N.N.W. to S.S.E.
d.	Chain of Alleghanies	N.E. to S.W.
e.	System of England and Portugal.	N. by W. to S. by E.
f.	System of Eastern Australia . .	N. by E. to S. by W.



[To be continued.]

XXIV. *On the Influence of Traces of Foreign Metals on the Electric Conducting Power of Mercury.* By A. Matthiessen, F.R.S.; and C. Vogt, Ph.D.*

THE fact that mercury, when alloyed with traces of foreign metals, shows an increment, and not, as most pure metals,

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* Communicated by the Authors.

a decrement of the conducting power, has induced us to make the following experiments.

The mercury employed was purified by allowing it to stand for a length of time under a solution of protonitrate of mercury, and before use heating it on a water-bath for about half an hour with dilute nitric acid, washing with distilled water, and drying on the water-bath.

Before commencing the experiments, it was necessary to test whether the assertion made by Siemens*, "that not only does *the absorbed oxygen,* but also all metallic impurities increase the conducting power of mercury," is correct. Is oxygen really absorbed by mercury? This question may be answered by the following experiments:—

I. Mercury which had been treated with dilute nitric acid on a water-bath was well washed with distilled water (previously boiled to expel the air) and carefully dried with bibulous paper; a part of it was heated on the water-bath for half an hour, being well stirred during that time, and part dried in a Liebig's drying tube at 100° C. in a current of dry and pure hydrogen. These specimens did not show the slightest difference in their conducting power.

II. Another portion, after being dried in the water-bath, was shaken with oxygen in a bottle at the ordinary temperature for twenty minutes, and allowed to stand for three hours, during which time it was repeatedly shaken up. This also had the same conducting power as the above.

III. Another portion of the same mercury was boiled in an evaporating dish in contact with air for a quarter of an hour, to allow the formation of suboxide. This also showed no alteration in the conducting power. It may be mentioned that the apparatus we employed for the determination of the resistances will distinctly show 0·01 per cent. difference in the resistance.

IV. If mercury absorbed oxygen, it is probable that it would give it out again on solidification, as in the case of silver. Rose, however, states in his paper "On the Spitting of Silver†," that he had often frozen large quantities of mercury, but never observed the phenomena which occur with the *spitting* of silver.

From the foregoing it would appear that mercury does not absorb oxygen or oxide; or if it does, only to so small an extent that its conducting power is not altered by it. Now, as we shall prove that a very minute quantity of foreign metal materially affects the conducting power of mercury, we think we are justified in stating that pure mercury will neither absorb oxygen nor dissolve either of the oxides of mercury. That the

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* Phil. Mag. January 1861.

† Poggendorff's *Annalen,* vol. lxviii. p. 290.

mercury was sufficiently purified by the process we subjected it to, was proved by comparing it with some distilled mercury, which after distillation was treated with dilute nitric acid and dried in a current of hydrogen. The experiments were made as follows:—

Thermometer-tubes were fused on to wide tubes and bent, as shown in the figure. The length of these was about 300 millims. Into the wide tubes dipped well-amalgamated copper wires (5 millims. thick), which reached to the bottom of the tubes at *a*, thereby closing, as it were, the ends of the thermometer-tubes with a plate of copper. It was found that the height of the mercury in the wide tubes made no difference in the results obtained. The weight of mercury taken for each determination was 50 grammes.

To obtain concordant results, the precautions taken were:—

I. The amalgam was made in the tube itself. This was filled with the requisite quantity of mercury, and its resistance determined: this was always repeated twice, to be sure no air-bubbles were in it; and either the solid metal was added, or, as in the case of the poorer amalgams, a certain weight of an amalgam of known composition, and then heated for a quarter to half an hour over a Bunsen-burner, during which time the mercury was allowed to flow continually from the one arm to the other, at the same time taking great care that the thermometer-tube remained always full; for if it became empty, the amalgam would leave a *tail* in it, and thereby injure the continuity of the column. The thick copper wires were heated before being dipped into those amalgams, which partially solidified on cooling. It is almost superfluous to add that the tubes were, after they were emptied, well washed with nitric acid and distilled water and carefully dried, and that the ends of the copper wires were cleaned after each determination, and, when necessary, reamalgamated.

II. As the conducting power of mercury is known, and as the resistances of the tubes filled with that metal were always determined, it was not necessary to measure the length or diameter of the tubes; for we obtain more concordant results, when experimenting with the different tubes, by comparing their resistances with that of the tubes filled with mercury, than if we had measured their respective lengths and diameters and brought these data into calculation.

III. The metals used for the experiments were pure.

IV. During the determination the tube filled with the amalgam was placed in a trough filled with water, the temperature of which was kept as near 13° C. as possible. Of course, the tube was first placed on the empty trough and allowed to cool gradually before the water was poured in.

V. Each amalgam was made twice, and its resistance determined in different tubes.

In the following Tables the results obtained are given. They have been compared with the gold-silver alloy*, whose conducting power was taken at 0°=100:—

TABLE I.—Mercury-Bismuth Series.

To 100 parts mercury were added	Volumes per cent.	Conducting power, observed.	Temperature.	Mean of the		Conducting power, calculated.	Difference.
				Conducting power.	Temperature.		
0·05	0·069	10·932 10·932	13°·0 13·1	10·932	13°·05	10·908	+0·024
0·1	0·138	10·946 10·947	13·3 13·0	10·9465	13·15	10·906	+0·040
0·2	0·276	10·984 10·973	12·8 13·3	10·9785	13·05	10·901	+0·077
0·5	0·686	11·066 11·063	13·5 13·4	11·0675	13·45	10·890	+0·177
1·0	1·13	11·199 11·200	13·2 13·3	11·1995	13·25	10·876	+0·323

TABLE II.—Mercury-Lead Series

To 100 parts mercury were added	Volumes per cent.	Conducting power, observed.	Temperature.	Mean of the		Conducting power, calculated.	Difference.
				Conducting power.	Temperature.		
0·01	0·0119	10·920 10·917	13°·2 13·2	10·9185	13°·2	10·915	+0·003
0·025	0·0298	10·929 10·928	13·0 13·1	10·9285	13·05	10·922	+0·006
0·05	0·0596	10·944 10·944	13·2 13·2	10·944	13·2	10·935	+0·009
0·1	0·119	10·972 10·973	13·3 13·3	10·9725	13·3	10·960	+0·012
0·2	0·238	11·037 11·038	13·2 13·6	11·0375	13·4	11·009	+0·028
0·5	0·593	11·219 11·225	13·2 13·0	11·222	13·1	11·157	+0·065
1·0	1·19	11·493 11·497	13·2 13·1	11·495	13·15	11·407	+0·083
2·0	2·33	11·702 11·708	13·1 13·2	11·705	13·15	11·882	−0·177
4·0	4·55	11·867 11·879	13·0 13·4	11·873	13·2	12·809	−0·936

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* Phil. Mag. February 1861.

TABLE III.—Mercury-Tin Series*.

To 100 parts mercury were added	Volumes per cent.	Conducting power, observed.	Temperature.	Mean of the		Conducting power, calculated.	Difference.
				Conducting power.	Temperature.		
0·01	0·0186	10·929 10·930	12°·2 12·1	10·9295	12°·15	10·922	+0·007
0·025	0·0465	10·946 10·945	12·4 12·6	10·9455	12·5	10·941	+0·005
0·05	0·0930	10·978 10·977	12·6 12·8	10·9775	12·7	10·973	+0·004
0·1	0·186	11·041 11·042	13·2 13·0	11·0415	13·1	11·036	+0·005
0·2	0·371	11·172 11·170	13·2 13·2	11·171	13·2	11·161	+0·010
0·5	0·929	11·528 11·529	13·2 13·0	11·5285	13·1	11·538	−0·010
1·0	1·83	11·796 11·788	13·2 13·2	11·792	13·2	12·147	−0·355
2·0	3·59	12·329 12·307	13·0 13·0	12·318	13·0	13·335	−1·017
4·0	6·93	13·204 13·130	12·8 12·8	13·167	12·8	15·595	−2·428

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* In my paper (Phil. Mag. Sept. 1861) the conducting power of the gold-silver alloy at 0° C. was brought into calculation by mistake as =226, instead of 100 as stated. The values given in Table V. in that paper must be divided by 2·26 in order to make them comparable with the above. Taking, as above, the gold-silver alloy 100° at 0°, the values found for the tin-mercury series, &c. ought to have been—

		Calculated conducting power.
"Pure mercury conducts	= 10·827 at 18° C.	
,, alloyed with 0·1 Bi	= 10·876 at 18·6	10·823
,, ,, 0·01 Sn	= 10·845 at 18·4	10·840
,, ,, 0·02 ,,	= 10·858 at 18·0	10·850
,, ,, 0·05 ,,	= 10·898 at 18·2	10·889
,, ,, 0·1 ,,	= 10·956 at 18·8	10·951
,, ,, 0·2 ,,	= 11·080 at 19·0	11·071
,, ,, 0·5 ,,	= 11·442 at 18·4	11·439
,, ,, 1·0 ,,	= 11·779 at 18·6	12·031
,, ,, 2·0 ,,	= 12·239 at 18·8	13·162
,, ,, 4·0 ,,	= 13·137 at 19·0	15·527

"Further, for the calculations the conducting power of tin was taken equal to 76·146, and that of bismuth 7·9115, &c." It must be borne in mind that the values given in the Table were deduced from determinations made at different temperatures from those given in the present paper; the differences may therefore be due chiefly to the temperature not being the same. The relative results, however, are in both cases the same. The values given for the conducting powers of tin and bismuth are those taken from my paper "On the Electric Conducting Power of the Metals" (Phil. Trans. 1858).—A. M.

Table IV.—Mercury-Zinc Series.

To 100 parts mercury were added	Volumes per cent.	Conducting power, observed.	Temperature.	Mean of the		Conducting power, calculated.	Difference.
				Conducting power.	Temperature.		
0·01	0·0190	10·928 10·927 10·932	13°·2 13·2 12·8	10·929	13°·07	10·943	−0·014
0·025	0·0474	10·949 10·950 10·953	13·2 13·2 12·8	10·9507	13·07	10·992	−0·0413
0·05	0·0948	10·990 10·993 10·993	13·2 13·4 12·8	10·992	13·13	11·075	−0·083
0·1	0·189	11·079 11·078 11·075	13·2 13·0 13·0	11·077	13·07	11·238	−0·241
0·2	0·378	11·240 11·236 11·230	13·1 13·1 13·2	11·235	13·13	11·564	−0·329
0·5	0·940	11·708 11·698 11·683	13·2 13·2 13·2	11·696	13·2	12·538	−0·842
1·0	1·86	12·462 12·458 12·431	13·4 13·0 13·4	12·450	13·27	14·131	−1·681
2·0	3·66	13·537 13·569 13·593	13·2 13·0 12·8	13·566	13·0	17·247	−3·681
4·0	7·06	14·644 14·651 14·678	13·3 13·0 13·0	14·658	13·1	23·133	−8·475

Table V.—Mercury-Gold Series.

To 100 parts mercury were added	Volumes per cent.	Conducting power, observed.	Temperature.	Mean of the		Conducting power, calculated.	Difference.
				Conducting power.	Temperature.		
0·01	0·0070	10·917 10·917	13°·2 13·4	10·917	13°·3	10·913	+0·004
0·025	0·0176	10·929 10·933	13·4 12·8	10·931	13·0	10·917	+0·014
0·05	0·0352	10·945 10·948	13·4 13·0	10·9465	13·2	10·924	+0·022
0·1	0·070	10·979 10·976	13·2 13·2	10·9775	13·2	10·937	+0·040
0·2	0·134	11·029 11·034	13·0 13·2	11·0315	13·1	10·962	+0·069
0·5	0·341	11·294 11·351	13·0 13·2	11·3225	13·1	11·041	+0·281
1·0	0·70	11·567 11·576	13·2 13·2	11·5715	13·2	11·180	+0·391

Table VI.—Mercury-Silver Series.

To 100 parts mercury were added.	Volumes per cent.	Conducting power, observed.	Temperature.	Mean of the Conducting power.	Mean of the Temperature.	Conducting power, calculated.	Difference.
0·01	0·0130	10·920 10·918	13°·2 13·0	10·919	13°·1	10·917	+0·002
0·025	0·0324	10·930 10·923	13·2 13·2	10·9265	13·2	10·927	0·000
0·05	0·0648	10·953 10·943	13·4 13·4	10·948	13·4	10·944	+0·004
0·1	0·129	10·985 10·983	13·0 13·0	10·984	13·0	10·978	+0·006
0·2	0·259	11·049 11·047	13·2 13·4	11·048	13·3	11·046	+0·002
0·5	0·644	11·208 11·192	12·8 13·2	11·200	13·0	11·247	−0·047
1·0	1·28	11·561 11·572	13·0 13·0	11·5665	13·0	11·581	−0·015

Unfortunately, owing to the tendency of the amalgams to solidify, or rather to crystallize, we were obliged to discontinue the determinations in some of the series much sooner than in the others, as it was impossible to obtain constant results. In the mercury-bismuth series the turning-point (for there must be one, as bismuth has a lower conducting power than mercury) occurs between 1 and 2 per cent.; this point, however, could not, for the above reason, be accurately determined.

For the calculations, the conducting powers and specific gravities employed are given in the following Table:—

Table VII.

	Conducting power at 13°[*].	Specific gravity[†].
Bismuth	7·915	9·822
Mercury	10·910	13·573
Lead	52·640	11·376
Tin	78·507	7·294
Zinc	184·064	7·148
Gold	494·684	19·265
Silver	633·327	10·468

From the foregoing it will be seen that mercury, when alloyed

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[*] The values given for the conducting powers are taken from a paper "On the Influence of Temperature on the Conducting Power of the Metals," by A. Matthiessen and M. v. Bose, which will shortly be published.

[†] Phil. Trans. 1860.

with very minute quantities of another metal, with the exception of zinc, has a *greater* conducting power; but when alloyed with larger quantities, has a *lower* conducting power than the mean of the conducting powers of their relative values.

That zinc behaves differently from other metals when alloyed with mercury is what might have been expected; for zinc will only alloy with mercury to a small extent, just as it does with lead and bismuth*; for when zinc is melted with either lead, bismuth, or mercury, they separate, after having been well mixed together by stirring, immediately into two layers,—the upper one being zinc alloyed with small quantities of the other metal, and the lower one being the other metal alloyed with small quantities of zinc.

Does mercury stand alone in its behaviour when alloyed with traces of other metals? Is it the only one whose conducting power increases with traces of foreign metals to such an extent that it is greater than the calculated one, or is this a property of all metals in a liquid state? The following experiments prove that in all probability mercury is the only metal that behaves in this manner.

I. Tin was melted in tubes, as above described, in a bath of Rose's metal. The tubes were about 150 millims. long. The metal was first fused in the one arm of the tube, this lying at first somewhat inclined in the bath to prevent the metal from flowing in the thermometer-tube; when fused, it was sucked up the length of tube, and heated copper wires placed in it. It was found that the copper wires might be removed and replaced without materially affecting the conducting power. The alloys were made by first filling the tubes with tin and determining its resistance, and then adding traces of other metals (from 0·125 to 4 per cent.) to it in the tube. The tube after each addition was slightly inclined several times, to allow the alloy to flow from the one arm to the other, care being taken, as with the amalgams, not to allow the thermometer-tube to become empty. We only give the qualitative results obtained, as we do not possess any means of measuring high temperatures with accuracy, as well as owing to the great difficulty in obtaining constant values for the conducting powers of the melted metals and their alloys. The qualitative results will, however, answer the question we proposed.

I. Tin, when fused, loses in conducting power with an increase of temperature, but at the point of solidification increases rapidly, as already observed by Siemens†, and just in the same way as potassium and sodium‡; and when solid, if allowed

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* Proceedings of the Royal Society, vol. xi. p. 430.

† Poggendorff's *Annalen*, vol. cxiii. p. 91.

‡ Phil. Mag. February 1857.

to cool further, still continues to increase, but in a much slower degree.

If to melted tin traces of lead or bismuth are added, a decrement in the conducting power is observed, in fact almost in the same ratio as the addition of the metal.

II. Lead, when fused and at its point of solidification, behaves in the same manner as tin; and if traces of tin are added, an increment, if bismuth, a decrement in the conducting power is observed.

III. Bismuth, when fused, behaves like tin or lead with an increment of temperature, but at its point of solidification decreases rapidly, and from this point gains again, on cooling, in conducting power. This fact has already been observed by Matteucci[*], and confirmed by one of us[†].

If to melted bismuth traces of tin or lead be added, a decrement, and, on further addition, an increment in the conducting power will be observed. This behaviour corresponds exactly with that of these metals in a solid state; in fact, if we had determined the conducting power of the melted alloys, we should have obtained similar curves to those found for the conducting power of these alloys in a solid form[‡]. We are sorry not to have been able to determine the conducting power of mercury and its alloys in a solid state, owing to the want of means and appliances to freeze them.

1 Torrington Street,
February 17, 1862.

XXV. *On Sugar in Urine.* *By* E. Schunck, *F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE attention of chemists and physiologists has of late been directed to the fact of the existence or formation of sugar, or some substance much resembling it, in various organs and secretions of the body, even when these are in a perfectly healthy state. The liver, for instance, is found to secrete a compound which, under certain circumstances, is capable of being converted into glucose, and which has received the name of *glycogene* or *hepatine*; and the urine is also supposed to contain sugar occasionally, even when no suspicion exists of the presence of disease. A fact connected with this subject, which was made known by myself more than four years ago, seems to have been left almost

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[*] *Ann. de Chim. et de Phys.* sér. 3. vol. xlii. p. 472.

[†] Phil. Mag. February 1857.

[‡] Phil. Trans. 1860, p. 161.

unnoticed, though it may, I think, serve to explain several phenomena recorded in physiological chemistry, the cause of which is still enveloped in obscurity. In a paper "On the Occurrence of Indigo-blue in Urine*," I stated that when urine which is free from sugar is mixed with sulphuric or muriatic acid and boiled, it deposits a large quantity of brown flocks, and that the liquid filtered from these flocks, after being made alkaline, gives the reaction of sugar with salts of copper. Hence I inferred that urine must contain some substance which, by decomposition with acids, yields sugar, the brown flocks representing the body or bodies with which the latter was originally associated in the manner so well known to chemists; and I ventured to surmise that this substance might be the so-called extractive matter of urine, regarding which so little is yet known. This supposition has been fully confirmed by further investigation. My experiments have led to the conclusion that human urine contains, as Berzelius supposed, no less than three distinct extractive matters. I have succeeded in separating these from one another, and from the other constituents of urine. When quite pure, their composition is uniform, even when they have been obtained from various sources at considerable intervals of time; and hence it is to be inferred that, like urea and uric acid, they must be classed among the last and simplest products of decomposition which the tissues are capable of affording under the circumstances usually occurring in the animal frame. When decomposed in watery solution by means of strong acids, they yield brown pulverulent or resinous bodies insoluble in water, which contain the whole of the small quantity of nitrogen originally present, and a species of glucose, which, though it has the composition and some of the properties of grape or diabetic sugar, differs from the latter in being uncrystallizable and insipid.

The bearing of these facts on several important questions in physiological chemistry will be apparent at once. The origin of the sugar occasionally found both in health and disease in various animal secretions is still a matter for speculation. It has been surmised that it may be formed by the decomposition of albuminous substances; but direct experiment has hitherto failed to confirm this supposition. The extractive matters of urine, however, probably represent the missing link connecting the tissues on the one hand with the sugar on the other; or, in other words, they contain that portion of the original complex (proteine) which is capable of forming sugar,—the latter being no doubt preserved by its association with nitrogenous matter from oxidation, which would, if it were completely uncombined from

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* Phil. Mag. S. 4. vol. xiv. p. 288.

the commencement, cause it to disappear before it could be eliminated in the urine. It is possible to conceive that, in certain morbid states of the system, a process of decomposition may be set up inducing a further metamorphosis in the extractive matters, and leading to the formation of a greater quantity of sugar than can be got rid of by oxidation. Some such process may perhaps serve to account for the large amount of sugar found in the urine in diabetes, the origin of which is still involved in so much obscurity. It is well known that in this disease hippuric acid is found in the urine in much greater abundance than in a state of health. Now the composition of two of the urinary extractive matters is such, that they may be supposed to split up into sugar, hippuric acid, and acetic acid; while in the case of the third it is only necessary to suppose that the acetic acid is replaced by formic acid. It seems to me not improbable that some such process of decomposition may take place in the body, perhaps under the influence of a peculiar ferment, and that the extractive matters are the immediate source of the sugar occurring in the urine in this disease, the remote source being of course the tissues themselves. The saccharine state of the urine, which, according to Dr. Pavy, is produced by the injection of acids into the circulation, may possibly be caused by the action of the acid on the urinary extractive matters, as well as on the glycogene of the liver.

I will not trouble you with any further speculation on the subject, as my experiments, which have suffered considerable interruption from unforeseen circumstances, will, I hope, soon be brought to a conclusion. On the present occasion I merely wish to announce the fact that ordinary healthy urine contains substances which, by decomposition with acids, yield among other products a species of sugar, and that these substances are simply the extractive matters always contained in the secretion.

I am, Gentlemen,
Your most obedient Servant,
E. SCHUNCK.

Kersall, Manchester,
January 27, 1862.

XXVI. *On the Composition of the Great Bhurtpoor Gun, stationed on the Royal Artillery Parade Ground, Woolwich; and of some other interesting Cannon.* *By* F. A. ABEL, *F.R.S.*[*]

IN 1828 the largest gun taken, shortly before, at Bhurtpoor, and which had been presented to George IV. by Viscount Combermere and the forces engaged in the capture of that

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[*] Communicated by the Author.

fortress, was placed by the King's desire in front of the Royal Artillery Barracks at Woolwich.

This gun is not only interesting as a trophy, but remarkable for its very considerable size,—its length being 16 feet 4 inches, its largest diameter (at the base ring) 3 feet 3 inches, and that of the muzzle 2 feet. Its calibre is 8 inches, and its weight about 17¾ tons. From the muzzle to a short distance behind the trunnions, the gun exhibits comparatively few and unimportant imperfections as a casting; but the portion which reaches from the cascable to a large ornamental ring (in the form of a coil) near the rear of the trunnions, is exceedingly imperfect, the metal being spongy, and exhibiting cavities of some size in several places. The thickness of this portion of the gun is considerably greater than that of the remainder; and at the ornamental coil-ring, above referred to, its thickness diminishes abruptly, while from this point to the muzzle the gun tapers slightly and uniformly. An ornamental scroll engraved round the gun immediately adjoins the coil-ring just spoken of, and seems as if it were partially and irregularly covered over by the latter. There is, moreover, an appearance of the metal having been roughly chiseled at this part, as if small projecting portions had been cut away, after the ornamental scroll had been cut upon the metal.

These points, added to the great dissimilarity between the appearances of the surface and the colour of the metal, at the breech, and those of the remainder of the gun, evidently show that a species of envelope, reaching from the cascable nearly to the trunnions, has been cast upon the original gun, for the purpose of either strengthening or adding to the weight of that portion; and the circumstance that the projecting coil-ring, which forms the boundary of this envelope, is partially superposed upon an ornamental scroll, appears to favour the supposition that the breech had been strengthened or thickened by the second casting, after the original gun had been, at any rate, fully completed. The gun is highly ornamented, and bears several Persic inscriptions, of which the following are the translations:—

1. On the chase, the name and titles of the monarch in whose reign the gun was cast. "The Father of Victory, the Reviver of Religion; Muhammad, Aurang-zeb, Alam-gir, The Warrior, the victorious King."

2. On the swell of the gun, the date of its production: "Year of the Hejira 1087, the 20th of the Reign." (This corresponds to A.D. 1677.)

3. Under the right trunnion, the name of the gun: "The Gun, the aid of Ali" (the hero-saint of the Indian Mohammedans).

4. Under the left trunnion, the weight of the powder-charge and of the projectile to be used: "According to the weights of the Shah Jehan: the Ball 30 Sirs, powder 10 Sirs." (The *sir*, a weight established by the Emperor Shah Jehan and still used in Hindustan, is equal to 2 lbs. avoirdupois*.)

A belief has been hitherto entertained that the metal of this gun contained a proportion of gold and silver, in consequence of an existing tradition that it was the custom of the high dignitaries to throw articles of gold and silver into the metal at the time of casting. The results of analysis have demonstrated the erroneous nature of this tradition, at any rate as applied to this particular instance, as no trace of the precious metals was discovered in the specimens of the gun examined.

Three specimens of the metal composing the gun have been submitted to quantitative analysis,—one sample having been taken from near the muzzle, a second from between the trunnions, and the third from the breech. The means of the analytical results obtained show that—

1. The metal of which the main casting itself consists is composed of lead, tin, and copper; and the proportions in which these metals have been employed vary to a very marked extent in different parts of the gun. The alloy composing the portion near the trunnions contains considerably less lead, and larger proportions of copper and tin, and is therefore comparatively harder and more tenacious, than that forming the front portion, or chase of the gun.

2. The mass of metal enclosing or forming the exterior of the breech of the gun consists of an alloy of copper and zinc, resembling ordinary brass in its composition, and containing small quantities of iron, lead, and tin, which probably existed as accidental impurities in the metals used. These results confirm the conclusion above arrived at, that the exterior of the breech consists of a distinct casting.

The per-centage results obtained by the analysis of the specimens above referred to are as follows:—

	Composition of the metal near the	
	Trunnions.	Muzzle.
Copper	86·10	81·73
Tin	4·64	3·33
Lead	9·12	15·10
Arsenic	0·15	trace
Antimony . . .	trace	trace

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* Mallet, 'On the Construction of Artillery.'

The specimen taken from the breech consisted of—

Copper	60·52
Zinc	36·72
Lead	1·38
Tin	0·25
Iron	1·01
Arsenic	trace

On either side of this large gun, in front of the Royal Artillery Barracks, are two very highly ornamented and perfect bronze cannon, of considerable size, though small as compared with the Bhurtpoor gun.

These four cannon were cast in Florence about the year 1750, by the sons of Andreas Morenus, for Francis, Duke of Lorraine (who, by marriage with Maria Therese, became Emperor of Austria). The guns are all exactly alike as regards size and ornamentation, the only mark of distinction being the name, which is engraven on the chase of each gun. These names are, *Violentum, Testudo* (the name of an ancient military engine), *Destructor*, and *Negans*.

Specimens were detached for analysis from one of these guns (the *Negans*) at three places, the muzzle, trunnions, and breech. The following are the per-centage results obtained:—

	Metal taken from the		
	Muzzle.	Trunnion.	Breech.
Copper	89·13	89·00	89·21
Tin	10·23	10·20	9·75
Lead	0·57	0·52	0·79
Arsenic	0·10	0·15	0·09
Antimony	trace	trace	trace

The above results show that the proportions of copper and tin employed in the preparation of the casting were almost identical with those adopted in France at the present day (100 of copper to 11 of tin). The composition of the metal is remarkably uniform at different parts of the gun,—the results of analysis of the specimens taken from the muzzle and trunnions being almost identical, while those furnished by the metal from the breech do not differ from the others so greatly as I have found to be frequently the case in the examination of a very large number of bronze guns of recent manufacture. The small proportion of lead present in the alloy is in all probability an accidental impurity.

A considerable number of large *Chinese* guns, believed to consist of bronze, were brought over to this country at the close of the last and the preceding war with China. Several of these guns are very interesting as specimens of the ingenuity and the decorative skill of the Chinese. Some of the largest are beautifully and very highly ornamented with elaborate carvings. In external form these resemble the heavy guns of bronze and iron produced in European countries, excepting that they are almost uniform in diameter throughout their length. The thickness of their metal is very considerable in proportion to their calibre. Other of the guns are devoid of ornament, with the exception of numerous projecting rings or narrow hoops, forming part of the casting and distributed over the entire length of the gun. Some of these particular guns contain linings of iron, the alloy which composes the gun having evidently been cast upon a cylinder of that metal.

The majority of these Chinese cannon consist of metal closely resembling brass in colour; but a few among those most recently brought to this country are even of a deeper colour than bronze. A sample taken from one of the light-coloured guns was found on analysis to have the following composition:—

Copper	61·20
Zinc	33·53
Lead	3·50
Tin	1·32
Iron	0·42
Arsenic Antimony	traces

This metal is therefore similar in composition to ordinary brass, but contains, in addition, somewhat considerable proportions of lead and tin, which metals, however, together with the iron, may have existed as impurities in the copper and zinc employed.

The metal composing the dark-coloured guns is very different from the above. Samples were taken for analysis from three different parts of a gun, for the purpose of ascertaining whether it consisted of the same metal throughout; these furnished the following per-centage results:—

	Metal taken from the		
	Muzzle.	Trunnion.	Breech.
Copper	95·88	97·33	97·28
Tin	0·15		
Antimony	1·52	0·46	0·36
Arsenic	0·01	0·22	0·14
Lead	0·68	0·54	0·66
Bismuth	0·01	0·01	0·03
Iron	0·20	0·10	0·22
Zinc	1·70	1·30	1·38

Neither silver nor sulphur could be detected in the metal.

It would seem from these results that the dark-coloured guns are produced from the commercial copper of the country, with the addition perhaps of a very small proportion of zinc. The front part of the gun examined appears to have been cast of a metal somewhat different from and inferior to the remaining portion. In several parts of the gun the metal was in an exceedingly porous condition, and appeared to have been cast at a very low temperature. Some portions contained many cavities lined with the oxidized metal.

XXVII. *On the Cohesion-Figures of Liquids. By* Charles Tomlinson, *Lecturer on Science, King's College School, London*[*].

IN a paper read before the British Association last year, and inserted in the October Number of this Journal, it is shown that when a drop of an independent liquid (*i. e.* not a solution) is gently deposited on the surface of another liquid (such as water), it flattens out into a peculiar and characteristic figure, which is the resultant of its cohesive force, its density, and the adhesion of the surface.

The principle of these *cohesion-figures* (as I name them) being established, facts and details may be multiplied to almost any extent. I have already ascertained the figures of about one hundred liquids, and have made some progress in establishing their value in analysis, and in the detection of adulteration, to which they seem well fitted. It was not my intention to have written again on the subject until the investigation had advanced further towards completion; but inquiries having been made consequent on failures in the production of these figures, I may perhaps be allowed on the present occasion to state a few more particulars.

The sources of failure are chiefly three:—1. *Want of purity in the vessels used.* Glass, &c. exposed to the air contracts an organic film, so that when a vessel is filled with water, the surface of the latter becomes covered with a portion of such film, and this prevents adhesion between the water and the drop deposited on it. The vessels must be washed from time to time with strong sulphuric acid, and rinsed after every experiment with a strong solution of caustic potash and then with water. 2. *Want of care in delivering the drop.* The drop must not be allowed to fall on the surface, but be gently *delivered* to it from the end of a glass rod, or from a dropping-tube. 3. *Want of care in preserving the surface of the water tranquil.* After the

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[*] Communicated by the Author.

water has been poured into the glass, it must be allowed to come to rest before the drop is deposited upon its surface.

It is desirable to have three or four glasses in regular work; these should be of the same size and shape. The form of glass used by me is a conical tumbler about 3½ inches in diameter at the mouth. The water need not be distilled, provided it be clean. The glass rods must be wiped after every experiment, and washed in caustic potash solution. The washing, &c. (which has been complained of as troublesome) really gives little or no trouble, provided suitable arrangements be made for it. My plan is to keep two glasses full of the potash solution, one a beaker, and the other a small Phillips's glass. When an observation is complete, the contents of the tumbler are emptied into a waste-pan, in such a way that little or none of the oil (if an oil-film be present) shall adhere to the glass. The glass is then rinsed out with a little water, filled up with potash solution, and emptied back into the potash vessel, taking care in pouring to turn the glass round so as to wash every part of the edge; for to this the oil is apt to cling, and soil the fresh water that is next poured in. The glass rods should be kept in the Phillips's jar; and when one is taken out for an experiment it must be wiped dry on a clean duster, then dipped into the oil and allowed to drain, so that one and only one drop shall be deposited on the water. The rod is then wiped on another duster and returned to the potash solution. The pipettes used for ethers, &c. should also be kept clean; a number standing upright in a cylindrical glass jar is a useful arrangement.

With respect to temperature, results are most consistent when it is pretty constant, such as the mean temperature of the air in summer, and the warmth of an ordinary room in winter. I shall, however, have more to say on this subject presently.

I am encouraged to believe that these details will not be thought trivial, when I call to mind that the minute details contained in my first instructor in chemistry ('Chemical Manipulation') were the production of as high a scientific genius as this country has probably ever produced.

I have stated above that the glasses used in these experiments present a surface of water 3½ inches in diameter. Now, as adhesion plays a considerable part in the production of cohesion-figures, it may be asked, what would be the effect of varying the adhesion by enlarging or contracting the surface-area?

In order to throw light on this question I selected four vessels —A, a small thick shallow foot-glass, into which was poured 2 oz. of water; B, a large dinner plate, containing 4 ozs. of water; C, my usual conical tumbler, containing 8 ozs. of water; and D, an evaporating dish, containing 16 oz. of water. Their surface-areas were respectively—A, 3 inches in diameter; B, 6 inches; C, 3½ inches; and D, 6 inches: another object in selecting D was to notice the effect of a great body of water as compared with A.

Sulphuric ether was washed, and drops from a pipette deposited on the centre of A. A moderately good figure was formed, but it was strongly attracted by the thick sides of the glass. When the ether was delivered to the centre of B, instead of the usual sharp defined figure, about the size of a florin, a much larger figure was produced, and this was distorted and torn by the attraction of the larger surface and the sides of the vessel, to which it bounded up with rapidity. The characteristics of the figure were evident in the midst of the distortion. On C the figure was most perfect and satisfactory. On D the figure was not so much distorted as on B. The attraction of the sides was not so strong; but it must be noted that the sides were thin and upright, whereas in B the raised inner edge of the plate was on a level with the surface of the water.

On mixing a portion of the washed ether with about one-fourth of alcohol, good steady figures were produced on all four surfaces. The figures were more nearly confined to one spot, and the attraction of the sides was less marked, in consequence of the stronger attraction of the water for the alcohol than for the pure ether.

Pipettes of two sizes were used, one with a small and another with a larger aperture, without any particular change in the structure of the ether figure.

The effect of varying the surface is very marked in the case of some of the essential oils. In general the larger surface produces the phenomena with much greater rapidity than the smaller; the patterns of the figures are also more minute, and the colours more freely developed. For example, a drop of oil of lavender on the water in the conical glass forms a large film, with a display of the beautiful Carrigeen-moss pattern, characteristic of this oil. The pattern sets in from holes in the film. When, however, the drop is deposited in the centre of B or D, it flashes out into a large film with a transient display of colour; the pattern is minute, and is produced with great rapidity, setting in from the edge and meeting the forms produced from holes in the central parts. On the larger surface D, a large shadowy outer film is produced more perfectly than on the other surfaces. This secondary film is formed by the more fluid and volatile portion of the oil, and is not an uncommon feature in the films of essential oils.

The influence of extended surface, as well as increased volume of water, may be seen by the phenomena attending the formation of other lavender films on the same surface. A second drop of oil on C formed a small film thick and bossy; after some minutes it spread somewhat wider, and produced the pattern in a sluggish, imperfect manner. It was, however, instructive as showing more clearly the mode in which this complicated pattern propagates itself. After about twenty minutes a third drop on C did not spread at all, but remained lenticular.

A second drop of oil on B acted well, but more slowly than the first. A third drop after some minutes scarcely acted at all, showing that adhesion was nearly satisfied.

The effect of the larger volume of water in favouring solution was shown in D. This surface received a second, a third, and a fourth drop, and each of the resulting films displayed the pattern admirably. When a fifth drop was added, the film was thick and bossy, but the pattern set in and continued to play sluggishly about for some time.

Similar trials were made with oil of bitter almonds and oil of spike, with somewhat similar results, varying, however, with the solubility of the oil. A fixed oil was also tried. A drop of castor oil on the plate B, in the very act of spreading, was attracted to the side, so that the figure was marred. The figure was very fine on C, but finest on D, the enlarged surface favouring the phenomena of this film, namely, a large shadowy outer film, within which is a very wide perforated border, with a beautiful lace-like pattern*, then a charming series of Newton's rings bounding a smooth colourless centre.

The cohesion-figure of balsam of copaiba, with its broad lustrous iridescent rings and sharply defined outer edge, shows wondrously well on the surfaces B, C, and D; but perhaps the colours are most lustrous on the smaller surface C.

There are other circumstances which modify the cohesion-figures of liquids. The cohesion-figure of a volatile oil, for example, is liable to certain variations, according to its density and some other circumstances. The figure in its main features may be produced so as to be easily recognized, from various specimens of oil, all differing somewhat in physical constitution, that is, supposing them to be pure. Thus the oil-of-lavender pattern may vary somewhat in different specimens, since it varies in density from 0·87 to 0·94. Its cohesion-figure may be more minute in the oil of one season, or of one maker, than in that of

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* I stated at the British-Association Meeting at Manchester during the reading of my first paper on this subject, that many of these cohesion-figures are calculated to give hints to the pattern-designer. I have shown these figures to artists, who pronounce some of them to be "perfect beauty," and agree with me that numerous shawl and other patterns, fringes and borders of great novelty and effect, both as to form and colour, might be got up from a study of these figures.

another; but there is no mistaking the cohesion-figure of any oil of lavender that I have tried for that of another oil, unless that other oil be made to resemble oil of lavender by artificial means. For example, oil of coriander (sp. gr. ·859 to ·871) has a cohesion-figure peculiar to itself; but if it be made to combine with a small portion of camphor under a gentle heat, so as to bring it to about the same density and texture as oil of lavender, it will display the lavender figure; with a certain other proportion of camphor the figure becomes again different: a film is formed which breaks up rapidly into flat disks; each disk becomes fringed with a silvery ring, which undergoes constant motion and change; the most lovely iridescent colours then take the place of the silver tint; the fringe becomes more copious, and the colours more lustrous, until the whole vanishes. With yet another proportion of camphor in coriander oil another set of effects is induced. The film splits into large disks; the edge of each disk is the thinnest part; and as cohesion reasserts itself (as shown by the shrinking of each disk), the edge, by its adhesion to the water, is left behind, it being cut off by a visible action, just as if a sharp instrument were being slowly passed round it. The piece thus cut off forms satellites to the parent disk; and as the latter continues to contract, there may thus be two or three concentric circles of satellites of different magnitudes; but they in their turn disappear by evaporation and solution, somewhat after the manner of the principal disk.

I have also produced the lavender pattern by the addition of camphor to ol. pimentæ and ol. citronellæ. A drop of the former produces a very eccentric film: the Carrigeen-moss or lavender pattern plays about in flattened disks; and the disk suddenly recovering its form, the pattern is produced from the other side of the same disk. The camphorated citronella displays the lavender pattern on a large scale.

Again, camphor in solution in oil of bitter almonds entirely changes the figure of that oil. The film quickly breaks up into disks, each of which throws out waving cilia; and these set the disks sailing about, while they rapidly disappear by solution and evaporation.

The action of cold upon the essential oils is not very apparent, either as respects the oils themselves or their cohesion-figures. I reduced a number of such oils to 0° F. Oil of peppermint became cloudy; ol. cassiæ cloudy and thick; anthemis slightly cloudy; cinnamon less fluid; patchouli viscid; but this low temperature did not appear to have any effect on the oils of juniper, menthis, cloves, citronella, anethum, myristica, lavender, coriander, rosemary, verbena, origanum, bitter almonds, and sassafras. Glycerine became a little more viscid, and benzole cloudy. Most of these oils, when taken out of the freezing mixture, and a drop deposited on water at + 35°, formed admirable and characteristic cohesion-figures, although, certainly, some of the figures which are usually active (such as that of oil of cloves) were less active than at a higher temperature.

On the other hand, the effects of a high temperature on the volatile oils are very decided. For example, a drop of the oil of bitter almonds deposited on hot water spins for a moment and scatters the more fluid portions of the oil with violence around; after which the peculiar features of the cohesion-figure are displayed with rapidity. A drop of oil of lavender on hot water produces its sea-weed pattern on a much smaller scale than usual.

But the effects of temperature in modifying the cohesion-figures are most remarkable when an essential oil is subjected to fractional distillation. The volatile oils have not, in general, a fixed boiling-point, any more than a fixed freezing-point. If a volatile oil, such as oil of camphor*, be heated, its boiling-point will go on rising until the whole of it has evaporated. An ounce and a half of this oil was distilled and the temperature noted.

At 140° F. it began to simmer.
„ 210 „ it was boiling.
„ 260 „ boiling briskly with spheroidal globules on the surface.
„ 300 „ boiling rapidly.
„ 350 „ „ „ a milky fluid distilled over.
„ 385 „ a transparent colourless fluid passing over.
„ 390 „ boiling rapidly and distilling.
„ 400 „ boiling. Temperature slowly rising.
„ 416 „ white fumes in the belly of the retort; transparent liquid distilling over; retort jerking.
„ 418 „ rapidly distilling.
„ 430 „ oil in retort deeper in colour.
„ 460 „ limpid transparent liquid distilling over. Nearly all the oil in the retort gone.

In another operation the distillate was collected in test-tubes, which were changed as soon as any alteration in its character

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* This oil, which is seldom seen in Europe, is the elæopten, while the ordinary solid camphor is the stearopten of the camphor laurel tree. The specimen furnished to me through the kindness of Messrs. Baiss Brothers, of Leadenhall Street, London, is of a dark-amber or brownish-orange colour, containing deposited feathery crystals of camphor. A drop of the oil smeared on white writing-paper disappeared without leaving any kind of stain. The sp. gr. of the oil is ·940. Exposed to the air it deposits solid camphor, and at 0° F. a portion becomes solid; the fluid part being drained off, the other part remained solid up to 28°, but just above this it liquefied.

was apparent. The tubes marked Nos. 1–5 contained,—No. 1, a milky, slightly coloured, very limpid fluid; No. 2, a milky colourless fluid, very limpid; No. 3, a perfectly transparent fluid, limpid; No. 4, an opalescent white jelly, gelatinous camphor in fact, slightly yellow; No. 5, colourless transparent fluid resting on perfectly colourless jelly. When the whole of the distillate is collected into one vessel, the gelatinous camphor does not appear, it being dissolved in the liquid portion.

The five liquids thus collected formed as many cohesion-figures. Nos. 1 and 2 resembled the figure of the crude oil; No. 3 formed a large film, or rather double film, viz. an outer faint one, and an inner thicker one; the latter opened with a display of iridescent rings, which disappeared when the film was perfectly formed. But no sooner was the film formed than cohesion began to reassert itself, and, adhesion struggling with it, the film broke up, forming a complicated pattern, which played about with a waving-to-and-fro motion so long that the eye became dizzy with looking at it. The disks then expanded and contracted with a display of colour until the whole disappeared.

No. 4 formed a film covered with a great number of specks of gelatinous camphor, one mass sending off minute globules in radial lines. After a short time a granular disk of camphor was formed, and sailed about round by the side of the glass.

No. 5 formed a film like No. 4, only it almost immediately gave a kind of convulsive start, split into a number of large irregular disks, which continued jerking and splitting and opening for some time, as if by a simultaneous movement.

A drop of the crude oil on water forms a good film with a row of small bosses at the edge, and a row of larger ones just within the edge,—a character that belongs to oil of turpentine and a number of the oils allied to it, of which oil of camphor is one. The film soon opens into well-shaped holes, each of which is surrounded by bosses; the holes become very numerous and open into each other, forming a kind of network; and this also breaks up, leaving a few small disks.

The oil-of-turpentine film presents very distinctive well-marked characters, which qualify it as the representative of a considerable number of oils which are related to it isomerically, and not greatly differing in density; but the heavier oil of cloves makes an entirely different figure. The moment a drop of the rectified oil of turpentine touches the water, it flashes out into a film with a beautiful display of rainbows. The colour disappears as soon as the film is fairly formed, and the edge becomes marked with a double row of bosses of unequal size, as above noticed. These bosses flatten out into disks, which are surrounded by small dots. A beautiful display of iridescent colours then sets in, beginning with the lower series and gradually ascending. The colours cover the film in irregular bands, producing exquisite harmony. Suddenly the film opens into holes, which, in the midst of the colours, have a striking effect. The colours disappear with the film, leaving a network or lace-pattern, the holes becoming ovoid and many-sided, and the colour a beautiful steel-blue.

A charming effect may be produced by holding a drop of turpentine from the end of a glass rod over the blue lace-pattern. The vapour condensing upon the lines, causes them to swell out and apparently to crowd closer together, while the colour becomes changed to a silvery white.

The effects above described succeed each other much more rapidly on a bright warm day than on a damp or dull one,—a remark that equally belongs to many other films.

The film of ol. sabinæ has the double row of bosses, and presents the other phenomena of the turpentine film, only modified in various particulars, which enable it to be distinguished from all other films.

The same remarks apply to oils of juniper, cajeput, bergamot, lemon, and some others.

In all these cases the characteristics of the turpentine film are distinctly recognizable. 1. They all open with a flash of rainbow colours. 2. They all have the double row of bosses about the edge, *i. e.* a very minute beading at the edge, and a larger beading just within it. 3. They all display bright iridescent colours as they become thinner by evaporation. 4. They all open into holes; and 5. They all form a network which lasts for a long time.

No two of these oils, however, are precisely alike in passing through these changes, except perhaps the oil of turpentine and the oil of spike; indeed it is often difficult to distinguish between these two oils, from the circumstance that the commercial oil of spike is composed of a mixture of three parts essential oil of turpentine and one part oil of lavender. Some oils do not give the same rainbow tints in opening as others. In some the double row of bosses at the edge is on a little larger scale than in the others. In some the colours that come over the film are brighter and more metallic than in others. In some the holes are smaller and more numerous than in others: the oil-of-juniper film, for example, displays a multitude of holes like needle-points closely crowded together. And lastly, the network is finer in the film of some oils than in that of others. In some cases this network resembles a vegetable structure, as in that of ol. myristicæ, where the lines are like long branches of very delicate moss, laid down so as to form a tolerably regular pattern.

Other groups of liquids also present interesting resemblances in their cohesion-figures. Thus we may compare the oil of bitter almonds with the oils of cinnamon, anise, cumin, &c. On the present occasion, however, I will only notice the oil of bitter almonds. This oil forms a good film with a shadowy outer film, formed by a more fluid and diffusive portion of the oil (an effect by no means rare with the essential oils). The principal film opens into holes, from which shoot out a number of processes like fingers or snails' horns; or, supposing a hole to represent a closed child's hand, and one, two, or three fingers to be quickly opened and shut, we get some idea of the behaviour of the bitter-almond-oil film. Some of the holes send out short processes from equidistant points, thus forming triangles with curved sides: occasionally long processes are shot out, and these in an odd way detach disks of the oil from the parent disk, the final result being the formation of a large number of disks at rest on the surface. These disappear by evaporation and solution with displays of iridescent rings.

Variations in cohesion-figures, arising from changes in the adhesion-surface, produce interesting results. A drop of water gently delivered to the surface of sulphuric acid from the end of a pipette flattens down into a well-defined disk about the size of a shilling, with radial lines which disappear at the centre, while the fragments remain for some seconds near the circumference. I have obtained three variations of this figure all of the same type, and am preparing drawings of these, and some of the other more striking figures. Alcohol, sulphuric ether, benzole, &c. on sulphuric acid give striking figures, and illustrate the truth of the principle on which these figures are formed, any variation in the adhesive force of the surface being accompanied by a change of figure.

When acetic acid is employed as the adhesive surface, a new set of figures is also obtained. Thus oil of camphor, which on water forms a large well-developed film, does not expand into a film at all on acetic acid (sp. gr. 1·045), but forms a small disk, which sails about with considerable agitation, throwing off globules in large number, which are whirled about with great rapidity under the action of the parent disk. This becoming attracted by the sides of the vessel, it will sail round a number of times in contact therewith.

Oil of lavender on acetic acid also forms a small disk, which becomes greatly agitated, and gathers itself up with strange contortions, thus illustrating in its own way the struggle that is going on between cohesion and adhesion. The oils of cajeput, bergamot, and some others form films on acetic acid with considerable agitation, while the oils of cloves and of bitter almonds are quite inert.

The modifications which take place in cohesion-figures by the mixture of one liquid with another in varied proportions are very striking. For example, balsam of copaiba, when pure, forms a figure consisting of very perfect concentric rings of great breadth and splendid metallic colours, changing and disappearing as the film becomes thicker. The outer edge of the film is quite sharp and perfect.

Now this balsam is soluble in alcohol, so that any adulteration of it by means of a fixed oil could be easily detected, except castor oil, which is also soluble in alcohol. The cohesion-figure of castor oil is also well marked; it has narrow iridescent rings around a colourless disk, and is fringed with a broad perforated pattern. A mixture of two-thirds balsam and one-third castor oil, made under a gentle heat, forms a blank white film of large size and clear edge quite destitute of colour. There is not a vestige of the brilliant bands of the copaiba, or the delicate halo and coloured fringe of castor. The film lies passively on the water, with no other indication of its origin than the gradual formation at its close of a very minute chain of colourless beads or bosses gradually enlarging within its clear edge, precisely similar to those which close the existence of the copaiba film, and still later a partial attempt at a fringe (but *without colour*) like castor.

King's College, London.
February 13, 1862.

XXVIII. *Note on the Solution of an Equation of the Fifth Order.*

By A. Cayley, *Esq.*[*]

I ACCIDENTALLY omitted to reply to Mr. Jerrard's remarks, May 1861, on my discussion of his alleged solution of an equation of the fifth order. In his last paper, "Remarks on M. Hermite's argument relating to the Algebraical Solution of Equations of the Fifth Degree," February 1862, Mr. Jerrard reverts to the subject, and he says, "But although $f(\iota)\,f(\iota^2)\,f(\iota^3)\,f(\iota^4)$ is, as well as its fifth power, a six-valued function, we cannot, with the aid of Lagrange's theory of homogeneous functions, establish a rational communication [? relation] between the two functions

$$f(\iota)\,f(\iota^2)\,f(\iota^3)\,f(\iota^4),\quad f^5(\iota)\,f^5(\iota^2)\,f^5(\iota^3)\,f^5(\iota^4),$$

as I pointed out to Mr. Cayley in the Philosophical Magazine for May 1861."

Putting, for shortness, $\alpha=f(\iota)\,f(\iota^2)\,f(\iota^3)\,f(\iota^4)$, I understand Mr. Jerrard to mean that α, α^5 being given as the roots of two

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[*] Communicated by the Author.

equations of the sixth order respectively, α *cannot* be expressed as a rational function of α^5 and of the coefficients of the two equations, or, what is the same thing, since the coefficients of each equation are rational functions of the coefficients of the given equation of the fifth order, that α *cannot* be expressed as a rational function of α^5 and of the coefficients of the equation of the fifth order. If this is not Mr. Jerrard's meaning, he will doubtless set me right; but that α *can* be so expressed seems so clear, that I must apologize for giving a formal proof of it. In fact, if $\alpha, \beta, \gamma, \delta, \varepsilon, \zeta$ are the six values of α, then, denoting by Σ_n the sum of the nth powers of these quantities, we have

$$\begin{aligned} \alpha + \beta + \gamma + \delta + \epsilon + \zeta &= \Sigma_1 \\ \alpha^5 . \alpha + \beta^5 . \beta + \gamma^5 . \gamma + \delta^5 . \delta + \epsilon^5 . \epsilon + \zeta^5 . \zeta &= \Sigma_6, \\ &\vdots \\ \alpha^{25} . \alpha + \beta^{25} . \beta + \gamma^{25} . \gamma + \delta^{25} . \delta + \epsilon^{25} . \epsilon + \zeta^{25} . \zeta &= \Sigma_{26} ; \end{aligned}$$

and thence

$$\begin{aligned} (\alpha^5-\beta^5)(\alpha^5-\gamma^5)(\alpha^5-\delta^5)(\alpha^5-\epsilon^5)(\alpha^5-\zeta^5)\alpha &= \Sigma_{26} - \Sigma\beta^5 . \Sigma_{21} \\ &+ \Sigma\beta^5\gamma^5 . \Sigma_{16} - \Sigma\beta^5\gamma^5\delta^5 . \Sigma_{11} + \Sigma\beta^5\gamma^5\delta^5\epsilon^5 . \Sigma_6 - \beta^5\gamma^5\delta^5\epsilon^5, \end{aligned}$$

where $\Sigma\beta^5$ denotes the sum $\beta^5+\gamma^5+\delta^5+\epsilon^5+\zeta^5$; and in like manner $\Sigma\beta^5\gamma^5$, $\Sigma\beta^5\gamma^5\delta^5$, $\Sigma\beta^5\gamma^5\delta^5\epsilon^5$ denote the sum of the products of the quantities β^5, γ^5, δ^5, ϵ^5, ζ^5, taken two and two, three and three, four and four together.

But $\Sigma_1, \Sigma_6, \ldots \Sigma_{26}$ are symmetrical functions of $\alpha, \beta, \gamma, \delta, \epsilon, \zeta$; that is, they are rational functions of the coefficients of the equation for α, or, what is the same thing, of the coefficients of the equation of the fifth order; and the product $(\alpha^5-\beta^5)(\alpha^5-\gamma^5)(\alpha^5-\delta^5)(\alpha^5-\epsilon^5)(\alpha^5-\zeta^5)$, and the coefficients $\Sigma\beta^5$, &c. *quà* symmetrical functions of β^5, γ^5, δ^5, ϵ^5, ζ^5, are rational functions of α^5 and of the coefficients of the equation for α^5; that is, they are rational functions of α and of the coefficients of the equation of the fifth order. The only case of failure would be if two or more of the quantities $\alpha, \beta, \gamma, \delta, \epsilon, \zeta$ were equal; but this is not the case, since we are only concerned with the general equation of the fifth order. Hence by the last equation, α is given as a rational function of α^5 and of the coefficients of the equation of the fifth order.

2 Stone Buildings, W.C.,
February 4, 1862.

XXIX. *Note on the Remarks of* Mr. Jerrard.

By JAMES COCKLE, *M.A. &c.*[*]

1. M. HERMITE'S results are reconciled with the possibility of solving binomial equations of the fifth degree

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[*] Communicated by the Author.

by the fact that, for such equations, u and v vanish, and the *réduite* is not an Abelian sextic.

2. The function designated by $\sqrt[5]{\mathrm{K}}$ is generally expressible in rational terms of K. The suggestion that that function is an irreducible surd cannot be reconciled with the fact that it is a root of an equation of the sixth degree with rational coefficients. Assume, however, for a moment, that it can. Then, as we learn from a theorem of Abel, four other roots will be obtained by multiplying $\sqrt[5]{\mathrm{K}}$ into the unreal fifth roots of unity. Hence, denoting the remaining root by θ and the absolute term by A, we find

$$\mathrm{A}=\mathrm{K}\theta, \quad \theta=\frac{\mathrm{A}}{\mathrm{K}}.$$

Next, change the signs of the quadratic surds in K, or multiply its cubic surds into unreal cube roots of unity, or perform both operations simultaneously, and denote the result by K′. Then, by Abel's theorem, $\frac{\mathrm{A}}{\mathrm{K}'}$ is a root of the sextic. Consequently, for some of the forms of K′, we may establish the relation

$$\frac{\mathrm{A}}{\mathrm{K}'}=\iota^m\sqrt[5]{\mathrm{K}},$$

in other words $\sqrt[5]{\mathrm{K}}$ is not an irreducible surd, and the argument of M. Hermite is unshaken.

3. Mr. Jerrard's paper in this Journal for May 1861 does not disprove a rational commmunication between the resolvent product and its fifth power. His equation (e'_1) is not illusory but identical, and we may expunge it and substitute u^5 for v in the second of the equations marked (e). The result of this substitution, reduced by means of (U), may be made to take the form

$$\lambda_5+\lambda_4 u+\lambda_3 u^2+\ldots+\lambda_0 u^5=0,$$

and the six linear, but not homogeneous, equations

$$\lambda_5=0, \quad \lambda_4=0, \ldots \lambda_0=0,$$

will determine the six quantities $\nu_5, \nu_4, \ldots \nu_0$.

4. Mr. Jerrard says that it follows from what has been demonstrated in his 'Essay' that the fifth power of the resolvent product depends directly upon an Abelian equation. But the supposed demonstration consists in applying to Ξ the suffix f upon no assigned grounds. The consequence of this arbitrary assumption is a result apparently so inexplicable that Mr. Jerrard deemed it necessary to verify it. The attempted verification in this Journal for February 1852 is, to me at least, as inexplicable as the result itself; and in his 'Essay' Mr. Jerrard gives no explanation whatever of the difficulty. That 'Essay,' I am

aware, contains results which have gained Mr. Jerrard a great and widely spread renown. But his undemonstrated proposition ought not to prevail against the well-considered argument of M. Hermite.

5. Mr. Cayley has calculated P. His value may, by means of Mr. Harley's cyclical symbol, be written $\Sigma' x_\alpha x_\beta (x_\alpha - x_\beta)$. Developing this expression, and making the substitution $(\alpha\underset{\cdot\cdot}{\,}\beta)(\gamma\underset{\cdot\cdot}{\,}\epsilon)$, we see that P^2 is a six-valued function. Hence, by Lagrange's theory of similar functions, we find

$$\Xi = \mathfrak{r}\{P^2\}, \quad \Xi_{f(\beta\underset{\cdot\cdot}{\,}\epsilon)} = \mathfrak{r}\{P^2{}_{f(\beta\underset{\cdot\cdot}{\,}\epsilon)}\}.$$

Speaking of my own personal convictions, I am satisfied that a relation of the latter form ought to supersede the erroneous result given in Mr. Jerrard's 'Essay.' But the burden of proof is on Mr. Jerrard, and he offers none. He simply (I say it with a deference which long familiarity with his writings may well inspire) intrenches himself behind a conjectural and inadmissible equation, leaving others to explain how an equation connecting the six and the twelve-valued function can exist at all. The actual equation, I may add, does not give consistency to the self-contradictory result which Mr. Jerrard has attempted to verify, nor does it lead to an algebraic solution of the quintic. Such a solution M. Hermite's argument proves, on Mr. Jerrard's own premises, to be impossible.

4 Pump Court, Temple, London,
February 3, 1862.

XXX. *On the Puzzle of the Fifteen Young Ladies. By the* Rev. T. P. Kirkman, *A.M., F.R.S., Hon. Mem. of the Literary and Philosophical Societies of Manchester and Liverpool**.

MY distinguished friend Professor Sylvester, at page 371 of the 21st volume of this Journal, volunteers *en passant* an hypothesis as to the possible origin of this noted puzzle under its existing form. No man can doubt, after reading his words, that he was in possession of the property in question of the number 15 when he was an Undergraduate at Cambridge. But the difficulty of tracing the origin of the puzzle, from my own brains to the fountain named (p. 371) at that University, is considerably enhanced by the fact that, when I proposed the question in 1849, I had never had the pleasure of seeing either Cambridge or Professor Sylvester. My own account of the origin of the problem may be seen at p. 260, vol. v., of the Cambridge and Dublin Mathematical Journal, 1850. No other account of

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* Communicated by the Author.

it has, so far as I know, been published in print except this guess of Prof. Sylvester's in 1861.

It may be worth while to show the connexion between this tactical problem and the Theory of Groups, which has lately acquired new interest by the competition of last year for the prize of the Imperial Institute of France on that subject, and which presents to the investigator a most valuable region of truth yet unexplored.

The three solutions of Mr. Woolhouse (Lady's Diary, 1862) are all derangements of the group G,

$$\begin{array}{ccccccccccccccc}
1 & 2 & 3 & 4 & 5 & 6 & 7 & \scriptstyle 1 & \scriptstyle 2 & \scriptstyle 3 & \scriptstyle 4 & \scriptstyle 5 & \scriptstyle 6 & \scriptstyle 7 & 0 \\
2 & 3 & 4 & 5 & 6 & 7 & 1 & \scriptstyle 2 & \scriptstyle 3 & \scriptstyle 4 & \scriptstyle 5 & \scriptstyle 6 & \scriptstyle 7 & \scriptstyle 1 & 0 \\
3 & 4 & 5 & 6 & 7 & 1 & 2 & \scriptstyle 3 & \scriptstyle 4 & \scriptstyle 5 & \scriptstyle 6 & \scriptstyle 7 & \scriptstyle 1 & \scriptstyle 2 & 0 \\
4 & 5 & 6 & 7 & 1 & 2 & 3 & \scriptstyle 4 & \scriptstyle 5 & \scriptstyle 6 & \scriptstyle 7 & \scriptstyle 1 & \scriptstyle 2 & \scriptstyle 3 & 0 \\
5 & 6 & 7 & 1 & 2 & 3 & 4 & \scriptstyle 5 & \scriptstyle 6 & \scriptstyle 7 & \scriptstyle 1 & \scriptstyle 2 & \scriptstyle 3 & \scriptstyle 4 & 0 \\
6 & 7 & 1 & 2 & 3 & 4 & 5 & \scriptstyle 6 & \scriptstyle 7 & \scriptstyle 1 & \scriptstyle 2 & \scriptstyle 3 & \scriptstyle 4 & \scriptstyle 5 & 0 \\
7 & 1 & 2 & 3 & 4 & 5 & 6 & \scriptstyle 7 & \scriptstyle 1 & \scriptstyle 2 & \scriptstyle 3 & \scriptstyle 4 & \scriptstyle 5 & \scriptstyle 6 & 0
\end{array}$$

of the seventh order made with fifteen elements, $1234567\,{\scriptstyle 1234567}\,0$, considered as *consecutive*, ${\scriptstyle 1}$ counting for 8, ${\scriptstyle 2}$ for 9, &c., on the partition

$$15 = 7 \cdot 2 + 1 \cdot 1 = \mathrm{A}a + \mathrm{B}b,$$

which is determined by the two vertical circular factors 1234567 and ${\scriptstyle 1234567}$, the fifteenth element 0 being undisturbed.

There are $\dfrac{\Pi 15}{7}$ different derangements of G_1, made by altering the horizontal order of the vertical rows. Three of them are

G_1.

$$\begin{array}{lllll}
124 & 3{\scriptstyle 25} & 5{\scriptstyle 16} & 6{\scriptstyle 34} & 7{\scriptstyle 7}0 \\
235 & 4{\scriptstyle 36} & 6{\scriptstyle 27} & 7{\scriptstyle 45} & 1{\scriptstyle 1}0 \\
346 & 5{\scriptstyle 47} & 7{\scriptstyle 31} & 1{\scriptstyle 56} & 2{\scriptstyle 2}0 \\
457 & 6{\scriptstyle 51} & 1{\scriptstyle 42} & 2{\scriptstyle 67} & 3{\scriptstyle 3}0 \\
561 & 7{\scriptstyle 62} & 2{\scriptstyle 53} & 3{\scriptstyle 71} & 4{\scriptstyle 4}0 \\
672 & 1{\scriptstyle 73} & 3{\scriptstyle 64} & 4{\scriptstyle 12} & 5{\scriptstyle 5}0 \\
713 & 2{\scriptstyle 14} & 4{\scriptstyle 75} & 5{\scriptstyle 23} & 6{\scriptstyle 6}0
\end{array}$$

G_2. G_3.

$$\begin{array}{lllll|lllll}
124 & 3{\scriptstyle 46} & 5{\scriptstyle 23} & 6{\scriptstyle 15} & 7{\scriptstyle 7}0 & 124 & 3{\scriptstyle 15} & 5{\scriptstyle 46} & 6{\scriptstyle 23} & 7{\scriptstyle 7}0 \\
235 & 4{\scriptstyle 57} & 6{\scriptstyle 34} & 7{\scriptstyle 26} & 1{\scriptstyle 1}0 & 235 & 4{\scriptstyle 26} & 6{\scriptstyle 57} & 7{\scriptstyle 34} & 1{\scriptstyle 1}0 \\
346 & 5{\scriptstyle 61} & 7{\scriptstyle 45} & 1{\scriptstyle 37} & 2{\scriptstyle 2}0 & 346 & 5{\scriptstyle 37} & 7{\scriptstyle 61} & 1{\scriptstyle 45} & 2{\scriptstyle 2}0 \\
457 & 6{\scriptstyle 72} & 1{\scriptstyle 56} & 2{\scriptstyle 41} & 3{\scriptstyle 3}0 & 457 & 6{\scriptstyle 41} & 1{\scriptstyle 72} & 2{\scriptstyle 56} & 3{\scriptstyle 3}0 \\
561 & 7{\scriptstyle 13} & 2{\scriptstyle 67} & 3{\scriptstyle 52} & 4{\scriptstyle 4}0 & 561 & 7{\scriptstyle 52} & 2{\scriptstyle 13} & 3{\scriptstyle 67} & 4{\scriptstyle 4}0 \\
672 & 1{\scriptstyle 24} & 3{\scriptstyle 71} & 4{\scriptstyle 63} & 5{\scriptstyle 5}0 & 672 & 1{\scriptstyle 63} & 3{\scriptstyle 24} & 4{\scriptstyle 71} & 5{\scriptstyle 5}0 \\
713 & 2{\scriptstyle 35} & 4{\scriptstyle 12} & 5{\scriptstyle 74} & 6{\scriptstyle 6}0 & 713 & 2{\scriptstyle 74} & 4{\scriptstyle 35} & 5{\scriptstyle 12} & 6{\scriptstyle 6}0
\end{array}$$

The derangements G_1, G_2, G_3 are the three solutions of the school-girl problem given by Mr. Woolhouse, if we write his files of young ladies horizontally instead of vertically.

The derangements

$$GP, \quad GQ, \quad GR, \&c.$$

of a group G are in general of two kinds—those which are, and those which are not *derived derangements.* The first are such that

$$GP=PG, \quad GQ=QG, \&c.;$$

in the other,

$$GP \text{ is not } PG, \quad GQ \text{ is not } QG, \&c.$$

A derived derangement GP of G is obtained either by the operation

$$PG,$$

which denotes the result of effecting on every substitution of G the substitution P, or by the operation

$$GP,$$

which denotes the result of effecting upon the substitution P in turn every substitution of G.

A derived derangement GP, of G by P, is both the derivate PG of G by P, and the derangement GP of G by P.

Any other derangement GR of G, which it will be convenient to call a *simple derangement* of G by R, is no derivate RG of G by R, nor is it a derivate of G by any substitution.

Thus the simple derangement G_1 above written is obtained by effecting upon the substitution

$$\theta=\frac{1243_{25}5_{16}6_{34}7_{7}0}{1234567_{1234567}0}$$

in turn every one of the substitutions of the *model group* G, namely by adding to θ the products

$$\frac{2345671_{2345671}0}{1234567_{1234567}0}\theta,$$

$$\frac{3456712_{3456712}0}{1234567_{1234567}0}\theta, \&c.$$

Or G_1 may be formed, by a known property of all groups and their derangements, by effecting upon *any arrangement* of G_1 in turn all the substitutions of G.

The same account can be given of the generation of G_2 and G_3. Further, G_2 can be obtained, and any derangement whatever of G can be obtained, as a certain derangement of the derangement G_1. That is, if

$$\theta' = 1234_{52}75_{431}6_{67}0,$$

$$\theta'' = 1234_{1}67_{523}5_{467}0,$$

we have

$$G_2=G_1\theta', \text{ and } G_3=G_1\theta'';$$

which merely affirms that G_2 is formed by writing in order the 1st, 2nd, 3rd, 4th, (^{5}th$=$)12th, (^{2}nd$=$)9th, &c. vertical rows of G_1, &c. Or, if we please, we may interpret $G_2=G_1\theta'$ as expressing the result of operating on the substitution θ' (with *unity*, that is, the *first line of* G supposed written under it) by every arrangement of G_1 (with unity so subscribed).

The three systems G_1, G_2, G_3 cannot, as Mr. Woolhouse justly observes, be mutually elicited by any *direct substitution* of the form

$$G_1=\phi G_2=\chi G_3;$$

but this does not prevent one being obtained from another by a *right-handed* operation with a substitution, as

$$G_2=G_1\theta', \quad G_1=G_2\theta'^{-1},$$

where

$$\theta'^{-1}=1234{\scriptstyle 15}7{\scriptstyle 4}6{\scriptstyle 32}5{\scriptstyle 67}0.$$

It will not be found difficult to demonstrate that the three systems given by Mr. Woolhouse are all the simple derangements of G which give different solutions of the problem, if we define *that two solutions are different when one* G′ *cannot be obtained from the other* G″ *by a direct substitution*, as $G'=PG''$.

But we need not begin with the model group G_1, whose two vertical circular factors are 1234567 and ${\scriptstyle 1334567}$.

The late Mr. Anstice has shown (Camb. and Dub. Journ. vol. vii. p. 285) that we may employ the two vertical circles 1234567 ${\scriptstyle 1235674}$, which determine the group H,

$$\begin{array}{ccccccccccccccc}
1&2&3&4&5&6&7&\scriptstyle 1&\scriptstyle 2&\scriptstyle 3&\scriptstyle 4&\scriptstyle 5&\scriptstyle 6&\scriptstyle 7&0\\
2&3&4&5&6&7&1&\scriptstyle 2&\scriptstyle 3&\scriptstyle 5&\scriptstyle 1&\scriptstyle 6&\scriptstyle 7&\scriptstyle 4&0\\
3&4&5&6&7&1&2&\scriptstyle 3&\scriptstyle 5&\scriptstyle 6&\scriptstyle 2&\scriptstyle 7&\scriptstyle 4&\scriptstyle 1&0\\
4&5&6&7&1&2&3&\scriptstyle 5&\scriptstyle 6&\scriptstyle 7&\scriptstyle 3&\scriptstyle 4&\scriptstyle 1&\scriptstyle 2&0\\
5&6&7&1&2&3&4&\scriptstyle 6&\scriptstyle 7&\scriptstyle 4&\scriptstyle 5&\scriptstyle 1&\scriptstyle 2&\scriptstyle 3&0\\
6&7&1&2&3&4&5&\scriptstyle 7&\scriptstyle 4&\scriptstyle 1&\scriptstyle 6&\scriptstyle 2&\scriptstyle 3&\scriptstyle 5&0\\
7&1&2&3&4&5&6&\scriptstyle 4&\scriptstyle 1&\scriptstyle 2&\scriptstyle 7&\scriptstyle 3&\scriptstyle 5&\scriptstyle 6&0
\end{array}$$

This group is one of the

$$\frac{\Pi 15}{6.2.7^2}=15.13.12.11.10.9.8.5.4.3.2.1$$

equivalents of G: *vide* art. 12 of my memoir "On the Theory of Groups and Many-valued Functions," Manchester Memoirs, 1861.

One derangement of the group H given as a solution by Mr. Anstice is

H_1.

$$\begin{array}{llllll}
134 & 6{\scriptstyle 12} & 7{\scriptstyle 46} & 2{\scriptstyle 37} & 5{\scriptstyle 5} & 0 \\
245 & 7{\scriptstyle 23} & 1{\scriptstyle 17} & 3{\scriptstyle 54} & 6{\scriptstyle 6} & 0 \\
356 & 1{\scriptstyle 35} & 2{\scriptstyle 24} & 4{\scriptstyle 61} & 7{\scriptstyle 7} & 0 \\
467 & 2{\scriptstyle 56} & 3{\scriptstyle 31} & 5{\scriptstyle 72} & 1{\scriptstyle 4} & 0 \\
571 & 3{\scriptstyle 67} & 4{\scriptstyle 52} & 6{\scriptstyle 43} & 2{\scriptstyle 1} & 0 \\
612 & 4{\scriptstyle 74} & 5{\scriptstyle 63} & 7{\scriptstyle 15} & 3{\scriptstyle 2} & 0 \\
723 & 5{\scriptstyle 41} & 6{\scriptstyle 75} & 1{\scriptstyle 26} & 4{\scriptstyle 3} & 0
\end{array}$$

If we operate with the substitution

$$\frac{6543217\ {\scriptstyle 1237456}}{1234567\ {\scriptstyle 1234567}}=\phi,$$

it becomes $J=\phi H_1=$

$$\begin{array}{llllll}
643 & 1{\scriptstyle 12} & 7{\scriptstyle 75} & 5{\scriptstyle 36} & 2{\scriptstyle 4} & 0 \\
532 & 7{\scriptstyle 23} & 6{\scriptstyle 16} & 4{\scriptstyle 47} & 1{\scriptstyle 5} & 0 \\
421 & 6{\scriptstyle 34} & 5{\scriptstyle 27} & 3{\scriptstyle 51} & 7{\scriptstyle 6} & 0 \\
317 & 5{\scriptstyle 45} & 4{\scriptstyle 31} & 2{\scriptstyle 62} & 6{\scriptstyle 7} & 0 \\
216 & 4{\scriptstyle 56} & 3{\scriptstyle 42} & 1{\scriptstyle 73} & 5{\scriptstyle 1} & 0 \\
175 & 3{\scriptstyle 67} & 2{\scriptstyle 53} & 7{\scriptstyle 14} & 4{\scriptstyle 2} & 0 \\
764 & 2{\scriptstyle 71} & 1{\scriptstyle 64} & 6{\scriptstyle 25} & 3{\scriptstyle 3} & 0
\end{array}$$

which has the set of triplets made with 1234567 which Mr. Woolhouse employs.

This J is by our definition the same solution as H_1; and it is a derangement of the model group formed with the two circular factors 1765432 and ${\scriptstyle 1234567}$, or, what is the same thing, with the circular factors 1234567 and ${\scriptstyle 1765432}$.

If we operate on J with the substitution

$$\frac{1234567\ {\scriptstyle 2176543}}{1234567\ {\scriptstyle 4567123}},$$

it becomes the third of Mr. Woolhouse's forms.

A rigorous mathematical discussion of the problem of arranging $12n+3$ young ladies, to walk out daily till every pair have once, and once only walked abreast, $6n+1$ being a prime number, such as to supply a direct method of finding and exhausting all the different solutions, has not hitherto been given.

The following method will be found rigorous and satisfactory. Mr. Anstice showed, in his elegant memoir in vol. vii. of the Cambridge and Dublin Journal, that n primary triplets $A_1B_1C_1$, $A_2B_2C_2\,.\,.\,A_4B_4C_4$ can always be found such that, by continual additions of unity to every element, all the duads possible with $6n+1$ capitals shall be once, and once only exhausted. Thus for thirteen capitals, **1, 2, 3, 4, 5 . . . 12, 13,** we may take the triplets **1 . 2 . 5, 4 . 6 . 12,** and the first day's arrangements of 27 young ladies will be

$$\mathbf{1.2.5}\ \mathbf{4.6.12}\ \mathbf{3}ab\ \mathbf{7}cd\ \mathbf{8}ef\ \mathbf{9}gh\ \mathbf{10}ij\ \mathbf{11}kl\ \mathbf{13}_{13}\mathrm{V}.$$

It is required to determine *ab, cd, ef, gh . . . kl,* which are in some order the numbers 1 2 3 . . . 12 (to be counted as 14, 15 . . . 25), in every possible way, so as to solve the problem.

I shall content myself here with enunciating the two arithmetical conditions, which are necessary and sufficient.

First, it is required that, supposing $a > b$, $c > d$, &c.,

$$(a-b),\ (c-d),\ (e-f),\ (g-h),\ (i-j),\ (k-l)$$

shall be six (or for the general case of $12n+3$ shall be $3n$) *different* numbers > 0.

Secondly, it is required that the twelve numbers (or for the general case the $6n$ numbers defined by the capitals and small letters)

$$a-\mathbf{3},\ b-\mathbf{3},\ c-\mathbf{7},\ d-\mathbf{7}\ \ldots\ k-\mathbf{11},\ l-\mathbf{11}$$

shall be in some order the twelve numbers 1 2 3 4 . . . 12, or for the general case the $6n$ numbers 1 2 3 . . . $6n$, when estimated as residues to modulus 13 (or $6n+1$).

To find the numbers a, b, c, &c., we have only to write out the congruences

$$(a-\mathbf{3})^m+(b-\mathbf{3})^m+(c-\mathbf{7})^m+(d-\mathbf{7})^m+\ldots+(k-\mathbf{11})^m+$$
$$(l-\mathbf{11})\equiv 1^m+2^m+3^m+\ldots+12^m,\ (\text{mod. } 13,)$$

for as many values of m as we require. We can thus obtain by a solution of these congruences every possible system of the $3n$ duads which can satisfy the second condition. The number of these systems which satisfy also the first, is that of the different solutions of the problem.

It is perfectly certain that for the case of $12n+3=15$, the only systems possible will thus turn out to be, for addition to the capitals **3**, **5**, **6**, those read in the first lines of G_1, G_2, G_3.

Mr. Anstice's method of constructing the n primary triplets of the $6n+1$ capitals is not proved to exhaust the solutions.

A direct and exhaustive method of finding them is to seek for their *difference circles*; that is, to seek for the *perfect sets of partitions in triplets* of the prime number $6n+1$.

Def.—A *perfect set of partitions in* r-*plets* of $\mathrm{N}=k(r^2-r)+1$ is a system of k r-plets,

$$(a_1\,a_2\,a_3\ldots a_r)_1+(a'_1\,a'_2\,a'_3\ldots a'_r)_2+\ \ldots\ +(a''^{\cdots}_1\ a''^{\cdots}_2\ldots a''^{\cdots}_r)_k,$$

such that every number, 1, 2, 3, . . . $\mathrm{N}-1$, can be made by addition of *consecutive elements* of some one r-plet, the sum of the elements of each r-plet being N. These partitions can be directly found by the solution of a system of congruences similar to that above written. (*Vide* a paper of mine "On the perfect r-partitions of $N=r^2-r+1$," Transactions of the Historic Society of Lancashire and Cheshire, vol. ix. 1857.)

For example, the perfect sets of triplets for N=13 are obtained by solution of the six congruences, made with six values of m,

$$a_1^m+a_2^m+a_3^m+b_1^m+b_2^m+b_3^m+(a_1+a_2)^m+(a_2+a_3)^m+(a_3+a_1)^m$$
$$+(b_1+b_2)^m+(b_2+b_3)^m+(b_3+b_1)^m\equiv 1^m+2^m+\ldots+12m, \text{(mod. 13,)}$$

of which one solution is

$$a_1a_2a_3+b_1b_2b_3=139+265.$$

Perfect partitions are the following:—

N=7, 1.2.4: N=13, 1.3.9+2.6.5: N=19, 1.7.11 +2.14.3+4.9.6:

N=13, 1.2.6.4; 1.7.2.3: N=21, 1.3.10.2.5: N=31, 1.5.12.4.7.2:

N=73, 1.2.4.8.16.5.18.9.10; 1.4.7.6.3.28.2.8.14; 1.16.22.23.4.6.8.11; 1.8.12.2.3.13.24.4.6.

Using 139+265, or 1264 as difference circles, we form the triplets 1, 2, 5+4, 6, 12, or the quadruplet 1, 2, 4, 10. If we now complete under every element the circle 12345 ... 13, we shall have two columns of triplets, or a column of quadruplets, in which the duads are once, and once only exhausted. In the same way all the above perfect partitions may be used.

Six solutions of the triplet problem for $12n+3=27$ are given by Mr. Anstice. They are all derangements of the same group of the thirteenth order. The solution of the same problem by Mr. Mease (Camb. and Dub. Journ. vol. v. 1850, p. 262) is the sum of certain derangements of two groups of the third and ninth order added to a derangement of unity.

The method above given, depending on the theory of difference circles, will exhaust the solutions of Mr. Anstice's form, and can easily be modified so as to exhaust those of Mr. Mease's.

Croft Rectory, near Warrington,
January 14, 1862.

XXXI. *Liquid Diffusion applied to Analysis.*
By Thomas Graham, *F.R.S., Master of the Mint**.

THE property of volatility, possessed in various degrees by so many substances, affords invaluable means of separation, as is seen in the ever-recurring processes of evaporation and distilla-

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* From the Philosophical Transactions for 1861, Part I. p. 183.

tion. So similar in character to volatility is the diffusive power possessed by all liquid substances, that we may fairly reckon upon a class of analogous analytical resources to arise from it. The range also in the degree of diffusive mobility exhibited by different substances appears to be as wide as the scale of vapour-tensions. Thus hydrate of potash may be said to possess double the velocity of diffusion of sulphate of potash, and sulphate of potash again double the velocity of sugar, alcohol, and sulphate of magnesia. But the substances named belong all, as regards diffusion, to the more "volatile" class. The comparatively "fixed" class, as regards diffusion, is represented by a different order of chemical substances (marked out by the absence of the power to crystallize), which are slow in the extreme. Among the latter are hydrated silicic acid, hydrated alumina, and other metallic peroxides of the aluminous class, when they exist in the soluble form; with starch, dextrine and the gums, caramel, tannin, albumen, gelatine, vegetable and animal extractive matters. Low diffusibility is not the only property which the bodies last enumerated possess in common. They are distinguished by the gelatinous character of their hydrates. Although often largely soluble in water, they are held in solution by a most feeble force. They appear singularly inert in the capacity of acids and bases, and in all the ordinary chemical relations. But, on the other hand, their peculiar physical aggregation, with the chemical indifference referred to, appears to be required in substances that can intervene in the organic processes of life. The plastic elements of the animal body are found in this class. As gelatine appears to be its type, it is proposed to designate substances of the class as *colloids*, and to speak of their peculiar form of aggregation as the *colloidal condition of matter*. Opposed to the colloidal is the crystalline condition. Substances affecting the latter form will be classed as *crystalloids*. The distinction is no doubt one of intimate molecular constitution.

Although chemically inert in the ordinary sense, colloids possess a compensating activity of their own arising out of their physical properties. While the rigidity of the crystalline structure shuts out external impressions, the softness of the gelatinous colloid partakes of fluidity, and enables the colloid to become a medium for liquid diffusion, like water itself. The same penetrability appears to take the form of cementation in such colloids as can exist at a high temperature. Hence a wide sensibility on the part of colloids to external agents. Another and eminently characteristic quality of colloids is their mutability. Their existence is a continued metastasis. A colloid may be compared in this respect to water while existing liquid at a temperature under its usual freezing-point, or to a supersaturated saline solution. Fluid colloids appear to have always a *pectous** modification, and they often pass under the slightest influences from the first into the second condition. The solution of hydrated silicic acid, for instance, is easily obtained in a state of purity, but it cannot be preserved. It may remain fluid for days or weeks in a sealed tube, but is sure to gelatinize and become insoluble at last. Nor does the change of this colloid appear to stop at that point; for the mineral forms of silicic acid, deposited from water, such as flint, are often found to have passed, during the geological ages of their existence, from the vitreous or colloidal into the crystalline condition (H. Rose). The colloidal is, in fact, a dynamical state of matter, the crystalloidal being the statical condition. The colloid possesses *energia*. It may be looked upon as the probable primary source of the force appearing in the phenomena of vitality. To the gradual manner in which colloidal changes take place (for they always demand time as an element), may the characteristic protraction of chemico-organic changes also be referred.

A simple and easily applicable mode of effecting a diffusive separation is to place the mixed substance under a column of water, contained in a cylindrical glass jar of 5 or 6 inches in depth. The mixed solution may be conducted to the bottom of the jar by the use of a fine pipette, without the occurrence of any sensible intermixture. The spontaneous diffusion, which immediately commences, is allowed to go on for a period of several days. It is then interrupted by siphoning off the water from the surface in successive strata, from the top to the bottom of the column. A species of cohobation has been the consequence of unequal diffusion, the most rapidly diffusive substance being isolated more and more as it ascended. The higher the water column, sufficient time being always given to enable the most diffusive substance to appear at the summit, the more completely does a portion of that substance free itself from such other less diffusive substances as were originally associated with it. A marked effect is produced even where the difference in diffusibility is by no means considerable, such as the separation of chloride of potassium from chloride of sodium, of which the relative diffusibilities are as 1 to 0·841. Supposing a third metal of the potassium group to exist, standing above potassium in diffusibility as potassium stands above sodium, it may be safely predicated that the new metal would admit of being

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* Πηκτὸς, *curdled*. As fibrine, caseine, albumen. But certain liquid colloid substances are capable of forming a jelly and yet still remaining liquefiable by heat and soluble in water. Such is gelatine itself, which is not pectous in the condition of animal jelly, but may be so as it exists in the gelatiniferous tissues.

separated from the other two metals by an application of the jar diffusion above described.

A certain property of colloid substances comes into play most opportunely in assisting diffusive separations. The jelly of starch, that of animal mucus, of pectine, of the vegetable gelose of Payen, and other solid colloidal hydrates, all of which are, strictly speaking, insoluble in cold water, are themselves permeable when in mass, as water is, by the more highly diffusive class of substances. But such jellies greatly resist the passage of the less diffusive substances, and cut off entirely other colloid substances like themselves that may be in solution. They resemble animal membrane in this respect. A mere film of the jelly has the separating effect. Take for illustration the following simple experiment.

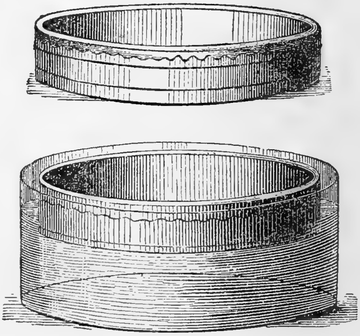
A sheet of very thin and well-sized letter-paper, of French manufacture, having no porosity, was first thoroughly wetted and then laid upon the surface of water contained in a small basin of less diameter than the width of the paper, and the latter depressed in the centre so as to form a tray or cavity capable of holding a liquid. The liquid placed upon the paper was a mixed solution of cane-sugar and gum-arabic, containing 5 per cent. of each substance. The pure water below and the mixed solution above were therefore separated only by the thickness of the wet sized paper. After twenty-four hours the upper liquid appeared to have increased sensibly in volume, through the agency of osmose. The water below was found now to contain three-fourths of the whole sugar, in a condition so pure as to crystallize when the liquid was evaporated on a water-bath. Indeed the liquid of the basin was only in the slightest degree disturbed by subacetate of lead, showing the absence of all but a trace of gum. Paper of the description used is sized by means of starch. The film of gelatinous starch in the wetted paper has presented no obstacle to the passage of the crystallized sugar, but has resisted the passage of the colloid gum. I may state at once what I believe to be the mode in which this takes place.

The sized paper has no power to act as a filter. It is mechanically impenetrable, and denies a passage to the mixed fluid as a whole. Molecules only permeate this septum, and not masses. The molecules also are moved by the force of diffusion. But the water of the gelatinous starch is not directly available as a medium for the diffusion of either the sugar or gum, being in a state of true chemical combination, feeble although the union of water with starch may be. The hydrated compound itself is solid, and also insoluble. Sugar, however, with all other crystalloids, can separate water, molecule after molecule, from any hydrated colloid, such as starch. The sugar thus obtains the liquid medium required for diffusion, and makes its way through the gelatinous septum. Gum, on the other hand, possessing as a colloid an affinity for water of the most feeble description, is unable to separate that liquid from the gelatinous starch, and so fails to open the door for its own passage outwards by diffusion.

The separation described is somewhat analogous to that observed in a soap-bubble inflated with a gaseous mixture composed of carbonic acid and hydrogen. Neither gas, as such, can penetrate the water-film. But the carbonic acid, being soluble in water, is condensed and dissolved by the water-film, and so is enabled to pass outwards and reach the atmosphere; while hydrogen, being insoluble in water, or nearly so, is retained behind within the vesicle.

It may perhaps be allowed to me to apply the convenient term *dialysis* to the method of separation by diffusion through a septum of gelatinous matter. The most suitable of all substances for the dialytic septum appears to be the commercial material known as vegetable parchment or parchment-paper, which was first produced by M. Gaine, and is now successfully manufactured by Messrs. De la Rue. This is unsized paper, altered by a short immersion in sulphuric acid, or in chloride of zinc, as proposed by Mr. T. Taylor. Paper so metamorphosed acquires considerable tenacity, as is well known; and when wetted it expands and becomes translucent, evidently admitting of hydration. A slip of 25 inches in length was elongated 1 inch in pure water, and 1·2 inch in water containing one per cent. of carbonate of potash. In the wetted state parchment-paper can easily be applied to a light hoop of wood, or, better, to a hoop made of sheet gutta percha, 2 inches in depth and 8 or 10 inches in diameter, so as to form a vessel like a sieve in form (fig. 1). The disk of parchment-paper used should exceed in diameter the hoop to be covered by 3 or 4 inches, so as to rise well round the hoop. It may be bound to the hoop by string, or by an elastic band, but should not be firmly secured. The parchment-paper must not be porous. Its soundness will be ascertained by sponging the upper surface with pure water, and then observing that no wet spots show themselves on the opposite side. Such defects may be remedied by applying liquid albumen, and then coagulating

Fig. 1.—Hoop Dialyser.



the same by heat. Mr. De la Rue recommends the use of albumen in cementing parchment-paper, which thus may be formed into cells and bags very useful in dialytic experiments. The mixed fluid to be dialysed is poured into the hoop, upon the surface of the parchment-paper, to a small depth only, such as half an inch. The vessel described (*dialyser*) is then floated in a basin containing a considerable volume of water, in order to induce egress of the diffusive constituents of the mixture. Half a litre of urine, dialysed for twenty-four hours, gave its crystalloidal constituents to the external water. The latter, evaporated by a water-bath, yielded a white saline mass. From this mass urea was extracted by alcohol in so pure a condition as to appear in crystalline tufts upon the evaporation of the alcohol.

1. *Jar Diffusion.*

The mode of diffusing more lately followed, which I have already alluded to as jar diffusion, is extremely simple, and gives results of more precision than could possibly be anticipated. The salt is allowed to rise from below into a cylindrical column of water, and after a fixed time, the proportion of salt which has risen to various heights in the column is observed. The water was contained in a plain cylindrical glass jar, of about 152 millimetres (6 inches) in height and 87 millimetres (3·45 inches) in width. In operating, seven-tenths of a litre of water were first placed in the jar, and then one-tenth of a litre of the liquid to be diffused was carefully conveyed to the bottom of the jar by means of a fine pipette. The whole fluid column then measured 127 millimetres (5 inches) in height. So much as five or six minutes of time were occupied in emptying the pipette at the bottom of the jar, and extremely little disturbance was occasioned in the superincumbent water, as could be distinctly seen when the liquid introduced by the pipette was coloured. The jar was then left undisturbed, to allow diffusion to proceed, the experiments being always conducted in an apartment of constant, or nearly constant temperature. When a certain time had elapsed, the diffusion was interrupted by drawing off the liquid from the top, by means of a small siphon, slowly and deliberately, as the liquid had been first introduced, in portions of 50 cubic centimetres, or one-sixteenth of the whole volume. The open end of the short limb of the siphon was kept in contact with the surface of the liquid in the jar, and the portion of liquid drawn off was received in a graduated measure. By evaporating each fraction separately, the quantity of salt which had risen into equal sections of the liquid column was ascertained. From the bottom of two jars, A and B for instance, a 10 per cent. solution of chloride of sodium was diffused for a period of fourteen days. The whole quantity of salt present in each jar was 10 grammes, which was found at the end to be distributed as follows in the different sectional strata of fluid, numbering them from the top downwards:—

In the first or highest stratum, 0·103 and 0·105 gramme of salt in A and B respectively; in the second stratum, 0·133 and 0·125; in the third stratum, 0·165 and 0·158; in the fourth stratum, 0·204 and 0·193; in the fifth stratum, 0·273 and 0·260; in the sixth stratum, 0·348 and 0·332; in the seventh stratum, 0·440 and 0·418; in the eighth stratum, 0·545 and 0·525; in the ninth stratum, 0·657 and 0·652; in the tenth stratum, 0·786 and 0·747; in the eleventh stratum, 0·887 and 0·875; in the twelfth stratum, 0·994 and 0·984; in the thirteenth stratum, 1·080 and 1·100; in the fourteenth stratum, 1·176 and 1·198; in the fifteenth and sixteenth strata together, 2·209 and 2·324 grammes. With differences so moderate in amount between corresponding strata in the two experiments, this method of observing diffusion may claim a considerable degree of precision.

In similar experiments made at the same time and temperature with sugar, gum-arabic and tannin of nut-galls, the final distribution of each substance was different in each case, and the results may be placed together in illustration of unequal diffusibility, as exhibited by this method of observation. Two experiments were made on each substance, as with chloride of sodium, but the mean result only need be stated.

Table I.—Diffusion of 10 per cent. solutions (10 grammes of substance in 100 cub. centims. of fluid) into pure water, after fourteen days, at 10° (50° Fahr.).

No. of stratum (from above downwards).	Chloride of sodium.	Sugar.	Gum.	Tannin.
1	·104	·005	·003	·003
2	·129	·008	·003	·003
3	·162	·012	·003	·004
4	·198	·016	·004	·003
5	·267	·030	·003	·005
6	·340	·059	·004	·007
7	·429	·102	·006	·017
8	·535	·180	·031	·031
9	·654	·305	·097	·069
10	·766	·495	·215	·145
11	·881	·740	·407	·288
12	·991	1·075	·734	·556
13	1·090	1·435	1·157	1·050
14	1·187	1·758	1·731	1·719
15 and 16	2·266	3·783	5·601	6·097
	9·999	10·003	9·999	9·997

The superimposed column of water being 111 millimetres (4·38 inches) in height, the chloride of sodium, it will be observed, has diffused in sensible quantity to the top, and could have risen higher; the upper layer being found to contain 0·104 gramme of salt, or 1 per cent. of the whole quantity present. The apex of the diffusion column of sugar appears to have just reached the top of the liquid in the fourteen days of the experiment, for ·005 gramme only of that substance is found in the first stratum, followed by ·008, ·012, ·016, and ·030 in the following strata. Again, no gum appears to be carried by diffusion higher than the seventh stratum (2·2 inches), which stratum contains ·006 gramme, followed by ·031 gramme in the eighth stratum. The minute quantities of substance shown in the first to the sixth stratum, and which do not altogether exceed ·020 gramme, are no doubt the result of accidental dispersion, arising probably from a movement of the upper fluid occasioned by slight inequalities of temperature. The diffusion of tannin is even less advanced than that of gum; but the former numbers are apparently influenced by a partial decomposition, to which tannin is known to be liable, and which gives rise to new and more highly diffusible substanccs.

Experiments continued, like those last described, for a constant time, do not exhibit the exact relative diffusibilities, although these could be obtained by proceeding to ascertain, by repeated trial, the various times required to bring about a similar distribution and equal amount of diffusion in all the salts. The numbers observed, however, may afford data for the deduction of the relative diffusibilities by calculation.

A particular advantage of the new method is the means which it affords of ascertaining the absolute rate or velocity of diffusion. It becomes possible to state the distance which a salt travels per second in terms of the metre. It is easy to see that such a constant must enter into all the chronic phenomena of physiology, and that it holds a place in vital science not unlike the time of the falling of heavy bodies in the physics of gravitation. It may therefore be not amiss to place here in a short tabular form the results observed of the diffusion of a few more substances, conducted in the same manner as the preceding.

Table II.—Diffusion of 10 per cent. solutions for fourteen days.

No. of stratum (from above downwards).	Sulphate of magnesia, at 10°.	Albumen, at 13° to 13°·5.	Caramel, at 10° to 11°.
1	·007		
2	·011		
3	·018		
4	·027		
5	·049		
6	·085	......	·003
7	·133	......	·005
8	·218	·010	·010
9	·331	·015	·023
10	·499	·047	·033
11	·730	·113	·075
12	1·022	·343	·215
13	1·383	·855	·705
14	1·803	1·892	1·725
15 and 16	3·684	6·725	7·206
	10·000	10·000	10·000

The sulphate of magnesia was anhydrous. The albumen was purified by Wurtz's method. The caramel was partly purified by precipitation by alcohol, as recommended by Fremy, and further by other means which will again be referred to. It will be remarked that the diffusion of sulphate of magnesia exhibited above is very similar to that of sugar in a former Table, but is slightly less advanced. The similarity in diffusibility of these two substances had already been observed in the experiments of former papers. The fall in rate on passing from these crystalloids to the colloids tannin, albumen, and caramel is very striking. The elevation in the liquid column attained by albumen or by caramel is moderate indeed compared with that of crystalline substances. Of albumen, which will be looked upon with most interest, no portion whatever was found in the seven higher strata. It appeared to the extent of 0·010 gramme in the eighth stratum, 0·015 in the ninth stratum, 0·047 in the tenth stratum, 0·113 in the eleventh stratum, 0·343 in the twelfth stratum; while the great mass of this substance remained in the four lower strata. The diffused albumen did not appear to lose its coagulability, or to be otherwise altered. It will be seen immediately that the diffusion of sugar advances as much in two days as the albumen above in fourteen days (Table IV.).

The diffusion of caramel is the slowest of all, and does not much exceed in fourteen days the diffusion of sugarin a single day.

It was considered useful to possess examples of the progress of diffusion, in one or two selected substances, for successive periods of time, so as to exemplify the continuous progress of diffusion in these substances. Such a chronological progress of diffusion in a particular substance becomes a standard of comparison for single experiments on the diffusion of other substances. The substances selected were chloride of sodium and cane-sugar.

Table III.—Diffusion of a 10 per cent. solution of Chloride of Sodium in different times.

No. of stratum.	In 4 days, at 9° to 10°.	In 5 days, at 11°·75.	In 7 days, at 9°.	In 14 days, at 10°.
1	·004	·004	·013	·104
2	·004	·006	·017	·129
3	·005	·011	·028	·162
4	·011	·020	·051	·198
5	·023	·040	·081	·267
6	·040	·075	·134	·340
7	·080	·134	·211	·429
8	·145	·233	·318	·535
9	·261	·368	·460	·654
10	·436	·589	·640	·766
11	·706	·762	·850	·881
12	1·031	1·090	1·057	·991
13	1·416	1·357	1·317	1·090
14	1·815	1·697	1·527	1·187
15 and 16	4·023	3·613	3·294	2·266
	10·000	9·999	9·998	9·999

Table IV.—Diffusion of a 10 per cent. solution of Cane-sugar in different times.

No. of stratum.	In 1 day, at 10°·75.	In 2 days, at 10°.	In 6 days, at 9°.	In 7 days, at 9°.	In 8 days, at 9°.	In 14 days, at 10°.
1	......	......	·001	·002	·002	·005
2	......	......	·002	·002	·003	·008
3	......	......	·002	·003	·003	·012
4	......	......	·002	·004	·004	·016
5	......	......	·003	·004	·007	·030
6	......	......	·005	·007	·012	·059
7	......	......	·011	·020	·031	·102
8	·002	·002	·024	·051	·072	·180
9	·002	·008	·071	·121	·154	·305
10	·005	·027	·170	·260	·304	·495
11	·024	·107	·376	·507	·555	·740
12	·133	·344	·727	·897	·858	1·075
13	·597	·930	1·282	1·410	1·365	1·435
14	1·850	1·940	1·930	1·950	1·955	1·758
15 and 16	7·386	6·641	5·392	4·760	4·674	3·783
	9·999	9·999	9·998	9·998	9·999	10·003

The scheme of the diffusion of the chloride of sodium may afford terms of comparison for the metallic salts, acids and other highly diffusible substances, while the scheme of sugar will be found more useful in appreciating the diffusion of organic and other less diffusible substances. In comparing the two Tables together, it appears that a fourteen days' diffusion of sugar is greater in amount than a four days' diffusion of chloride of sodium, but less than a five days' diffusion of the same substance. The diffusion of chloride of sodium appears to be pretty nearly three times greater (or more rapid) than that of sugar.

The following experiments were made upon hydrochloric acid and chloride of sodium at a somewhat lower temperature and for times which are different, but which give a nearly equal diffusion for each substance.

TABLE IV. *bis.*—10 per cent. solutions.

No. of stratum.	Hydrochloric acid, in grammes. 3 days at 5°.	Chloride of sodium, in grammes. 7 days at 5°.
1	·003	·003
2	·006	·009
3	·012	·010
4	·022	·026
5	·043	·055
6	·086	·082
7	·162	·165
8	·308	·270
9	·406	·403
10	·595	·595
11	·837	·823
12	1·080	1·085
13	1·163	1·270
14	1·578	1·615
15 and 16	3·699	3·589
	10·000	10·000

The diffusion of hydrochloric acid in three days corresponds closely with the diffusion of chloride of sodium in seven days. The times of equal diffusion for these two substances, at the temperature of the experiment, appear accordingly to be 1 (hydrochloric acid) and 2·33 (chloride of sodium). Hydrochloric acid and the allied hydracids, with other monobasic acids, are the most diffusive substances known. The general results of several series of experiments may be expressed approximately by the following numbers:—

Approximate times of equal diffusion.

Hydrochloric acid	1
Chloride of sodium	2·33
Sugar	7
Sulphate of magnesia	7
Albumen	49
Caramel	98

It is curious to observe the effect of changing the liquid atmosphere in which diffusion takes place, which is water in all these experiments, and replacing it by another fluid, namely alcohol. Two substances were diffused in the usual manner, but with this difference, that the substances were dissolved in alcohol, and the solutions placed under a column of the same liquid in the jar. The alcohol was of sp. gr. 0·822 (90 per cent.).

TABLE V.—Diffusion in Alcohol of 10 per cent. solutions of Iodine and of Acetate of Potash in seven days.

No. of stratum.	Iodine at 14°.	Acetate of potash, at 14° to 15°.
1	·028	·055
2	·033	·057
3	·046	·061
4	·038	·063
5	·037	·064
6	·039	·066
7	·081	·070
8	·143	·071
9	·263	·072
10	·417	·095
11	·637	·285
12	·936	·619
13	1·235	1·157
14	1·506	1·907
15 and 16	4·561	5·358
	10·000	10·000

TABLE V. *bis.*—Diffusion in Alcohol of a 10 per cent. solution of Resin, for seven days, at 14°·5.

No. of stratum.	Diffusate, in grammes.
1	·017
2	·017
3	·018
4	·017
5	·019
6	·020
7	·022
8	·024
9	·025
10	·080
11	·210
12	·498
13	·992
14	1·700
15 and 16	6·341
	10·000

The experiments were conducted in the absence of light, and there is no reason to believe that the iodine acted chemically upon the alcohol. The diffusion is more advanced in the iodine than in the acetate of potash, but in both is moderate in amount, confirming the early experiments with phials, which appeared to show that the diffusion process was several times slower in alcohol than in water. The small quantities of iodine found in each of the six superior strata are nearly equal, and were no doubt accidentally elevated by the mobility of this fluid, arising from its high dilatability by heat compared with that of water at the same low temperature. The diffusion may be considered then as confined to the nine lower strata, and considerably resembles that of sugar in water for eight days.

The diffusion of acetate of potash is still less advanced than that of iodine, and is probably confined to the six lower strata, the salt found in the higher strata presenting in its distribution the appearance of having been carried there by a movement of the fluid consequent upon heat-dilatation, and not by diffusion. The diffusion of acetate of potash in alcohol observed during seven days approximates to that of sugar in water during six days (Table IV.).

I now proceed to observations of the simultaneous diffusion of two substances in the same fluid. The great object of this class of experiments was to separate salts of unequal diffusibility, and to test the application of diffusion as an analytical process. A mixture of two salts being placed at the bottom of the jar, it may be expected that the salts will diffuse pretty much as they do when they are diffused separately; the more diffusive salt travelling most rapidly, and showing itself first and always most largely in the upper strata. The early experiments of diffusion from phials had shown indeed that inequality of diffusion is increased by mixture, and the actual separation is consequently greater than that calculated from the relative diffusibilities of the mixed substances. Chlorides of potassium and sodium diffuse nearly in the proportion of 1 to 0·841, according to the earlier experiments. They may afford, therefore, the means of observing the amount of separation that may be produced by a very moderate difference in diffusibility. A mixture of 5 grammes of each salt in the usual 100 cub. cent. of water was diffused.

Table VI.—Diffusion of a mixture of 5 per cent. of Chloride of Potassium and 5 per cent. of Chloride of Sodium, for seven days, at 12° to 13°.

Number of stratum.	Chloride of Potassium.	Chloride of sodium.	Total diffusate.
1	·018	·014	·032
2	·025	·015	·040
3	·044	·014	·058
4	·075	·017	·092
5	·101	·034	·135
6	·141	·063	·204
7	·185	·104	·289
8	·252	·151	·403
9	·330	·212	·542
10	·349	·351	·700
11	·418	·458	·876
12	·511	·559	1·070
13	·552	·684	1·236
14	·615	·772	1·387
15 and 16	1·385	1·551	2·936
	5·001	4·999	10·000

In the upper part of the Table chloride of potassium always appears in excess, but not in so large a proportion in the first three strata as in the fourth. This inequality may be partly owing to mechanical dispersion of the mixed solution, but is to be referred chiefly, I believe, to errors of analysis from a loss of the chloride of potassium difficult to avoid in the determination of minute proportions of that salt by means of chloride of platinum. Of 92 milligrammes of salt found in the fourth stratum, 75 milligrammes, or 81·5 per cent., are chloride of potassium. The first six strata contain together 561 milligrammes, of which 404 milligrammes, or 72 per cent., that is nearly three-fourths, are chloride of potassium. We have to descend to the tenth stratum before the salts are found in equal proportions. The progression is then inverted, and chloride of sodium comes to preponderate in the lower strata.

It is evident that the preceding experiment might be so conducted as to diffuse away the chloride of potassium and leave below a mixture containing chloride of sodium in relative excess, to as great an extent as the chloride of potassium is found above, in the last experiment.

Further, the mixture in which chloride of potassium was concentrated in the experiment described, so as to form 72 per cent. of the whole mixture, might be subjected again to diffusion in the same manner. In an experiment upon a mixture of 7·5 grammes of chloride of potassium and 2·5 grammes of chloride of sodium, the six upper strata gave 640 milligrammes of salt, of which 610 milligrammes, or 95·3 per cent., were chloride of potassium. It is obvious that by repeating this diffusive rectification a sufficient number of times, a portion of the more diffusive salt might be obtained at last in a state of sensible purity.

The preceding example illustrates the separation of unequally diffusive metals or bases; the following example, on the other hand, the separation of unequally diffusive acids united with a common base. Chloride of sodium and sulphate of soda diffuse separately in the phial experiments in the proportion of 1 to 0·707.

TABLE VII.—Diffusion of 5 per cent. of Chloride of Sodium and 5 per cent. of anhydrous Sulphate of Soda, for seven days, at 10° to 10°·75.

Number of stratum.	Chloride of sodium, in grammes.	Sulphate of soda, in grammes.	Total diffusate, in grammes.
1	·009	......	·009
2	·013	·001	·014
3	·024	·002	·026
4	·038	·003	·041
5	·060	·006	·066
6	·095	·012	·107
7	·141	·029	·170
8	·203	·059	·262
9	·278	·115	·393
10	·360	·205	·565
11	·473	·317	·790
12	·560	·507	1·067
13	·637	·694	1·331
14	·718	·909	1·627
15 and 16	1·390	2·141	3·531
	4·999	5·000	9·999

Here the separation is still more sensible than before with the bases. The six upper strata contain 263 milligrammes of salt, of which 239 milligrammes, that is 90·8 per cent., are chloride of sodium. The salt of the upper eight strata amounts to 695 milligrammes, of which 583 milligrammes, or 83·9 per cent., are chloride of sodium.

How long the diffusion should be continued in a liquid column of limited height, such as in these experiments, so as to produce the greatest separation, is a question of some interest, which can only be answered by experiment. The last diffusion was accordingly repeated, with the difference that it was continued for double the former time.

Table VIII.—Diffusion of 5 per cent. of Chloride of Sodium and 5 per cent. of Sulphate of Soda, for fourteen days, at 10° to 11°.

Number of stratum.	Chloride of Sodium, in grammes.	Sulphate of soda, in grammes.	Total diffusate, in grammes.
1	·077	·005	·082
2	·089	·009	·098
3	·105	·014	·119
4	·130	·026	·156
5	·161	·044	·205
6	·199	·072	·271
7	·240	·111	·351
8	·289	·173	·462
9	·337	·241	·578
10	·392	·334	·726
11	·433	·433	·866
12	·487	·539	1·026
13	·525	·646	1·171
14	·555	·745	1·300
15 and 16	·979	1·609	2·588
	4·998	5·001	9·999

The salt contained in the three upper strata amounts to 299 milligrammes, of which 271, or 90·6 per cent. of the whole, are chloride of sodium. The upper five strata yield 660 milligrammes of salt, of which 562 milligrammes, or 85·1 per cent., are chloride of sodium. These proportions are not dissimilar to those deduced from the former Table, and show that little is gained in the way of separation by extending the diffusion-period from seven to fourteen days, unless, indeed, the column of fluid be increased in height at the same time.

It might be worth observing whether the separation of two unequally diffusive metals can be favoured by varying the acid, or form of combination—whether, for instance, the hydrates of potash and soda would not separate to a greater extent than has been observed of the chlorides of potassium and sodium, the separate diffusibilities of the former substances being as 1 to 0·7, while that of the latter are as 1 to 0·841. I have not, however, pursued this branch of the subject.

The separation of the same metals from each other may possibly be favoured in another manner. In the preceding experiments (Table VI.) the two metals were in union with the same acid, or rather both were in the state of chloride. But the metals might be used in combination with different acids, and these acids themselves might be of equal or of unequal diffusibility. If of equal diffusibility, such as nitric and hydrochloric acids, no reason appears why the acids should affect the amount of separation. But if the acids are unlike in diffusibility, the case is not so clear. If, for instance, the potassium were in the form of chloride and the sodium in that of sulphate, might not the diffusion of the potassium be promoted by the highly diffusive chlorine with which it is associated, and the diffusion of the soda, on the other hand, be retarded by its association with the slowly diffusive sulphuric acid? Will, in fine, the separation of the metals be greater from a mixture of chloride of potassium and sulphate of soda, or even from sulphate of potash and chloride of sodium, than from the two chlorides or from the two sulphates? The inquiry, it will be remarked, raises the whole question of the distribution of acid and base in solutions of mixed salts. It will be illustrated by a comparison of the diffusion of chloride of potassium mixed with sulphate of soda, with the diffusion of sulphate of potash mixed with the chloride of sodium, the salts being taken in equivalent proportions.

TABLE IX.—Diffusion of a mixture of 5·12 per cent. of Chloride of Potassium and 4·88 per cent. of Sulphate of Soda (equivalent proportions), for seven days, at 14°.

No. of stratum.	Potassium, in grammes.	Sulphuric acid, in grammes.	Total diffusate, in grammes.
1	·028	·002	·024
2	·035	·002	·030
3	·048	·004	·045
4	·064	·009	·066
5	·092	·016	·097
6	·128	·032	·149
7	·174	·058	·215
8	·242	·105	·316
9	......	......	·441
10	......	......	·615
11	......	......	·815
12	......	......	1·042
13	......	......	1·290
14	......	......	1·517
15 and 16	......	......	3·346
			10·008

The weight of the mixed salt was always 10 grammes. The diffusions exhibited in Tables IX. and X. are strikingly similar, and indeed may be considered as identical. It thus appears that the diffusion of the metals is not affected by the acid with which they are in combination. The result is quite in harmony with Berthollet's view, that the acids and bases are indifferently combined, or that a mixture of chloride of potassium and sulphate of soda is the same thing as a mixture of sulphate of potash and chloride of sodium, when the mixtures are in a state of solution. With two acids very unequal in their affinity for bases, the result possibly might be very different.

TABLE X.—Diffusion of a mixture of 4·01 per cent. of Chloride of Sodium and 5·99 per cent. of Sulphate of Potash (equivalent proportions), for seven days, at 14°.

No. of stratum.	Potassium, in grammes.	Sulphuric acid, in grammes.	Total diffusate, in grammes.
1	·028	·002	·023
2	·034	·002	·030
3	·049	·004	·044
4	·064	·009	·065
5	·092	·015	·096
6	·128	·031	·149
7	·172	·059	·219
8	·242	·104	·315
9	......	......	·435
10	......	......	·600
11	......	......	·797
12	......	......	1·025
13	......	......	1·261
14	......	......	1·480
15 and 16	......	......	3·467
			10·016

2. *Effect of Temperature on Diffusion.*

Diffusion is promoted by heat; and separations may accordingly be effected in a shorter time at high than at low temperatures. In a series of observations made upon hydrochloric acid, the diffusion of that substance was carefully determined at 15°·5 (60° F.), and at three higher points, advancing by 11°·11 (20° F.). The ratios of the diffusions observed were as follows:—

Diffusion of hydrochloric acid at 15°·55 (60° F.), 1
" " at 26°·66 (80° F.), 1·3545
" " at 37°·77 (100° F.), 1·7732
" " at 48°·88 (120° F.), 2·1812

The increments of diffusibility, 0·3545, 0·4187, and 0·408 for equal increments of temperature, are probably affected by small errors of observation, but they appear to indicate that the diffusion increases at a higher, although not greatly higher, rate than the temperature. The average increase of diffusibility for the whole range of temperature observed is 0·03543, or $\frac{1}{28}$ for each degree (0·01969, or $\frac{1}{50}$ nearly for 1° F.).

The preceding experiments were made by diffusing a 2 per cent solution of hydrochloric acid from wide-mouth phials immersed in a jar of water, as in my former experiments*. The times were observed in which an equal amount of the acid (0·777 gramme from three phials) was diffused out. These times of equal diffusion were 72 hours at 15°·55 (60° F.); 53·15 hours at 26°·66 (80° F.); 40·6 hours at 37°·77 (100° F.); and 33 hours at 48°·88 (120° F.).

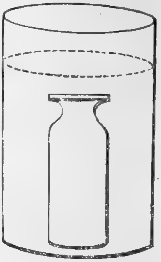


Fig. 2.

The diffusate from a 2 per cent. solution of chloride of potassium in similar circumstances was 0·6577 gramme

In 101·75 hours at 15°·55 (60° F.); and
In 41·93 hours at 48°·88 (120° F.).

The diffusate from a 2 per cent. solution of chloride of sodium was 0·6533 gramme

In 124·75 hours at 15°·55 (60° F.);
In 49·60 hours at 48°·88 (120° F.).

In equal times the diffusate would be

For chloride of potassium	at 15°·55 (60° F.),	1
„ „	at 48°·88 (120° F.),	2·426
For chloride of sodium	at 15°·55 (60° F.),	1
„ „	at 48°·88 (120° F.),	2·5151.

As the ratio between the diffusates of hydrochloric acid, at the same two temperatures, was 1 to 2·1812, it appears that the acid is less increased in diffusibility than the salts at the higher temperature; chloride of sodium also is slightly more increased than chloride of potassium. The more highly diffusive the substance, the less does it appear to gain by heat. Chloride of sodium appears to be sensibly $2\frac{1}{2}$ times more diffusible at 48°·88 (120° F.) than at 15°·55 (60° F.): this gives an average increase of 0·014, or $\frac{1}{71}$ for 1 degree (0·025 for 1° F., or $\frac{1}{40}$). The inequality of diffusion which the three substances referred to exhibit at a low temperature becomes therefore less at high temperatures; and it would appear to be the effect of a high temperature to assimilate diffusibilities. Heat, then, although it quickens the operation of diffusion, does not appear otherwise to promote the separation of unequally diffusive substances.

The results in such experiments are less disturbed by changes of temperature, if at all gradual, than might be supposed. A sensible separation was obtained of hydrochloric acid and chloride of sodium from each other, in a solution containing 2 per cent. of each substance, when the water-jar was heated up from 15°·55

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* Philosophical Transactions, 1850, p. 25.

to 95° C. in two hours, and maintained at the latter temperature during four hours more. Diffusion appeared to be accelerated about six times at the higher temperature.

At low temperatures, again, diffusion is proportionally slow. The ratio of diffusibilityof the following salts at two different temperatures appeared to be,—

For chloride of potassium at 5°·3 (41°·5 F.), 1; at 16°·6 (62°·0 F.), 1·4413
For chloride of sodium at 5°·3 (41°·5 F.), 1; at 17°·4 (63°·4 F.), 1·4232
For nitrate of soda at 5°·3 (41°·5 F.), 1; at 17°·4 (63°·4 F.), 1·4475
For nitrate of silver at 5°·3 (41°·5 F.), 1; at 17°·4 (63°·4 F.), 1·3914.

The salts are unequally affected to a sensible extent; and it will be observed that the superiority of chloride of potassium over chloride of sodium, in diffusibility, is increased at the low temperature.

Within the range of temperature of the preceding experiments, the diffusibility of chloride of sodium being taken as 1 at 17°·4 (63°·4 F.), it becomes 0·7026 at 5°·3 (41°·5 F.); or it diminishes 0·0246, or $\frac{1}{40\cdot7}$, for a depression of 1° (0·0136, or $\frac{1}{73\cdot5}$, for a depression of 1° F.).

[To be continued.]

XXXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxii. p. 552.]

March 14, 1861.—Major-General Sabine, R.A., Vice-President and Treasurer, in the Chair.

THE following communications were read:—

"On an Application of the Theory of Scalar and Clinant Radical Loci." By Alexander J. Ellis, Esq., B.A., F.C.P.S.

"A Seventh Memoir on Quantics." By Arthur Cayley, Esq., F.R.S.

"On the Secular Change in the Magnetic Dip in London, between the years 1821 and 1860." By Major-General Edward Sabine, R.A., Treas. and V.P.R.S.

I propose in this communication to bring together and discuss four determinations at different epochs, in which I have myself been either directly or indirectly concerned, which have had expressly in view the object which forms the title of the paper.

Epoch of 1821.—The experiments on this occasion were made in a part of the Regent's Park, then occupied as the nursery garden of Mr. Jenkins: an unexceptionable locality in all respects, and far distant at that time from buildings or iron implements, railing, or pipes. The experiments, ten in number, were made on six different days, between the 3rd and 10th of August 1821; and all between 8 A.M. and 4 P.M. The circle employed was 11½ inches in diameter, made by Nairne, a celebrated artist in his day for instruments of this description: the needle was made by Dollond on Professor Tobias

Meyer's principle, described in the Göttingen Transactions for 1814. The size of the small spheres, or their distance from the needle, was varied in the different experiments, so as to bring different parts of the axle to rest on the agate planes. The mean of the ten experiments was 70° 02′·9 N., corresponding to the epoch 1821.65: the extremes being 70° 00′·1 and 70° 05′·9. The whole of the experiments were made by myself, and are detailed in a paper in the Phil. Trans. for 1822, Art. 1.

Epoch of 1838.—The experiments on this occasion were made on different days in 1837 and 1838, in the course of the magnetic survey of Great Britain, by Messrs. Robert Were Fox and John Phillips, Captain (since Admiral) Sir James Clark Ross, Captain Edward Johnson of the Royal Navy, and myself. The instruments employed were those of Robinson, Gambey, and Jordan: the particulars are recorded in the 8th volume of the Reports of the British Association for the Advancement of Science (1839), Table 10, p. 64. The localities in which the experiments were made were—1. The same spot in the Regent's Park where those of 1821 had been made. 2. Kew Gardens. 3. Westbourne Green, a locality which has been since built over. Separate determinations were made on 13 days between May 30, 1837, and December 10, 1838, the mean epoch being 1838.3, and the mean dip 69° 17′·3 N. The extremes of all the observers and of all the instruments were 69° 13′·3, and 69° 23′·9.

Epoch of 1854.—The experiments on which this determination rests were made by the late Mr. John Welsh, of the Kew Observatory, and myself in August and September 1854, with two inclinometers made by Mr. Henry Barrow (successor to Mr. Robinson), fitted according to the modern English construction with verniers and microscopes, and each having two needles. The localities selected were—1. The station in the Regent's Park already named as that of the experiments in 1821, and of a part of those in 1838; and 2. the magnetic house of the Kew Observatory. The experiments had a double purpose, viz. 1, to ascertain the difference, if any, in the dip in the Regent's Park and in the magnetic house at Kew; and 2, to obtain a determination of the dip in August 1854 which might be strictly comparable with the result obtained in August 1821. The experiments were made on five different days, and comprised eighteen determinations, ranging between 68° 29′·25 and 68° 33′·73; the mean being 68° 31′·13 N., corresponding to the epoch 1854.65. The mean of eight determinations in the Regent's Park was 68° 30′·55, and of ten determinations at Kew, 68° 31′·6; the difference of either from the mean being 0′·52, which is within the limits of probable error. A detailed notice of these experiments was published in 1855 in an Editor's note in p. 364 of the translation, edited by myself, of Arago's Meteorological Essays.

Epoch of 1859.5.—The dip corresponding to July 1, 1859 (now first discussed), is derived from 282 determinations made in the magnetic house at Kew on 121 different days between November 1857 and December 1860 inclusive, chiefly by four observers, viz. Mr. John Welsh, late Director of the Kew Observatory, Mr. Balfour Stewart, its present Director, Mr. Chambers, Assistant in the Observatory, and Dr. Bergsma, Director of the Magnetical and Meteorological Observatory of the Netherlands Government in Java.

There were employed in these determinations, on different occasions, twelve circles and twenty-four needles, all of the same form and pattern; the circles being 6 inches in diameter, fitted with verniers and microscopes, and the needles $3\frac{1}{2}$ inches in length; they were all made by Mr. Henry Barrow. Every determination was complete in the eight different positions of the circle and needle, as described in Appendix 2 of the Article "Terrestrial Magnetism" in the 3rd edition of the 'Admiralty Manual of Scientific Inquiry.' The individual results are shown in the subjoined Tables, whereof Table I. contains 115 determinations comprised between November 1857 and December 1858; Table II. 96 determinations between January and December 1859; and Table III. 71 determinations between January and December 1860. The results in each year are reduced to the 1st of July in the same year, employing the proportional parts of an annual secular change of $-2'\cdot 6$: those which were obtained in the four winter months, November, December, January, and February, have also received a correction of $-0'\cdot 8$, and those obtained in the summer months, May, June, July, and August, a correction of $+0'\cdot 8$ in compensation for annual variation, agreeably to an investigation contained in the sequel. The Tables exhibit in every case the date, the particular circle and needle employed, the azimuths in which the observations were made, the name of the observer, the observed dip, the reduction to a common epoch, the correction for annual variation, and finally, the corrected result.

The opportunity afforded at the Kew Observatory, of testing the degree of accordance which may be expected in the results of different instruments constructed on the plan which has been for several years past approved and adopted at Kew, has thus been profited by, and the conclusions appear such as to merit the consideration of those who are desirous to possess reliable instruments. Several of the circles are the property of foreign governments or of individuals, at whose request they were provided subject to a verification at Kew. The observations here recorded were for the most part made for the purpose of such verifications, and were entered as they were made in the books of the Kew Observatory, from which they are now taken. No observation has been omitted. The circles were distinguished by the numbers 20, 23, 27, 28, 30, 31, 33, 34, 35 and 36; and two unnumbered, one known as the Kew Circle, the other an inclinometer employed by Admiral Sir James Clark Ross in his recent magnetic survey of a part of England. No. 20 was made for Professor Hansteen of Christiania, and is now in his possession; 23 is the circle used by the late Mr. Welsh in his magnetic survey of Scotland; 27 was supplied to the Austrian frigate 'Novara' for her voyage of circumnavigation; 28 was made for the Russian Government; 30 was used by myself in the recent magnetic survey of England, and has been since supplied to the Observatory at the "Isle Jesus" near Montreal in Canada; 31 was made for Padre Secchi of the Collegio Romano, and is now at Rome; 32 was made for the Rev. Alfred Weld of

Stonyhurst College, and is now in the Observatory of that College; 34 was supplied to the Government of the United States of America, and is now in the possession of Dr. Alexander Dallas Bache, Superintendent of the Coast Survey; Nos. 35 and 36 were made for the Netherlands Government, one for Utrecht, and one for Java; the "Kew Circle" was in regular use for the monthly determinations of the Dip at Kew, from the commencement of those observations until August 1859, when it was exchanged for No. 33, which has subsequently been, and is now, in regular employment for that purpose.

Besides the four principal observers already noticed, a few determinations were made, as is shown in the Tables, by Mr. Valentine Magrath, Assistant in the Observatory, by Captain Haig of the Royal Artillery, practising at Kew preparatory to his employment on the Boundary Commission between the United States and the British possessions on the West Coast of North America, and by Lieut. Goodall of the Royal Engineers, who attended at Kew to practise the manipulation of magnetical instruments.

TABLE I.—Observations of the Magnetic Dip, at the Kew Observatory, in 1857 and 1858, with Circles of the English Construction, fitted with Verniers and Microscopes.

Date.	Circle.	Needle.	Azimuth.	Observer.	Observed Dip.	Reduction to Epoch.	Correction for Annual Variation.	Corrected Dip, July 1, 1858.
1857.								
Nov. 2	27	1	0° & 180°	Mr. Welsh.	68° 23′·4	−1′·8	−0′·8	68° 20′·8
2	27	1	30 ,, 120	,,	24·9	1·8	0·8	22·3
2	27	1	60 ,, 150	,,	24·5	1·8	0·8	21·9
2	27	2	0 ,, 180	,,	25·2	1·8	0·8	22·6
2	27	2	30 ,, 120	,,	23·8	1·8	0·8	21·2
2	27	2	60 ,, 150	,,	24·0	1·8	0·8	21·4
2	Kew	2	0 ,, 180	Mr. Chambers.	27·0	1·8	0·8	24·4
2	Kew	2	30 ,, 120	,,	24·0	1·8	0·8	21·4
2	Kew	2	60 ,, 150	,,	24·0	1·8	0·8	21·6
3	23	1	0 ,, 180	Mr. Welsh.	26·3	1·8	0·8	23·7
3	23	1	30 ,, 120	,,	26·6	1·8	0·8	24·0
3	23	1	60 ,, 150	,,	25·6	1·8	0·8	23·0
3	23	2	0 ,, 180	,,	26·6	1·8	0·8	24·0
3	23	2	30 ,, 120	,,	25·5	1·8	0·8	22·9
3	23	2	60 ,, 150	,,	25·0	1·8	0·8	22·4
3	27	3	0 ,, 180	Mr. Chambers.	22·0	1·8	0·8	19·4
3	27	3	30 ,, 120	,,	23·5	1·8	0·8	20·9
3	27	3	60 ,, 150	,,	23·5	1·8	0·8	20·9
4	28	1	0 ,, 180	,,	22·3	1·8	0·8	19·7
4	28	1	30 ,, 120	,,	25·1	1·8	0·8	22·5
4	28	1	60 ,, 150	,,	27·2	1·8	0·8	24·6
5	28	2	0 ,, 180	,,	28·1	1·8	0·8	25·5
5	28	2	30 ,, 120	,,	25·3	1·8	0·8	22·7
5	28	2	60 ,, 150	,,	28·1	1·8	0·8	25·5
7	28	1 & 4	Mer., and at right angles.	,,	25·6	1·8 1·8	0·8	23·0
10	27	1 & 4	Mer., and at right angles.	,,	34·2	1·8 1·8	0·8	31·6
27	28	1	0 & 180	,,	24·9	1·4	0·8	22·7
27	28	2	,,	,,	68 29·4	−1·4	−0·8	68 27·2

TABLE I. (*continued.*)

Date.	Circle.	Needle.	Azimuth.	Observer.	Observed Dip.	Reduction to Epoch.	Correction for Annual Variation.	Corrected Dip, July 1, 1858.
1858.			° °		° ′	′	′	° ′
Jan. 1	Kew	1	0 & 180	Mr. Chambers.	68 20·7	−1·4	−0·8	68 18·5
1	Kew	2	„	„	24·7	1·4	0·8	22·5
5	Kew	2	„	„	24·6	1·4	0·8	22·4
5	Kew	1	„	Mr. Welsh.	20·8	1·4	0·8	18·6
8	Kew	1	„	Cap. Bedingfield, R.N.	25·1	1·4	0·8	22·9
14	Kew	2	„	Capt. Haig, R.A.	24·9	1·3	0·8	22·8
14	Kew	2	„	„	24·6	1·3	0·8	22·5
16	Kew	1	„	„	23·6	1·3	0·8	21·5
18	23	1	„	„	28·4	1·3	0·8	26·3
18	23	1	„	„	22·8	1·3	0·8	20·7
Feb. 3	?		„	Mr. Chambers*.	27·2	1·2	0·8	25·2
4	Kew	1	„	„	22·9	1·2	0·8	20·9
4	Kew	2	„	„	26·9	1·2	0·8	24·9
27	Kew	1	„	„	20·3	1·0	0·8	18·5
27	Kew	2	„	„	22·5	1·0	−0·8	20·7
Mar. 1	Kew	1	„	Mr. Welsh.	24·4	1·0	0·0	23·4
1	Kew	2	„	„	24·3	1·0	0·0	23·3
4	Kew	2	„	„	25·4	0·9	0·0	24·5
5	Kew	1	„	„	22·5	0·9	0·0	21·6
22	Kew	1	„	„	23·7	0·7	0·0	23·0
22	Kew	2	„	„	25·9	0·7	0·0	25·2
27	Kew	1	„	„	23·7	0·7	0·0	23·0
27	Kew	2	„	„	26·1	0·7	0·0	25·4
27	Kew	1	„	Mr. Chambers.	21·6	0·7	0·0	20·9
30	20	1	„	Mr. Welsh.	25·4	0·7	0·0	24·7
30	30	1	30 & 120	„	28·3	0·7	0·0	27·6
30	30	1	60 „ 150	„	23·0	0·7	0·0	22·3
30	30	1	0 „ 180	„	23·2	0·7	0·0	22·5
30	20	1	30 „ 120	„	21·3	0·7	0·0	20·6
30	30	1	60 „ 150	„	25·2	0·7	0·0	24·5
30	30	2	0 „ 180	Mr. Chambers.	25·1	0·7	0·0	24·4
30	30	2	30 „ 120	„	26·9	0·7	0·0	26·2
30	30	2	60 „ 150	„	27·0	0·7	0·0	26·3
Apr. 22	Kew	2	0 „ 180	Mr. Magrath.	23·7	0·5	0·0	23·2
22	Kew	1	„	„	24·0	0·5	0·0	23·5
27	Kew	1	„	Mr. Welsh.	20·9	0·5	0·0	20·4
27	Kew	2	„	„	21·7	0·5	0·0	21·2
28	Kew	1	„	Mr. Chambers.	22·8	0·5	0·0	22·3
28	Kew	2	„	„	21·7	0·5	0·0	21·2
May 20	Kew	1	„	„	22·3	0·3	+0·8	22·8
20	Kew	2	„	„	23·9	0·3	0·8	24·4
26	Kew	1	„	Mr. Welsh.	20·5	0·3	0·8	21·0
26	Kew	2	„	„	24·1	0·3	0·8	24·6
27	Kew	1	„	Mr. Chambers.	21·6	0·3	0·8	22·1
27	Kew	2	„	„	21·3	0·3	0·8	21·8
27	Sir J. Ross	1	„	Mr. Welsh.	25·4	0·3	0·8	25·9
28	Sir J. Ross	1	„	„	26·8	0·3	0·8	27·3
28	Sir J. Ross	1	30 & 120	„	20·3	0·3	0·8	20·8
28	Sir J. Ross	1	60 „ 150	„	24·0	0·3	0·8	24·5
June 9	30	1	0 „ 180	„	68 21·1	−0·2	+0·8	68 21·7

* Marked "Doubtful."

TABLE I. (*continued.*)

Date.		Circle.	Needle.	Azimuth.	Observer.	Observed Dip.	Reduction to Epoch.	Correction for Annual Variation.	Corrected Dip, July 1, 1858.
1858.									
June	9	30	2	0° & 180°	Mr. Welsh.	68° 21'·6	−0'·2	+0'·8	68° 22'·2
	22	Kew	1	,,	Mr. Chambers.	19·9	0·1	0·8	20·6
	22	Kew	2	,,	,,	20·7	0·1	0·8	21·4
	22	Kew	2	,,	,,	19·8	0·1	0·8	20·5
	22	Kew	1	,,	,,	18·2	0·1	0·8	18·9
	22	23	1	,,	Mr. Welsh.	27·4	0·1	0·8	28·1
	22	23	2	,,	,,	23·0	0·1	0·8	23·7
	22	23	2	,,	,,	23·1	0·1	0·8	23·8
	22	23	2	30 & 120	,,	25·2	0·1	0·8	25·9
	22	23	2	60 ,, 150	,,	24·7	−0·1	0·8	25·4
	28	23	2	0 ,, 180	,,	25·1	0·0	0·8	25·9
	28	Kew	1	,,	Mr. Chambers.	23·4	0·0	0·8	24·2
	29	23	2	,,	Mr. Welsh.	25·1	0·0	0·8	25·9
	29	23	1	,,	,,	23·9	0·0	0·8	24·7
	29	23	1	,,	Mr. Chambers.	21·2	0·0	0·8	22·0
July	3	32	1	,,	,,	24·3	0·0	0·8	25·1
	6	32	1	,,	,,	25·5	0·0	0·8	26·3
	6	32	2	,,	,,	22·9	0·0	0·8	23·7
	8	32	2	,,	,,	22·7	+0·1	0·8	23·6
	8	32	1	,,	,,	23·1	0·1	0·8	24·0
Aug.	3	Kew	2	,,	,,	20·1	0·3	0·8	21·2
	10	Kew	1	,,	,,	20·7	0·3	0·8	21·8
	10	Kew	2	,,	,,	21·3	0·3	0·8	22·4
	20	Kew	1	,,	,,	21·7	0·4	0·8	22·9
	20	Kew	2	,,	,,	23·5	0·4	+0·8	24·7
Sept.	16	Kew	1	,,	,,	22·1	0·6	0·0	22·7
	17	Kew	2	,,	,,	20·8	0·6	0·0	21·4
Oct.	13	Kew	1	,,	,,	23·4	0·7	0·0	24·1
	14	Kew	2	,,	,,	24·2	0·7	0·0	24·9
Nov.	4	Kew	1	,,	,,	20·0	0·9	−0·8	20·1
	4	Kew	2	,,	,,	23·4	0·9	0·8	23·5
	5	23	1	,,	,,	25·6	0·9	0·8	28·7
	6	23	2	,,	,,	26·1	0·9	0·8	26·2
	29	23	2	,,	,,	23·0	1·0	0·8	23·2
	30	23	1	,,	,,	24·3	1·0	0·8	24·5
Dec.	10	Kew	2	,,	,,	20·8	1·1	0·8	21·1
	29	31	1	,,	,,	22·3	1·3	0·8	22·8
	29	31	2	,,	,,	68 20·4	+1·3	−0·8	68 20·9
July 1, 1858, mean of 115 observations									68 23·2

TABLE II.—Observations of the Magnetic Dip at the Kew Observatory in 1859.

Date.	Circle.	Needle.	Azimuth.	Observer.	Observed Dip.	Reduction to Epoch.	Correction for Annual Variation.	Corrected Dip, July 1, 1859.
1859.			° °		° ′	′	′	° ′
Jan. 4	31	1	0 & 180	Mr. Chambers.	68 22·5	−1·3	−0·8	68 20·4
4	31	2	,,	,,	21·0	1·3	0·8	18·9
11	Kew	1	,,	Mr. V. Magrath.	23·6	1·3	0·8	21·5
11	30	1	,,	Mr. Chambers.	23·7	1·3	0·8	21·6
11	30	1	,,	,,	24·6	1·3	0·8	22·5
11	Kew	1	,,	,,	21·6	1·3	0·8	19·5
12	30	2	,,	,,	23·7	1·3	0·8	21·6
12	Kew	1	,,	,,	22·2	1·3	0·8	20·1
12	30	2	,,	,,	24·4	1·3	0·8	22·3
24	30	1	,,	,,	22·7	1·2	0·8	20·7
24	Kew	2	,,	,,	19·8	1·2	0·8	17·8
24	30	1	,,	,,	21·6	1·2	0·8	19·6
25	30	2	,,	,,	24·1	1·2	0·8	22·1
25	Kew	2	,,	,,	20·4	1·2	0·8	18·4
25	30	1	,,	,,	22·2	1·2	0·8	20·2
25	Kew	1	,,	,,	18·9	1·2	−0·8	16·9
Mar. 7	Kew	1	,,	,,	19·6	0·8	0·0	18·8
7	Kew	2	,,	,,	20·1	0·8	0·0	19·3
8	30	1	,,	,,	21·7	0·8	0·0	20·9
8	30	2	,,	,,	22·2	0·8	0·0	21·4
8	20	1	,,	,,	20·8	0·8	0·0	20·0
8	20	2	,,	,,	22·8	0·8	0·0	22·0
10	20	2	,,	,,	24·6	0·8	0·0	23·8
10	33	2	,,	,,	20·5	0·8	0·0	19·7
11	20	1	,,	,,	24·1	0·8	0·0	23·3
11	33	1	,,	,,	25·0	0·8	0·0	24·2
11	33	1	,,	,,	22·2	0·8	0·0	21·4
11	33	2	,,	,,	22·3	0·8	0·0	21·5
11	34	1	,,	,,	21·0	0·8	0·0	20·2
11	34	2	,,	,,	21·6	0·8	0·0	20·8
12	34	1	,,	,,	21·4	0·8	0·0	20·6
12	34	2	,,	,,	20·4	0·8	0·0	19·6
12	23	1	,,	,,	25·0	0·8	0·0	24·2
12	23	2	,,	,,	21·8	0·8	0·0	21·0
14	23	1	,,	,,	24·6	0·8	0·0	23·8
14	23	2	,,	,,	22·7	0·8	0·0	21·9
15	33	1	,,	,,	22·9	0·8	0·0	22·1
15	33	2	,,	,,	21·7	0·8	0·0	20·9
15	Kew	1	,,	,,	21·5	0·8	0·0	20·7
15	Kew	2	,,	,,	26·3	0·8	0·0	25·5
16	33	1	,,	,,	22·9	0·8	0·0	22·1
16	33	2	,,	,,	21·8	0·8	0·0	21·0
16	34	1	,,	,,	21·5	0·8	0·0	20·7
16	34	2	,,	,,	19·5	0·8	0·0	18·7
17	34	1	,,	,,	24·8	0·7	0·0	24·1
17	34	2	,,	,,	18·4	0·7	0·0	17·7
17	30	1	,,	,,	21·7	0·7	0·0	21·0
17	30	2	,,	,,	68 24·4	−0·7	0·0	68 23·7

TABLE II. (*continued.*)

Date.	Circle.	Needle.	Azimuth.	Observer.	Observed Dip.	Reduction to Epoch.	Correction for Annual Variation.	Corrected Dip, July 1, 1859.
1859. Mar. 18	20	1	0° & 180°	Mr. Chambers.	68° 23'·3	−0'·7	0'·0	68° 22'·6
18	20	2	,,	,,	25·0	0·7	0·0	24·3
18	Kew	2	,,	,,	21·5	0·7	0·0	20·8
19	Kew	1	,,	,,	21·5	0·7	0·0	20·8
19	20	1	,,	,,	23·7	0·7	0·0	23·0
19	20	2	,,	,,	26·7	0·7	0·0	26·0
April 20	23	2	,,	Dr. Bergsma.	22·5	0·5	0·0	22·0
?	?	?	,,	,,	21·2	0·5	0·0	20·7
?	?	?	,,	,,	19·2	0·5	0·0	18·7
30	33	2	,,	,,	21·7	0·5	0·0	21·2
May 16	33	1	,,	,,	18·8	0·3	+0·8	19·3
16	33	2	,,	,,	18·1	0·3	0·8	18·6
16	Kew	1	,,	Mr. Chambers.	19·0	0·3	0·8	19·5
16	Kew	2	,,	,,	19·4	0·3	0·8	19·9
17	Kew	1	,,	Dr. Bergsma.	18·2	0·3	0·8	18·7
17	Kew	2	,,	,,	20·3	0·3	0·8	20·8
17, 18	33	1	,,	Mr. Chambers.	20·7	0·3	0·8	21·2
17, 18	33	2	,,	,,	20·7	−0·3	0·8	21·2
July 7	23	1	,,	,,	21·3	0·0	0·8	22·1
7	23	2	,,	,,	21·9	0·0	0·8	22·7
Aug. 10	Kew	1	,,	,,	17·4	+0·3	0·8	18·5
10	33	1	,,	,,	18·3	0·3	0·8	19·4
15	33	2	,,	,,	21·8	0·3	0·8	22·9
15	Kew	2	,,	,,	17·2	0·3	0·8	18·3
22	34	1	,,	,,	25·3	0·3	0·8	26·4
22	Kew	2	,,	,,	20·6	0.3	0·8	21·7
22	Kew	1	,,	,,	22·6	0·3	0·8	23·7
22	34	2	,,	,,	21·9	0·3	+0·8	23·0
Sept. 12	34	2	,,	,,	21·9	0·6	0·0	22·5
12	34	1	,,	,,	20·4	0·6	0·0	21·0
21	33	2	,,	,,	23·6	0·6	0·0	24·2
21	33	1	,,	,,	22·2	0·6	0·0	22·8
Oct. 29	33	1	,,	,,	25·6	0·9	0·0	26·5
29	34	2	,,	,,	24·1	0·9	0·0	25·0
29	34	1	,,	,,	22·8	0·9	0·0	23·7
29	33	2	,,	,,	23·7	0·9	0·0	24·6
Nov. 17	33	1	,,	,,	23·9	1·0	−0·8	24·1
17	34	1	,,	,,	22·1	1·0	0·8	22·3
17	34	2	,,	,,	22·4	1·0	0·8	22·6
17	33	2	,,	,,	24·3	1·0	0·8	24·5
17	34	1	,,	Mr. Magrath.	21·0	1·0	0·8	21·2
17	33	1	,,	,,	24·3	1·0	0·8	24·5
17	33	2	,,	,,	24·3	1·0	0·8	24·5
17	34	2	,,	,,	21·2	1·0	0·8	21·4
18	34	1	,,	,,	20·5	1·0	0·8	20·7
18	34	1	,,	Mr. Chambers.	20·1	1·0	0·8	20·3
Dec. 21	34	1	,,	,,	20·2	1·2	0·8	20·6
21	34	2	,,	,,	68 21·4	+1·2	−0·8	68 21·8
July 1, 1859, Mean of 96 observations								68 21·5

TABLE III.—Observations of the Magnetic Dip at the Kew Observatory in 1860.

Date.	Circle.	Needle.	Azimuth.	Observer.	Observed Dip.	Reduction to Epoch.	Correction for Annual Variation.	Corrected Dip, July 1, 1860.
1860.								
Jan. 20	33	1	0° & 180°	Mr. Chambers.	68° 21′·9	−1′·2	−0′·8	68° 19′·9
20	33	2	”	”	22·9	1·2	0·8	20·9
Feb. 17	33	1	”	”	20·7	1·0	0·8	18·9
17	33	2	”	”	21·5	1·0	−0·8	19·7
Mar. 16	33	1	”	”	20·8	0·8	0·0	20·0
17	33	2	”	”	21·2	0·8	0·0	20·4
April 2	33	1	”	Lt. Goodall, R.E.	21·9	0·7	0·0	21·2
2	33	2	”	”	24·7	0·7	0·0	24·0
18	33	2	”	Mr. Chambers.	22·8	0·6	0·0	22·2
18	33	1	”	”	21·6	0·6	0·0	21·0
26	36	1	”	”	17·3	0·5	0·0	16·8
27	36	1	”	Mr. Stewart.	18·5	0·5	0·0	18·0
27	36	2	”	”	18·6	0·5	0·0	18·1
27	35	2	”	”	20·4	0·5	0·0	19·9
27	35	2	”	Mr. Chambers.	20·6	0·5	0·0	20·1
27	36	2	”	”	14·8	0·5	0·0	14·3
28	35	1	”	”	23·9	0·5	0·0	23·4
May 2	36	1	”	Mr. Stewart.	18·1	0·5	+0·8	18·4
15	35	1	”	”	20·5	0·3	0·8	21·0
15	33	1	”	Mr. Chambers.	19·5	0·3	0·8	20·0
15	33	2	”	”	20·5	0·3	0·8	21·0
22	35	1	”	Dr. Bergsma.	19·2	0·3	0·8	19·7
22	35	2	”	”	19·0	0·3	0·8	19·5
23	36	1	”	”	18·0	0·3	0·8	18·5
24	36	2	”	”	19·7	0·3	0·8	20·2
June 18	33	2	”	Mr. Chambers.	20·1	0·1	0·8	20·8
18	33	1	”	”	19·1	0·1	0·8	19·8
19	36	1	”	Dr. Bergsma.	16·6	0·1	0·8	17·3
20	35	1	”	”	17·7	0·1	0·8	18·4
21	35	1	”	”	19·3	0·1	0·8	20·0
21	35	1	”	”	18·8	0·1	0·8	19·5
22	35	2	”	”	19·2	0·1	0·8	19·9
22	35	2	”	”	18·8	−0·1	0·8	19·5
25	35	1	”	”	23·2	0·0	0·8	24·0
25	35	1	”	”	22·3	0·0	0·8	23·1
26	35	2	”	”	18·4	0·0	0·8	19·3
26	35	2	”	”	17·8	0·0	0·8	18·6
29	36	2	”	”	19·9	0·0	0·8	20·7
29	36	2	”	”	15·1	0·0	0·8	15·9
30	36	1	”	”	19·5	0·0	0·8	20·3
30	36	1	”	”	19·7	0·0	0·8	20·5
July 2	35	1	”	”	21·7	0·0	0·8	22·5
2	36	1	”	”	22·6	0·0	0·8	23·4
3	36	2	”	”	17·7	0·0	0·8	18·5
5	36	2	”	”	18·2	0·0	0·8	19·0
5	35	2	”	”	22·9	0·0	0·8	23·7
6	36	2	”	”	15·4	0·0	0·8	16·2
7	36	2	”	”	68 15·7	+0·1	+0·8	68 16·6

TABLE III. (*continued.*)

Date.	Circle.	Needle.	Azimuth.	Observer.	Observed Dip.	Reduction to Epoch.	Correction for Annual Variation.	Corrected Dip, July 1, 1860.
1860.								
July 9	35	1	0° & 18°	Dr. Bergsma.	68° 20'·4	+0'·1	+0'·8	68° 21'·3
9	35	2	,,	,,	17·0	0·1	0·8	17·9
9	36	1	,,	,,	14·5	0·1	0·8	15·4
9	36	2	,,	,,	13·2	0·1	0·8	14·1
23	33	1	,,	Mr. Chambers.	18·6	0·2	0·8	19·6
23	33	2	,,	,,	21·5	0·2	0·8	22·5
Aug. 16	33	1	,,	,,	16·4	0·4	0·8	17·6
16	33	2	,,	,,	16·9	0·4	+0·8	18·1
Sept. 14	33	2	,,	,,	19·9	0·6	0·0	20·5
14	33	1	,,	,,	18·9	0·6	0·0	19·5
Oct. 19	33	2	,,	,,	21·2	0·9	0·0	22·1
19	33	1	,,	,,	20·5	0·9	0·0	21·4
22	30	1	,,	Mr. Stewart.	18·0	0·9	0·0	18·9
22	30	2	,,	,,	20·6	0·9	0·0	21·5
22	30	2	,,	,,	21·2	0·9	0·0	22·1
22	30	1	,,	,,	14·9	0·9	0·0	15·8
23	30	1	,,	,,	17·6	0·9	0·0	18·5
29	30	1	,,	,,	19·2	0·9	0·0	20·1
30	30	2	,,	,,	23·0	0·9	0·0	23·9
Nov. 24	33	1	,,	Mr. Chambers.	20·3	1·0	−0·8	20·5
26	33	2	,,	,,	21·3	1·0	0·8	21·5
Dec. 18	33	2	,,	,,	19·1	1·2	0·8	19·5
19	33	1	,,	,,	68 18·0	+1·2	−0·8	68 18·4
July 1, 1860, Mean of 71 observations								68 19·8

Correction for Annual Variation.—Wherever, in the middle latitudes of the northern hemisphere, observations of the dip have been made with sufficient care, it has been found that, after elimination of the effects of secular change, the north dip is somewhat greater in winter than in summer. In the 3rd volume of the Toronto Observations, pp. cxxii and cxxiii, the following Table is given as the result of fifteen years of careful observation made throughout at the same spot and according to the same method of observation, comprising 1920 independent determinations nearly equally distributed in the different months, and averaging about 128 determinations for each of the twelve months; by combining the months equidistant from July (or the middle of the year), the influence of secular change is eliminated:—

Mean of January and the following December	75° 18'·90	N.
Mean of February and the following November	75 18·98	,,
Mean of March and the following October	75 18·63	,,
Mean of April and the following September	75 18·71	,,
Mean of May and the following August	75 17·70	,,
Mean of June and the following July	75 17·25	,,

Hence on the 1st of July the mean dip at Toronto would be derived as follows, viz.:—

	° ′
From the four winter months, November to February ..	75 18·97
From the four summer months, May to August	75 17·47

Showing an excess of 1′·5 in the winter months above the summer months.

The annual variation at Kew, as it may be derived from the 282 determinations in Tables I., II. and III., does not differ materially from this conclusion. There are in these Tables 87 results obtained in the four winter months of the different years, and 93 results obtained in the four summer months of those years. If we collect into separate means the results in the winter months of 1857–58, 1858–59, and 1859–60, numbering them (1), (3), and (5),—and into separate means the results in the summer months of 1858, 1859 and 1860, numbering them (2), (4), and (6),—and if we compare (1) and (3) with (2), (3) and (5) with (4), (2) and (4) with (3), and (4) and (6) with (5), (by which comparisons the effects of secular change are eliminated), we find an excess of 1′·7 in the mean dip of the winter months over that obtained from the summer months. The mean of the two corrections, thus separately obtained at Toronto and Kew, is 1′·6; of which the half, or 0′·8, has been applied in the Tables with the − sign to the results in the winter months, and with the + sign to the results in the summer months.

Probable error of a single determination of the Dip.—It may be desirable to state the probable error of a single determination as it may be derived from the observations in the Tables, before and after the application of the correction for annual variation. It will be seen that the probable error is diminished by the application that has been made of a correction on this account,

	When uncorrected for annual variation.	When corrected for annual variation.
	′	′
From the 115 Results in Table I. ..	±1·50..........	±1·49
From the 96 Results in Table II.	±1·44..........	±1·39
From the 71 Results in Table III.	±1·57..........	±1·46
	±1·50..........	±1·45

The probable error thus obtained represents all the diversities ascribable to the employment of different instruments (all of the one construction),—to the supposed peculiarities of different observers,—to the occasional presence of magnetic disturbance (for which no correction has been attempted),—and to differences due to different *hours* of observation;—in addition to what may be more strictly viewed as "observational errors." It may thus serve in some measure as a guide to those engaged in similar researches, as to the degree of accuracy which is attainable in such experimental inquiries, when proper care is taken in the procurement of a reliable inclinometer and in its manipulation.

For the purpose of comparing the probable error thus obtained with inclinometers of the later English pattern with that of the instruments of earlier construction, four of the latter were selected, viz. a 9-inch circle by Robinson, a 9-inch circle by Barrow, and two 6-inch circles by Robinson, all in good order. Each circle was furnished with two needles of the same length as the diameter of the circle, and read by a lens in lieu of verniers and microscopes. Table IV. contains the particulars of 20 determinations made with these instruments in 1860 by Messrs. Stewart and Chambers. Their mean result is 68° 20′·04 reduced to the epoch 1860.5, and corrected for annual variation. The mean result of the 71 determinations at the same epoch in Table III. is 68° 19′·8. There is therefore no notable difference in the mean results obtained by the two classes of instruments; but there is a considerable difference in the probable error; as from the 20 determinations in Table IV. we obtain ±3′·65 as the probable error of a single determination with the instruments of the earlier pattern, whilst ±1′·5 has been shown to be the probable error when inclinometers of the more recent pattern were employed.

TABLE IV.—Observations of the Magnetic Dip, at the Kew Observatory, in 1860, with 9- and 6-inch Circles (Robinson's and Barrow's), without Verniers.

Date.	Circle.	Needle.	Observer.	Observed Dip.	Reduction to Epoch.	Correction for Annual Variation.	Corrected Dip, July 1, 1860.
1860.				° ′	′	′	° ′
Mar. 16	Robinson's 9-inch	1	Mr. Stewart.	68 29·4	−0·8	0·0	68 28·6
17	”	1	”	24·7	0·8	0·0	23·9
17	”	1	”	18·0	0·8	0·0	17·2
19	”	2	”	14·2	0·8	0·0	13·4
19	”	2	”	13·5	0·8	0·0	12·7
19	”	2	”	22·7	0·8	0·0	21·9
21	Barrow's 9-inch	1	”	11·0	0·8	0·0	10·2
21	”	1	”	16·5	0·8	0·0	15·7
21	”	2	”	24·0	0·8	0·0	23·2
21	Robinson's 9-inch	1	Mr. Chambers.	16·6	0·8	0·0	15·8
21	”	2	”	21·8	0·8	0·0	21·0
22	Barrow's 9-inch	2	Mr. Stewart.	26·5	0·8	0·0	25·7
22	”	2	Mr. Chambers.	23·2	0·8	0·0	22·4
22	”	1	”	18·0	0·8	0·0	17·2
May 2	Robinson's 6-inch, No. 1	1	”	26·3	0·5	+0·8	26·6
4	”	2	”	30·7	0·5	+0·8	31·0
4	Robinson's 6-inch, No. 2	1	”	18·1	0·5	+0·8	18·4
4	”	2	”	17·5	−0·5	+0·8	17·8
Nov. 26	”	1	Mr. Stewart.	19·0	+1·1	−0·8	19·3
26	”	2	”	68 18·5	+1·1	−0·8	68 18·8

July 1, 1860, mean of 20 observations .... 68 20·04

The observations were all made in the plane of the magnetic meridian.

The values obtained for the Dip at the epochs of 1821 and 1854, having been derived from observations made at the close of August and beginning of September in those years, require a small correction for annual variation, to bring them into strict comparison with the values at the two other epochs of 1838 and 1859, which have beenderived from observations distributed generally throughout the years. A correction of $+0'\cdot5$ has been applied to each on this account.

Corrected Dip at the several Epochs.—We have then the observed Dips, finally corrected, at the several epochs as follows:—

	° ′	
1821.65	70 03·4 N.	(1)
1838.3	69 17·3	(2)
1854.65	68 31·6	(3)
1859.5	68 21·5	(4)

Between No. 1 and No. 4 we have an interval of 37.85 years, and a mean annual secular change of $-2'\cdot69$; mean epoch, 1840.6.

Between No. 1 and No. 2, comprising an interval of 16.65 years, we have a mean secular change of $-2'\cdot77$; mean epoch, 1830.0.

Between No. 2 and No. 4, comprising an interval of 21.2 years, we have a mean secular change of $-2'\cdot63$; mean epoch, 1848.9.

Hence we may infer that the yearly diminution of the Dip from secular change, though very nearly uniform throughout the whole interval of 37.85 years, was somewhat greater in the earlier part of the interval than in the later; and that the rate of diminution may admit of being more exactly represented by the introduction of a second term.

If then we take the year 1840.0 as a convenient middle epoch $=t_0$, and call its dip θ_0; and if we further call the observed dip at the several observational epochs t_1, t_2, t_3 and t_4, respectively θ_1, θ_2, θ_3, θ_4, we shall have four equations of the form

$$\theta_1=\theta_0+x(t_1-t_0)+y(t_1-t_0)^2;$$

and giving double weight to the equation furnished by the epoch 1859.5, inasmuch as it is derived from so much greater a body of observations than the results at the other three epochs, we obtain by least squares,

$$\theta_0=69^\circ\ 11'\cdot95;\qquad x=-2'\cdot713;\qquad y=+0'\cdot00057.$$

Hence we have the general formula for computing the dip between the years 1820 and 1860,

$$\theta=69^\circ\ 11'\cdot95-2'\cdot713\ (t-t_0)+0'\cdot00057\ (t-t_0)^2,$$

t_0 being 1840.0, and t being any other time for which the dip θ is required.

Using this formula, we have the differences between the computed and the observed dips at the several epochs of observation as follows:—

	Computed.	Observed.	Computed—Observed.
	° ′	° ′	′
1821.65	70 03·6	70 03·4	+0·2
1838.3	69 16·8	69 17·3	−0·5
1854.65	68 33·4	68 31·6	+1·8
1859.5	68 21·2	68 21·5	−0·3

And the dips corresponding to every tenth year within the period specified are as follows:—

1820.0	70° 07′·3
1830.0	69 39·6
1840.0	69 11·9
1850.0	68 45·9
1860.0	68 19·9

The progressive diminution of the Dip in London during the last forty years has thus been traced and followed by the observations recorded and discussed in this paper; and the further progress of the research will now devolve on the systematic observations which are made for that purpose monthly at the Observatory at Kew.

The rate of diminution in the last forty years does not appear to differ materially from the mean rate in the preceding hundred years. The experiments of Mr. George Graham between March and May, 1723, recorded in the Philosophical Transactions for 1725, No. 389, give a mean dip in London at that epoch of "nearly" 74° 40′. Comparing this with 69° 11′·95 in 1840.0, we have a difference of 5° 28′·1 in 116·7 years, equivalent to a uniform diminution of 2′·81 annually; or if the formula

$$\theta=69^\circ\,11'{\cdot}95-2'{\cdot}713\,(t-t_0)+0'{\cdot}00056\,(t-t_0)^2$$

be employed, it gives the dip in March 1723.3 equal to 74° 36′·1, being a difference of less than 4′ from the result of Mr. Graham's experiments; which difference is doubtless less than the probable error of that gentleman's determination with the instruments then in use.

An expectation appears to have prevailed in some quarters that the decrease of the Dip in London should have ceased, and its subsequent increase have commenced, contemporaneously with the alteration which took place in the secular change of the *Declination* in the early part of this century, when the increase of west declination, which had been continuous in the British Islands for about two centuries, ceased, and was succeeded by a decrease of the same. But this supposition is by no means in accordance with that general view and interpretation of the phenomena of terrestrial magnetism for which we are indebted to Dr. Halley, and which, since its promulgation in 1683, has received so much confirmation in various and distant parts of the globe. In accordance with that hypothesis, the diminution of the Dip in London might be expected to continue until the epoch should arrive when, by the easterly movement of translation of the minor magnetic system in the northern hemisphere, the disparity of the magnetic force prevailing in the European and American portions of the hemisphere should have attained its maximum:—which is certainly not yet the case.

Is there then, in the secular change of the Dip, no feature in which, in conformity with the Halleian hypothesis, an alteration might be expected to synchronize with the reversal in the direction of the secular change of the declination? Assuredly there is; and the facts which recent investigations have brought to our knowledge manifest that such an alteration has taken place. I proceed to describe it.

If we have recourse to those extensive generalizations which, under the name of "Isoclinal Lines corresponding to particular Epochs," present a connected view of the changes which have taken place from time to time in the magnetic lines of the Dip over large portions of the earth's surface, and enable us to anticipate with some degree of confidence the changes which may be expected to take place in years to come, we notice generally that the lines undergo two species of modification, or peculiarities of change, which it is necessary to keep separately and distinctly in view. In the British Islands, for example, the Isoclinal Lines for little less than two centuries past have been steadily advancing towards the north by a gradual movement of translation. This is one feature of the secular change; but there is a second feature, which, if not at first sight equally striking, is yet equally regular and systematic in its operation; viz. the *direction* of the isoclinal lines as they pass across our country from the south-west towards the north-east undergoes a small but sensible change from year to year, by which, in the lapse of several years, the angle at which they cut the geographical meridians is materially altered. By the joint operation of these two processes, the *general configuration of the lines* over large portions of the earth's surface, as well as *their values in particular localities,* are both subject to systematic alteration; a remark which is not limited to the isoclinal lines alone, but is the case also in the isogonic and isodynamic lines. Those who are conversant with Dr. Halley's writings, will be aware that,—in correspondence with his views,—between the epochs when the Dip in London should attain, respectively, the maximum and the minimum amount which constitute its limits under the system of secular change, an *intermediate* epoch might be anticipated, when the isoclinal lines passing across the British Islands should attain their least angle of inclination to the geographical meridian; towards which they should have progressively advanced, and from which they would as progressively recede. Now, if we compare the line of 70° of dip in the Isoclinal Map of 1780 of the Magnetismus der Erde with that of 1840 in Mr. Keith Johnstone's Physical Atlas, plate 23, we may fix on a point in about 42° North Latitude and 30° West Longitude, in which the Dip has remained nearly stationary, and through which the line of 70° of Dip passed, at both epochs; and we may perceive that, in its easterly course from that point or pivot, this line passed in 1780 *through the middle of France considerably to the South of Paris* (where the Dip was then between 71° and 72°); whereas in 1840 it passed *across England considerably to the north of London* (where the Dip had diminished to little more than 69°). Therefore in the sixty years which had elapsed between the two epochs, 1780 and 1840, the direction of the lines as they impinged upon Western Europe had become much less inclined to the geographical meridian (*i. e.* forming a greater angle with the parallels of latitude) in 1840 than in 1780: and if we consult still earlier maps, we find that a change in the same direction had been progressive from a still earlier period. The particular year in which this feature attained its limit, and an opposite change commenced, cannot now perhaps be precisely determined; it was probably somewhat earlier than 1840. But from the comparison of the magnetic surveys of the British Islands in 1836–37 and 1857–58, it is certain that the change in the direction of the isoclinal lines in this part of the globe has entered upon the contrary phase to that which had previously existed. The observations of the late Mr. Welsh in Scotland in 1857–58 (Brit. Assoc. Reports, 1859), when compared with those of the Scotch Survey made in 1836–37, published in the British Association Reports for 1836, show, according to Mr. Balfour Stewart's calculation, that an increase of several degrees in the angle at which the lines cut the meridians in passing across Scotland has taken place between the epochs of the earlier and the later surveys. The same general conclusion follows from a comparison of the magnetic surveys of England at nearly the same epochs; everywhere near the west coast of England the mean annual secular change in the twenty years has been greater, and near the east coast less than its mean value at Kew; showing that the general direction of the isoclinal lines more nearly approaches a parallelism to the equator now than it did twenty years ago. The ascertainment of the exact value of the secular change at a particular locality by a well-conducted system of periodical observations is the duty of a magnetic *observatory*; the direction of the magnetic lines passing across a country is supplied by magnetic *surveys*; which, for that purpose, ought to be repeated from time to time, as they have now been in this country, at intervals of perhaps twenty or twenty-five years.

It has been imagined that the secular changes of the magnetic elements may be due to some alteration taking place either in the distribution or in the condition of the materials in the interior of the globe. But the regularity and uniformity with which the secular magnetic changes continue through long intervals of time, together with their sudden periodic reversals,—and their corresponding features in the northern and southern hemispheres, which add greatly to the apparent consistency and systematic character of the whole as parts of a uniform general system,—wear more the aspect of effects of some yet unascertained *cosmical* cause. One of the British Colonial Observatories, St. Helena, having the advantage of both a large secular change and a small amount of magnetic disturbance, has afforded a very striking example of the great regularity with which the secular change takes place, maintaining a steady uniformity, traceable not only from year to year, but from month to month, and even from week to week; so that it is not too much to say that, from observations made during a single fortnight, an annual secular change which has existed almost without variation for more than a century, may be ascertained and measured with very considerable precision. (Magnetic Observations at St. Helena, vol. ii. p. ix.)

March 21.—"On the Relations of the Vomer, Ethmoid, and Intermaxillary Bones." By John Cleland, M.D.

"On the Structure and Growth of the Tooth of *Echinus*." By S. James A. Salter, M.B. Lond., F.L.S., F.G.S.

GEOLOGICAL SOCIETY.

[Continued from vol. xxii. p. 405.]

November 6, 1861.—Sir R. I. Murchison, V.P.G.S., in the Chair.

The following communications were read:—

1. "Note on the Bone-Caves of Lunel-Viel, Hérault." By M. Marcel de Serres. Communicated by the President.

These bone-caves in Miocene limestone, on the Mazet estate, near Montpellier, discovered about 1823, and described in 1839 by MM. Marcel de Serres, Dubreuil, and Jean-Jean, comprise a large cave and some smaller fissures, containing a red earth with pebbles and an abundance of bones and coprolites of Hyæna, Lion, Bear, Wolf, Fox, Otter, Boar, Beaver, Rhinoceros, Horse, Deer, Ox, &c., with Birds and Reptiles. The author expressed his belief anew that the association of pebbles with the bones in caves is a common phenomenon, and an evidence of the accumulation of the materials, gnawed bones and coprolites included, by the running water of violent inundations,—the caverns being of Tertiary origin, the detritus being contemporary with the old alluvium of the Rhone, and the fauna indicated by the bones having been antecedent to the latter.

2. "On the Petroleum-springs in North America." By Dr. A Gesner, F.G.S.

After some observations on the antiquity of the use of mineral oil in North America and elsewhere, and on the present condition of the oil- and gas-springs and the associated sulphur- and brine-springs in the United States, the author stated that 50,000 gallons of mineral oil are daily raised for home-use and for exportation. The oil-region comprises parts of Lower and Upper Canada, Ohio, Pennsylvania, Kentucky, Virginia, Tennessee, Arkansas, Texas, New Mexico, and California. It reaches from the 65th to the 128th degree of long. W. of Greenwich, and there are outlying tracts besides.

The oil is said to be derived from Silurian, Devonian, and Carboniferous rocks. In some cases the oil may have originated during the slow and gradual passage of wood into coal, and in its final transformation into anthracite and graphite,—the hydrogen and some carbon and oxygen, being disengaged, probably forming hydrocarbons, including the oils. In other cases, animal matter may have been the source of the hydrocarbons.

Other native asphalts and petroleums were referred to by the author, who concluded by observing that these products were most probably being continually produced by slow chemical changes in fossiliferous rocks.

3. "Notice of the Discovery of some additional Land Animals in the Coal-measures of the South Joggins, Nova Scotia." By Dr. J. W. Dawson, F.G.S.

Two additional fossil stumps of trees have been examined by the author from the same group of the Coal-measures as that which has already afforded Reptilian, Molluscan, and Myriapodal specimens.

These trees stand on the 6-inch coal in Group XV. One (*Sigillaria Brownii*) has yielded indications of six skeletons of *Dendrerpeton acadianum* (one probably perfect), a jaw of a new species, two skeletons of *Hylonomus Lyellii*, one of *H. Wymani*, a number of specimens of *Pupa vetusta* and *Xylobius Sigillariæ*, and some remnants of Insects (in coprolites).

In a lower bed (1217 feet beneath—in Group VIII.), a Stigmarian under-clay 7 feet thick, the *Pupa* was found abundantly in a thickness of 2 inches—with fragments of Reptilian bones. The coal-seams between the trees and this bed indicate that this *Pupa* must have existed during the growth and burial of at least twenty forests.

4. "On a Volcanic Phenomenon observed at Manilla, Philippine Isles." By J. G. Veitch, Esq. In a Letter to Dr. J. D. Hooker, F.G.S.

On the 1st of May 1861, the River Pasig, at Manilla, from 15 to 18 feet deep, was disturbed by a violent ebullition from 6 to 10 A.M. for a distance extending to a quarter of a mile. Its temperature here was 100° to 105° Fahr. (elsewhere 80°). A bank of fetid mud was thrown up several feet above the water, and had a temperature of 60° to 65° only.

The Chairman remarked that a bank of mud, 30 feet high, and more than a mile long, had lately been thrown up in the southern portion of the Caspian.

November 20, 1861.—Sir R. I. Murchison, V.P.G.S., in the Chair.

The following communications were read:—

1. "On the Bovey Basin, Devonshire." By J. H. Key, Esq.

The author first described the physical features of the Bovey Basin, and then the strata, as proved by borings and diggings for clay and lignite. Having pointed out the evidences that exist of the basin having once been a lake in which the several strata of clay, sand, lignite, gravel, &c. were deposited, and having considered the probable conditions of such a lake having been gradually filled up by fluviatile deposits brought down from neighbouring granitic hills, the author remarked:—1st. that the Bovey deposits are composed of materials almost identical with the component parts of granite. 2. The strata run, for the most part, parallel with the outline of the marginal hills, and dip from the sides towards the centre, often thinning away in that direction. 3. The finer material is deposited towards the sides, and the coarser towards the centre. 4. Where the basin is contracted the finer beds often disappear, but thicken where the basin widens. 5. That the upper beds of the northern part are coarser than those of the middle and lower portions. 6. On the eastern side the fine-clay beds are more developed than on the western side. 7. The various beds run in the direction of, and seem to point to, the River Bovey as the source from whence they were derived; but the old outlet of the lake was towards Torbay, and not along the Teign as it is at present. Some observations on the peculiar absence of animal-remains in these deposits, often rich with vegetable-remains, concluded the paper, which was illustrated by several original plans, sections, and sketches.

2. "On two Volcanic Cones at the Base of Etna." By Signor G. G. Gemmellaro.

These two cones occur at Paternò and Motta (Sta. Anastasia); and the existing remains of their craters and nuclei were described in detail. The author concludes that these were two contemporaneous doleritic volcanic cones, that were formed in the Post-pliocene period, previous to the deposition of the calcareous tuff of the vicinity of Paternò,—also that they were cones of eruption, and not of elevation, for the neighbouring strata are not disturbed—and that they were independent eruptions, and not parasitical cones of Etna.

3. "On some Fossil Brachiopoda of the Carboniferous Rocks of the Punjâb and Kashmir, collected by A. Fleming, M.D., &c., and W. Purdon, Esq., F.G.S." By T. Davidson, Esq., F.R.S., F.G.S.

Dr. Fleming's geological researches on the Salt-range and elsewhere in the Punjâb, in 1842–52, are recorded in the Journal of the Society for 1853, in the Journ. Bengal Asiat. Soc. 1853, and in his Report on the Salt-range, 1854. The species of Carboniferous Brachiopoda collected by Dr. Fleming and described and figured by Mr. Davidson, are *Terebratula* (vel *Waldheimia*) *Flemingii*, Dav., *T. problematica*, Dav., *T. subvesicularis*, Dav., *Retzia radialis*, var. *grandicosta*, Dav., *Athyris Royssii*, L'Ev., *A.* (vel *Merista*) *subtilita*, Hall, var. *grandis*, Dav., *Spirifera striata*, Martin, *Spiriferina octoplicata*, Sow., *Orthis resupinata*, Martin, *Streptorhynchus crenistria*, Phil., var. *robustus*, Hall, *St. pectiniformis*, Dav., *Productus striatus*, Fisch., *P. longispinus*, Sow., *P. contortus*, Sow.

Mr. Purdon's collection comprises, besides several of the foregoing, *Terebratula Himalayensis*, Dav., *Spirifera Moosakailensis*, Dav., *Sp. lineata*, Martin, var., *Camarophoria Purdonii*, Dav., *Productus Purdonii*, Dav., *P. Humboldtii*, D'Orb., *Aulosteges Dalhousii*, Dav., and *Strophalosia Morrisiana* (?), King, var.

December 4.—Sir R. I. Murchison, V.P.G.S., in the Chair.

The following communication was read:—

"On the Bracklesham Beds of the Isle of Wight Basin." By the Rev. O. Fisher, M.A., F.G.S.

After noticing the researches of Prestwich and Dixon, the author proceeded to state that most of the "Bracklesham Beds" are displayed at low water at Bracklesham Bay; but other and higher beds belonging to the same series are to be observed in the New Forest, at Stubbington, and in the Isle of Wight. By means of the fossils, for the most part, Mr. Fisher divides the series into four groups:—A. The uppermost abounds in *Gasteropoda*, and has several fossil-beds. One of these, in the eastern part of its range, is full of *Num-*

mulina variolaria (bed No. 16 of Mr. Prestwich's Section of Whitecliff Bay, Quart. Journ. Geol. Soc. vol. ii. pl. 9); the *N. variolaria* bed of Selsea and of Stubbington; and the Shepherd's Gutter Bed at Bramshaw, New Forest. The beds above the last-named are—1st, a portion of No. 19 of the Whitecliff Bay section and the Coral-bed of Stokes Bay and Hunting Bridge (New Forest); and 2nd, the Shell-bed at Hunting Bridge, and Pebble-bed, with shell-casts, at Highcliff. The lowest bed of this group is the Cypræa-bed of Selsea, the Cardita-bed of Stubbington, and the Brook-bed in the New Forest. B. This group is more sandy than the last; it has two fossil-beds, one of which contains *Cerithium giganteum* (at Hillhead, Stubbington; and half-a-mile west of Thorney Station, Bracklesham Bay). C. This is a sandy group, and is remarkable for the profusion of *Nummulina lævigata* in its principal fossil-bearing beds. D. This embraces the lowest fossiliferous sands of Bracklesham Bay. Its distinctive shells are *Cardita acuticosta* and *Cypræa tuberculosa.*

Some species of Molluscs pass upwards from the Bracklesham into the Barton series; yet the fauna of the Bracklesham Beds has a sufficiently distinct facies; and the following species range through this series and are confined to it—*Cardita planicosta, Sanguinolaria Hollowaysii, Solen obliquus, Cytherea suberycinoides, Voluta Cithara,* and *Turritella sulcifera*. *Pecten corneus* is also characteristic, but is met with higher up.

The Bracklesham Beds seen at Whitecliff Bay were first treated of, and Mr. Prestwich's section referred to in detail. No. 6 (a pebble-bed) of this published section is regarded by Mr. Fisher as the base of the Bracklesham series, the upper limit being somewhere in No. 19. Descriptions followed of the beds seen at Bracklesham Bay; the eastern side of Selsea; at the Mixen Rocks; at well-sinkings near Bury Cross; at Stubbington (including the Cerithium-bed at Hillhead, discovered by the author in 1856); Netley; Bramshaw, Brook, and Hunting Bridge (where H. Keeping has lately found a fossil-bed high in the series), in the New Forest. Indications of the western range of the marine shells of "Bracklesham" age were quoted as occurring at Lychett near Poole, and as very rare (one *Ostrea*) near Corfe.

Bracklesham Beds, containing marine forms, seen at Alum Bay (Isle of Wight) and at Highcliff (near Christchurch) were then described in full. The Bracklesham series is regarded by Mr. Fisher as commencing in both these sections a few feet beneath a dark-green clay (part of No. 29 of Mr. Prestwich's section of Alum Bay) containing a peculiar variety of *Nummulina planulata* and many shells of the Barton fauna.

Remarks were also made on the estuarine condition of the lower Bracklesham Beds in their western area; on the probable sources of their materials; on the successive deepenings of the old sea-bottom, and the formation of the pebble-beds; and lastly on the fitness of the Bracklesham and Barton series as a field for research in the history of Molluscan Species.

The paper was illustrated by a series of Specimens from the Author's Collection.

January 8, 1862.—Sir C. Lyell, F.G.S., in the Chair.

The following communications were read:—

1. "On the Carboniferous Limestone of Oreton and Farlow, Clee Hills, Shropshire." By Professor John Morris, V.P.G.S., and George E. Roberts, Esq. With a Note upon a new species of *Pterichthys*; by Sir P. de M. G. Egerton, Bart., M.P., F.G.S.

The rocks described in this paper are a series of thin beds of limestone and sandstone lying between the Old Red Sandstone of South Shropshire and the Millstone Grit which forms the basement of the Titterstone Clee Coal-field.

In consequence of the opening of new quarries and the cutting of a roadway through the Farlow ridge, transversely to the strike of these deposits, the authors were enabled to add somewhat to the description of the locality given in 'The Silurian System.' The series of deposits from the Old Red "cornstone," upwards, was shown by them to be:—1. Laminated yellow sandstones, with pebble-beds and sands. 2. Bright-yellow sandstones containing *Pterichthys*. 3. Brecciated yellow sandstones, pebble-beds, sandy layers, and laminated sandstones. 4. Sandy and concretionary limestone. 5. Grey oolitic limestones, containing palatal teeth of great size. 6. Clays, with ferruginous bands. 7. Shaly Crinoidal limestones. 8. Clays with limestone-concretions, and shaly limestones. Against the last-mentioned bed, the Millstone Grit rests unconformably.

These beds thicken out at Oreton, a mile East of this Farlow section, and are there extensively worked for various economic purposes, the oolitic limestones, locally termed "jumbles," being used for decorative purposes under the name of Clee Hill marble. In describing the physical conditions of the localities, mention was made of the "mole river," which, losing itself at the West end of the ridge, takes a subterranean course nearly parallel with its axis, and re-appears at its lower end, a mile distant. An interesting fact was communicated to the authors by the Rev. J. Williams of Farlow, of an accidental accumulation in the hollow of its inlet, of a body of water estimated at 1,635,000 cubic feet, the whole of which was carried away in 48 hours by the sudden clearance of the channel.

In describing the palæontology of these rocks, the authors specially drew attention to the fortunate discovery in the Yellow Sandstone of Farlow, of *Pterichthys macrocephalus* (spec. nov., Egerton), made while reducing the thickness of a large ripple-marked slab sent them by Mr. Weaver Jones in illustration of the physical conditions of the deposit. This *Pterichthys* proving identical with the fragment previously found in the Farlow sandstone by Thomas Baxter, Esq., F.G.S., they attached to the paper a descriptive note on that fossil, by Sir Philip Egerton, in which the Farlow *Pterichthys* was contrasted with that of Dura Den, and additional proof given of the identity of the genera *Pamphractus* and *Pterichthys*. In addition to Pterichthyoid remains, scales of two species of *Holoptychius*, one probably new, had been found by them.

The richness of the overlying limestones in palatal teeth was shown by a fine series of examples, amongst which *Orodus ramosus*, of unusual size and in perfect condition, and an undescribed *Pœcilodus*, of great magnitude, were most conspicuous. Other genera represented were *Helodus*, *Psammodus*, *Cladodus*, *Cochliodus*, *Petalodus*, and *Ctenoptychius*. Ichthyodorulites, of large size and rich ornament, chiefly belonging to the genera *Ctenacanthus* and *Oracanthus*, accompany these teeth.

The notices of the invertebrate fauna given by the authors proved the assumed lowness of the Oreton limestones in the Mountain-limestone series,—the zone of *Rhynchonella pleurodon* being well-marked, Crinoidal and Bryozoan remains abundant though fragmentary, and Corals nearly absent.

A large series of *Pterichthyes* and of rock-specimens were exhibited in illustration by Mr. George E. Roberts; and a collection of palatal teeth was liberally sent for exhibition by W. Weaver Jones, Esq,, of Cleobury Mortimer, and by Edward Baugh, Esq., of Bewdley.

2. "On some Fossil Plants, showing Structure, from the Lower Coal-measures of Lancashire." By E. W. Binney, Esq., F.R.S., F.G.S.

After noticing the views taken of the structure of *Lepidodendron* by Hooker and others, the author proceeded to describe three portions of calcified stems, Lepidodendroid in external appearance, two of which exhibit in section a central axis composed, not of cellular tissue, but of large, transversely barred, hexagonal vessels. These two specimens the author refers to a new species, *Sigillaria vascularis*. The third specimen differs from the others in the absence of the thin radiating cylinder of barred vessels around the central axis; this he terms *Lepidodendron vasculare*.

Microscopical preparations and photographs of sections were supplied by the author.

3. "Supplemental Notes on the Plant-beds of Central India." By the Rev. S. Hislop. In a Letter to the Assistant-Secretary.

Mr. Hislop, in noticing the discovery of more remains of Plants, Insects, and Fishes at Kota on the Pranhita, stated that he certainly now thought that the ichthyolitic beds of Kota (probably Lower Jurassic in age) are higher in relative position than the plant-sandstone of Nagpur, which, with the Sironcha sandstone underlying the Kota limestone, belong to the Damuda Group. He remarked also that, in his opinion, the *Tæniopteris* of Kampti would prove that the Damuda and Rajmahal groups cannot be widely separated.

XXXII. *Intelligence and Miscellaneous Articles.*

ON A DEW-BOW SEEN ON THE SURFACE OF MUD.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THERE was seen today (February 13, 1862) by myself and some other persons in this neighbourhood, a very beautiful phenomenon, of which the cause is obvious, and of such a nature that one would expect the phenomenon to occur frequently; but I do not remember to have yet seen any instance of it recorded in any scientific publication. I refer to a prismatically-coloured hyperbolic iris, or bow of the first order, exactly resembling that sometimes seen on a field of dewy grass; but in this case it was displayed on the muddy surface of a by-road near Glasgow, and on the less trodden parts of an adjoining turnpike road, throughout a distance of more than a mile. The time was between $12^h\ 30^m$ and 1^h P.M. Greenwich time; the morning had been hazy, but the mist had cleared away, and the sun was shining brightly.

The angular dimensions of the iris were obviously the same with those of a rainbow of the first order; its colours were complete, from red to violet, and very bright and distinct, especially where the mud was softest and moistest; where a sheet of water, how thin soever, covered the mud, the iris vanished. No trace of an iris could be seen on the grass, in the sky, or anywhere but on the mud; and on those parts of the turnpike road where the mud had been much disturbed no iris was visible.

The necessary conclusion from this appearance is, that the surface of the mud must have been thickly covered with globules of pure water, perfectly spherical, and not in absolute contact with the mud, although resting on it; but those globules must have been extremely minute, for they were invisible to the closest inspection with the naked eye.

I am, Gentlemen,
Your most obedient Servant,
Glasgow, February 6, 1862. W. J. Macquorn Rankine.

NOTE ON THE THEORY OF SPHERICAL CONDENSERS.

BY M. J. M. GAUGAIN.

I have indicated in my preceding communications* a general principle by means of which all questions relative to condensers may be transformed into questions of propagation, and thus brought within the domain of Ohm's law. The exactitude of this principle has been already demonstrated experimentally,—1, in the case of concentric cylindrical condensers; 2, in the case of eccentric cylindrical condensers; 3, in the case of plane condensers (*Comptes Rendus*, Feb. 18, April 29, and June 17, 1861). I have now verified

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* See Phil. Mag. vol. xxi. p. 539.

it for a new class of condensers, that of concentrical spherical condensers.

The problem which I proposed to solve was as follows:—Given two concentrical spheres, suppose that the inside sphere, whose radius is r, is placed in contact with a constant source of electricity, and that the external sphere whose radius is R, is placed in contact with the ground; it is required to express the charge of the inside sphere in terms of the radii r and R.

In order to obtain the corresponding question of propagation, it is sufficient to suppose that the insulating substance, which separates the two spheres in the case of condensation, is replaced by a medium which is a conductor, but to a far less extent than the substance of which the spheres consist. The problem then consists in finding an expression for the intensity of the current transmitted from the internal sphere to the external one; and this latter question is easily solved.

Suppose two concentric spheres very near each other, and having the radius x and $x+dx$ (x being less than R and greater than r); the resistance of the medium comprised between these two spheres will be expressed by $\frac{kdx}{x^2}$, k being a constant coefficient; and this resistance will be the differential of the total resistance of the spherical ring comprised between the spheres of the radii r and x. This total resistance will thus have the value $k\left(\frac{1}{r}-\frac{1}{x}\right)$, that of the ring comprised between the spheres of the radius r and radius R will be expressed by $k\left(\frac{1}{r}-\frac{1}{\mathrm{R}}\right)$, and consequently the intensity of the current transmitted will be proportional to $\frac{\mathrm{R}_r}{\mathrm{R}-r}$. Now, from the principle which I have adduced at starting, the charge expressed in the case of condensation ought to be proportional to this same expression. It was necessary to ascertain experimentally if this were so.

To do this I have compared, two by two, six concentrical spherical condensers, the armatures of which had the following dimensions:—

	Internal sphere.	External sphere.
	Diameter in millimetres.	Diameter in millimetres.
No 1	61·5	89·0
2	61·5	118·5
3	61·5	161·0
4	90·5	118·5
5	90·5	161·0
6	120·0	161·0

I charged these six apparatus successively by placing them in communication with the same source, and I determined the charge accumulated on the sphere by the methods described in my preceding Notes. Through a circular aperture, 30 millims. in diameter, in each of the external spheres, wires can be introduced by which the sphere is either charged or discharged. The following are the results obtained:—

	Charge obtained experimentally.	Ratio of the charges obtained.	Ratio of the calculated charges.
No. 5	16	0·457	0·438
6	35		
3	7·5	0·214	0·211
6	35		
4	27	0·828	0·812
6	32·6		
1	13	0·433	0·424
6	30		
2	10	0·609	0·618
5	16·4		

The differences obtained between the calculated and observed charges are small, considering the imperfections of the modes of measurement; and we see that the formulæ deduced from the theory of propagation may be applied to the case of condensation, as well in the case of spherical condensers as in the case of the cylindrical and plane condensers with which I have been previously occupied. I believe that the proposition may without temerity be generalized, and be considered applicable to condensers of any shape.

To appreciate the interest of this principle, it is important to observe that the theory of condensers, which has been usually presented as a branch of statical electricity, comprehends really the whole of this subject. When an insulated and electrified conductor is placed in any room, the electricity with which it is charged is usually called *free* electricity; but, as Mr. Faraday has shown by numerous experiments, this electricity is no more *free* than that on the inner coating of a Leyden jar. The insulated conductor is only the inner coating of a large condenser, the external coating of which is constituted by the whole of the adjacent conductors. In fine, all questions relating to the distribution of electricity which is said to be *free*, depend on the theory of condensers, and may therefore be solved by means of the theory of propagation. By the principle which I have propounded, all questions of statical electricity may be resolved into questions of dynamics, and *vice versâ*.

The want of apparatus has prevented me from investigating whether the numerous results which Coulomb obtained in his researches relative to the distribution of electricity may be made to coincide with the ideas which I have laid down; but I have already been able to verify the very simple law which expresses the free charge. We have just seen that the quantity of electricity accumulated on the inner coating of a spherical condenser is proportional to $\frac{\mathrm{R}r}{\mathrm{R}-r}$. This expression is reduced to r if we suppose R to be infinite; but it seemed evident that if the outer armature becomes infinitely large, its form becomes indifferent. We are thus led to admit that when a sphere is placed in an envelope which is either infinite or simply very large, the charge communicated to it by a given source is proportional to its radius; which amounts to saying in Coulomb's language, that the thickness of the electric layer is inversely as its radius. To verify this conclusion, I took four spheres of brass, the diameters of which were 61·5, 90·5, 120, and 161 millims.; I placed them successively on an insulating support in the centre of a very narrow chamber, and charged them by connecting them with a constant source by means of a metallic wire; I then tested them by employing, as usual, a discharging electroscope. The charges obtained were 5·2, 7·6, 11, and 14·7. The law of proportionality would have given 5·2, 7·6, 10·1, and 13·6. These two series of numbers are not identical; but they differ so little, that we might expect that the law of proportionality would be exactly verified in a larger envelope.

At first we might suppose that this law of proportionality is opposed to one of the results obtained by Coulomb; but this contradiction is only apparent. In Coulomb's experiments, in the case of two unequal spheres, the spheres touched at the moment at which they were electrified, and were only separated after being charged; their mutual action necessarily modifies the distribution of electricity. The problem I have treated is quite different, and much simpler, as the spheres on which I worked were only charged successively.—*Comptes Rendus,* September 30, 1861.

ON THE ACTION OF NITRATE OF SODIUM ON SULPHIDE OF SODIUM AT DIFFERENT TEMPERATURES. BY DR. PH. PAULI, UNION ALKALI WORKS, ST. HELENS.

The mother-liquor obtained in the manufacture of soda-ash contains, as is well known, large quantities of sulphide of sodium. In ordert o oxidize that compound, nitrate of sodium is used. As long as the boiling-point of the liquid is between 280° and 290° F., the sulphide is quietly oxidized to sulphate, nitrite of sodium being formed.

But if the nitrate is added when the temperature of the boiling liquid is about 310° F., a violent evolution of ammonia takes place, according to the following equation:—

$$2\,NaS + Na\,NO^6 + 4\,HO = 2\,Na\,SO^4 + Na\,HO^2 + NH^3.$$

As the liquor contains a large amount of sulphide, the quantity of ammonia is so considerable that it may prove worth while to connect the evaporating-pot with a tower filled with coke, over which a stream of water or dilute acid is running.

If the nitrate be added when the liquor has been heated to a temperature much above 310°, a violent evolution of pure nitrogen occurs.

$$5\,NaS + 4\,Na\,NO^6 + 4\,HO = 5\,Na\,SO^4 + 4\,NaO^2\,H + 4\,N.$$

—*From the Proceedings of the Literary and Philosophical Society of Manchester,* January 21, 1862.

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[FOURTH SERIES.]

APRIL 1862.

XXXIV. *On the Passage of Radiant Heat through moist Air, and on the Hygroscopic Properties of Rock Salt.* By G. Magnus[*].

THE deportment of our atmosphere in reference to the passage of the solar heat through it appeared especially important to me in the investigation on the propagation of heat in gases, of which I gave an account to the Königliche Academie on the 30th of July 1860[†], and 7th of February 1861. Although it could be foreseen that the small quantity of aqueous vapour which the air can take up at the ordinary temperature would exercise a scarcely perceptible influence on the passage of the thermal rays, yet, after it had been made out that, under the same circumstances, olefiant gas only transmits half as many rays as oxygen, and ammoniacal gas still fewer, it appeared desirable to investigate whether that anticipation was well founded or not. Experiment completely confirmed it: neither by the application of a source of heat of 100° nor by using a strong gas-flame could any difference be perceived in the transmission of heat through air, whether it was dry or saturated with vapour. This was the more surprising, inasmuch as a paper by Dr. Tyndall[‡], "On the Absorption and Radiation of Heat by Gases and Vapours," published simultaneously with the above investigation, contained the statement that undried atmospheric air, on a certain day, exhibited fifteen times as great an absorption as dried air. Dr. Tyndall has subsequently been further engaged on this subject, as appears from a letter to Sir John Herschel[§] recently published. In this Dr. Tyndall says, "The results from which the opacity of the air has been inferred are all to be ascribed to extraneous matters diffused in the

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[*] Translated from Poggendorff's *Annalen*, No. 12, 1861.

[†] Phil. Mag. vol. xx. p. 510.

[‡] Ibid. vol. xxii. p. 169.

[§] Ibid. vol. xxii. p. 377.

atmosphere, and mainly to the aqueous vapour." He has, he continues, led by the experiments which I have made, again investigated this subject, and the experiments have shown that the action of aqueous vapour was enormous. On the 10th of October he found that the absorption by the air of the laboratory consisted of three components. If the first, which is due to pure air, be designated by the number 1, the second, produced by the transparent aqueous vapour is 40, and the third, caused by the effluvia of the laboratory and the carbonic acid, is 27. The total action of foreign substances on that day was certainly 67 times as great as that of the atmospheric air alone, and that of the aqueous vapour was certainly 40 times as great.

This statement has caused me to repeat the experiments in this direction which I published in Poggendorff's *Annalen*, vol. cxii. p. 539 (Phil. Mag. vol. xxii. p. 85). But neither by using the apparatus described in page 87 and depicted on Plate I. fig 2, in which the heated bottom of a glass vessel sent its heat to the pile through the air without the intervention of any plates, nor even when the heat of a lamp passed through a tube closed with glass plates, could any difference be perceived between air saturated with aqueous vapour at 15° C., and perfectly dried air. It follows again from this, that aqueous vapour, so long as it is not separated as fog, exercises at 15° C. no appreciable influence on the transmission of thermal rays, and that the rays of the sun, so long as the air is clear, reach the earth in the same manner, whether the atmosphere is saturated with vapour or not.

Besides the above experiments I have made similar ones with plates of rock salt, but I soon found that the use of the latter presents considerable difficulties; for rock salt in saturated air readily attracts moisture, and becomes covered with a solution of salt which may become so considerable as to drip off. If a plate of rock salt be placed in an inclined position under a bell-glass, under which is a vessel containing water, the solution gradually collects towards the lower parts, and falls in drops into a vessel placed underneath. The attraction of the water was observed in this manner between 10° and 25° C.; the water underneath the bell-glass had no higher temperature. For the sake of comparison, a glass plate was placed each time near the rock-salt plate under the same bell-glass, but it never showed a trace of moisture. The plates of rock salt used were all quite white and transparent. Plates from Northwich in Cheshire were principally used; but plates from Wielicza, from Stassfurth, Ischl and Hall in Tyrol, which I happened to possess, showed the same deportment; and salt from Barcelona, prepared from sea-water, behaved in a similar manner.

If the plate of salt, after it has become covered in a moist atmosphere with a solution of salt, be placed in dry air, the water evaporates and the salt becomes again dry. In the experiments I made, it was merely necessary to expose the plates in the laboratory to obtain them quite dry in a few hours.

Melloni* found that a layer of pure water of a millimetre in thickness transmits no heat which comes from an obscure source of heat. Of the rays of ignited platinum, 5·7 per cent. are transmitted. In Melloni's experiments †, a saturated solution of rock salt transmitted $\frac{1}{11}$ more of the rays from an Argand lamp, than a layer of water of the same thickness, and in Dr. Franz's experiment $\frac{1}{9}$ more. As far as I know, no experiments have been published as to the proportion of rays transmitted through a very thin layer of solution of rock salt; but the quantity which is transmitted must in any case be excessively small. Hence even the thinnest layer of solution on the plate hinders the passage of heat.

In order to investigate how far this is the case, the following experiment was made:—A tube of strong glass, a metre in length, was closed at both ends by plates of English rock salt a millimetre thick. It was first filled with dry air, by sending through it, by means of an aspirator, such a quantity of air, which had been dried by passing through several chloride-of-calcium tubes, as to make it quite certain that all the original air had been displaced. Thereupon the deflection of the galvanometer was observed, which was produced when the rays from a flask blackened on the outside and filled with water kept boiling by passing a current of steam through it reached the thermo-pile after passing through the tube full of dry air. Then, while the rest of the experiment was unchanged, air, which had previously passed through a tube filled with moistened pumice, was sent through the same tube. So soon as only a small quantity of this air filled the tube with the plates of rock salt, the quantity of the heat which reached the thermo-pile, after traversing this air, decreased. If, then, dry air was again transmitted through the tube, the deflection again increased and finally attained its original value. It is perhaps superfluous again to remark that, when the tube was closed with glass plates instead of rock-salt plates, nothing was observed of such a difference in the transmission of the thermal rays. By a continued transmission of moist air through the tube with the rock-salt plates, the quantity of heat which traversed it could easily be reduced to $\frac{1}{4}$. In that case the plates, when removed from the tube, were found to be covered on the inside with moisture. A decrease such as Mr. Tyndall mentions‡, to $\frac{1}{40}$ or to $\frac{1}{67}$, has not, could not be attained in these experiments, not

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* *La Thermochrose*, 207.

† Ibid. 165.

‡ Phil. Mag. vol. xxii p. 377.

even when the outer sides of the plates of rock salt were simultaneously kept moist.

I do not venture to maintain that the remarkable results which Dr. Tyndall obtained on the 10th of October of this year, depend on the hygroscopic properties of his plates of rock salt, since I neither know sufficiently the quality of these plates, nor the precautions which Dr. Tyndall took in his experiments. My only object is to call attention to the difficulties incidental to the use of plates of rock salt in such experiments.

XXXV. *Remarks on Recent Researches on Radiant Heat.*

By JOHN TYNDALL, *F.R.S.*[*]

§ 1. THE last Number of Poggendorff's *Annalen* contains a short paper by Professor Magnus, "On the Passage of Radiant Heat through moist air," a translation of which appears in the present Number of the Philosophical Magazine. This paper has excited considerable interest and some discussion among the scientific men of London, and it is on many accounts desirable that I should not delay attempting to offer an explanation of the differences which exist between my eminent friend and myself. A brief sketch of the history of the subject is also considered desirable; and this, as far as the extremely limited time at my disposal will admit of, I shall also endeavour to supply.

§ 2. On the first perusal of Melloni's admirable work *La Thermochrose*, which came into my hands soon after its publication, the thought of investigating the action of gases on radiant heat occurred to me. Melloni, it will be remembered, failed to obtain any evidence of the absorption of radiant heat by a column of atmospheric air 18 or 20 feet long. My attention was further fixed upon this subject by the discussion carried on in 1851 between Professors Stokes and Challis, regarding Laplace's correction for the theoretic velocity of sound. Professor Challis, it will be remembered, contended that Laplace had no right to his correction, because the heat evolved in condensation would be instantly wasted by radiation in a mass of air of indefinite extension. In the first lecture of my first course at the Royal Institution in 1853, I proposed compressing air in a rock-salt syringe to decide the question; and in a paper presented quite recently to the Royal Society, I have solved this point in a manner which I hope Professor Challis himself will deem conclusive, the mode of solution resembling in some respects my device of 1853. In 1854 the action of gases

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[*] Communicated by the Author.

and vapours on radiant heat was a frequent subject of conversation between my scientific friends and myself; and some of these still remember my remarks at the time; the hopes I entertained regarding the subject, and the devices by which I proposed to meet its difficulties. I was, however, prevented by other engagements from attacking the subject at this time; and not till the early spring of 1859 were my ideas brought to practical definition. Then, however, I devised and applied the apparatus which, with some modifications and improvements, I have used ever since.

This apparatus immediately opened to me a large and rich field of experimental inquiry; and the greatest pleasure this discovery gave me, and which I often expressed to Mr. Faraday at the time, was, that it placed me in possession of a subject in the prosecution of which I could not possibly interfere with the claims of any previous investigator. The first notice of my researches is published in the 'Proceedings of the Royal Society' for May 26, 1859. On the 10th of June following, I made the investigation the subject of a Friday evening discourse at the Royal Institution. The late lamented Prince Consort was present on this occasion, and with characteristic goodness interested himself afterwards to obtain plates of rock-salt for me. I then executed many of my experiments in presence of a large audience; and an account of the discourse is published in the 'Proceedings of the Royal Institution' of the date referred to. I also communicated an account of the investigation to my friend Professor De la Rive, and he published a translation of my communication in the *Bibliothèque Universelle.* The investigation was also described in *Cosmos,* in the *Nuovo Cimento,* and in other Journals. When I reached Paris in 1859, I found that the subject had attracted a greater degree of attention than I could have hoped to see bestowed upon it. In short, the publicity of my mode of experiment and results was quite general.

I will here ask permission to cite a number of these results obtained during the month of July 1859, after the main difficulties of my apparatus had been surmounted. The method employed was substantially the same as that described in my last memoir*. The heat passed from the radiating surface through a vacuum into the experimental tube; the principle of compensation was also employed; the length of the tube used to receive the gas was 12 inches; and from the galvanometric deflection consequent on the admittance of the gas or vapour its absorption was deduced.

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* Phil. Trans., February 1861; Phil. Mag., Sept. 1861.

I. *Gases.*

Name of gas.	Deflection.
Atmospheric air	6°.
Oxygen	8°; 8°; 7°; 7°.
Nitrogen, 20th July	6°; 5°.
Again, 25th July	7°; 7°.
Hydrogen	10°; 10°.
Carbonic oxide	34°; 34°; 34°.
Carbonic acid	37°·5; 35°; 37°·5; 37°.
Nitrous oxide	57°·5; 57°·5; 57°·5
Olefiant gas, 1 inch tension	43°; 43°.
,, ,, 5 inches	62°·5; 62°·5.
,, ,, 30 inches	74°.
Coal-gas, 1 inch tension	28°.
,, 5 inches	54°; 53°.
,, 30 inches	74°; 74°.
Total heat	79°·8.

The figures separated from each other by semicolons indicate the results of different experiments; and their close agreement shows the accuracy which, even in this early stage of the inquiry, the experiments had attained. The above deflections represent the following absorptions, at a common tension of 30 inches of mercury.

II.

Name of gas.	Absorption.
Atmospheric air	6
Nitrogen	6
Oxygen	7
Hydrogen	10
Carbonic oxide	34
Carbonic acid	37
Nitrous oxide	110
Olefiant gas	345
Coal-gas	345

The vapours of the following substances were also examined in the same month, at a common tension, and the annexed results were obtained.

III. *Vapours.*

Name of vapour.	Deflection.
Bisulphide of carbon	16°; 17°.
Bichloride of carbon	33°; 33°.
Iodide of methyle	37°·5; 37°·5.
Chloroform	40°; 41°; 40°.
Benzole	43°; 43°; 44°.
Amylene	55°; 55°.

Table (*continued*).

Name of vapour.	Deflection.
Wood-spirit	55°; 55°.
Methylic alcohol (from Dr. W.)	55°·5; 55°
,, ,, (from Dr. H.)	63°·5; 64° (impure).
Ethylic ether	63°·5; 63°.
Absolute alcohol	64°·5; 64°·5.
Ethyl-amylic ether	65°; 65°.
Sulphuric ether	67°; 67°.
Propionate of ethyle	68°; 68°.
Acetate of ethyle	70°; 70°.
Double brass screen	79°·8.

These deflections correspond to the following absorptions, omitting decimals:—

IV.

Name of vapour.	Absorption.
Bisulphide of carbon	17
Bichloride of carbon	33
Iodide of methyle	38
Chloroform	44
Benzole	50
Amylene	84
Pure methylic alcohol	84
Ethylic ether	200
Absolute alcohol	210
Ethyl-amylic ether	216
Sulphuric ether	237
Propionate of ethyle	252
Acetate of ethyle	282

These results, which followed many thousand undescribed experiments, were all obtained before the end of July 1859; and I should certainly have published them and many others *in extenso* at the time, had I not felt that the wide circulation the general description of the inquiry had obtained relieved me from this necessity. I wished to impart the last finish to my apparatus, and to pursue the subject with that deliberation and thoroughness which its difficulty and importance demanded. Not until the close of 1860 was the full account of the investigation drawn up; and the memoir in which it was embodied bears the receipt of the Royal Society for the 10th of January, 1861. It afterwards formed the Bakerian Lecture for the year.

For months I was harassed by the discordant results obtained with gases generated in different ways. The nitrogen obtained from the passage of air over heated copper turnings gave me at first many times the effect of the air itself; that obtained from the combustion of phosphorus in air differed from both; while the nitrogen obtained from the nitrate of potassa could not be made to agree with its fellows. In like manner, the oxygen obtained from the chlorate of potash and peroxide of manganese differed from electrolytic oxygen; the hydrogen obtained from sulphuric acid and zinc differed from electrolytic hydrogen; the carbonic oxide obtained from chalk and carbon differed from that generated from the ferrocyanide of potassium, while carbonic acid from different sources showed similar anomalies. It will be borne in mind that at this time nothing whatever was known of the vast action which a small amount of certain impurities can exert, and that my own experiments were the first to exhibit this action.

Further, my drying apparatus first consisted of sixteen feet of glass tubing filled with chloride of calcium, and a large U-tube filled with fragments of pumice-stone moistened with sulphuric acid. Sometimes the chloride of calcium was used alone, sometimes the sulphuric acid, and sometimes both were used together. Every morning it was necessary to allow the air to pass through the drying apparatus, and fill the experimental tube several times before the results became constant; and even after they had become tolerably constant with the chloride of calcium, the introduction of the sulphuric acid caused a considerable variation of the absorption. This might naturally be ascribed to the more perfect desiccation of the air by the acid, but this does not account for the effects which I obtained. For when both were used, the magnitude of the absorption was found to depend on the circumstance whether the air entered the sulphuric-acid tube or the chloride-of-calcium tube *first*. I will here give an example of this irregularity.

	Absorption.
Air passed through $Ca\,Cl$ alone	7
When SO^3 was added	4
Through new $Ca\,Cl$ tube	7
New SO^3 tube added	4
Through another $Ca\,Cl$ tube alone	7
A fresh tube of SO^3 added	5
Reversed current of air, and sent it through SO^3 first	10

The fluctuations above referred to are here distinctly exhibited; and the last experiment shows that, without changing the tubes in any way, but merely by reversing the direction in which the current of air passed through them, the absorption was doubled. Difficulties almost innumerable of this kind had to be overcome. I had finally to abandon the chloride of calcium and the pumice-stone altogether, and make use of fragments of pure marble for my caustic potash, and of pure glass for my sulphuric acid. But with these also a long time elapsed before I was master of the anomalies which from time to time made their appearance. The dust of a cork; a fragment of sealing-wax, so minute as almost to escape the eyesight; the moisture of the fingers touching the neck of the U-tube, in which the sulphuric acid was contained —these, and many other apparently trivial causes, were sufficient entirely to vitiate my results in delicate cases, giving me on many occasions effects which I knew to be large multiples of the truth. Thus, while perfectly safe as regards the stronger gases whose energy of action masked small errors, prolonged experiment was needed to connect these with the feebler ones, and to refer them to air as a standard. In short, I thought it due both to the public and myself to abstain from giving more than a clear general account of my inquiry until I had mastered every anomaly that had arisen. I cannot regret having exercised this patience, more especially when I find one of the ablest and most conscientious experimenters of modern times falling, as I believe, into error on some of the points which most perplexed me.

A few weeks subsequent to the receipt of my paper by the Royal Society, that is to say, on the 7th of February, 1861, an account of experiments on the transmission of radiant heat through gases was communicated by Professor Magnus to the Academy of Sciences in Berlin. In this inquiry the absorption of heat by vapours was left untouched, nor did it embrace the reciprocity of radiation and absorption which my investigation revealed. But as regards absorption by gases, Professor Magnus and myself had operated on the same substances; and considering the totally different methods employed, the correspondence between our results must be regarded as very remarkable.

Previous to occupying himself with the transmission of heat through gases, Prof. Magnus had made an investigation on the *conduction* of heat by gases, and he was led naturally by this inquiry to take up the question of gaseous diathermancy. My knowledge of his great skill and extreme caution as an experimenter entirely ratifies a statement which he has repeated more than once in his published memoir, namely, that his results on the diathermancy of gases were already obtained at the time he communicated his results on conduction to the Academy of which he is a member, that is to say, in the month of July 1860; in fact the very experiments intended to determine their conduction, really revealed the absorption of the gases. I am quite persuaded that the results of Prof. Magnus are independent of mine, and that, had I published nothing on the subject, his own inquiries would have led him to the discoveries which he has announced. That my researches preceded his by more than a year, is simply to be ascribed to the fact of my attention having been directed to the radiation of heat through gases long before even his researches on conduction had commenced. It is needless to dwell upon the value of such a general corroboration as that which subsists between Prof. Magnus and myself. However private interests may fare, science is assuredly a gainer when independent courses of experiment lead, as in the present instance, to the same important results.

§ 3. But while furnishing, by an independent method, a highly valuable general corroboration of my results, there are some special points on which Prof. Magnus differs from me; and one of these (the action of aqueous vapour on radiant heat) he has made the subject of special examination. My first experiment gave the action of the vapour of the London air on a November day to be 15 times that of the air itself. Only a few weeks subsequently Prof. Magnus announced, and cited very clear experiments in support of his statement, that the amount of aqueous vapour capable of being taken up by air at a temperature of 15° C. has no influence whatever upon the absorption. This announcement caused me to repeat my experiments with more than usual care; and I found the absorption of the vapour not 15 times, but 40 times that of the air. This result was mentioned incidentally in my letter to Sir John Herschel; and Prof. Magnus, induced by this mention to take up the question again, corroborates his former result, and finds, by repeated experiments, that the aqueous vapour of the atmosphere has no influence whatever upon radiant heat, "and that the rays of the sun, so long as the air is clear, reach the earth in the same manner whether the atmosphere is saturated with vapour or not."

The more I experiment, the further I seem to retreat from the position of my friend; for in a paper quite recently presented to the Royal Society, I have set down the action of the air of the laboratory of the Royal Institution, not at 15, nor at 40, but often at 60 times that of perfectly dry air. In fact, the more experienced I become, and the greater the precautions I take to exclude impurities, the more does atmospheric air, in its action on radiant heat, approach the character of a vacuum, and consequently the greater, by comparison, becomes the action of the aqueous vapour of the air.

In the paper which has suggested this communication, Prof. Magnus assigns as a possible source of error on my part, that the aqueous vapour may have been precipitated in a liquid form upon my plates of rock-salt. He cites experiments of his own to show the hygroscopic nature of this substance; and refers to Melloni's experiments in proof of the highly opake character of a solution of rock-salt for the obscure rays of heat. Such a solution on the surfaces of my plates might account in part for the extraordinary absorption which I have observed. In a series of experiments made with the express intention of wetting the plates of salt by precipitation, Prof. Magnus exalts the absorption to 4 times that of air; but though the plates were visibly wet, no nearer approach than this could be made to my result, which makes the absorption of aqueous vapour 40, 50, and even 60 times that of air. It was only on the inner surface of the salt, which came into contact with the saturated air, that the moisture was precipitated in the experiments of Prof. Magnus; the outer surface, which was in contact with the common air of his laboratory, remained dry; and even the wetted surface, when exposed for a time to the same air, became dry also. I would here, at the commencement, remark that *it is with this common outer air, and not with air artificially saturated with moisture, that I find the absorption of aqueous vapour to be* 50 *or* 60 *times that of the air in which it is diffused.* In fact, if I am correct, the action of aqueous vapour upon radiant heat might be applied in the construction of a hygrometer surpassing in delicacy any hitherto devised.

I think it would be hardly possible for a person of any experimental aptitude whatever, to work, as I have done, for three years with plates of rock-salt, which must be kept polished and bright, without becoming aware of all the circumstances referred to by Prof. Magnus. But the truth is that I was well acquainted with the peculiarities of rock-salt many years before this investigation commenced*. A slight consideration of the conditions of the case will, I think, show how improbable it is that a precipitation, such as that surmised, could take place in my experiments. First, then, the common air of the laboratory, according to Prof. Magnus, does not produce the effect which he considers may be active in my case; this, as already stated, is the air which I have employed in all kinds of weather, dry as well as moist. Secondly, this air is introduced into a tube through which is passing a flux of heat from the radiating source. Thirdly, the air on entering the tube is heated by the stoppage of its own motion, and thereby rendered more capable of maintaining its vapour in a transparent state. The exterior surface of my terminal plate of salt was, moreover, always open to inspection, and it was never found wet; much less could the inner surface be wetted, because the temperature within the tube was higher than that without.

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* The action of moisture upon rock-salt was unhappily made strikingly evident to me some months ago; for through a chink in the roof of the laboratory some water entered, which destroyed two of my plates, and left me more or less a cripple ever since.

But I have not relied on the inspection of the outer surface alone of my rock-salt plates. I have taken my apparatus asunder fifty times and more, on occasions when I had most reason to expect precipitation, but have not been able to find a trace of moisture on my plates.

This, however, did not entirely satisfy me, and I therefore made an arrangement of the following kind:—An India-rubber bag was filled with air and subjected to gentle pressure. By a suitable arrangement of cocks and T-pieces, this air could be forced either through a succession of tubes containing fragments of marble moistened with caustic potash and fragments of glass moistened with sulphuric acid; or through a similar series in which fragments of glass were moistened with distilled water. A current of either dry air or damp air could be thus obtained at pleasure; and my object then was to get either the dry air or the wet air, under precisely the same conditions, into an *open tube.* To effect this, matters were so arranged that either current could be discharged into the same narrow glass tube. This glass tube was left in undisturbed connexion with one end of my experimental tube, while the other end was connected with the air-pump. *The plates of salt were entirely abandoned,* the experimental tube was separated from the "front chamber" described in my memoir, and a distance of a foot intervened between the radiating surface and the adjacent open end of the tube. In front of the other open end of the experimental tube was my thermo-electric pile, the "compensating cube" being applied in the usual way. By pressing the bag and gently working the pump, I could, to a great extent, displace dry air by moist, and moist air by dry. And in this way, *without any plates of rock-salt whatever,* I verified all the results that I had obtained with them. I have executed similar experiments in the case of all other vapours that I have examined, and find that with them, as well as with aqueous vapour, my plates of rock-salt are perfectly to be relied on.

Whence, then, the difference between Prof. Magnus and myself? I am quite persuaded that no greater care could be bestowed upon scientific work than Prof. Magnus bestows upon his; and it is the perfectly accurate nature of his experiments which renders the explanation of the differences between us an easy task.

Let me, however, first ask attention to what I may call a case of *internal evidence.* I think the mere inspection of the drawing of my apparatus in the 'Philosophical Transactions' will show that there was a good deal of thought and labour expended in the construction of it. To one part of it especially I would direct attention. In front of the experimental tube is a chamber which is always kept exhausted, the radiant heat thus passing through a vacuum into the experimental tube. To obtain that chamber gave me great trouble: I had to unite its anterior wall with silver solder to its sides; and this, moreover, had to be done for every special source of heat employed. I had to cause this chamber to pass through a copper vessel, soldering it water-tight at its place of entrance and of exit. This vessel I had to connect by a tube 20 feet long with the water-pipes of the Institution, so as to get a supply; and to carry off the water, I had the stone floor of the laboratory perforated, and one of our drains connected by a second tube with the vessel. As already known, this vessel was intended to prevent the heat of the source from reaching my first plate of rock-salt. The introducing of this plate air-tight between the front chamber and the experimental tube was also a difficult matter, which required special means to meet it. Now let me ask what could have induced me to go to all this trouble? The obtaining of suitable plates of rock-salt has been one of my greatest difficulties; why then did I expend my time in seeking for a pair of them? Why did I not content myself with a single plate to stop the remote end of my tube, and allow the latter to form a continuous whole from the radiating surface to the remote end? Nay, why did I not abandon both plates, and simply cement my pile air-tight into the remote end of my tube? All these devices passed through my mind and formed subjects of experiment at an early stage of this inquiry. These experiments taught me that by bringing the gas whose deportment I wished to examine *into direct contact with my source of heat*, or into direct contact with the face of my pile, I entirely vitiated my results. And this arrangement, which in my case would have been perfectly fatal as far as accuracy is concerned, is that which Prof. Magnus has adopted, and is, I believe, the sole source of the differences which have shown themselves between his results and mine.

His chief apparatus may be thus described*:—A glass vessel fits like a receiver with its ground edge on the plate of an air-pump. To the top of this receiver a second glass vessel is fused, and partially filled with water. Into this water steam is conducted, which causes the water to boil—a temperature of 100° C. being thus imparted to the bottom of the vessel, which is at the same time the top of the receiver. On the plate of the air-pump a thermo-electric plate is fixed with its face turned upwards, so as to receive the radiation from the heated top of the receiver. The face of the pile can be screened off at pleasure from the radiation from above. From the pile, wires proceed through the plate of the air-pump

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* The apparatus itself is drawn, and a translation of the paper to which it refers is published, in the Philosophical Magazine, vol. xxii. pp. 1, 81, Pl. I. fig. 2.

to the galvanometer. The receiver is first exhausted and the screen removed; the consequent deflection gives the amount of heat radiated against the pile through a vacuum. Air, or some other gas, is then admitted, and the reduction of the deflection is regarded as due to the absorption of the gas*.

Air at the common laboratory temperature is here admitted into direct contact with the radiating source possessing a temperature of 100° C.; chilling of that source is the immediate consequence. And no matter how long the gas may remain there, the hot surface can never attain its pristine temperature. Prof. Magnus, it will be observed, experiments in the ordinary way, making use of one face only of his pile. I entirely failed to obtain any absorption by air or any of the elementary gases by this mode of experiment, while Prof. Magnus obtains for oxygen and air an absorption of 11 per cent., and for hydrogen an absorption of 14 per cent. My apparatus enables me to measure an absorption of 0·1 per cent.; and surely with it an action so gross as the above could never have escaped me. Nor could it have escaped Melloni, who operated upon a column of air fifteen times the length of that used by Prof. Magnus, and still found no absorption. With a column of air more than double the length of his I obtain for oxygen only $\frac{1}{110}$th of the absorption ascribed to it by Prof. Magnus, and only $\frac{1}{140}$th of what he finds for hydrogen.

The greater action of hydrogen is quite in accordance with the known chilling-power of that gas. While ascribing their results to a different cause, some experiments of my own, which I have briefly described in the paper recently presented to the Royal Society, completely corroborate those of Prof. Magnus. In these experiments the gases were allowed to come into direct contact with the radiating source, and here the action of hydrogen bore to that of oxygen the precise ratio found by Prof. Magnus. The tube used in these experiments was 8 inches long; and had I been tempted to ascribe the results to absorption, I should have found in a tube of the above length fifty times the effect observed in a tube 33 inches long, in which the gases were withdrawn from contact with the source.

The negative results of Prof. Magnus, as regards aqueous vapour, are now sufficiently intelligible. The action which he observed in the case of air being due to direct chilling by contact—a process in which the mass of the chilling agent is the most important consideration—the action of the minute quantity of aqueous vapour present in the air becomes a vanishing quantity. He makes air more than a hundred times what it ought to be, and the action of the vapour practically disappears.

It is curious and instructive to observe the contrast of opinion between Prof. Magnus and myself. He concludes that even if his experiments did not actually prove it, it must be evident that the small amount of aqueous vapour in the air cannot sensibly affect the absorption; and I apply the same consideration of smallness of quantity to account for the neutrality of the aqueous vapour, when mixed with air, as a chilling agent by contact. With regard to absorption, however, the quantity of vapour usually afloat in the atmosphere is quite enormous in comparison with some of the quantities with which I work. Indeed it is common with me to operate with quantities of various vapours which, multiplied thousands of times, would not equal in volume the vapour of the atmosphere.

Further, an inspection of my experiments showed me long ago that those substances which, in the liquid condition, are highly absorbent of radiant heat, are also highly absorbent in the vaporous condition. Indeed, prompted by this fact, I have already commenced experiments for the purpose of examining whether the same amount of matter does not exert the same absorption, whether it be in the liquid or the vaporous state. Now, water is proved by Melloni to be the most opake liquid that he had examined; and it would be perfectly anomalous to me, on *à priori* grounds, if the vapour of this liquid proved so utterly neutral as the experiments of Prof. Magnus would make it.

But I have also spoken of the exposure of the naked face of the pile to the gas experimented with. My experience of this arrangement is not without instruction.

I had a square aperture cut into a tin tube, and the face of a pile introduced into the aperture, and cemented air-tight all round. The tube was closed at the ends and put in connexion with an air-pump. The tube being exhausted and the needle of the galvanometer connected with the pile at zero, on allowing air to enter, its motion was soon arrested, and an equivalent amount of heat was generated. This heat, communicated to the face of the pile, was sufficient to dash my needles against the stops at 90°. I do not entertain a doubt of being able to cause my needles to swing through an arc of 500° by the heat thus generated. When, on the contrary, the tube was *full* at the commencement, and the needle at zero, two or three strokes of the pump sufficed to send the needle up against the stops, the deflection now being due to the chilling of the inner face of the pile. In fact this very deportment of a gaseous body on entering an exhausted receiver, and on being pumped out of a full one, has enabled me to solve the paradoxical problem of determining the radiation and absorption of a gas or vapour without any source of heat external to the gaseous body itself. The pile of Prof. Magnus was exposed to a similar action to that here described, though he never, to my knowledge, refers to it. It would be quite impossible for me to carry out my experiments with a pile thus circumstanced; for after the instrument had been either heated or chilled dynamically, it required in some cases hours for the needle to return to zero. I may add that I have made these experiments on dynamic heating and chilling with my needles loaded with pieces of paper, so as to render their motion visible to the most distant members of the large audience of the Royal Institution.

§ 4. In addition to the experiments made with the apparatus which I have described, Prof. Magnus has made two other series with a glass tube one metre in length, and stopped at its ends by plates of glass. His source of heat in this case was a powerful Argand lamp, the rays of which were collected by a parabolic mirror placed behind it. In one series the tube was covered within by a coating of blackened paper, while in the other this coating was removed, the radiation through the tube being in this case augmented by the reflexion from its sides. With the blackened tube, Prof. Magnus corroborates the results already obtained for air by Dr. Franz, who makes the absorption of a column of nearly the same length as that employed by Prof. Magnus 3 per cent. of the incident heat.

The difference between this result and that obtained with the other apparatus of Prof. Magnus, which gave an absorption of 11 per cent., might naturally be ascribed to the different kinds of heat employed in the respective cases. But in the series of experiments made with his *unblackened* tube, and in which the lamp above described was also his source of heat, he finds the absorption of oxygen and of air to be 14·75 per cent.; and of hydrogen to be 16·23 per cent. of the incident heat. This great difference between the blackened and the unblackened tube, Prof. Magnus ascribes to a change of quality which the heat has undergone by reflexion at the interior surface of the tube, and which has rendered the heat more capable of absorption. I have tried to obtain this result with a glass tube of nearly the same length as that used by Prof. Magnus, but have failed to do so. The absorption of oxygen and air in his tube is 140 times, and the absorption of hydrogen is 160 times what they show themselves to be in mine.

Whence these differences? They are plainly to be referred to a source the same in kind as that which rendered an account of the former ones; indeed I know not a more instructive example of a single defect running through a long series of experiments faithfully made, and so completely accounting for all the observed anomalies. Prof. Magnus stops his tube with plates of glass 4 millimetres in thickness. Now Melloni has shown that 61 per cent. of the rays of a Locatelli lamp are absorbed by a plate of glass 2·6 millimetres in thickness. It is therefore almost certain that 70 per cent. of the entire heat emitted by the lamp of Prof. Magnus were lodged in his first glass plate. A much less quantity of the direct heat would be absorbed by his second plate; but here the amount absorbed would be most effective as a secondary source of heat, on account of the proximity of this plate to the thermo-electric pile.

With the blackened tube, then, we had three sources of heat acting directly or indirectly upon the pile—the lamp, the first plate of glass, and the second plate. In reality, however, the sources reduce themselves to *two*. For, glass being opake to the radiation from glass, the heat emitted by the first plate was expended in exalting the temperature of the second, close to which the pile was placed. On admitting air at the ordinary temperature into this tube, an effect similar in kind to that which takes place in the other instrument of Prof. Magnus must occur: the heated glass plates would be chilled, and they would be chilled more by the hydrogen than by the air, thus giving us the exact results recorded by Prof. Magnus.

The same considerations applied to the unblackened tube, explain perfectly the singular result obtained with it. On theoretic grounds it is extremely difficult, if not impossible, to conceive of such a change of quality as that above referred to. But there appears to be no reason to call in its aid. Prof. Magnus himself finds that the quantity of heat transmitted through his unblackened tube is 26 times the quantity which gets through his blackened one where the oblique radiation is cut off. In the case therefore of the naked tube, the flux of heat sent down by the heated glass plate adjacent to the lamp, to its fellow at the other end, and likewise the heat sent directly from the lamp to the same plate, are greatly superior to what they are in the case of the blackened tube. The plate adjacent to the pile becomes therefore more highly heated in the case of the naked tube; and as its chilling is approximately proportionate to the difference of temperature between it and the cold air, the withdrawal of heat will be greatest when the tube is unblackened within. While leaving myself open to correction, I would offer this as the explanation of the extraordinary result which Prof. Magnus has obtained. It is, I submit, not a case of absorption, but of direct chilling by the cold air.

It is hardly necessary to say that similar remarks to those made with reference to the blackened tube of Prof. Magnus apply to the experiments of Dr. Franz. Dr. Franz, if I am correct, never touched the absorption by air at all; his effects are entirely due to chilling by contact. The mistaking of chilling for absorption causes him to find the same effect in a tube

45 centimetres long as in a tube of 90 centimetres. He ranks carbonic acid as low as air, and makes bromine-vapour a greater absorbent than nitrous acid, whereas the absorption by the compound gas is vastly greater than that by the elementary one. The heat rendered latent by the evaporation of his bromine, augmented the effect which in reality he was measuring. In fact all the differences between the German philosophers and myself appear to be strictly accounted for by reference to a source of error which the application of plates of rock-salt enabled me from the outset to avoid*.

Royal Institution,
March 1862.

XXXVI. *Note on the Transformation of a certain Differential Equation.* By A. Cayley, *Esq.*[†]

THE differential equation

$$(1+\theta^2)\frac{d^2y}{d\theta^2}+\theta\frac{dy}{d\theta}-m^2y=0,$$

if we put therein $i\theta=2x^2+1$ ($i=\sqrt{-1}$ as usual), becomes

$$(1+x^2)\frac{d^2y}{dx^2}+x\frac{dy}{dx}-4m^2y=0.$$

In fact an integral of the second equation is $(\sqrt{1+x^2}+x)^{2m}$; this is

$$=(\sqrt{(2x^2+1)^2-1}+2x^2+1)^m;$$

or putting $\iota\theta=2x^2+1$, it is

$$=(\sqrt{-\theta^2-1}+i\theta)^m,$$

which is

$$=\{i(\sqrt{\theta^2+1}+\theta)\}^m;$$

so that an integral of the transformed equation in θ is

$$=(\sqrt{\theta^2+1}+\theta)^m.$$

And writing in the second equation θ for x, and $\frac{1}{2}m$ for m, we

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* I should be willing to pay a heavy price for a clear specimen of this substance. Results of the very highest interest are, I believe, quite within the reach of any experimenter who may be fortunate enough to possess a suitable prism and one or two lenses of transparent rock-salt; and I am practically disabled at the present moment through my inability to procure a moderate quantity of this precious material. A pair of plates, or even one plate, of rock-salt, 3 inches in diameter and an inch thick, would also be of the greatest use to me.

[†] Communicated by the Author.

see that the last-mentioned function, viz. $(\sqrt{\theta^2+1}+\theta)^m$, is an integral of

$$(1+\theta^2)\frac{d^2y}{d\theta^2}+\theta\frac{dy}{d\theta}-m^2y=0;$$

whence the transformed equation in θ must be this very equation, that is, it must be the first equation. I have for shortness used the particular integral $(\sqrt{1+x^2}+x)^{2m}$; but the reasoning should have been applied, and it is in fact applicable, without alteration, to the general integral

$$\mathrm{C}(\sqrt{1+x^2}+x)^m+\mathrm{C}'(\sqrt{1+x^2}-x)^m.$$

There is of course no difficulty in a direct verification. Thus, starting from the first equation, or equation in θ, the relation $i\theta=2x^2+1$ gives

$$\frac{dy}{d\theta}=\frac{i}{4x}\frac{dy}{dx},\quad \frac{d^2y}{d\theta^2}=\frac{i}{4x}\frac{d}{dx}\left(\frac{i}{4x}\frac{dy}{dx}\right)=-\frac{1}{16x^2}\left(\frac{d^2y}{dx^2}-\frac{1}{x}\frac{dy}{dx}\right),$$

$$1+\theta^2=-4x^2(1+x^2);$$

so that the equation becomes

$$\frac{1}{4}(1+x^2)\left(\frac{d^2y}{dx^2}-\frac{1}{x}\frac{dy}{dx}\right)+\frac{1+2x^2}{4x}\frac{dy}{dx}-m^2y=0.$$

Or multiplying by 4,

$$(1+x^2)\frac{d^2y}{dx^2}+\left(-\frac{1+x^2}{x}+\frac{1+2x^2}{x}\right)\frac{dy}{dx}-4m^2y=0;$$

that is,

$$(1+x^2)\frac{d^2y}{dx^2}+x\frac{dy}{dx}-4m^2y=0,$$

the second equation. But the first method shows the reason why the two forms are thus connected together.

2 Stone Buildings, W.C.,
February 19, 1862.

XXXVII. *Elementary Proof, that Eight Perimeters, of the Regular inscribed Polygon of Twenty Sides, exceed Twenty-five Diameters of the Circle.* By Professor Sir William Rowan Hamilton, *LL.D., &c.**

It was proved by Archimedes that 71 perimeters, of a regular polygon of 96 sides inscribed in a circle, exceed 223 diameters; whence follows easily the well-known theorem, that eight circumferences of a circle exceed twenty-five diameters, or that $8\pi>25$. Yet the following elementary proof, that eight perimeters of the regular inscribed polygon of *twenty sides* are

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* Communicated by the Author.

greater than twenty-five diameters, has not perhaps hitherto appeared in any scientific* work or periodical; and if a page of the Philosophical Magazine can be spared for its insertion, some readers may find it interesting from its extreme simplicity. In fact, for completely understanding it, no preparation is required beyond the four first Books of Euclid, and the few first Rules of Arithmetic, together with some rudimentary knowledge of the connexion between arithmetic and geometry.

1. It follows from the Fourth Book of Euclid's 'Elements,' that the rectangle under the side of the regular decagon inscribed in a circle, and the same side increased by the radius, is equal to the square of the radius. But the product of the two numbers, 791 and 2071, whereof the latter is equal to the former increased by 1280, is less than the square of 1280 (because 1638161 is less than 1638400). If then the radius be divided into 1280 equal parts, the side of the inscribed decagon must be greater than a line which consists of 791 such parts; or briefly, if the radius be equal to 1280, the side of the decagon exceeds 791.

2. When a diameter of a circle bisects a chord, the square of the chord is equal, by the Third Book, to the rectangle under the doubled segments of that diameter. But the product of the two numbers, 125 and 4995, which together make up 5120, or the double of the double of 1280, is less than the square of 791 (because 624375 is less than 625681). If then the radius be still represented by 1280, and therefore the doubled diameter by 5120, and if the bisected chord be a side of the regular decagon, and therefore greater (by what has just been proved) than 791, the lesser segment of the diameter is greater than the line represented by 125.

3. The rectangle under this doubled segment and the radius, is equal to the square of the side of the regular inscribed polygon of twenty sides. But the product of 125 and 1280 is equal to the square of 400; and if the radius be still 1280, it has been proved that the doubled segment exceeds 125; with this representation of the radius, the side of the inscribed polygon of twenty sides exceeds therefore the line represented by 400; and the perimeter of that polygon is consequently greater than 8000.

4. Dividing then the numbers 1280 and 8000 by their greatest common measure 320, we find that if the radius be now represented by the number 4, or the diameter by 8, the perimeter of the polygon will be greater than the line repre-

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* A sketch of the proof was published, at the request of a friend, in an eminent literary journal last summer, but in a connexion not likely to attract the attention of mathematical readers in general. At all events, it pretends to no merit but that of brevity, and the simplicity of the principles on which it rests.

sented by 25; or in other words, that *eight perimeters of the regular inscribed polygon of twenty sides* (and by still stronger reason, *eight circumferences of the circle* itself) *exceed twenty-five diameters.*

Observatory, March 7, 1862.

XXXVIII. *On the Form and Distribution of the Land-tracts during the Secondary and Tertiary periods respectively; and on the effects upon Animal Life which great changes in Geographical Configuration have probably produced.* *By* SEARLES V. WOOD, *Jun.*

[Continued from p. 171.]

SECTION 3.—*The Changes in the Geographical Configuration which resulted from Post-cretaceous Volcanic Action.*

WE have numerous evidences that, since the close of the secondary period, the volcanic energy has, with the exception of the chain of the Cordillera, its continuation in the Rocky Mountains, and the coast ranges of the latter, been exerted in a diametrically opposite direction to that which I have attempted to show prevailed throughout the secondary period, the tertiary bands having been, with these exceptions, from east to west. I have collected below the various tertiary geological systems and anticlinals of which I am cognizant, and also the existing volcanic bands, omitting the Andes, with the average direction they possess: the strictest accuracy in direction is not pretended, the point of the compass being given which coincides nearest with the general strike or direction of the chain or band.

The following are the systems or anticlinals whose direction is due to post-cretaceous action. (The figures on the left hand refer to the diagram.)

1.	The Antilles	E. to W.
2.	Venezuela and Trinidad	E. to W.
3.	The Isles of Portland, Purbeck, and Wight, and Weald of Kent	E. to W.
4.	The Pyrenees	E. to W.
5.	The Balearic Isles	W.S.W. to E.N.E.
6.	The Atlas	W.S.W. to E.N.E.
7.	Malta and Gozo	N.W. to S.E.
8.	The principal Alps, and the Noric Alps	E.N.E. to W.S.W.
9.	The Apennines, and the Julian and Dinaric Alps.	N.W. to S.E.
10.	Bosnia and Albania	N.W. to S.E.
11.	The Carpathians (Northern)	W.N.W. to E.S.E.
12.	The Carpathians (Southern)	W.S.W. to E.N.E.
13.	The Balkan	E. to W.
14.	The Caucasus and Crimea	W.N.W. to E.S.E.
15.	Armenia and Ararat	W.N.W. to E.S.E.
16.	Galicia and Cappadocia	W. by S. to E. by N.
17.	The Turco-Persian frontier	N.W. to S.E.
18.	Crete	E. to W.

19. Cyprus	E. by N. to W. by S.
20. Himalayah	W.N.W. to E.S.E.
21. The Salt-range, Punjab	W. by N. to E. by S.
22. The Trappean range of Nerbudda	E. by N. to W. by S.
23. The Lebanon and Anti-Lebanon	N. by E. to S. by W.
24. Corsica	N. to S.

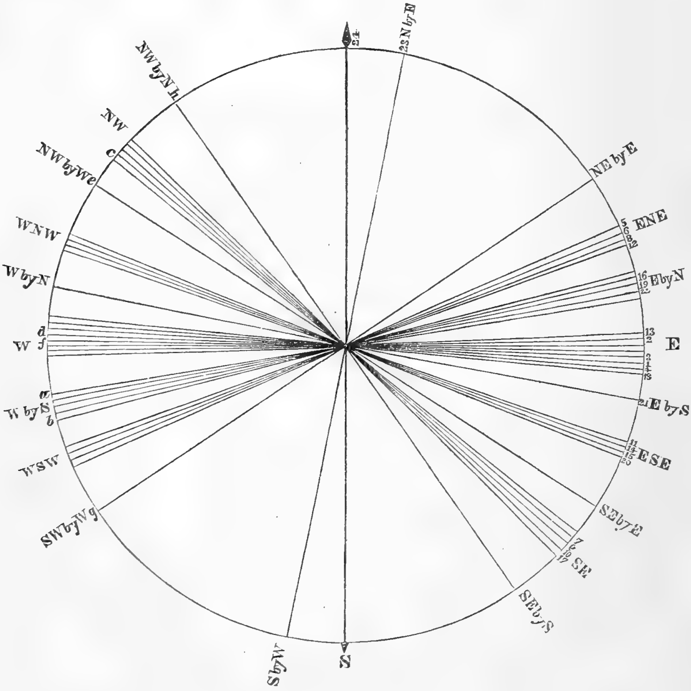
The following are volcanic bands. (The letters on the left hand refer to the diagram.)

a. The Aleutian Isles	W. by S. to E. by N.
b. The Canaries	W. by S. to E. by N.

The band from Andaman to the Society Isles, divided as follows:—

c. Andaman to Java	N.W. to S.E.
d. Java to New Guinea	W. to E.
e. New Britain to New Hebrides	N.W. by W. to S.E. by E.
f. The extinct band of South Australia and Victoria (Australia), extending probably to New Zealand	E. to W.
g. The band from the Aleutian Isles to Formosa, including Japan	N.E. by E. to S.W. by W.
h. The band from Formosa to the Moluccas	N.W. by N. to S.E. by S.*

The Andes, the Rocky Mountains, and the Pacific-coast ranges of North America are omitted.



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* The following authorities may be consulted in reference to the ages of

It is remarkable how the general direction of all the tertiary anticlinals, with two exceptions, coincides with the direction of the existing volcanic bands (other than the Andes), the most im-

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the above systems, anticlinals, and bands, the numbers following referring to the corresponding numbers above :—

1. Heneken, "On St. Domingo," Quart. Journ. Geol. Soc. vol. ix. p. 115. Cuba, Yucutan, and the chain of active volcanoes crossing Central Mexico from E. to W. belong probably to this system.
2. Wall, Quart. Journ. Geol. Soc. vol. xvi. p. 460.
3. Weald : Hopkins, Trans. Geol. Soc. vol. vii. p. 1 et auctorum.
4. Durocher, *Comptes Rendus*, 1851, p. 163. Noulet, in same, 1857 ; also in *Bull.* vol. xv. p. 284. D'Archiac, *Bull.* vol. xiv. p. 507.
5. La Marmora, " On Majorca and Minorca." Turin, 1834.
6. Coquand, *Comptes Rendus*, 1847, vol. xxiv. p. 857. Nicaise, *Bull.* vol. viii. p. 263. As to the coast-ranges forming the sub-Atlan region, see the sections of Laurent's paper " On the Sahara," *Bull.* vol. xiv. p. 616. Consult also Pomel, *Bull.* vol. xii. p. 489 ; Ville, *Bull.* vol. ix. p. 362.
7. Spratt, Proc. Geol. Soc. vol. iv. p. 225.
8. Auctorum. See, however, Murchison, Quart. Journ. Geol. Soc. vol. v. p. 157.
9. Cocchi, *Bull. Soc. Géol. d. France*, vol. xiii. p. 226. Murchison, Quart. Journ. Geol. Soc. vol. v. p. 281. Ponzi, *Bull.* vol. x. p. 196.
10. Viquesnel, *Mém. Géol. Soc. France*, 1842, p. 35.

11 & 12. Boué, *Mém. Géol. Soc. France*, 1834, p. 224. Lilienbach, *Mém. Géol. Soc. France*, 1833, p. 224. Murchison, Quart. Journ. Geol. Soc. vol. v. p. 259.

13. The Balkan region is coloured cretaceous in Murchison and Nicol's Geological Map of Europe, but I have not met with the authority. See, however, Leonhard, *Bull.* vol. xii. (Old Series). Also Spratt, Quart. Journ. Geol. Soc. p. 79.
14. For Caucasus, see Abich, *Vergleichende geologische Grundzüge der Kaukasischen Armenischen und Nord-Persischen Gebirge*, St. Petersburg, 1858. Also in *Comptes Rendus*, 1856, p. 227. For Crimea, see De Verneuil, *Mém. Géol. Soc. France*, vol. iii. (1838).
15. See Abich, as above.
16. Hamilton, Quart. Journ. Geol. Soc. vol. v. p. 369. Tchihatcheff, *Bull. Soc. Géol. d. France*, vol. xi. p. 366.
17. Loftus, Quart. Journ. Geol. Soc. vol. xi. p. 247.
18. Raulin, *Bull.* vol. xiii. p. 439.
19. Gaudry, *Bull.* vol. xi. p. 120.
20. Strachey, Quart. Journ. Geol. Soc. vol. vii. p. 292, vol. x. p. 249. Vicary, Quart. Journ. Geol. Soc. vol. ix. p. 70.
21. Fleming, Quart. Journ. Geol. Soc. vol. ix. p. 192.
22. Hislop, Quart. Journ. Geol. Soc. vol. xi. p. 350. Sankey, Quart. Journ. Geol. Soc. vol. x. p. 55. Calder and Coulthard, 'Asiatic Researches,' Calcutta, 1833, pp. 13 and 47.
23. Botta, *Mém. Géol. Soc. France*, 1833, p. 135.
24. Collomb, *Bull.* vol. xi. p. 63.

f Smyth, Quart. Journ. Geol. Soc. vol. xiv. p. 227. Woods, Quart. Journ. Geol. Soc. vol. xvi. p. 253. Trans. Phil. Inst. Victoria, vol. iii. p. 85. Heaphy, Quart. Journ. Geol. Soc. vol. xvi. p. 242.

For the other volcanic bands, *a*, *b*, *c*, *d*, *e*, *g*, and *h*, see Mallet, Reports of British Association, 1852 to 1858.

portant and marked of which may be considered as parts of one large band, which extends from the Western Isles, with a varying breadth of from 10 to 20 degrees of latitude, through the Mediterranean, Black, and Caspian Seas, and is continued through Southern Asia, under the form of intense earthquake action, down to the head of the Bay of Bengal, and thence in its most active form through the Indian Archipelago to the centre of the South Pacific*. The general conformity in direction of the great tertiary mountain systems of Europe and Asia with that of this great volcanic band forms a striking coincidence. The reports of geologists upon most of the mountain systems of Europe and Asia show, with considerable precision, the part which these systems have played in the formation of the present Europeo-Asiatic continent.

I have in the introductory section alluded to the way in which the elevatory forces have been exerted in foci, forming volcanic bands and afterwards mountain chains, contorting violently the strata within a limited area only, but desiccating over great areas the pre-existing sea-bottom; thus it seems to have been with the bed of the cretaceous ocean, at least in Europe and Asia. Over the whole of Southern Europe and South-Western Asia the sections published show, with the exception of the Carpathians (where Sir R. Murchison has described the nummulitic deposits as resting unconformably upon the secondary beds), that the older tertiary and secondary formations, although thrown into the greatest disorder in the mountain chains, in some by older, but in most by middle and newer tertiary volcanic action, have a general conformability to each other. This, coupled with the well-known hiatus which exists between the fauna of the cretaceous and that of the older tertiary periods, justifies, I think, the conclusion that over the whole of this area the bed of the cretaceous sea must have been desiccated by the effect of elevatory forces having their foci separated by a wide interval, and the whole sea-bed (in order to have preserved its horizontality up to the time when it was again submerged to form the basin of the tertiary sea) have been formed into a continent unmarked by any considerable irregularity of surface. If the view advanced in the introductory section, as to the cause of the contiguity of the sea to volcanic foci, be well founded, this undisturbed condition of the desiccated bed of the cretaceous sea, coexisting with a gap in the geological succession of very great duration, is what we should *à priori* expect to find, by reason that, the volcanic bands of the period being remote from the area in question, an undisturbed permanence of level was permitted; and this level being that of dry land, we should find no formations until the area was again subjected to the direct action of the volcanic bands, and with that

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* See Mallet, 'Reports of British Association,' 1852 to 1858.

to a return of the sea. We may to some extent trace the line of volcanic band to which this elevation was due (or rather in which it had its focus), along the northern border of the tract. Thus the great chain of the Northern Carpathians, although in convulsion during later tertiary periods, appears, from the sections of Sir Roderick Murchison, to have been upheaved, and the cretaceous strata to have acquired a considerable inclination, prior to the formation of the nummulitic deposits*. The Pyrenees also appear† to have undergone their principal elevation prior to the newer cretaceous period. Mr. Prestwich, again, has found reason to infer that the Weald anticlinal had begun prior to the close of the upper cretaceous formation‡. From these, and also from the system of the Jura or Côte d'Or, which, coming into existence in the early part of the cretaceous age§, possessed a direction from N.E. to S.W., or intermediate between those characteristic of the secondary and tertiary periods respectively, it appears that the movements which elevated the old secondary sea-bed, and brought into existence a continent which endured for a period long enough to change the complexion of the higher orders of the animal kingdom, had begun towards the close of the secondary period. To what other volcanic bands the elevation of this continent was due we have not at present the evidence to show; but the general conformity, between the tertiary and cretaceous beds in Southern Europe, to which I have adverted would point to these bands being further to the south than any of the places hitherto examined.

We see that the Maestricht, and also some other deposits of limited extent || which some geologists have referred to the newest cretaceous age, were formed in the contiguity of what appears to me to have been the volcanic band from which the elevation of the secondary sea-bed was proceeding; and their limited character thus becomes intelligible, as they would only endure during the comparatively brief period before the secondary sea-bed became converted into a continental tract, when, the volcanic forces to which that elevation was due becoming quiescent, no further deposits took place until these forces again burst forth and prevailed during the tertiary period over the areas occupied by the

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* See the sections in Murchison, Quart. Journ. Geol. Soc. vol. v. p. 259.

† D'Archiac, *Bull.* vol. xiv. p. 507.

‡ Quart. Journ. Geol. Soc. vol. viii. p. 257.

§ See Lory, *Bull.* vol. xi. p. 780; Benoit, in vol. xv. p. 315.

|| The equivalent of the Maestricht is said by M. Coquand to occur in the Charentes (*Bull.* vol. xiv. p. 571). The late Mr. Sharpe also referred some sands at Farringdon to the same epoch (Quart. Journ. Geol. Soc. vol. x. p. 176); but his views are disputed by others. See Davidson, *Bull.* vol. xi. p. 180.

mountain systems of Southern and Central Europe and South-Western Asia. The absence of deposits of a thickness sufficient to withstand subsequent degradation during a period of elevation has been urged by Mr. Darwin*; and if true (as it may well be in the sense of a general continental elevation, although not in that of the gradual shoaling of such gulfs as those which received the secondary deposits of France and England), we see in it an explanation of the limited extent of the newest secondary (supracretaceous) deposits, since it would only be on the skirts of the continent formed out of the cretaceous sea-bed where these would occur; and this skirt, except on its northern border, has not yet been explored. The intra-cretaceous and tertiary deposits would, I conceive, be taking place in the contiguity of any of the volcanic bands then in activity; and we may still therefore look for their discovery, unless they should now be beneath the ocean. The Cordillera of the Andes, Mexico, California and Oregon, places where, according to the views before discussed, the direction of the coast-line of America has remained since the secondary period unaltered in its main features, and perhaps even Southern India, offer probable sites for the occurrence of intra-cretaceous and tertiary deposits†.

We have seen that the conformability between the newer secondary and the tertiary formations, from the British Isles as far east as India, shows that the tertiary sea over that area returned mainly to the same bed as that occupied by the secondary sea; it differed, however, essentially in one particular, viz. in being shut in to the north by a barrier of land, no inconsiderable portion of which was composed of elevated cretaceous deposits: we find in the nummulitic deposits of Southern Europe, Northern Africa, Southern and South-Western Asia, the evidence of a vast gulf (interspersed with numerous islands) stretching from the Bay of Bengal north-west through Hindostan and Persia, across Asia Minor into Europe and North Africa, including within it the present Mediterranean, Black, and Caspian Seas; while, fringing the barrier of land which bounded it on the north, we find the richly stocked marine deposits of the English, Belgian, and French eocene basins generally associated with estuarine and fluviatile beds of contemporaneous age‡. Fringing land composed of elevated Jurassic deposits,

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* Origin of Species, pp. 300 and 327.

† I entertain considerable confidence that some of the beds associated with the great lignite formation of North-Western America, California, and Vancouver will eventually prove to be of intra-cretaceous and tertiary date.

‡ The same association of fluviatile and estuarine beds with the nummulitic deposit seems to exist wherever an insular tract of land occurred in this gulf—as in the Pyrenees, where remains of eocene mammalia have been

and which in India bounded this gulf on the south, we have the nummulitic eocene beds of Cutch similarly connected with the fluviatile deposits intercalated with trap which occupy a considerable area in Western India. There seems to me every reason to infer that the suggestion of M. d'Archiac, quoted by Sir Roderick Murchison*, affords the true explanation of the phenomena presented, viz. that the eocene formations of Western Europe were but the littoral deposits of the great nummulitic gulf†, and were formed by the sand and mud of rivers debouching into the gulf at the spots where these formations occur, the deltas of which rivers have furnished the estuarine and fluviatile beds which are associated with these deposits. The mollusca of the eocene formations of England, France, and Belgium appear‡ to have all their affinities with the existing mollusca of the present Eastern seas (being those to which we trace the junction of the nummulitic gulf), but exhibit a dissimilarity to the mollusca of the eocene formations of America. M. Abich, in his 'Palæontology of Asiatic Russia' (*Mém. de l'Académie des Sciences de St. Pétersbourg*, 6^me^ série, vol. vii.), figures thirty-two eocene species of mollusca, *of which twenty-six are*, he considers, *identical with English, French, and Belgian eocene forms*, two are given by him as indeterminate, and four only as new species, being respectively 81·25, 6·25, and 12·5 per cent. on the whole number of species described by him. These fossils, obtained from beds reposing on nummulitic rock and overlain by middle tertiary in the neighbourhood of the Sea of Aral, a district contiguous to the southern extremity of the Oural region (which formed the land fringing the sea these forms inhabited), lived at a distance from the English, Belgian, and French basins of upwards of 2500 miles, and strongly confirm the inference (arising from the outcrop across Russia of older strata uncovered by eocene) of a continuous coast-line joining these distant places, lying as

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found. Similar fluviatile and estuarine deposits will doubtless hereafter be discovered associated with the eocene beds of the Aral Sea and Araxes. The return of the sea after the long intra-cretaceous interval to parts of its old secondary bed appears to have been very gradual, and the formation of the great nummulitic deposit to have been preceded by local tertiary formations, mostly fluviatile and estuarine. This, at least, was the case according to M. d'Archiac (*An. Foss. de l'Inde*, p. 77); his remark, however, admits of many exceptions, as the nummulitic deposits frequently repose immediately on the secondaries or other older rocks.

[*] Quart. Journ. Geol. Soc. vol. v. p. 301.

[†] MM. Hébert and Renesier also (*Bull.* vol. xi. p. 604) regard their upper division of the nummulitic deposit of the Alps as the marine equivalent of the upper eocene of the Paris basin, and probably also of the oldest miocene (Mayence, Limbourg, Sables de Fontainebleau, &c.).

[‡] See Introduction to Eocene Bivalves, p. 10; Palæontographical Society's Volume for 1859.

they do under nearly the same latitude, this coast-line being what I have termed the northern shore of the great nummulitic gulf. It is worth observing, also, that such an identity of forms at so great a distance is, so far as I know, unexampled during other tertiary epochs (although it is conspicuous during the palæozoic period), and can hardly have existed, except by virtue of a similarity of conditions over the whole area and of easy communication by coast-line. M. Abich also describes the older tertiaries of the valley of the Araxes as containing a large proportion of the species of the mollusca common to corresponding horizons of the Paris basin, and, intermingled with them, many species agreeing with mollusca from India described by M. d'Archiac*, and containing also well-known forms of nummulites characteristic of the Pyrenean and other South-European nummulitic deposits. The proportion of the mollusca in the beds of the Araxes valley common to the older tertiaries of England, France, and Belgium is not so large as in the case of the beds of the Aral-Sea region; but the intermixture of Indian species much assists the proof of the continuous extension of the nummulitic gulf in the form I have described. The contiguity of the Aral-Sea region to the northern coast-line of the gulf (which, beginning perhaps to the north of the Indian beds described by M. d'Archiac†, extended to the Aral region at the extremity of the Oural chain, and thence to England) would account for the somewhat larger per-centage there of the shells of the basins of North-Western Europe. The beds of the Araxes valley, on the other hand, appear to have occupied a position more towards the centre of the gulf, in the vicinity of insular land (formed by the palæozoic plateau of that region which is uncovered by eocene deposits), but remote from the great coast-lines. The extraordinary range of the mollusca of the older tertiary period over the region filled by this sea tends, moreover, to show that the sea-bed formed by the submergence of the post-cretaceous continent—a continent which I have suggested was a vast tract uninterrupted by great mountains—was shallow over its whole area, the tertiary mountain chains of Southern Europe and South-Western Asia, which have since elevated portions of its bed into land, and deepened other portions into the Caspian, Black, and Mediterranean Seas, not having come into existence until a later date.

While we have thus evidence of a great gulf or land-girt sea stretching, at the dawn of the tertiary period, from the Bay of Bengal in a north-westerly direction to the British Isles, fringed

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* *Animaux fossiles du terrain Nummulitique de l'Inde.* Paris, 1853.

† These beds are, Hala in Scinde, the Cashmere valley, and the range of Subathoo (part of Himalayan chain).

to the northward by a continuous shore, formed of deposits which had been land since the close of the secondary period, and closed from any connexion with the North American seas, we have in the Vertebrata of the period most satisfactory evidence of a continuous land-connexion between the American and the Europeo-Asiatic continents. Associated in the same bed at Kyson in Suffolk, there have been found remains of the *Macacus* (*Eopithecus*), an exclusively eastern genus of monkey, and the *Didelphis*, an exclusively American form of marsupial. In the fluviatile deposit of Hordle in Hampshire, the remains of a type of crocodile resembling the American form (the cayman) occur; at the not far distant locality of Bracklesham, in the marine though slightly older portion of the same delta, the true Asiatic gavial has been found; and in the London clay the true Eastern form of crocodile. In the same fluviatile of Hordle there occurs in the greatest abundance the remains of the peculiar freshwater fish the *Lepidosteus*, now an exclusively American form; and associated with these Vertebrata, a land-shell (*Helix labyrinthica*) now existing only in North America; and the river in whose deposits these forms occur, discharged into a sea containing mollusca whose affinities, as I have shown, are entirely with the East. Continuing eastward into Asia from the European termination of this Atlantic bridge, by following the line of secondary formations, which extend through Northern Europe and Western Asia uncovered by any eocene deposits (they having been already traced as far east as the Aral Sea), we perceive the wide stretch of land which at the dawn of the tertiary period connected the Asiatic region with America. The dissimilarity between the mollusca of the European and American eocene formations, to which I have already adverted, militates against any hypothesis of a coast-line joining the seas in which such formations were respectively deposited; and this agrees with what might be inferred from the indications afforded by the configuration of the secondary strata which skirt the eocene basins of England, Belgium, and France, which is, that the latter countries formed the head of the nummulitic gulf, the coast-line connecting England with the shore of the American eocene sea being on the other side of the land thus closing in that gulf.

The extension of the European continent westward at the dawn of the tertiary period, in the manner I have attempted to describe, that is, in the form of a tract cutting off the nummulitic gulf from the Atlantic, is further shown by the circumstance of the European and American fauna becoming more assimilated when they occur in formations which were due to the sediment of one common ocean, the Atlantic. Thus Sir Charles Lyell long ago showed* the connexion of the American and European *miocene* formations by the presence of several marine molluscous forms in common. Now, the absence of any similar connexion between the eocene marine mollusca of the two continents, while so close a connexion exists between the terrestrial fauna of the eocene of Europe with that now existing in America, seems only intelligible upon the hypothesis of a land tract at once joining the continents, but severing the seas. That this land-connexion has been gradually disappearing since the eocene period, is shown by the agreement among naturalists that the molluscous fauna of the shores of the Western Isles, the Madeiras, and of Portugal, affords evidence of the extension of Western Europe in this direction between the miocene and the pleistocene epochs, forming a province to which they have given the name of Lusitanian†.

The probability of the configuration, at the dawn of the tertiary period, which I have described, receives support also from a consideration of the climatal conditions which the fossils of that period indicate.

It has not unfrequently been remarked, as inconsistent with any theory of a gradual refrigeration of climate during geological periods down to the pliocene, that the eocene fauna of Europe, both vertebrate and invertebrate, should at so late a stage in the geological succession, present at least as tropical a character as that presented by the fauna of any preceding stage in our latitudes. The explanation of this fact, standing out as it does at variance with any law of gradual refrigeration, should, I think, be sought in a consideration of the geographical configuration of the period. It is, with reference to this subject, also worthy of remark that we do not find this tropical eocene fauna extending up into high latitudes, as has been the case with the fauna of more ancient deposits, as the carboniferous of Spitzbergen and Melville Island, and even some of the secondary formations, whose fauna in our latitudes presents perhaps a less tropical appearance than does that of the eocene. In seeking the explanation of this tropical character of the eocene fauna of Northern Europe, we may refer to the existing conditions of such gulfs as the Arabian Sea and Gulf of Persia, the latter of which represents on a very small scale what I conceive the nummulitic gulf

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* Proc. Geol. Soc. vol. iv. p. 554.

† See Forbes in 'Memoirs of Geological Survey of United Kingdom,' vol. i. 1846, p. 406 & pl. 7, who indicates the land as far west as the meridian of 30° W. See also Woodward, 'Rudimentary Treatise on Recent and Fossil Shells,' Weale, London, 1856, pp. 361, 385. See also this view, of the extension of the miocene land into the Atlantic, adopted, from other considerations, by De Verneuil and Collomb, *Bull.* vol. x. p. 77.

to have been. Now, the shores of these two seas or gulfs are the hottest of those of any seas on the globe, although the half of the former and the whole of the latter are extra-tropical. Let us conceive the nummulitic gulf thus extending from its mouth open to the tropical ocean at some point, how far east we have not yet materials to decide, but beyond the region of the Aral Sea, to its head in England and Belgium, and we may realize the effect which would result from that configuration. Not only would the tropical waters have free access and be closed by land from the contact of cold currents from the north, but the shores of this sea would be heated by the accumulation of land surrounding the greater part of it*, while at places on its shore the rivers which formed the deposits of the English and French and other eocene formations had their deltas, the more open portions of the sea furnishing the nummulitic deposits.

The view taken, that this formation of continent in the opposite direction to that theretofore prevailing commenced in the closing epoch of the secondary period contemporaneously with the first outbursts of the east and west bands which have governed the alignement during the post-cretaceous period (that is to say, with the band of the Pyrenees), seems supported by the greater approximation between the faunas of the eastern and western extremities of the Europeo-Asiatic continent which the newer cretaceous beds afford over those of the older and of the Jurassic deposits. The late Edward Forbes first remarked this in the comparison of the fossils from the cretaceous deposits of Verdachellam and Trinconopoly†, since which M. d'Archiac‡ has found in the fossils of the uppermost cretaceous beds of Bains de Rennes, in the Pyrenees, a few species closely resembling forms described by Mr. Forbes from these Indian beds; and M. Abich also gives several upper-cretaceous forms from the Caucasus identical with, or closely resembling, species described from the cretaceous beds of England and France.

The remarks of Mr. Forbes on the fossils from Southern India are so germane to the views discussed in this section, and indeed lend so much support to them, that I am tempted by the weight always attached to the opinions of that deceased naturalist, to subjoin the following extract:—"Considered in regard to the

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* The northern shore I have attempted to describe; the southern shore would be the peninsula of Southern India, where nummulitic or other marine eocene deposits do not occur, and probably Central Africa (which I have, in Section 2, referred to as land probably formed at the commencement of the secondary period), and that region, now sea, referred to in Section 4, containing the islands of Mauritius, &c., in which birds of the secondary continent have been preserved.

† Proc. Geol. Soc. vol. iv. p. 326.

‡ *Bull.* vol. xi. p. 202; see also his remarks, p. 204.

distribution of animal life during the cretaceous era, this collection is of the highest interest. It shows that during two successive stages of that era the climatal influence, as affecting marine animals, did not vary in intensity in the Indian, European, and American regions, whilst the later of the two [Verdachellam and Trinconopoly stage] had specific relations with the seas of Europe which are absent from the earlier [Pondicherry stage]. The cause of this remarkable fact is not to be sought for in a more general distribution of animal life at one time than at another, but rather in some great change in the distribution of land and sea, *and in a greater connexion of the Indian and European seas* during the epoch of the deposition of the upper greensand than during that of the lower. To this cause must also be attributed the peculiar tertiary aspect of the Indian collections, depending upon the presence of a number of forms usually regarded as characteristic of tertiary formations, such as *Cypræa, Oliva, Triton, Pyrula, Nerita,* and numerous species of *Voluta,* the inference from which, since not one of the species is identical with any known tertiary form, should not be that the deposits containing them are either tertiary or necessarily connected with the tertiary, but that the genera in question commenced their existence earliest in the eastern seas." By the expression I have copied in italics, the author, I apprehend, meant a greater connexion of the seas by a more continuous shore line, affording facility for migration of mollusca; and this, in order to join these regions, would necessarily be in an easterly and westerly direction. The origin of the characteristic eocene molluscous forms in the east, and their subsequent development westerly, thus suggested by Mr. Forbes, seems to me to lend support to the view that I have taken, of the connexion of the seas of Western Europe with those of Eastern Asia, in the form of a gulf stretching from Eastern Asia as far at least as the most westerly limits of Europe, at the epoch when the sea again occupied a part of the area which had been continent during the intra-cretaceous and tertiary interval.

If at the commencement of the tertiary period we find the evidences of a tropical climate extending northwards to the 52nd parallel, due to the peculiar geographical configuration of the period, how excessive may we conceive the climate of the intra-cretaceous and tertiary period to have been, when a vast level tract of desiccated sea-bottom, uninterrupted by mountain chains of any importance, extended through the whole region between the tropic of Cancer and the parallel of 50° N., from England on the west to the Bay of Bengal on the east, and (from the evidence of the eocene land-fossils) appears to have been continued westward in a lower latitude to America. Whether this hypertropical belt of continent was continued eastward beyond the Aralian region, we have not any evidence to affirm; the opening-up of Central Asia will alone disclose this, and until then the limit of this extension cannot be realized; but it should not be overlooked that the great region of Oceanica, which Mr. Darwin has shown to consist of submerged mountain chains of immense extent, and to be now in a state of elevation and depression in alternate bands, is traversed by that great volcanic band to the operation of which has been due the formation of the major part of the Europeo-Asiatic continent, that is, the part which is composed of cretaceous and tertiary formations. It is to the extreme climate and widely different conditions to which this configuration must have given rise, that I venture to think may be attributed those complete changes in animal life which took place in the intra-cretaceous and tertiary interval. The effect of a continent stretching east and west, and lying in low latitudes, would operate not merely to exaggerate the terrestrial heat, and produce those interferences with the trade-winds which cause the monsoons and bring the alternations of extreme aridity and extreme moisture, but to affect the marine conditions by arresting the interchange of the tropical with the polar waters,—an example of the effects produced by such causes being now perceptible in the condition of the southern border of the Asiatic continent, and, to a less degree, in that of Africa, where the Bight of Benin washes its southern shore.

We have seen that, as in the palæozoic period, so in the secondary; the complete changes in the direction of the volcanic bands, which took place towards the termination of those periods respectively, did not occur absolutely at their close, but rather heralded it by occurring prior to the last of their epochs (assuming the Permian to be an epoch of the palæozoic period). In the former case, the changes occurred between the carboniferous and the Permian, and in the latter we have seen that the volcanic bands from east to west had come into existence, in the case of the Pyrenees, prior to the formation of the upper cretaceous deposits, while the system of the Jura (which seems to have originated during the cretaceous epoch, from the occurrence in it of detached portions of older cretaceous beds conformable to the Jurassic) possessed a direction midway between those opposite ones characteristic of the secondary and tertiary periods, being from N.E. to S.W. Both in the palæozoic and secondary periods, therefore, the complete changes in the fauna which marked their termination do not appear to have been immediate upon the changes of the geographical alignement, but to have required the lapse of an epoch for their fulfilment; and the completeness of that change is perhaps not less the indirect result of the altered alignement, by the formation of continents where seas had been, and the opening out of new seas for the habitation of marine animals, thereby causing a gap in the geological records so far as they have hitherto been discovered, than the direct result of the changed conditions to which the inhabitants of the seas, and even those of the lands, came to be subject on account of the entire change in the alignement of the land over the globe.

[To be continued.]

XXXIX. *Theorems in the Calculus of Symbols.*

By W. H. L. Russell, *Esq., A.B.*[*]

THE following theorems in differentiation and integration may be proved by means of the Calculus of Symbols. They are a development of results which I gave in a Memoir published in the Philosophical Transactions for the year 1861, and will, I hope, be found interesting to mathematical readers. The proof of these theorems will be suggested by the original memoir.

$$x^{2n+1}\left\{\frac{d}{dx}+\frac{n}{x}\right\}^{2n+1}u=$$

$$\frac{d}{dx}x^3\frac{d}{dx}\cdot\frac{1}{x^2}\cdot\frac{d}{dx}x^5\frac{d}{dx}\cdot\frac{1}{x^4}\cdots\frac{d}{dx}x^{2n+1}\frac{d}{dx}\cdot\frac{1}{x^{n-1}}\frac{du}{dx}$$

$$\frac{d^{nr}u}{dx^{nr}}=\left\{\left(\frac{d}{x^{r-1}\,dx}\right)^n x^{nr+1}\right\}^r\frac{u}{x^{(n+1)r}}$$

$$\frac{d^{2n}u}{dx^{2n}}=\frac{1}{x^{2n+2}}\left(x^2\frac{d^2}{dx^2}\right)^n\frac{u}{x^{2n-2}},$$

$$(-1)^n\int\frac{dx}{x^2}\int\frac{dx}{x^2}\int\frac{dx}{x^2}\cdots\int dx\,x^{2n}\frac{d^nu}{dx^n}=\int dx\,.\,u$$

$$-n(n+1)\frac{1}{x}\int dx\,.\,xu+n\,.\,\frac{n-1}{2}\,.\,(n+1)\,.\,\frac{n+2}{2}\,\frac{1}{x^2}\int dx\,.\,x^2u$$

$$-n\,.\,\frac{n-1}{2}\cdot\frac{n-2}{3}(n+1)\,.\,\frac{n+2}{2}\cdot\frac{n+3}{3}\cdot\frac{1}{x^3}\int dx\,x^3\,u+\ldots,$$

the integral sign in the first member of the equation being repeated $(n+1)$ times.

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[*] Communicated by the Author.

$$(-2)^n \int \frac{dx}{x^3} \int \frac{dx}{x^3} \dots \int dx\, x^{3n} \frac{d^n u}{dx} =$$

$$\int dx\, u - n(n+1)\frac{n+2}{2} \cdot \frac{1}{x^2} \int dx \,.\, x^2 u$$

$$+ n \,.\, \frac{n-1}{2} (n+1) \frac{n+2}{2} \cdot \frac{n+3}{3} \cdot \frac{n+4}{4} \cdot \frac{1}{x^4} \int dx \,.\, x^4 u + \dots,$$

the integral sign in the first member being repeated $(n+1)$ times.

$$(-2)^n \int x\, dx \int x\, dx \int x\, dx \dots \int \frac{dx}{x^{2n+1}} u =$$

$$\frac{1}{1.2.3\dots n} \int \frac{dx \,.\, u}{x} - \frac{1}{1.1.2.3\dots(n-1)} x^2 \int \frac{dx \,.\, u}{x^3}$$

$$+ \frac{1}{1.2} \cdot \frac{1}{1.2.3\dots(n-2)} x^4 \int \frac{dx \,.\, u}{x^5} - \dots,$$

the integral sign in the first member being repeated $(n+1)$ times.

$$\frac{1}{x} \int \frac{dx}{x^2} \int dx\, x + \frac{1}{x^2} \int \frac{dx}{x^2} \int dx \,.\, x^2 u + \frac{1}{x^3} \int \frac{dx}{x^2} \int dx\, x^3 u$$

$$+ \dots + \frac{1}{x^{n-1}} \int \frac{dx}{x^2} \int dx\, x^{n-1} u =$$

$$\frac{1}{x} \int dx \,.\, u - \frac{1}{x^n} \int dx \,.\, x^{n-1}\, u.$$

XL. *Experimental Researches on the Laws of Evaporation and Absorption.* *By* THOMAS TATE, *Esq.*

[Continued from p. 135.]

Maximum absorption of water by different substances. Measure of the porosity of different substances.

THE amount of water which a substance is capable of absorbing depends upon the capacity of its pores, or the volume of its interstices so far as they are permeable by water. In most cases the volume of water which a substance absorbs is equal to the volume of the air expelled. But this is not universally true; for I have found that water will filter through heavy sandstone in opposition to an excess of atmospheric pressure. In order to saturate perfectly certain substances with water, they must be boiled in a vessel exhausted of air.

The capacity of different substances for absorbing and retaining moisture is very various. The following substances being thoroughly dried, I have found that calico, linen, unsized paper, and other substances of this kind absorb and retain about their own weight of water; woollen cloth about twice its own weight; bath brick and fine sand about one-third their own weight; plaster of Paris, after being set, about six-tenths; common deal, six-tenths; laurel-wood rather more than two-thirds; pine-wood nearly six-sevenths; and bran, about three and three-quarters.

In order to determine the porosity of different substances, so far as regards their capacities for absorbing water, we have the following formulæ:—

$$v=\frac{w_2-w}{w_2-w_1}\times V;\ \text{and}\ v=(s_2-s_1)V;$$

where $V=$ the volume of the body; $v=$ the capacity of the interstices of the body, or the volume of that portion of it penetrated by the water absorbed; $w=$ the weight of the body in a dry state, s_1 being its corresponding specific gravity; $w_2=$ its weight when perfectly saturated with water, s_2 being its corresponding specific gravity; and $w_1=$ the weight of the body in water.

By means of these formulæ and the experimental data, I have found the following results:—

For woollen cloth, $v=\frac{3}{4}V$; that is, the interstices are three-fourths of the whole volume of the substance.

For bath brick, $v={}^{2}V$; that is, the interstices are two-fifths of the whole volume of the substance.

For fine sand, $v=\cdot 47$; that is, the interstices are nearly one-half of the whole volume of the substance.

For deal, $v=\frac{1}{3}V$; for laurel-wood, $v=\cdot 45V$; and for pine-wood, $v=\frac{3}{8}V$. In these cases, however, a slight allowance must be made for the expansion of the wood by the absorption.

Contraction and elongation of textile fabrics by the absorption of moisture.

Under a constant stretching force, calico, linen, flax, and unsized paper undergo contraction upon the absorption of moisture, whilst certain woollen fabrics undergo elongation. Thus strips of linen and calico were contracted about the $\frac{1}{100}$th part of their length, whilst strips of woollen cloth were elongated nearly the same proportional part of their lengths by the absorption of moisture. Threads of cotton and flax were contracted about $\frac{1}{80}$th part of their lengths.

The strips as well as the threads were suspended from one extremity, and had metal plates attached to their lower extremities, so as to give the material a uniform tension. The divisions of the scale, measuring the lengths of the strips, were divided into tenths of an inch, so that with a vernier the contraction or elongation, as the case might be, could be read off to hundredths of an inch. After the strips had become dry, they *nearly* returned to their original lengths.

A curious experiment.—Attach an oblong plate of metal to one extremity of a cotton thread, and suspend it from a loop formed in the other extremity: immerse the thread in a deep glass jar filled with water; then *the plate will revolve rapidly in a direction contrary to the direction of the twist of the thread*: the rotation will go on for some time after the thread has been taken out of the water. When the rotation has ceased, let the plate be restrained from any further rotation; then after the thread has become dry, remove the obstruction placed against the plate, and it will again rotate in the same direction, and not, as might have been expected, in a direction contrary to that in which it had at first revolved. The cause of this rotation I apprehend to be as follows:—The moisture, by causing the thread to contract, thereby tends to tighten the twist of the thread, and, as a necessary consequence, the thread tends to revolve in a direction contrary to that of the twist.

Spontaneous absorption of moisture from the atmosphere by different absorbents.

If an absorbent be thoroughly dried and then exposed to a humid atmosphere at or near to the dew-point, it will gradually absorb moisture from the air. The moisture thus absorbed by some substances is something considerable. Woollen cloth absorbs one-seventh of its own weight of moisture from air whose temperature is about one degree above that of the dew-point; bran about one-eighth of its weight; calico about one-tenth; and so on. The amount of absorption increases as the temperature of the air approaches that of the dew-point: the maximum quantity of moisture absorbed takes place in an atmosphere saturated with the vapour of water.

1. The rates of absorption by different absorbents presenting equal surfaces are proportional to their respective maximum quantities of absorption. Moreover, the moisture absorbed by two equal surfaces of the same material, but of different thicknesses, is proportional to their respective weights.

Thus two equal surfaces of black cloth and flannel, weighing respectively 400 grains and 300 grains, absorbed during the same time of exposure 41 and 32 grains respectively, and their maximum quantities of absorption were found to be 60 and 47 grains of moisture respectively. Here we have—ratio of weights $=\frac{300}{400}=\cdot75$; ratio of rates of absorption $=\frac{32}{41}=\cdot78$;

and ratio of maximum absorptions $= \frac{47}{60} = {\cdot}78$. Again, two equal surfaces of calico, but the one double the weight of the other, absorbed during the same time of exposure to the air, weights of moisture very nearly proportional to their respective weights.

2. The weights of moisture absorbed by an absorbent exposed to a humid atmosphere in equal successive intervals of time are (nearly) in geometrical progression.

Thus, for example, during successive intervals of thirty minutes, the weights of moisture absorbed by a piece of black cloth, presenting a surface of 150 square inches to the air, were found to be as follows:—

	Moisture absorbed, in grains.
During the 1st interval	17·4
,, 2nd ,,	$13{\cdot}5 = 17{\cdot}4 \times \frac{3}{4}$ nearly.
,, 3rd ,,	$10{\cdot}0 = 17{\cdot}4 \times (\frac{3}{4})^2$,,
,, 4th ,,	$7{\cdot}0 = 17{\cdot}4 \times (\frac{3}{4})^3$,,

Absorption of moisture by sulphuric acid from an atmosphere saturated with the vapour of water.

Equal measures of strong sulphuric acid diluted with different proportions of water were introduced into a beaker $2\frac{3}{4}$ inches in diameter, the liquid being half an inch from the edge of the glass; and the beaker with the acid was placed on a tray containing water, and covered over with a large receiver about 1 foot in diameter. The absorption by strong sulphuric acid, during twenty-four hours, being first determined, the strong acid was successively diluted with four equivalents of water, and the weights of moisture absorbed in twenty-four hours by the respective liquids were determined as follows:—

Dilution of the acid.	Corresponding weight of water absorbed in twenty-four hours, in grains. a.	Value of a from the formula $a=44{\cdot}8\times{\cdot}6k$.
0	44·8	44·8
4HO	26·6	26·8
8HO	16·0	16·1
12HO	9·2	9·6
16HO	6·0	5·8

where $k = \frac{n\text{HO}}{4\text{HO}}$. Here it will be observed that, *whilst the dilution advances according to an arithmetical progression, the* *capacity for absorption decreases according to a geometrical progression.*

It will be hereafter seen that a similar law applies to the solution of absorbent salts.

It was further determined that, *the strength of the acid being constant, the amount of absorption in equal times varies* (approximately) *inversely as the depth of the liquid from the edge of the vessel in which it is placed.*

Other things being the same, the rate of absorption increases with the temperature. It was also found that the rate of absorption, other things being the same, increases with the decrease of the atmospheric pressure.

Absorption of moisture by solutions of chloride of sodium from an atmosphere saturated with the vapour of water.

In this case 2824 grains of a saturated solution of the salt were successively diluted with 1000 grains of water, the diameter of the vessel exposed to the humid air of the receiver being 6 inches: thus the second solution contained 2824 grains of the saturated, or first solution, with the addition of 1000 grains of water; the third solution contained 3824 grains of the last solution, with the addition of 1000 grains of water; and so on to the other solutions. The results of experiment were as follows:—

In twenty-four hours, the first, second, third, and fourth solutions respectively absorbed 25, 15·4, 9·0, 6·0 and 4·1 grains of moisture.

Now these results are approximately expressed by the formula

$$a = 25 \times \cdot 6k,$$

where $k = \frac{1}{1000}$ of the weight of the water added to the saturated solution.

It will be seen that this law of absorption is similar to that determined for sulphuric acid. It appears, therefore, that *the rate of absorption has a determinate atomic relation.*

Spontaneous evaporation of moisture from different surfaces exposed to the atmosphere.

It has been shown in one of the foregoing papers, that the evaporation from absorbents saturated with moisture is for the most part uniform, the temperature and hygrometric state of the air being constant.

1. The rate of evaporation of moisture from damp porous substances, of the same material, is proportional to the extent of the surface presented to the air, without regard to the relative thicknesses of the substances.

Thus two pieces of calico, presenting 150 square inches of surface, but the one folded double whilst the other was single, were saturated with moisture and suspended in a quiescent atmosphere. After the lapse of two hours the single thickness of calico had lost 41·6 grains of moisture by evaporation, whilst the double thickness had lost 42 grains.

In this respect spontaneous evaporation differs entirely from spontaneous absorption, which, as we have seen, is dependent (within certain limits) upon the thickness or weight of the material, and not upon the extent of its surface.

2. The rate of evaporation from different substances mainly depends upon the roughness of, or inequalities on, their surfaces, the evaporation going on most rapidly from the roughest or most uneven surfaces; in fact, *the best radiators of heat are the best vaporizers of moisture.*

Woollen cloth, calico, unsized paper, bran, and fine sand, of equal surfaces, are very nearly the same as regards the rate at which moisture is evaporated from them. Calico is a better vaporizer than flannel, and very much better than water. The mean result of a considerable number of experiments performed in a quiescent atmosphere was, that the evaporation from the surface of still water is nearly four-fifths of the evaporation from an equal surface of calico.

3. The evaporation from equal surfaces composed of the same material is the same, or very nearly the same, in a quiescent atmosphere, *whatever may be the inclination of the surfaces.*

Equal pieces of woollen cloth, each containing 25 square inches, were cemented to thin tin plates, and after being saturated with moisture and weighed, one plate was placed horizontally with its damp face *upwards*, another plate was placed upon upright rods with its damp face *downwards*: in the same time the loss from evaporation in both cases was the same, or very nearly the same.

Thus, at a summer temperature, when the air was somewhat humid, the evaporation per hour from the damp surface placed *upwards* was 4·78 grains, whilst the evaporation per hour from the damp surface placed *downwards* was 4·71 grains; and when the air was unusually dry, the evaporation from the former was 6·6 grains, and from the latter 6·5 grains.

Again, at a winter temperature, the evaporation per hour from the former was 2·5 grains, whilst from the latter it was 2·46 grains.

The fact here enunciated is highly significant. It shows that vapour is carried into the air from a damp surface for the most part by the principle of *diffusion,* and not, as it is commonly supposed, by the force of an ascensional current of vapour. If the damp surface exposed to the air were higher in temperature than the surrounding air, then we should most certainly have an ascensional current of vapour, which would facilitate the process of evaporation; but this can only take place during continued sunshine: in other cases the temperature of the damp surface (owing to the cooling effect of evaporation) is from 1 to 6 degrees below that of the surrounding air; and under such circumstances, according to the results of these experiments, we can have little or no ascensional current of vapour tending to promote the process of evaporation.

The temperature and hygrometric state of the air being constant, the process of evaporation is very much accelerated by aërial currents and by direct sunshine.

4. The rate of evaporation from a damp surface is very much affected by the elevation at which the surface is placed above the ground.

About half an hour before sunset, with a clear sky and a calm air, the two tin plates, with their damp surfaces somewhat below saturation, were placed as follows: one about an inch above the grass, the other at an elevation of three feet, both damp surfaces being placed upwards. In the course of two hours the former had *gained* 7·5 grains of moisture by absorption, whilst the latter had *lost* 5 grains of moisture by evaporation. In another experiment, the temperature of the air being higher, the former had lost 12·7 grains by evaporation, whilst the latter had lost 21·2 grains by evaporation. Here the damp cloth at the surface of the ground was cooled down with the grass by radiation, whilst the upper cloth was nearly maintained at the temperature of the air at that elevation.

5. The rate of evaporation is affected by the radiation of surrounding bodies.

The plates, as described, were placed in the shade at the height of six inches above the ground, with their damp surfaces upwards, the sky being clear, but one plate had a screen placed over it. In the course of two hours the latter had lost 8 grains by evaporation, whereas the former had lost only 3·5 grains; that is, the evaporation from the surface with the screen was about double that from the surface without the screen. On another occasion the evaporation from the surface with the screen over it was found to be 32·6 grains, whilst from the other surface it was only 25 grains.

Hastings, February 16, 1862.

[To be continued.]

XLI. *Liquid Diffusion applied to Analysis.*
By Thomas Graham, *F.R.S., Master of the Mint.*

[Continued from p. 223.]

3. *Dialysis.*

PASSING from liquid diffusion in the water-jar, I may advert first to the diffusion of crystalloids through a gelatinous or colloid mass, the circumstances of the experiment being varied as little as possible from those of jar diffusion.

Ten grammes of chloride of sodium and 2 grammes of the Japanese gelatine, or gelose of Payen, were dissolved together in so much hot water as to form 100 cub. centims. of fluid. Introduced into the empty diffusion-jar and allowed to cool, this fluid set into a firm jelly, occupying the lower part of the jar, and containing of course 10 per cent. of chloride of sodium. Instead of placing pure water over this jelly, it was covered by 700 cub. centims. of a solution containing 2 per cent. of the same gelose, cooled so far as to be on the point of gelatinizing,—the jar at the same time being placed in a cooling mixture, in order to expedite that change. The jar with its contents was now left undisturbed for eight days at the temperature 10°. After the lapse of this time the jelly was removed from the jar in successive portions of 50 cub. centims. each from the top, and the proportion of chloride of sodium in the various strata ascertained. The results were very similar to those obtained in diffusing the same salt in a jar of pure water. The diffusion in the gelose appeared more advanced in eight days than diffusion in water for seven days, as will be seen by comparing the gelose experiment below with a water experiment on chloride of sodium, which had been conducted at nearly the same temperature (Table III.).

Table XI.—Diffusion of a 10 per cent. solution of Chloride of Sodium in the jelly of gelose, for eight days, at 10°.

No. of stratum.	Diffusate, in grammes.
1	·015
2	·015
3	·026
4	·035
5	·082
6	·130
7	·212
8	·350
9	·486
10	·630
11	·996
12	1·172
13	1·190
14	1·203
15 and 16	3·450
	9·992

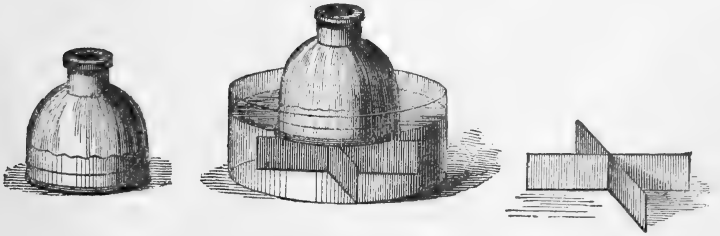
Diffusion of a crystalloid thus appears to proceed through a firm jelly with little or no abatement of velocity. With a coloured crystalloid, such as bichromate of potash, the gradual elevation of the salt to the top of the jar is beautifully illustrated. On the other hand, the diffusion of a coloured colloid such as caramel, through the jelly, appeared scarcely to have begun after eight days had elapsed. The diffusion of a salt into the solid jelly may be considered as cementation in its most active form.

Numerous experiments were made on the diffusion of crystalloids through various dialytic septa, such as gelatinous starch, coagulated albumen, gum-tragacanth, besides animal mucus and parchment-paper, which all tended to prove how little the diffusive process was interfered with by the intervention of colloid matter. Salts appeared to preserve their usual relative diffusibility unchanged. The same partial separation of mixed salts was observed as in the water-jar. With a mixture, for instance, of equal parts of chlorides of potassium and sodium in the dialyser, the first tenth part of the mixture which passed through was found to consist of 59·17 per cent. of chloride of potassium and 40·83 per cent. of chloride of sodium. Double salts also, such as alum, and the sulphate of copper and potash, which admit of being resolved into pairs of unequally diffusive salts, were largely decomposed upon the dialyser, as they are in the water-jar. The effect of heat in promoting diffusion appeared, however, to be diminished in dialysis, at least with a parchment-paper septum. Thus the diffusion from a 2 per cent. solution from chloride of sodium in a constant period of three hours was,—

		Ratio.
At 10°	0·738 grm.	1
At 20	0·794 grm.	1·07
At 30	0·892 grm.	1·20
At 40	1·017 grm.	1·37.

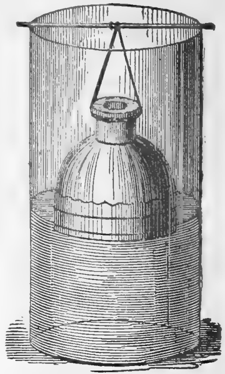
The rate of diffusion in water alone, without the septum, would have been doubled by an equal rise of temperature instead of being increased one-third only as above.

Fig. 3.—Bulb Dialyser.



The small glass bell-jar (figs. 3, 4) formerly used as an osmometer,

Fig. 4.



was conveniently applied to dialytic experiments. Two sizes of the bulb were employed, 3·14 and 4·44 inches in diameter respectively, and of which the dialytic septa possessed an area very nearly of $\frac{1}{100}$th and $\frac{1}{200}$th of a square metre (15·6 and 7·8 square inches). With 100 cub. centims. of fluid in the osmometer (the volume usually employed), the septum of the smaller instrument was covered to a depth of about 20 millimetres (0·8 inch), and the septum of the larger to a depth of 10 millimetres (0·4 inch). The thinner the stratum, the more exhaustive the diffusion in a given time. It is generally unadvisable to cover the septum deeper than 10 or 12 millimetres (half an inch), where a considerable diffusion is desired within twenty-four hours. The following practical observations may be found useful in applying the dialyser to actual cases of analysis. They refer to the parchment-paper septum, which is much the most convenient for use.

With a 2 per cent. solution of chloride of sodium, containing 2 grammes of the salt, and covering a septum of nearly 0·01 square metre (15·6 square inches) in area, to a depth of 10 millimetres, the salt which diffused in five hours amounted to 0·75 gramme, and in twenty-four hours to 1·657 gramme, leaving behind 0·343 gramme, or 17·1 per cent. of the original salt. The following experiments, made with the same osmometer and solution, show the effect of reducing the volume of liquid placed in the dialyser. The proportion of salt which diffused out in twenty-four hours was—

From 100 cub. centims. of solution 86 per cent.
From 50 cub. centims. of solution 92 per cent.
From 25 cub. centims. of solution 96 per cent.

In all cases the volume of water outside into which the salt escaped was ample, being from five to ten times as much as the volume of fluid placed in the dialyser, and it was changed during the continuance of the experiment. A much less volume of external water suffices, provided it is changed at intervals of a few hours. The temperature was 10° to 12°. It will be observed that these volumes correspond to a depth of liquid in the dialyser of 0·4, 0·2, and 0·1 inch respectively.

The time of travelling through the thickness of the parchment-paper itself may be observed, and is worthy of remark.

Of the quality of parchment-paper always used in these experiments, as quare metre, when dry, weighed 67 grammes, and when charged with water 108·6 grammes. Taking the specific gravity of cellulose at 1·46, that of the lighter woods, the parchment-paper described will, in the humid state, have a thickness of 0·0877 millimetre, or $\frac{1}{11\cdot41}$ of a millimetre. Wet parchment-paper so thin is highly translucent. Gelatinous starch, slightly coloured with blue litmus, was applied by a brush to one side of the wet parchment-paper. Immediately afterwards a drop of water, containing $\frac{1}{1000}$th part of *hydrochloric acid,* was applied on the point of the finger to the other (the lower) side of the paper. The time required by the acid to affect the litmus, in five successive trials, was 6 seconds, 5·5 seconds, 6 seconds, and 5 seconds. The mean is 5·7 seconds, which is therefore the time required by hydrochloric acid, diluted already 1000 times, to travel a distance of 0·0877 of a millimetre, by the agency of diffusion. The temperature was 15°.

With hydrochloric acid diluted twice as much as before (water containing 0·0005 dry acid), the average time of passage was 10·4 seconds, or nearly double the preceding time.

Water containing $\frac{1}{1000}$th of sulphuric acid (an acid less rapidly diffused than hydrochloric acid) reddened the litmus in 9·1 seconds, and when doubly diluted, in 16·5 seconds.

These results are not affected, it is believed, by any sensible diffusive movement on the part of the litmus. The diffusion of that colouring matter, in a colloid medium, is so slow that it may be entirely disregarded. The acid, therefore, is not met in its way by the litmus, but really travels the entire distance expressed by the thickness of the parchment-paper. The first experiments related give a diffusive velocity, in water, to hydrochloric acid, already diluted one thousand times, of 0·0154 millimetre per second, and 0·924 millimetre in one minute.

The few following dialytic experiments may be recorded for the sake of the practical points which they bring out. They were made in the smaller osmometer, with 100 cub. centims. of a solution containing 10 grammes of each of the various substances. The area of the parchment-paper septum was 0·005 square metre, and the depth of the stratum of fluid placed upon it 20 millimetres. The substances diffused were all crystalloids, with the exception of gum-arabic.

Table XII.—Dialysis through Parchment-paper during twenty-four hours, at 10° to 15°.

Ten per cent. solutions.	Diffusate, in grammes.	Relative diffusate.	Osmose, in grammes of water.	Relative osmose.
Gum-arabic	0·029	·004	5·0	·263
Starch-sugar	2·000	·266	17·0	·894
Cane-sugar	1·607	·214	15·3	·805
Milk-sugar	1·387	·185	15·0	·789
Mannite	2·621	·349	17·6	·926
Glycerine	3·300	·440	17·6	·926
Alcohol	3·570	·476	7·6	·400
Starch-sugar (second experiment)	2·130	·284	16·8	·884
Chloride of sodium	7·500	1	19·0	1

The experiments were all made through the same portion of parchment-paper, and in the order of the Table—gum-arabic first, and chloride of sodium last. After every experiment the bulb was immersed in water for twenty-four hours, to purify the septum, before it was again used. The diffusion of starch-sugar was repeated early and late in the series of experiments, with little change in the result, showing considerable uniformity in the action of the parchment-paper,—the first diffusate of starch-sugar being 2 grammes, and the second 2·13 grammes. Yet the parchment-paper had been in contact with water or some solution for a whole fortnight between the two observations referred to.

A layer of animal mucus, taken from the stomach of the pig, 12 millimetres in thickness (10 grammes of humid mucus being spread over 0·005 square metre of surface), was applied, between two discs of calico, to the diffusion-bulb used above, the parchment-paper being first removed.

TABLE XIII.—Dialysis through Animal Mucus during twenty-five hours, at 10° to 15°.

Ten per cent. solutions.	Diffusate, in grammes.	Proportional diffusate.	Osmose, in grammes of water.
Gum-arabic	·023	·004	+29
Starch-sugar	1·821	·360	+ 7·6
Cane-sugar	1·753	·347	+ 4·6
Milk-sugar................	1·328	·262	+ 7·1
Mannite................	1·895	·375	+ 5·0
Alcohol	2·900	·573	+ 7·2
Starch-sugar	1·765	·349	+ 7·0
Glycerine	2·554	·505	+ 7·5
Chloride of sodium	5·054	1	− 0·2

The relative diffusibilities of the different substances present a considerable degree of similarity in the two Tables, and are equally analogous to the diffusibilities of the same substances observed in pure water. The intervention of a colloid septum cannot be said to have impeded much the diffusion of any of these substances except the colloid gum.

The dialysis through parchment-paper of several other organic substances, both crystalloids and colloids, maybe brought together in comparison with the chloride of sodium as a standard. The larger osmometer bulb was used, and the parchment-paper was now changed in each experiment. The substance in solution amounted to 2 grammes, the depth of fluid in the dialyser to 10 millimetres (0·4 of an inch), and the surface of the septum to 0·01 square metre (15·6 square inches).

Table XIV.—Dialysis through Parchment-paper during twenty-four hours, at 12°.

Two per cent. solutions.	Diffusate, in grammes.	Proportional diffusate.
Chloride of sodium	1·657	1
Picric acid	1·690	1·020
Ammonia	1·404	·847
Theine	1·166	·703
Saiicine	·835	·503
Cane-sugar	·783	·472
Amygdaline	·517	·311
Extract of quercitron	·305	·184
Extract of logwood	·280	·168
Catechu	·265	·159
Extract of cochineal	·086	·051
Gallo-tannic acid	·050	·030
Extract of litmus	·033	·019
Purified caramel	·009	·005

Picric acid and theine were actually diffused from 1 per cent. solutions, and the numbers observed are multiplied by 2. The crystallizable principles, theine, salicine, and amygdaline, appear greatly more diffusible than gallo-tannic acid, or than gum, as has been already seen. Such inequality of rate is likely to facilitate the separation of vegetable principles by the agency of dialysis.

4. *Preparation of Colloid Substances by Dialysis.*

The purification of many colloid substances may be effected with great advantage by placing them on the dialyser. Accompanying crystalloids are eliminated, and the colloid is left behind in a state of purity. The purification of soluble colloids can rarely be effected by any other known means, and dialysis is evidently the appropriate mode of preparing such substances free from crystalloids.

Soluble Silicic Acid.—A solution of silica is obtained by pouring silicate of soda into diluted hydrochloric acid, the acid being maintained in large excess. But in addition to hydrochloric acid, such a solution contains chloride of sodium, a salt which causes the silica to gelatinize when the solution is heated, and otherwise modifies its properties. Now such soluble silica, placed for twenty-four hours in a dialyser of parchment-paper, to the usual depth of 10 millimetres, was found to lose in that time 5 per cent. of its silicic acid, and 86 per cent. of its hydrochloric acid. After four days on the dialyser, the liquid ceased to be disturbed by nitrate of silver. All the chlorides were gone, with no further loss of silica. In another experiment 112 grammes of silicate of soda, 67·2 grammes of dry hydrochloric acid, and 1000 cub. cents.

of water were brought together, and the solution placed upon a hoop dialyser, 10 inches in diameter. After four days the solution had increased to 1235 cub. centims. by the action of osmose, colloid bodies being generally highly osmotic. The solution now gave no precipitate with nitrate of silver, and contained 60·5 grammes of silica, 6·7 grammes of that substance having been lost. The solution contained 4·9 per cent. of silicic acid.

The pure solution of silicic acid so obtained may be boiled in a flask, and considerably concentrated, without change; but when heated in an open vessel, a ring of insoluble silica is apt to form round the margin of the liquid, and soon causes the whole to gelatinize. The pure solution of hydrated silicic acid is limpid and colourless, and not in the least degree viscous, even with 14 per cent. of silicic acid. The solution is the more durable the longer it has been dialysed and the purer it is. But this solution is not easily preserved beyond a few days, unless considerably diluted. It soon appears slightly opalescent, and after a time the whole becomes pectous somewhat rapidly, forming a solid jelly transparent and colourless, or slightly opalescent, and no longer soluble in water. This jelly undergoes a contraction after a few days, even in a close vessel, and pure water separates from it. The coagulation of the silicic acid is effected in a few minutes by a solution containing $\frac{1}{10,000}$th part of any alkaline or earthy carbonate, but not by caustic ammonia, nor by neutral or acid salts. Sulphuric, nitric, and acetic acids do not coagulate silicic acid; but a few bubbles of carbonic acid passed through the solution produce that effect after the lapse of a certain time. Alcohol and sugar, in large quantity even, do not act as precipitants; but neither do they protect silicic acid from the action of alkaline carbonates, nor from the effect of time in pectizing the fluid colloid. Hydrochloric acid gives stability to the solution: so does a small addition of caustic potash or soda.

This pure water-glass is precipitated on the surface of a calcareous stone without penetrating, apparently from the coagulating action of soluble lime-salts. The hydrated silicic acid then forms a varnish, which is apt to scale off on drying. The solution of hydrated silicic acid has an acid reaction, somewhat greater than that of carbonic acid. It appears to be really tasteless (like most colloids), although it occasions a disagreeable persistent sensation in the mouth, after a time, probably from precipitation.

Soluble hydrated silicic acid, when dried in the air-pump receiver, at 15°, formed a transparent glassy mass of great lustre, which was no longer soluble in water. It retained 21·99 per cent. of water after being kept two days over sulphuric acid.

The colloidal solution of silicic acid is precipitated by certain other soluble colloids, such as gelatine, alumina, and peroxide of iron, but not by gum or caramel. As hydrated silicic acid, after once gelatinizing, cannot be made soluble again by either water or acids, it appears necessary to admit the existence of two allotropic modifications of that substance, namely, soluble hydrated silicic acid, and insoluble hydrated silicic acid, the fluid and pectous forms of this colloid.

The ordinary soluble silicate of soda is not at all colloidal, but diffuses as readily through a septum as the sulphate of soda does. Several crystalline hydrated silicates of soda are known (Fritzsche).

The amorphous silicic acid obtained by drying and calcining the jelly, and the vitreous acid obtained by igneous fusion, have both a specific gravity of about 2·2, according to H. Rose*, and appear to be the same colloidal substance; while the specific gravity of crystalloidal silicic acid (rock-crystal and quartz) is about 2·6.

Soluble silicic acid forms a peculiar class of compounds, which, like itself, are colloidal, and differ entirely from the ordinary silicates. The new compounds are interesting from their analogy to organic substances, and from appearing to contain an acid of greatly higher atomic weight than ordinary silicic acid. Like gallo-tannic acid, gummic acid, and the other organic colloidal acids, silicic acid combines with gelatine—the last substance appearing to possess basic properties. *Silicate of gelatine* falls as a flaky, white and opake precipitate when the solution of silicic acid is gradually added to a solution of gelatine in excess. The precipitate is insoluble in water, and is not decomposed by washing. Silicate of gelatine prepared in the manner described, contains 100 silicic acid to about 92 gelatine. This is a greater proportion of gelatine than in the gallo-tannate of gelatine, and requires for soluble silicic acid a higher equivalent than that of gallo-tannic acid. In the humid state the gelatine of this compound does not putrefy.

The acid reaction of 100 parts of soluble silicic acid is neutralized by 1·85 part of oxide of potassium, and by corresponding proportions of soda and ammonia. The *colli*-silicates or *co*-silicates thus formed are soluble and more durable than fluid silicic acid, but they are pectized by carbonic acid, or by an alkaline carbonate, after standing for a few minutes. The co-silicate of potash forms a transparent hydrated film on drying *in vacuo*, which is not decomposed by water, and appears to require about ten thousand parts of water to dissolve it. The silicate of soda which Forchhammer obtained by boiling freshly precipitated silicic acid with carbonate of soda, and collecting the precipitate which falls on cooling, contains 2·74 per cent. of soda, and is

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* Phil. Mag. S. 4. vol. xix. p. 32.

represented by $NaO + 36SiO^2$ (Gmelin). This silicate is probably a co-silicate of soda in the pectous condition. Soluble silicic acid produces a gelatinous precipitate in lime-water, containing 6 per cent. and upwards of the basic earth. This and the other insoluble earthy co-silicates appear not to be easily obtained in a definite state. They gave out a more basic silicate to water on washing. The composition of these salts and that also of the co-silicate of gelatine were found to vary according as the mode of preparation was modified. When a solution of gelatine was poured into silicic acid in excess, the co-silicate of gelatine formed gave, upon analysis, 100 silicic acid with 56 gelatine, or little more than half the gelatine stated above as found in that compound prepared with the mode of mixing the solutions reversed. The gallo-tannate of gelatine is known to offer the same variability in composition.

The gelatine used in the preceding experiments was isinglass (colle de poisson) purified by solution in hydrochloric acid and subsequent dialysis. As the acid escapes by diffusion, a jelly is formed in the dialyser. This jelly is free from the earthy matter, amounting to about 0·4 per cent., in isinglass, and is not liable to putrefaction.

Cosilicic acid also precipitates both *albuminic acid* and pure *caseine.*

Soluble Alumina.—We are indebted to Mr. Walter Crum for the interesting discovery that alumina may be held in solution by water alone in the absence of any acid. But two soluble modifications of alumina appear to exist, alumina and metalumina. The latter is Mr. Crum's substance.

A solution of the neutral chloride of aluminium ($Al^2 Cl^3$), placed on the dialyser, appears to diffuse away without decomposition. But when an excess of hydrated alumina is previously dissolved in the chloride, the latter salt is found to escape by diffusion in a gradual manner, and the hydrated alumina, retaining little or no acid, to remain behind in a soluble state. A solution of alumina in chloride of aluminium, consisting at first of 52 parts of alumina to 48 of hydrochloric acid, after a dialysis of six days, contained 66·5 per cent. of alumina; after eleven days 76·5 per cent.; after seventeen days 92·4 per cent.; and after twenty-five days the alumina appeared to be as nearly as possible free from acid, as traces only of hydrochloric acid were indicated by an acid solution of nitrate of silver. But in such experiments the alumina often pectizes in the dialyser before the hydrochloric acid has entirely escaped.

Acetate of alumina with an excess of alumina gave similar results. The alumina remained fluid in the dialyser for twenty-one days, and when it pectized was found to retain 3·4 per cent.

of acetic acid, which is in the proportion of 1 equivalent of acid to 28·2 equivalents of alumina.

Soluble alumina is one of the most unstable of substances—a circumstance which fully accounts for the difficulty of preparing it in a state of purity. It is coagulated or pectized by portions, so minute as to be scarcely appreciable, of sulphate of potash and, I believe, by all other salts; and also by ammonia. A solution containing 2 or 3 per cent. of alumina was coagulated by a few drops of well-water, and could not be transferred from one glass to another, unless the glass was repeatedly washed out by distilled water, without gelatinizing. Acids in small quantity also cause coagulation; but the precipitated alumina readily dissolves in an excess of the acid. The colloids gum and caramel also act as precipitants.

This alumina is a mordant, and possesses indeed all the properties of the base of alum and the ordinary aluminous salts. A solution containing 0·5 per cent. of alumina may be boiled without gelatinizing, but when concentrated to half its bulk it suddenly coagulated. Soluble alumina gelatinizes when placed upon red litmus-paper, and forms a faint blue ring about the drop, showing a feeble alkaline reaction. Soluble alumina is not precipitated by alcohol nor by sugar. No pure solution of alumina, although dilute, remained fluid for more than a few days.

Like hydrated silicic acid, then, the colloid alumina may exist either fluid or pectous, or it has a soluble and insoluble form, the latter being the gelatinous alumina as precipitated by bases. It is evident that the extraordinary coagulating action of salts upon hydrated alumina must prevent the latter substance from ever appearing in a soluble state when liberated from combination by means of a base.

Colloidal alumina possesses also, I believe, a high atomic weight, like cosilicic acid. The chloride of aluminium with excess of alumina referred to above appears to be, either in whole or in part, a colloidal hydrochlorate of alumina, containing the latter substance with its large colloidal equivalent, and may be really neutral in composition. The soluble basic persalts of iron, tin, &c. are likewise all colloidal, and have no doubt a similar constitution. Such colloidal salts are themselves slowly decomposed on the dialyser, being resolved into the crystalloidal acid which escapes and the colloidal oxide which remains behind.

Soluble Metalumina.—Mr. Crum first pointed out a singular relation of acetic acid to alumina, which has never been explained. Sulphate of alumina, when precipitated by acetate of lead or baryta, gives a binacetate of alumina, with one equivalent of free acetic acid—the neutral teracetate of alumina not appearing to exist. It was further observed that, by keeping a solution of this binacetate in a close vessel at the boiling-point of water for several days, nearly the whole acetic acid came to be liberated, without any precipitation of alumina occurring at the same time. Mr. Crum boiled off the free acetic acid, or the greater part of it, and thus obtained his soluble alumina. The same result may be arrived at by dialysing a solution of acetate of alumina that has been altered by heat. In three days the acetic acid was reduced on the dialyser to 11 per cent., giving 1 equiv. acetic acid to 8 equivs. alumina; in six days to 7·17 per cent. acid; in thirteen days to 2·8 per cent. acid, or 1 equiv. acid to 33 equivs. alumina. The alumina exists in an allotropic condition, being no longer a mordant, and forming, when precipitated, a jelly that is not dissolved by an excess of acid. Metalumina resembles alumina in being coagulated by minute proportions of acids, bases, and of most salts. Mr. Crum found the solution of metalumina to require larger quantities of acetates, nitrates, and chlorides to produce coagulation than of the former substances. The solution of metalumina is tasteless, and entirely neutral to test-paper, according to my own observation.

Like alumina, the present colloid has therefore a fluid and a pectous form—the liquid soluble metalumina, and the gelatinous insoluble metalumina.

Soluble Peroxide of Iron.—A solution of hydrated peroxide of iron may be obtained by a process exactly analogous to that for soluble alumina. Perchloride of iron in solution is first saturated with hydrated peroxide of iron, added by small quantities at a time; or carbonate of ammonia may be added in a gradual manner to perchloride of iron, so long as the precipitated oxide continues to be redissolved on stirring. These red solutions of iron have lately been carefully investigated by Mr. Ordway (Silliman's Journal, 3 ser. vol. xxxix. p. 197), by M. Béchamp (*Annales de Chimie,* 3 sér. vol. lvii. p. 293), and by M. Scheurer-Kestner (ib. vol. lv. p. 330). It is observed that the act of solution of the hydrated peroxide by the chloride of iron is a gradual process, demanding time. The quantity of oxide taken up will go on increasing for a long time, if digestion in the cold is continued. Mr. Ordway found chloride of iron to take up so much as 18 equivalents of peroxide of iron in the course of five months. This slowness of action is highly characteristic of colloids. Only monobasic acids, such as hydrochloric and nitric, serve for preparing such solutions,—sulphuric and other polybasic acids giving insoluble subsalts with excess of ferric oxide, or of any other aluminous oxide. The red liquid so obtained is already a colloidal hydrochlorate of peroxide of iron, but requires to be dialysed for a sufficient time. Such a compound possesses an element of instability in the extremely unequal diffusibility of its constituents. Beginning with perchloride of iron, containing five or six equivalents of peroxide in solution, the whole solid matter also amounting to 4 or 5 per cent. of the liquid, and the latter forming a stratum of the usual depth of about half an inch in the dialyser, it was found that hydrochloric acid diffused out accompanied only by a small proportion of the iron. After eight days, the deep-red solution in the dialyser contained peroxide of iron and hydrochloric acid, in the proportion of 97·6 per cent. of the former to 2·4 per cent. of the latter. In nineteen days the hydrochloric acid was reduced to 1·5 per cent., which gives 1 equiv. of acid to 30·3 equivs. peroxide of iron. The last solution was transferred to a phial, in which it remained fluid for twenty days, and then spontaneously pectized.

The peracetate of iron, prepared by double decomposition, is incapable of dissolving hydrated peroxide of iron, as is well known, but still may be made a source of soluble peroxide, as the salt referred to is itself decomposed to a great extent by diffusion on the dialyser. About one-half of the iron was lost by a diffusion of eighteen days, in a particular experiment, leaving on the dialyser a red liquid, in which ninety-four parts of peroxide of iron were still associated with six parts of acetic acid.

Water containing about 1 per cent. of hydrated peroxide of iron in solution has the dark-red colour of venous blood. The solution may be concentrated by boiling to a certain point, and then pectizes. The red solution is coagulated in the cold by traces of sulphuric acid, alkalies, alkaline carbonates, sulphates and neutral salts in general, but not by hydrochloric, nitric, and acetic acids, nor by alcohol or sugar. The coagulum is a deep-red-coloured jelly, resembling the clot of blood, but more transparent. Indeed the coagulation of this colloid is highly suggestive of that of blood, from the feeble agencies which suffice to effect the change in question, as well as from the appearance of the product. The coagulum formed by a precipitant, or, in the course of time, without any addition having been made to the solution of peroxide of iron, is no longer soluble in water, hot or cold; but it yields readily to dilute acids. It is, in short, the ordinary hydrated peroxide of iron. Here then, again, we have a soluble and insoluble form of the same colloidal substance. Native hematite, which presents itself in mammillary concretions, is no doubt colloidal.

Soluble Metaperoxide of Iron.—The soluble peroxide of iron of M. Péan de Saint-Gilles* appears to be the analogue of metalumina. It was also prepared by the prolonged action of heat upon a pure solution of the acetate. The characteristic

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* *Comptes Rendus,* 1855, p. 568.

properties of this substance, which indicate its allotropic nature, are the orange-red colour and the opalescent appearance of its solution. The metaperoxide of iron is entirely precipitated, of a brown ochreous appearance, by a trace of sulphuric acid or of an alkaline salt, and is insoluble in all cold acids, even when the latter are concentrated. The solubility of metaperoxide of iron in water appears to be more precarious, if possible, than that of the colloid alumina. It would no doubt be more safely prepared by diffusing away the acetic acid of the altered acetate of iron, than it is by boiling off that acid, as the solution is said to become precipitable by heat before the whole acetic acid is expelled.

Ferrocyanide of Copper.—Many of the insoluble ferrocyanides are crystalline precipitates; but the compound above named, and the different varieties of prussian blue, appear to be strictly colloidal.

Certain anomalous properties long observed in these compounds come thus to be explained. The ferrocyanide of copper precipitated from ferrocyanide of potassium and sulphate of copper, is a reddish-brown gelatinous precipitate, and carries down a portion of the potash salt. It is obtained of greater purity, like the other insoluble ferrocyanides, by the use of ferrocyanic acid as the precipitant. Ferrocyanide of copper is then darker in colour, and still more highly gelatinous. It is well known that this substance appears as a transparent almost colourless jelly when precipitated from strong solutions. This colloidal matter assumes colour on the addition of water, in consequence of further hydration, following in this respect the analogy of the crystalloid salts of copper. The ferrocyanide of copper, when once precipitated, may be washed without loss, and exhibits no symptoms of solubility. But it has been remarked that the same salt, when produced by mixing the precipitating salts dissolved in not less than two or three thousand times their weight of water, gives a wine-red solution with no precipitate. This is the soluble condition of the colloid. When the red solution is placed in the dialyser, the salt of potash diffuses out and the whole ferrocyanide of copper is retained behind in solution.

Precipitated ferrocyanide of copper is not dissolved by oxalic acid, nor by oxalate of potash, but dissolves freely in about one-fourth of its weight of neutral oxalate of ammonia. The ferrocyanide of copper must be washed beforehand, to ensure solubility. A solution holding 3 or 4 per cent. of ferrocyanide of copper is of a dark reddish-brown colour, intermediate in tint between the acetate and meconate of iron. The solution is transparent, but assumes a peculiar appearance of opacity when seen by light reflected from its surface. The same appearance was observed by Péan de Saint-Gilles in his metaperoxide of iron.

When a red solution, such as that described, was dialysed, the oxalate of ammonia came away in a gradual manner; 30·6 per cent. of the oxalate of ammonia were found in the colourless diffusate of the first twenty-four hours, 31 per cent. of the same salt in the diffusate of the next three days, and 18·2 per cent. in the diffusates of the following seven days, making altogether 79·8 per cent., or four-fifths, of the whole oxalate of ammonia originally introduced. A small portion of the ammoniacal salt is retained with force, as might be expected from a ferrocyanide. Although the diffusate appeared colourless, it was found to contain a little oxide of copper, namely, 0·041 gramme (of which 0·022 gramme diffused out in the first twenty-four hours), from 2 grammes of ferrocyanide of copper placed in the dialyser.

The liquid ferrocyanide of copper, both before and after being dialysed, may be heated without change, but it is pectized by foreign substances with extreme facility. This effect is produced by a minute addition of nitric, hydrochloric, and sulphuric acids in the cold, and of oxalic and tartaric acids with the aid of a slight heat. It is remarkable that acetic acid does not pectize the ferrocyanide of copper and many other colloids. Sulphate of potash, sulphate of copper, and metallic salts generally appear to pectize the red liquid. The oxalate of ammonia, if any is present, remains in solution.

Neutral Prussian Blue.—The blue precipitate from perchloride of iron and ferrocyanide of potassium, or ferrocyanic acid, is a bulky hydrate, which dries up into gummy masses, so far resembling a colloid. The precipitate dissolves readily with the aid of a gentle heat, in one-sixth of its weight of oxalic acid, giving the well-known solution of prussian blue, used as an ink. Prussian blue is equally soluble in the oxalate and binoxalate of potash. When the solution of prussian blue in oxalic acid was placed on the dialyser, no colouring matter came through, but 28 per cent. of the oxalic acid diffused away in the first twenty-four hours, accompanied by traces of peroxide of iron. The oxalic acid appears to leave the colloidal solution very slowly and incompletely, 8 per cent. diffusing away in the second twenty-four hours, 11 per cent. in the next four days, and 2 per cent. in the following six days. The colloidal solution of prussian blue was pectized by small additions of sulphate of zinc and several other metallic salts, but required larger quantities of the alkaline salts for precipitation.

Ferridcyanide of Iron.—The blue precipitate from the ferridcyanide of potassium and a protosalt of iron is soluble in oxalic acid and the binoxalate of potash, but not in the neutral oxalates.

This blue liquid is quite incapable of passing through the dialyser, and is equally colloidal with ordinary prussian blue. So also is *basic prussian blue* prepared by the spontaneous oxidation of precipitated ferrocyanide of protoxide of iron. This last colloid might probably be purified with advantage upon the dialyser.

The ammonio-tartrate of iron, ammonio-citrate of iron, and similar pharmaceutical preparations are chiefly colloidal matters.

Sucrate of Copper.—The deep-blue liquid obtained by adding potash to a mixed solution of chloride of copper and sugar appears to contain a colloidal substance. Placed on a dialyser for four days, the blue liquid became green, and no longer contained either potassium or chlorine; it in fact consisted of oxide of copper united with twice its weight of sugar. The external liquid remained colourless, and gave no indication of copper when tested with sulphuretted hydrogen. The colloidal solution of sucrate of copper was sensitive in the extreme to pectizing agents. Salts and acids generally gave a bluish-green precipitate; even acetic acid had that effect. The precipitate, or pectous sucrate, after being well-washed, consisted of oxide of copper with about half its weight of sugar, and is therefore a subsucrate. When the green liquid is heated strongly, it gives a bluish-green precipitate, and does not allow the copper to be readily reduced to the state of suboxide. The subsucrate of copper possesses considerable vivacity of colour, and might be used as a pigment. A solution of sucrate of copper absorbs carbonic acid from the air with great avidity.

The sucrate of copper dries up into transparent films of an emerald-green colour. These films are not altered in appearance or dissolved in cold or boiling alcohol. In water they are resolved into sugar and the pectous subsucrate of copper.

Sucrate of Peroxide of Iron.—The perchloride of iron with an addition of sugar is not precipitated by potash, provided the temperature is not allowed to rise. The peroxide of iron combined with the sugar is colloidal, and remains on the dialyser without loss. At a certain stage, however, the sugar appears to leave the peroxide of iron, and a gelatinous subsucrate of iron pectizes. The subsucrate of iron thrown down from the soluble sucrate by the addition of sulphate of potash consisted of about 22 parts of sugar to 78 parts of peroxide of iron.

Sucrate of Peroxide of Uranium.—A similar solution may be obtained by adding potash to a mixture of the nitrate or chloride of uranium with sugar, avoiding heat. The solution is of a deep orange-yellow colour, and on the dialyser soon loses the whole of its acid and alkali. This fluid sucrate has considerable stability, but is readily pectized by salts, like the sucrate of copper. The subsucrate pectized has considerable solubility in pure water.

Sucrate of Lime.—The well-known solution of lime in sugar forms a solid coagulum when heated. It is probably, at a high temperature, entirely colloidal. The solution obtained on cooling passes through the septum, but requires a much longer time than a true crystalloid like the chloride of calcium.

The blue solution of tartrate of copper in caustic potash contains a colloidal compound, which has not been fully examined.

Soluble Chromic Oxide.—The definite terchloride of chromium, being a crystalloid, diffuses away entirely when placed in solution upon the dialyser. This salt dissolves, with time, a certain portion of freshly-precipitated hydrated chromic oxide, and becomes of a deeper green colour. Such a solution, after dialysis for twenty-two days, contained 8 hydrochloric acid to 92 chromic oxide; and after thirty-days, 4·3 acid to 95·7 oxide, or 1 equiv. acid to 10·6 equivs. oxide. After thirty-eight days, the solution gelatinized in part upon the dialyser, and then contained 1·5 acid to 98·5 oxide, or 1 equiv. acid to 31·2 equivs. chromic oxide. This last solution, which may be taken to represent soluble chromic oxide, is of a dark-green colour, and admits of being heated, and also of being diluted with pure water without change. It was gelatinized with the usual facility by traces of salts and other reagents which affect colloid solutions, and was then no longer soluble in water, even with the assistance of heat. It appeared to be the green hydrated oxide of chromium as that substance is usually known. A metachromic oxide may possibly be obtained by heating and dialysing the acetate, but I have not attempted to form it.

Mr. Ordway succeeded in dissolving an excess of the hydrated *uranic oxide* and of *glucina* in the chloride of uranium and of glucinum respectively. The dialysis of such solutions may be reasonably expected to yield soluble uranic oxide and soluble glucina.

It appears, then, that the hydrated peroxides of the aluminous type, when free, are colloid bodies; that two species of each of these hydrated oxides exist, of which alumina and metalumina are the types—one derived from an unchanged salt, and the other from the heated acetate of the base; further, that each of these species has two forms—one soluble, and the other insoluble or coagulated. This last species of duality should be well distinguished from the preceding allotropic variability of the same peroxide. The possession of a soluble and an insoluble (fluid and pectous) modification is not confined to hydrated silicic acid and the aluminous oxides, but appears to be very general, if not universal, among colloid substances. The double form is typified in the fibrine of blood.

The precipitated and gelatinous *peroxide of tin* is largely soluble in the bichloride of the same metal. Such a solution, when placed in the dialyser, allows the whole chlorine of the salt and a portion of the tin to diffuse away. Peroxide of tin, or stannic acid, remains behind, but not in a soluble state. It forms in the dialyser a semitransparent gelatinous cake, which after a few days is entirely free from chlorine. The original solution, containing excess of stannic acid, was diluted to various degrees, but was dialysed always with the same result. The coagulum was insoluble in hot or cold water, but dissolved readily in dilute acids. It was evidently the peroxide of tin unaltered.

The *metastannic acid,* or nitric-acid peroxide of tin of Berzelius, forms a solid compound with a small quantity of hydrochloric acid. This compound is not dissolved by an excess of acid, but is soluble in pure water. The solution placed in the dialyser is readily decomposed, and leaves behind a semitransparent gelatinous mass of pure hydrated metastannic acid, insoluble both in water and acids. There appears, then, to be no soluble form of either hydrated stannic or metastannic acid, although both are colloidal substances.

Precipitated *titanic acid* was dissolved in hydrochloric acid and submitted to dialysis. The hydrochloric acid readily diffused away, leaving hydrated titanic acid, gelatinous and insoluble, upon the dialyser. The proportion of titanic acid which escaped from the dialyser and was lost amounted to 0·050 gramme out of 2·5 grammes. Titanic acid thus resembles stannic acid in not presenting itself in the form of a fluid colloid.

Metallic protoxides are not soluble in their neutral salts, and therefore cannot be submitted to dialysis in the same conditions as the preceding peroxides. It was observed, however, that oxide of copper and oxide of zinc, when dissolved in ammonia, are capable of diffusing through a colloidal septum, and are therefore not colloids themselves. The water outside the dialyser should be charged with ammonia in such an experiment.

[To be continued.]

XLII. *On Projections for Maps applying to a very large extent of the Earth's Surface. By* Colonel Sir HENRY JAMES, *R.E., Director of the Ordnance Survey; and* Captain ALEXANDER R. CLARKE, *R.E.*[*]

[With a Plate.]

IN reading the "Explanation of a Projection by Balance of Errors for Maps applying to a very large extent of the Earth's Surface, and comparison of this projection with other

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[*] Communicated by the Authors.

projections," by G. B. Airy, Esq., Astronomer Royal, which appeared in the Philosophical Magazine for December 1861, and in examining the numerical results given in the Tables in which the relative advantages of the Projection by Balance of Errors, by Equal Radial Degrees, by Unchanged Areas, and in the Stereographic (attributed to Hipparchus), and my Projection of two-thirds of the sphere, I was struck with the fact that the numbers given as representing the Radial distances from the centre of the Map, the Exaggeration of the projected areas, and the Distortion of the form, did not show such advantages in favour of the Projection by Balance of Errors as I was naturally led to expect from the ingenious method employed by Mr. Airy for obtaining them.

I therefore requested Captain Alexander R. Clarke, R.E., to go through the mathematical process employed by Mr. Airy, and examine the numerical results given in the Tables. I subjoin the result of Captain Clarke's examination; and it will be observed that, from a mistake inadvertently made in one of the constants, the projection by Balance of Errors has greater advantages than Mr. Airy has given it in his Tables.

The fundamental equation of this very beautiful method of development is readily obtained in the following manner. Let P be the point on the sphere which is to be the centre of the map, and let Q be any other point on the sphere such that the arc $PQ=\theta$; if Q′ be the representation of Q on the Development, $PQ'=r$. Suppose a very small circle, radius ω, described on the sphere having its centre at Q, then the representation of this circle in the map will be an ellipse having its minor axis in the line PQ′ and its centre at Q′. The lengths of the semi-axes will be

$$\omega\frac{dr}{d\theta}, \quad r\frac{\omega}{\sin\theta}.$$

The differences between these quantities and that (ω) which they represent are

$$\omega\left(\frac{dr}{d\theta}-1\right), \quad \omega\left(\frac{r}{\sin\theta}-1\right);$$

and the sum of the squares of these errors is the measure of the misrepresentation at Q′. The sum for the whole surface from $\theta=0$ to $\theta=\beta$ is proportional to

$$\int_0^\beta \left\{\left(\frac{dr}{d\theta}-1\right)^2+\left(\frac{r}{\sin\theta}-1\right)^2\right\}\sin\theta\,d\theta,$$

which is to be a minimum.

Putting $r-\theta=y$, and giving to y only a variation subject to the condition $\delta y=0$ when $\theta=0$, the equations of solution are

$$\mathrm{N}-\frac{d(\mathrm{P})}{d\theta}=0; \quad \mathrm{P}_\beta=0,$$

P_β being the value of $2p\sin\theta$ when $\theta=\beta$; hence

$$\sin^2\theta\frac{d^2y}{d\theta^2}+\sin\theta\cos\theta\frac{dy}{d\theta}-y=\theta-\sin\theta, \quad . \quad . \quad (1)$$

$$\left(\frac{dy}{d\theta}\right)_\beta=0; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

from which

$$y=-\theta-2\cot\frac{\theta}{2}\log\cos\frac{\theta}{2}+\mathrm{C}\tan\frac{\theta}{2}+\mathrm{C}'\cot\frac{\theta}{2}, \quad . \quad (1)$$

$$0=\operatorname{cosec}^2\frac{\beta}{2}\log\cos\frac{\beta}{2}+\frac{1}{2}\mathrm{C}\sec^2\frac{\beta}{2}-\frac{1}{2}\mathrm{C}'\operatorname{cosec}^2\frac{\beta}{2}. (2)$$

Now y must vanish with θ; therefore $\mathrm{C}'=0$, and, from (2),

$$\mathrm{C}=\cot^2\frac{\beta}{2}\log\sec^2\frac{\beta}{2},$$

which completely determines r. At the centre of the map, where $\theta=0$,

$$\frac{dr}{d\theta}=\frac{1+\mathrm{C}}{2}.$$

This quantity in the Astronomer Royal's paper is inadvertently made $=1$, and consequently the computed Tables, pp. 415, 416, 417, are incorrect, and the Development appears under disadvantage. The limiting radius of the map is

$$\mathrm{R}=2\mathrm{C}\tan\frac{\beta}{2}.$$

This quantity does not increase indefinitely, but is a maximum when $\beta=126^\circ\ 24'\ 53''$: for higher values of β, R diminishes. When $\beta=120^\circ$, $\mathrm{R}=1{\cdot}6007$. When $\beta=113^\circ\ 30'$, which is the limit of Sir H. James's map, $\mathrm{R}=1{\cdot}5760$.

Let us now compare this Development with the Equal Radial and Sir Henry James's Projection. In order to do this, we must suppose these to be drawn on such a scale that the limiting radius shall be 1·5760, and then form the values of

$$\left(\frac{dr}{d\theta}-1\right)^2+\left(\frac{r}{\sin\theta}-1\right)^2.$$

The values of this quantity, which call U, are given in the following Table:—

θ.	Balance of Errors.		Sir H. James's.		Equal Radial.		θ.
	U.	U sin θ.	U.	U sin θ.	U.	U sin θ.	
0°	·1168	·0000	·1182	·0000	·0836	·0000	0°
5	·1164	·0101	·1172	·0102	·0832	·0072	5
10	·1149	·0200	·1147	·0199	·0819	·0142	10
15	·1126	·0291	·1106	·0286	·0799	·0207	15
20	·1093	·0374	·1050	·0359	·0771	·0264	20
25	·1052	·0444	·0980	·0414	·0737	·0311	25
30	·1002	·0501	·0897	·0448	·0696	·0348	30
35	·0945	·0542	·0804	·0461	·0651	·0373	35
40	·0881	·0566	·0704	·0453	·0603	·0387	40
45	·0812	·0574	·0601	·0425	·0553	·0391	45
50	·0740	·0567	·0499	·0382	·0506	·0387	50
55	·0667	·0547	·0404	·0331	·0464	·0380	55
60	·0598	·0518	·0325	·0281	·0432	·0374	60
65	·0536	·0486	·0270	·0244	·0418	·0379	65
70	·0491	·0461	·0251	·0236	·0430	·0404	70
75	·0471	·0455	·0285	·0275	·0479	·0463	75
80	·0492	·0485	·0391	·0385	·0582	·0573	80
85	·0575	·0573	·0597	·0594	·0759	·0756	85
90	·0752	·0752	·0937	·0937	·1041	·1041	90
95	·1068	·1064	·1461	·1456	·1469	·1463	95
100	·1593	·1569	·2240	·2206	·2099	·2067	100
105	·2412	·2359	·3371	·3256	·3013	·2910	105
110	·3784	·3552	·5002	·4700	·4329	·4068	110
115	·5904	·5352	·7350	·6661	·6223	·5640	114

From these quantities we may infer the values of

$$\int_0^\beta \mathrm{U} \sin\theta \,.\, d\theta = \mathrm{M}$$

for the different systems; they are

Sir H. James's . . .	M=0·1718,	or as 109·5
Equal Radial	=0·1643,	,, 104·7
Balance of Errors . .	=0·1569,	,, 100·0

By inspection of the Table it will be seen that from 0° to 45° the Equal-Radial Projection has the advantage; from 50° to 80° Sir H. James's Projection has the advantage; from 80° upwards the Balance of Errors has the advantage.

The first of these projections may, however, be greatly improved. The general expression for the radius, namely

$$r = \frac{k \sin\theta}{h + \cos\theta},$$

involves two arbitrary quantities, and we may so dispose of them as to render the integral M an absolute minimum; that is,

$$\int_0^\beta \left\{ \left(\frac{k}{h + \cos\theta} - 1 \right)^2 + \left(\frac{k(h\cos\theta + 1)}{(h + \cos\theta)^2} - 1 \right)^2 \right\} \sin\theta \,.\, d\theta$$

must be a minimum with respect to h and k. Effecting the integration, we get

$$M = k^2 H_1 + 2kH_2 + 4\sin^2\frac{\beta}{2},$$

where the symbols H_1, H_2 are

$$H_1 = \frac{1+h^2}{N} - \frac{h(h^2-1)}{N^2} + \frac{(h^2-1)^2}{3N^3} - \frac{1}{3}\cdot\frac{(1-h)^2}{1+h} - 1,$$

$$H_2 = (1+h)\log\frac{N}{1+h} + \frac{h^2-1}{N} - h + 1,$$

and $N = h + \cos\beta$. Now

$$\frac{dM}{dk} = 0; \quad \therefore kH_1 + H_2 = 0;$$

$$\frac{dM}{dh} = 0; \quad \therefore k\frac{dH_1}{dh} + 2\frac{dH_2}{dh} = 0;$$

$$\therefore k = -\frac{H_2}{H_1}; \ H_2\frac{dH_1}{dh} - 2H_1\frac{dH_2}{dh} = 0;$$

hence

$$M = 4\sin^2\frac{\beta}{2} - \frac{H_2^2}{H_1};$$

and h must be determined so that

$$\frac{H_2^2}{H_1} = \text{maximum}.$$

This is most easily determined by calculating the values corresponding to assumed values of h. We have the following:—

h.	$\log H^2_2 - \log H_1$.
1·35	0·420732
1·36	0·420756
1·37	0·420762
1·38	0·420747
1·39	0·420665

By interpolation the maximum is found to be

h.	$\log H^2_2 - \log H_1$.
1·36763	0·4207623

$$\therefore \frac{H^2_2}{H_1} = 2{\cdot}634889;$$

and consequently

$$M = 0{\cdot}16261;$$

and in this case the misrepresentation is to that in the Balance of Errors as 103·6 : 100·0. The point of sight or of projection is here at the distance of $\frac{11}{30}$ of the radius from the surface instead of $\frac{1}{2}$ the radius.

The expression then for r is

$$r=\frac{1{\cdot}66261\sin\theta}{1{\cdot}36763+\cos\theta}.$$

When $\theta=113^\circ\ 30'$, this becomes $R=1{\cdot}5737$, which is very near to the size of the Balance of Errors development, viz. $R=1{\cdot}5760$.

The values of r are as follows:—

θ.	r.	θ.	r.
0°	0·0000	60°	0·7710
5	0·0613	65	0·8417
10	0·1227	70	0·9138
15	0·1844	75	0·9874
20	0·2464	80	1·0623
25	0·3090	85	1·1385
30	0·3722	90	1·2157
35	0·4361	95	1·2935
40	0·5009	100	1·3713
45	9·5666	105	1·4484
50	0·6335	110	1·5233
55	0·7016	115	1·5945

A. R. C.

I have had the projection by Balance of Errors and my projection of two-thirds of the surface of the sphere drawn of the exact same size to facilitate the comparison of their relative merits (Plate IV.); and I have drawn circles in their centres, that the extent to which figures are distorted in form and exaggerated in area may be seen, by comparing them with the elliptical figures into which circles are projected towards the limits of the map*.

My projection of two-thirds of the surface of the sphere is described in the Corps' Papers of the Royal Engineers in 1858, and in the *Mittheilungen* for the same year. It is a true geometrical or optical projection, in which the sphere is supposed to be hollow, the plane of projection drawn parallel to and at the distance of 23° 30′ from the plane of any great circle, and the point of sight or projection is at the distance of half the radius from the surface of the sphere. In my published maps the plane of projection is drawn parallel to the plane of the ecliptic.

Maps drawn on this projection have consequently a true perspective effect, and all the circles are represented by true elliptical arcs.

But in the projection, or, to speak more correctly, in the

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* The diagrams (Plate IV.) have been reduced from larger diagrams, and printed by photo-zincography at the Ordnance Survey Office, Southampton.

Development by Balance of Errors, for it neither has a point of sight nor a plane of projection, we have no such effect, but, on the contrary, we have in it some of the circular arcs thrown into lines of contrary flexure; and this I conceive is fatal to this method of representing large portions of the earth's surface.

For the projection of the surface of a hemisphere, the distance of the point of sight which I have adopted is the best possible; for in it the distortion of form and the exaggeration of area—or, as we may call these two defects of all projections, the misrepresentation is a minimum; but for extending the projection from 90° to 113° 30′, Captain Clarke has shown that the distance of the point of sight to give the least misrepresentation should have been $\frac{11}{30}$ instead of $\frac{15}{30}$, in which case the misrepresentation would have been 103·6 as compared to 100·0 in the projection by Balance of Errors, the ratio in the projection as drawn by me being 109·5 to 100·0.

In deciding to adopt half the radius as the distance for my point of projection, I knew that this was not the best possible point, but that it was so near to it that, for all practical purposes and the simplicity of its definition, it was the best to adopt; and it is the very best projection up to 90° from the centre. For the same reason I adopted the limit of the tropics for the position of the plane of projection, because this is very definite, and near the limit which in prudence I could give to the projection; and I have called it a projection of two-thirds of the sphere, whilst in reality the surface represented is seven-tenths of the whole surface.

A comparison of the two projections will satisfy any practical person that, if the ratio of the misrepresentation in them is as 109·5 to 100·0, I should gain very little by making it in the ratio of 103·6 to 100·0; in fact the eye would not detect the difference.

I have prepared a map of the world and a map of the stars on my projection, each ten feet in diameter, for the Great Exhibition of this year; and the public will have a full opportunity of judging of its merits.

XLIII. *On the Regelation of Snow-granules.*
By John Tyndall, *F.R.S.*[*]

I THIS morning (March 21) noticed an extremely interesting case of regelation. A layer of snow between 1 and 2 inches in thickness had fallen on the glass roof of a small greenhouse, into which a door opened from the mansion to which the green-

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[*] Communicated by the Author.

house was attached. Air slightly warmed, acting on the glass surface from underneath, melted the snow in immediate contact with the glass, and the layer in consequence slid slowly down the glass roof. The inclination of the roof was very gentle, and the motion correspondingly gradual. When the layer overshot the edge of the roof it did not drop off, but bent like a flexible body, and hung down over the edge for several inches. The continuity of the layer was broken into rectangular spaces by the inclined longitudinal sashes of the roof, and from local circumstances one side of the roof was warmed a little more than the other; hence the subdivisions of the layer moved with different velocities, and overhung the edge to different depths. The bent and down-hanging layer of snow in some cases actually curled up inwards.

Faraday has shown that when small fragments of ice float on water, if two of them touch each other they instantly cement themselves at the point of contact; and on causing a row of fragments to touch, by laying hold of the terminal piece of the row you can draw all the others after it. A similar cementing must have taken place among the particles of snow now in question, which were immersed in the water of liquefaction near the surface of the glass. But Faraday has also shown that, when two fragments of ice are thus united, a hinge-like motion sets in when you try to separate the one from the other by a lateral push: one fragment might in fact be caused to roll round another, like a wheel, by incipient rupture and the re-establishment of regelation. The power of motion thus experimentally demonstrated rendered it an easy possibility for the snow in question to bend itself in the manner observed. The lowermost granules, subjected to pressure when the support of the roof had been withdrawn, rolled over each other without a destruction of continuity, and thus enabled the snow layer to bend as if it were viscous. The curling up was evidently due to a contraction of the inner surface of the layer, produced, no doubt, by the accommodation of the granules to each other as they slowly diminished in size.

Waverley Place, St. John's Wood,
March 21, 1862.

XLIV. *On the Principles of Theoretical Physics. By the* Rev. J. Challis, *M.A., F.R.S., F.R.A.S., Plumian Professor of Astronomy in the University of Cambridge*[*].

THE great progress that is being made at the present time in experimental philosophy is remarkable in respect both to the skill and ingenuity displayed in making the experiments,

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[*] Communicated by the Author.

and the success with which new facts are elicited and new laws established. In consequence, perhaps, of the striking character of these achievements, compared with the slow and uncertain steps with which theoretical physics have of late advanced, the idea seems to be gaining ground that theory may be dispensed with, and that the domain of natural philosophy includes only the discovery of facts and educing laws out of them. On this point I beg to offer a few observations.

Natural philosophy, as the history of its progressive steps seems clearly to point out, consists of two parts, related but distinct—viz. the experimental and theoretical. The kind of relation existing between them may be illustrated by reference to the history of physical astronomy. The labours of Kepler are exclusively in the province of experiment, or observation; those of Newton in the province of theory. The former do not involve the idea of force, while in the latter this idea is fundamental. By observations, carried on with wonderful patience and perseverance, Kepler established three laws relating to the motions of the planets about the sun. The knowledge of these laws was not necessary for the discovery of the principle of gravitation: Newton, in fact, did not use them for that purpose. They were rather *problems* for solution, which Newton succeeded in solving by differential calculation applied to the hypothesis of a gravitating force varying according to the law of the inverse square of the distance. It may, however, be doubted whether the reasoning by which the solutions were effected would ever have been discovered if the problems had not been proposed. But as soon as the proper calculation was employed and the proper hypothesis made, a few steps of symbolic reasoning sufficed for the demonstration of the laws which it cost Kepler so many laborious years to arrive at by observation. From Newton's time, with the exception perhaps of some futile attempts of Flamsteed, astronomical observation has not been directed towards the investigation of laws, but has almost exclusively been employed in furnishing the data indispensable for making theoretical calculations applicable to actual bodies and actual instances of motion, and in giving the means of correcting previous data by comparisons of observed with calculated celestial positions. The general result of the combination of observation with theory is the demonstration of the law of gravity. This law could not be shown to be a physical reality either by observation alone, or by theory alone.

From this survey it would appear that in Kepler's time astronomical observation was in advance of theoretical calculation, and was occupied with the investigation of laws because the means of doing so by theory, to which such investigation properly belongs, was not yet discovered. Turning now to other departments of physics—the phenomena of light, heat, electricity, galvanism, magnetism, and diamagnetism,—it may, in the first place, be stated that with respect to all these theory is in a condition analogous to that of physical astronomy in the time of Kepler. Experiment has established the existence of a great number of facts and laws, which are only so many problems that wait for solution by some theoretical generalization. The process for effecting such generalization must be of the same kind as that which has been so successful in physical astronomy. Some hypothesis, or hypotheses, suggested by the antecedents of physical science, must be thought of, and be made the basis of appropriate calculation, in order that the truth of the hypotheses may be tested by comparison of the results of the calculation with experimental facts.

I am not aware that any general physical theory, supported by mathematical reasoning, and comprehensive of all the physical forces, has hitherto been proposed, excepting that of which I have given an outline in communications made from time to time to this Magazine. In the supplementary Number for last June I have expressed the intention of going through a revision of the proofs of certain propositions in hydrodynamics which are essential to the general theory. Before, however, carrying out this intention, I propose in this communication, in the first place, to re-state the fundamental hypotheses of the theory, for the purpose of showing that, in conformity with the principles above laid down, they have regard to antecedent physical science, and are proper for being the basis of mathematical calculation. Then I shall endeavour to indicate in general terms the process to be followed in order to found on these hypotheses a general physical theory, and the requirements which such a theory must satisfy.

The hypotheses are of two distinc tkind—sone relating to the agency by which the physical forces act, and the other to the qualities of the ultimate constituents of material substances. These two classes must be regarded separately, as they require the application of different tests, and admit of different degrees of verification.

The principal hypothesis of the first class is, that a very rare and elastic fluid, called the æther, of uniform elasticity throughout, pervades all space not occupied by the atoms of visible and tangible substances, and that under all circumstances its pressure is proportional to its density. The theory does not recognize any active force which is not resident in this medium.

The conception of a medium of much greater tenuity and elasticity than the air is of very long standing, and was entertained especially by Descartes, who even applied it to account for the transmission of light from one point of space to another. And certainly this is a reasonable and a philosophical idea. For let us consider what is the most general and patent fact in regard to light that we have to account for. Some impulse, or action, originates at a position in space, for instance, at a star unnumbered millions of miles distant, and after a time is *felt* by a spectator on the earth's surface. The rate of transmission, it has been ascertained, is nearly two hundred thousand miles in one second. By what means, it may be asked, has the impulse been transmitted from so distant a point at so rapid a rate? To say that small particles of matter are violently driven off from the star in all directions, must be pronounced to be a clumsy explanation compared with that suggested by Descartes, especially as we now know, from our acquaintance with the phenomena of sound, that a dynamical effect may be transmitted through space with great velocity by an elastic medium, without the transmission of matter. I do not hesitate to say that this antecedently known fact is ample justification of the *hypothesis* that light is transmitted through space by an elastic medium analogous in constitution to air. Yet this very reasonable hypothesis meets with no favour from the mathematicians of the present day. No one, as far as I am aware, except myself, has endeavoured to trace the consequences of it. Both experimentalists and theorists have not hesitated to express their disapproval of it *as a hypothesis.* Let them argue against it as much as they please from the consequences to which it leads, but to object antecedently to a hypothesis which is suggested and made intelligible by ascertained facts, is, I maintain, wholly contrary to right rules of philosophizing.

It is true that a medium of a different kind has been invented to account for the transmission of light through space, and for other of its phenomena. According to the assumed constitution of this medium, it more resembles a solid than a fluid. It is, however, not exactly like any known body, a particular atomic arrangement and constitution having been assigned to it expressly to account for the polarization of light. The phenomena of light are in this theory referred to the vibrations of *discrete* atoms. I have from time to time given reasons for concluding that there are phenomena of polarization which are incompatible with such movements, and that this theory must consequently be abandoned. My reasons remain unanswered. I seem, therefore, to have a right to ask that attention should be given to the different course of reasoning which I have proposed, viz. that of investigating mathematically the vibratory movements of a *continuous* elastic fluid, and referring to them the phenomena of light. This course has required the discovery of new principles in the application of partial differential equations to the determination of fluid motion; and I venture to assert that, till these were known, the science of hydrodynamics was in so imperfect a state that an undulatory theory of light was not possible.

There will appear to be the more reason for making the above request when the nature of the task which I have, to a considerable extent, accomplished is stated. It is known that there are certain phenomena of light which are dependent only on properties of the medium in which it is generated, and through which it is transmitted. The following is a list of them: (1) the uniformity of the rate of propagation; (2) the identity of rate for rays of different intensity; (3) the difference of intensity of different rays; (4) the variation of the intensity, according to a certain law, with variation of distance from a centre; (5) the co-existence at the same instant of different portions of light in the same portion of space; (6) the interference of different rays according to given circumstances; (7) the composite character of light; (8) its colour; (9) results of compounding colours; (10) the different kinds of polarized light; (11) the circumstances of the interference and non-interference of polarized light. To account for all these facts, the single hypothesis is made that the medium which is the vehicle of light is a perfect fluid pressing proportionally to its density. Out of this one hypothesis the explanations of all these different phenomena are to be evolved by mathematics alone; and if any well-ascertained mathematical result be contradicted by any one of the facts, the whole theory must be abandoned. Now the hydrodynamical propositions that I have spoken of, if my demonstrations of them be true, do, in fact, give these explanations. Since, however, the principles on which the demonstrations rest are in some respects in advance of any previously proposed, additional elucidation and confirmation may reasonably be required. It is on this account that I intend, as already intimated, to revise both the propositions and their demonstrations.

We have now to consider what hypotheses of the second class—those relating to the qualities of the constituents of bodies—may be allowable and sufficient for the foundation of a general physical theory. First, it may be assumed that the constitution of material substances is *atomic*, the adoption hypothetically of this constitution being justified by the facts of chemical combination and analysis. As the known inertia of masses must be due to the inertia of the constituent parts, it may also be assumed that the atoms are *inert*. It is necessary to make a supposition respecting the *forms* of the atoms, otherwise the mutual action between them and the æther cannot be brought within the province of mathematical calculation. I have made the supposition that they are all *spherical,* but not without regard to antecedently known facts, such as the following. The properties of bodies in a fluid or gaseous state are in no respect altered by any change of the relative positions of the parts—a fact which would hardly be conceivable if the atoms had any other than the spherical form, because in that case their mutual actions would have relation to directions in space. Light is found to traverse some substances without modification, or change of velocity, on changing the direction of its passage through them; and though this is not the case in other substances, yet as the latter are known to be crystalline, it is reasonable to attribute the circumstance to the arrangement of the atoms, and not to deviations from the spherical form. Lastly, I have adopted the Newtonian doctrine that inertia is an essential quality, but not quantitative, and that consequently *all atoms have the same specific inertia.* According to this and the preceding hypothesis, atoms differ from each other only in *magnitude.*

The foregoing hypotheses relating to the æther and the qualities of atoms are the only ones that I have employed in laying the foundation of a general mathematical theory of the physical forces. This circumstance, while it may give an idea of the difficulty of the undertaking, at the same time affords a presumption that the hypotheses are true, it being extremely improbable, if that were not the case, that in the varied and extensive applications that have been made of them, some obvious and fatal contradiction would not have been encountered. The Newtonian dictum, "hypotheses non fingo," must be taken with reservation, as it is not possible to frame a physical theory without hypotheses. The theory of gravitation, for instance, rests on the hypothesis of the law of the inverse square. But they ought, no doubt, to be few and fundamental, and to be such as admit of being tested by means of mathematical reasoning founded on them. The multiplication of hypotheses, and making them *pro re natâ,* are sure signs of a failing theory.

It will be right here to draw a distinction as to kind and degree between the verifications which the two classes of hypotheses admit of in the actual state of science. The verification of the first class may be effected by comparing results deduced from them by rigid mathematical reasoning directly with observed phenomena. If, for instance, such deductions admit of being brought into satisfactory comparison with the eleven different kinds of phenomena of light which I previously enumerated (as, I believe, may be done), there would be a strong presumption, almost amounting to a proof, of the reality of the existence of the æther, and of its being such as it was assumed to be. And to arrive at this conclusion with so much of certainty as to allow of taking it for granted in prosecuting further researches, would certainly be a great step in theoretical physics. The other class of hypotheses do not admit of the same kind of verification, because phenomena (such as are some of those of light) which depend on the qualities of the constituents of bodies, require in general for their direct theoretical explanation the knowledge of the mutual action between the æther and the atoms, and the number, magnitudes, and arrangements of the latter. But this knowledge cannot be furnished in the present state of physical science, and ought rather to be looked for as the final result of physical inquiry pursued in different channels and by all available means.

Such being the account of the hypotheses of the proposed physical theory, I proceed now to speak briefly and in general terms of the course of reasoning required in their application to different classes of phenomena, and the demands which they will have to satisfy. First, let it be conceded that the before-mentioned explanations of phenomena of light have given strong presumptive evidence of the existence of the æther, and of its being such that variations of its pressure are proportional to variations of its density. Next we must take into account the matter of fact that light-bearing rays are also heat-bearing, and that consequently the æther must be the vehicle of the transmission both of light and of *heat*. The explanation which the undulatory theory gives at once of this fact is, that in a ray there exist conjointly transverse and direct vibrations, and that the former expound light, and the latter heat. Again, as heat is known by experience to act as a repulsive *force,* the æther which accounts for other of its phenomena must account for this also. It must be borne in mind that the æther was assumed to be a highly *elastic* medium, and its dynamic action cannot therefore be overlooked. In fact it is reasonable to attribute the sensation of light to the dynamic effect of the ætherial vibrations on the nervous system of the eye. But such vibrations, when we calculate their effect only to the first power of the velocity, are found to produce simply oscillations of small spherical bodies submitted to their action, and not motions of translation. To account for the latter, it is necessary to proceed to the consideration of effects due to the second power of the velocity. This I have attempted to do, and to found a theory of the force of heat on a mathematical investigation of the dynamic effect of pressures corresponding to the *square* of the velocity in ætherial *vibrations.* The investigation showed that the result of such action on small spherical bodies might under some circumstances be repulsion from a centre, under others, attraction towards a centre. Thus the theory was found to embrace the *forces of aggregation* which hold the constituent atoms of bodies in equilibrium. Between these forces and the force of *galvanism* there is, as experiment shows, a close relation, which the theory, if true, will account for. But clearly it does account for a relation by merely supposing that the two kinds of forces are modifications of the dynamical action of the *same* ætherial medium. I may even go further, and state, as a result to which my investigations point, that while the forces of aggregation depend on the square of the velocity in *vibrations*, galvanic force depends on the square of the velocity in *currents*. Moreover, it is matter of experience that galvanic, electric, magnetic, and diamagnetic forces have some bond of connexion; and obviously this circumstance also may be referred to their being modes of action of the same medium. My researches indicate, further, that these forces are all expounded by the dynamic action of ætherial *currents*, and that they differ from each other only in the conditions and circumstances under which the currents are generated. Lastly, there is yet another physical force, the relations of which to an ætherial medium, and to other modes of force, are not readily made out: I mean the force of gravity. If, however, all the other forces are modifications of ætherial pressure, it is reasonable to suppose that this one is of the same kind. I have ventured to reason on this supposition, and have attempted to deduce (I think with success) the known laws of gravity from the dynamical action of ætherial waves of much larger magnitude than those which correspond to molecular forces. It will be seen from these explanations that very large demands are made on the hypothesis of a universal æther, so large, indeed, that it seems impossible to account for its meeting them in any degree excepting on the supposition that it is a reality.

From the foregoing discussion, one general inference of an important character may be drawn. If the principles of the proposed theory be admitted, it will follow that, previous to the theoretical explanation of a vast number of facts and laws which modern experiments have discovered, it will be necessary to investigate, by mathematical reasoning applied to the æther, the modes of its action under given circumstances. To illustrate this remark, I may refer to the problem of the generation and permanence of the sun's heat, and to that of the development of the tails of comets, both of which have recently attracted the attention of theorists. Now, as I conceive, neither of these problems can be at all approached without the antecedent possession of a mathematical theory of the force of heat, such as that which I have deduced from the properties of an elastic medium. If this course were pursued, it might perhaps be found to be unnecessary to suppose that the sun's heat is maintained by the impact of minute bodies whose existence even is not ascertained; and to account for the elongation of comets' tails, it might be equally unnecessary to invent *pro hâc vice* a new kind of repulsive force emanating from the sun.

I take this opportunity of adverting to an assertion which has been made and reiterated respecting the science of hydrodynamics, to the effect that, compared with other departments of natural philosophy, it is of minor importance, and has produced "meagre" results. As this assertion is probably only the expression of an opinion, entertained by those who have made it, respecting the course which physical research may most profitably take, I claim the right in the interests of science to state an opposite opinion, formed after having long made hydrodynamics a special subject of inquiry, viz. that it is precisely the determination, by the application and solution of partial differential equations, of the motion and pressure of fluids, which is required for the theoretical explanation of the present large accumulation of experimental facts, and that the discovery and successful solution of the hydrodynamical problems proper for this purpose hold the same place with respect to actual theoretical physics as the solution which Newton first effected of appropriate dynamical problems held with respect to physical astronomy.

I beg permission to close this communication by referring to a fact of observation which appears to be a singular confirmation of the new principles which I have applied in hydrodynamical research. It will be unnecessary to indicate here the process by which the velocity of sound has been determined on those principles, as it is given at length in a communication to the Philosophical Magazine for December 1852. For the present purpose it will suffice to state that the velocity (a_1) of the propagation of sound is there given by the equation

$$a_1{}^2=a^2+\frac{b^2}{q^2}+v^2\left(\frac{2q^2a^2}{3b^2}+\frac{5}{12}\right),$$

in which v is the maximum velocity of the propagated wave, and the value of $\frac{b^2}{q^2}$ is $\frac{4a^2}{\pi^2}$. Hence, substituting V for $a\sqrt{1+\frac{4}{\pi^2}}$, it will readily be found that

$$a_1=\mathrm{V}.\left(1+1\cdot0308\,\frac{v^2}{\mathrm{V}^2}\right).$$

It thus appears, as a theoretical result, that the rate of propagation depends in some degree on the loudness of the sound, the louder being propagated with the greater velocity. I was not aware that such had been observed to be the fact, till my attention was drawn to the experiments for the determination of the velocity of sound made in the arctic regions under the superintendence of Captain Parry, by the reference made to them in Mr. Earnshaw's paper "On the Theory of Sound," in the Transactions of the Royal Society (Part I. 1860, p. 139). It is stated that several times in the experiments made on February 9, 1822, the word of command to fire was heard *after* the report of the gun; and though the same circumstance was not remarked on other days, it is to be said that on that day the number of experiments was greater than on any other, the distance was intermediate to what it was on most of the other days, and the air was still and barometer low. Taking all circumstances into account, the experiment seems to establish the fact of an actual difference of rate of propagation in waves of different intensities.

Cambridge, March 21, 1862.

XLV. *Notices respecting New Books.*

A Manual of Chemistry, Descriptive and Theoretical. By W. ODLING, *M.B., F.R.S.* Part I. London: Longman and Co. 1861.

THE Unitary notation of Laurent and Gerhardt, although it has made many disciples, does not as yet possess a complete literature of its own. It is true that there constantly appear, in British and foreign scientific journals, memoirs in which the unitary formulæ and an appropriate nomenclature are used. Yet although this has been the case for several years, and many fresh converts from among the most eminent chemists have joined the new sect, the doctrines of Gerhardt, with the modifications and additions made from subsequent experience, have not been embodied in a series of text-books adapted for the instruction of the young student, nor of works of reference for the more advanced. This is the more remarkable since it seems almost customary for every professor of chemistry to write a manual of his science, which he can oblige his own pupils to buy, even if he is unable to persuade the scientific world to do so. Nine out of every ten such works could well be spared: for they resemble one another very closely; and even of some of those whose success was at first merited, later issues have retained old fallacies and omitted newly-discovered facts; for when will chemical authors cease to talk of sulphuric and oxalic acids as monobasic, and to introduce these bodies quite commonly into descriptions and equations as SO^3 and $C^2 O^3$? But we are in real want of one complete set of treatises in which the various branches of chemical science proper shall be treated systematically according to the new view; and we welcome the first instalment of Professor Odling's contribution to the series.

As necessary chemical works, we may suggest the following:—1. A short, simple, introductory book explaining the scope and language of chemistry, and describing fully the way of performing a few easy and instructive experiments; such a book as every student might go through previously to attending lectures or entering the laboratory. It might also be used as the elementary school-book of chemistry. 2. A text-book of the most important chemical facts and theories. 3. A complete dictionary or work of reference. 4. A set of manuals of analysis, qualitative and quantitative. As to the first of these works, we do not at present possess in the English language a simple compact introduction to chemistry on the unitary system; but Prof. Odling has undertaken to supply the second book of the series; for the third we must wait; while the commencement of a set of analytical manuals was made more than three years ago. For in November 1858 an important original work was published, which Prof. Odling by a strange oversight neglects to notice, although he mentions in his preface a much smaller handbook which was afterwards issued, of no particular excellence, but owing any merit it may possess to the German work of Prof. Will, on which it is based. This adaptation by Mr. Conington attempts to embrace too much, while it lacks the clearness and precision attained in the systematic treatise of Messrs. Northcote and Church. The plan of the latter volume seems to anticipate in some of its details the subsequent work of Prof. Odling.

The author, in the portion of his Manual now before us, devotes thirty pages of Chapter I. to a brief outline of the generalities of the science. These, however, are not treated with such fulness and simplicity as to enable the beginner to read the present volume without previous initiation into the general principles and terminology of chemistry. For at the twentieth line of Chapter I. the student is introduced precipitately to "chlorous and basylous functions," and then told what is meant by this delightful expression in a couple of sentences which imply previous acquaintance on the student's part with several chemical truths. This reminds one of Dr. Miller's plan of drawing the attention of the young student of organic chemistry in the first place to *glycyrrhizin*, a substance whose constitution and relations are, it must be admitted, somewhat obscure.

In the paragraphs on Combination by Volume, Comparable Volumes, Equivalent Substitutions, and Molecular Types, many important principles are tersely explained; while the descriptions given of Homologous, Isologous, and Heterologous series are illustrated by well-selected examples. We may cite the two following sets of heterologues, remarking *en passant* that Prof. Odling has shown, by an experiment of his own, that it is possible actually to pass, by a process of direct oxidation, from the first to the second member of the hydrochloric acid series:—

$H\,Cl$	Hydrochloric acid.	$H^3\,P$	Phosphamine.
$H\,ClO$	Hypochlorous acid.	$H^3\,PO$	Phosphoric aldehyd.
$H\,ClO^2$	Chlorous acid.	$H^3\,PO^2$	Hypophosphorous acid.
$H\,ClO^3$	Chloric acid.	$H^3\,PO^3$	Phosphorous acid.
$H\,ClO^4$	Perchloric acid.	$H^3\,PO^4$	Phosphoric acid.

§§ 16–22 are devoted to the consideration of acids and salts. The author's general formula for an acid ($H_x R_y O_z$) can scarcely be adapted to the non-oxygenized, binary, or hydrogen acids; for it renders necessary the qualification that "O_z may range from 0." The basicity of acids is clearly explained by appropriate instances, such as the following, drawn from the monobasic, bibasic, and tribasic acid of phosphorus:—

		$K H^2 PO^2$	$H^3 PO^2$	Hypophosphorous	acids
	$K^2 HPO^3$	$K H^2 PO^3$	$H^3 PO^3$	Phosphorous	and
$K^3 PO^4$	$K^2 HPO^4$	$K H^2 PO^4$	$H^3 PO^4$	Phosphoric	salts.

As instances of derived acids, we have $Na^2 CO^3$ and $Na^2 CS^3$, (oxy)carbonate and sulphocarbonate of sodium: $K^3 PO^4$ and $K^3 PO^3 S$, (oxy)phosphate and sulphoxyphosphate of potassium. The relation of anhydrides to acids is shown by the following scheme:—

	Acids.			Anhydrides.
Periodic......	$2H\ IO^4$	$-\ H^2 O$	$=$	$I^2\ O^7$
Sulphuric	$H^2 SO^4$	$-\ H^2 O$	$=$	$S\ O^3$
Phosphoric ..	$2H^3 PO^4$	$-\ 3H^2 O$	$=$	$P^2 O^5$
Silicic	$H^4 SiO^4$	$-\ 2H^2 O$	$=$	$Si\,O^2$

A class of curious bodies, the true relations of which have not been generally discerned, find suitable places in some of the series given in the present volume. Among these substances the aldehydes are most conspicuous. They are usually found as chlorine-substitution products of the normal aldehydes, and bear the same relations to their respective oxacids as common aldehyde bears to acetic acid. In the following Table, the connexion between certain chloraldehydes and their acids is traced:—

	Chlor-aldehyds.			Acids.		
Nitric......	$Cl\ NO^2$	$+\ H^2 O =$	HCl	$+\ H\ NO^3$		$(HO)\ NO^2$
Sulphuric ..	$Cl^2 SO^2$	$+\ 2H^2 O =$	$2HCl$	$+\ H^2 SO^4$	or	$(HO)^2 SO^2$
Phosphoric..	$Cl^3 PO$	$+\ 3H^2 O =$	$3HCl$	$+\ H^3 PO^4$		$(HO)^3 PO$

"It is observable that the conversion of normal aldehyds into acids by oxidation may be represented as an exchange of hydrogen H, for peroxide of hydrogen HO, analogous to the above-illustrated exchange of chlorine for peroxide of hydrogen."

The following illustrations of the four primary types put their relations into a very clear light:—

Prot-equivalent radicles.	Bi-equivalent radicles.	Ter-equivalent radicles.	Tetr-equivalent radicles.
$HCl - H = Cl'$ Chlorine.			
$H^2 O - H = HO'$ Eurhyzene.	$H^2 O - H^2 = O''$ Oxygen.		
$H^3 N - H = H^2 N'$ Amidogen.	$H^3 N - H^2 = HN''$ Imidogen.	$H^3 N - H^3 = N'''$ Nitrogen.	
$H^4 N - H = H^3 C'$ Methyl.	$H^4 C - H^2 = H^2 C''$ Methylene.	$H^4 C - H^3 = HC'''$ Formyl.	$H^4 C - H^4 = C''''$ Carbon.

Professor Odling, with Laurent, Gerhardt, Kekulé, and others, regarding double decomposition as the great type of chemical action, enlarges the usual definition of it so as to include the direct union of two elements, the substitution of one element for another, the breaking up of a compound into its elements, and the liberation of a single element in the free state.

Compound radicals are regarded by our author as not necessarily existent in bodies, but as molecular groupings capable of transference from one body to another. Thus substances known to be mutually related in derivation and behaviour are capable of being viewed as related to one another in constitution also.

The general considerations conclude with an account of crystallography, of the various states in which chemical substances occur, and of atomic volume, atomic heat, and the diffusion of gases.

In the systematic description of the elements and their compounds, hydrogen, as the great typical element, comes first, the other elements, beginning with the non-metallic, being considered in order according to the gradually increasing complexity of their relations to hydrogen. An excellent feature of the volume is here apparent, namely, the strict order which is invariably followed in treating each subject; but this feature becomes still more marked when our author passes on to the consideration of the "monhydric elements." He gives us the requisite information about each element in the same manner:—1. Distribution. 2. Preparation. 3. General Properties. 4. Relations. And at the end of each group of elements, such as that which includes chlorine, bromine, iodine, and fluorine, we have a useful summary of their general properties and relations. The remainder of the volume is occupied by the dihydric and trihydric elements and their compounds,—the dihydric elements being oxygen, sulphur, selenium, and tellurium: and the trihydric, nitrogen, phosphorus, arsenic, antimony, and bismuth. In the summary of the nitrogen group, with which Part I. concludes, we have some notes on "Mixed Types," and the following interesting contrast between the parallel oxacid compounds to which chlorine, sulphur, and phosphorus respectively give origin:—

Monhydric.	Dihydric.	Trihydric.
HCl	$H^2 S$	$H^3 P$
$HClO$	$Cl^2 SO$	$Cl^3 PO$
$HClO^2$	$Cl^2 SO^2$	$H^3 PO^2$
$HClO^3$	$H^2 SO^3$	$H^3 PO^3$
$HClO^4$	$H^2 SO^4$	$H^3 PO^4$

A few remarks, in conclusion, as to the nomenclature of the work under review. Prof. Odling has made but few and slight alterations in the commonly received names of chemical compounds: the examples given below illustrate some of the more conspicuous changes:—

Chloronitrous gas	$NO\,Cl$,	*becomes* nitrous chlor-aldehyd.
Terchloride of phosphorus ..	$Cl^3 P$,	*becomes* phosphorous chlor-aldehyd.
Oxychloride of phosphorus ..	$Cl^3 PO$,	*becomes* phosphoric chlor-aldehyd.
Sulphochloride of phosphorus	$Cl^3 PS$,	*becomes* sulpho-phosphoric chlor-aldehyd.
Chlorosulphurous acid......	$Cl^2 SO$,	*becomes* sulphurous chlor-aldehyd.

While the author feels no hesitation in calling *hyposulphite* of sodium *thiosulphate*, a change which will be scarcely relished by photographers, he has scruples about altering *sulphamide* into *sulphodiamide*, saying (p. 228), "sulphamide ought analogically to be called sulphodiamide, and sulphimide sulphamide; but the use of these two words to signify the compounds expressed above is too general to allow of their alteration." We confess that we have not heard or seen much either of sulphamide or sulphimide; but our chemical experience has been perhaps too limited. The free use, as an equivalent notation, of dashes attached to the symbols, is an important feature of the work, and has already been employed with great advantage by Prof. Kekulé in his *Lehrbuch der Organischen Chemie.*

The volume before us is characterized by a force and precision of style, and by a happy originality of view, which it would require long quotations properly to illustrate, and which render the work a valuable contribution to English chemical literature.

XLVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 238.]

April 25, 1861.—Major-General Sabine, R.A., Treasurer and Vice-President, in the Chair.

THE following communication was read:—

"On the Synthesis of Succinic and Pyrotartaric Acids." By Maxwell Simpson, Esq., M.B.

Since my last communication to the Society*, I have succeeded in obtaining the cyanide of ethylene in a state of purity by a slight modification of the process I have already given. A detailed account of it will be found in the paper which accompanies this abstract.

This is, I believe, the first example of a diatomic cyanide. It has the following properties in addition to those I have already enumerated:—Below the temperature of 37° Cent. it is a crystalline solid of a light-brown colour, above that temperature it is a fluid oil. Its specific gravity at 45° Cent. is 1·023. It has an acrid disagreeable taste. It is neutral to test-paper. It is decomposed by potassium, cyanide of potassium being formed. Its solution in water is not affected by nitrate of silver. Heated with nitric acid, it gives succinic acid and nitrate of ammonia. Heated with muriatic acid, it yields the same acid and muriate of ammonia. It forms an interesting compound with nitrate of silver, which was obtained in the following manner:—About three equivalents of crystallized nitrate of silver were rubbed up in a mortar with one equivalent of pure cyanide of ethylene and a considerable quantity of ether. The ether was then poured off, and the residual salt dissolved in boiling alcohol. On cooling, the alcohol became a mass of brilliant pearly plates. Submitted to analysis, these yielded results agreeing with the for-

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* Phil. Mag. S. 4. vol. xxii. p. 66.

mula $C_4H_4Cy_2+4(AgO, NO_5)$. The crystals are soluble in water and alcohol, insoluble in ether. When heated, they melt and explode like gunpowder. They do not detonate on percussion. This compound may possibly throw some light on the constitution of the fulminates.

I have also slightly modified the process I gave in my last note for succinic acid. The modified process is very productive, and yields the acid at once in a state of purity. From 1500 grains of bromide of ethylene I obtained 480 grains of succinic acid, or nearly 33 per cent. It gave on analysis 40·54 instead of 40·67 per cent. of carbon.

We are now enabled, thanks to the researches of Messrs. Perkin and Duppa and of M. Kekulé*, to build up three highly complex organic acids (succinic, paratartaric, and malic) from a simple hydrocarbon; and, what is more important, we are enabled to do this by processes every stage of which is perfectly intelligible.

With the view of ascertaining whether or not the homologues of succinic acid could be obtained in a similar manner, I have endeavoured to prepare pyrotartaric acid from the cyanide of propylene, propylene being the radical of propylglycol.

Preparation of Cyanide of Propylene.—A mixture of one equivalent of bromide of propylene and two of cyanide of potassium, together with a considerable quantity of alcohol, was exposed to the temperature of a water-bath for about sixteen hours. The alcohol was then filtered and distilled. A liquid residue was thus obtained, which was dissolved in ether. The body left on evaporating the etherial solution was then submitted to distillation. Almost the entire liquid passed over between 265° and 290° Cent. The fraction distilling between 277° and 290° Cent. was collected apart and analysed. It gave 62·0 instead of 63·8 per cent. of carbon. This body cannot be obtained purer by distillation under atmospheric pressure, as it suffers partial decomposition during the process.

The properties of this cyanide very much resemble those of the preceding. It differs, however, in its physical state, which is that of a liquid at the ordinary temperature of the air. It is soluble in water, alcohol, and ether. It has an acrid taste. It is neutral to test-paper. It is decomposed by potassium, cyanide of potassium being formed. Its solution in water does not precipitate nitrate of silver. Heated with potash, it is resolved into an acid and ammonia.

Formation of Pyrotartaric Acid.—A mixture of one volume of cyanide of propylene and about 1½ volume of strong muriatic acid was exposed in a sealed tube to the temperature of a water-bath for a few hours. On cooling, the contents of the tube became a mass of crystals. These were dried and dissolved in absolute alcohol. The residue obtained on evaporating the alcoholic solution was then twice crystallized from water, and finally digested with ether. The body left on distilling off the ether is the acid in

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* Quarterly Journal of the Chemical Society, July 1860; and Bulletin de la Société Chimique de Paris du Août, 1860, p. 208.

question. The numbers obtained on analysis agree very well with the formula of pyrotartaric acid; I got 44·6 instead of 45·4 per cent. of carbon. It had also all the properties ascribed to this acid by Pelouze and Arppe. The crystals were colourless, and very soluble in water, alcohol, and ether. It had an agreeable acid taste. It became semi-fluid at 100° Cent., and melted completely a few degrees above that temperature. Long-continued ebullition in a glass tube converted it into an oil, which was insoluble in cold water, and no longer affected litmus-paper, but which gradually dissolved in hot water, recovering at the same time its acid reaction. The following equation will explain the reaction which gives birth to this acid:

$$C_6\,H_6\,Cy_2+2\,H\,Cl+8\,HO=C_{10}\,H_8\,O_8+2\,(N\,H_4\,Cl).$$

It is highly probable that there exists a series of isomeric acids running parallel to these, which may be obtained by similar processes from the diatomic radicals contained in the aldehydes. Thus from cyanide of ethylidene ($C_4\,H_4\,Cy_2$) we may hope to get an isomer of succinic acid.

I propose to continue my researches in this direction, and to extend them to the cyanides of the triatomic radicals.

May 2.—Major-General Sabine, R.A., Treasurer and Vice-President, in the Chair.

The following communication was read:—

"On Internal Radiation in Uniaxal Crystals." By Balfour Stewart, Esq., A.M.

The well-known theory of exchanges, which was proposed by the late Prof. Prevost of Geneva, is built upon the fact that a substance placed anywhere within an enclosure of a constant temperature will ultimately attain the temperature of the enclosure.

In his theory M. Prevost supposes that a constant, mutual, and equal interchange of radiant heat takes place between the body and the enclosure which surrounds it, so that, receiving back precisely that heat which it gives away, the former is enabled to remain at a constant temperature.

With respect to this radiation, which is thus supposed to be constantly taking place between substances at the same temperature, it had until lately been conceived of as proceeding mainly, if not entirely, from the surface of bodies—a very thin film or plate of any substance being supposed to furnish the maximum amount of radiation which that substance was capable of affording.

It lately occurred to the author of this paper, reasoning from the theory of exchanges, that mere surface radiation is not sufficient to account for the equilibrium of temperature which exists between a body and the enclosure which surrounds it.

These theoretical conclusions have been amply verified by experiment, and the subject has been discussed in a paper published in the 'Transactions of the Royal Society of Edinburgh' for the year 1858. As the chain of reasoning by which this fact is deduced theoretically from the law of exchanges, and the experimental evidence upon which it rests, are both of a very simple nature, it has been thought well to restate them here before proceeding further in this investigation.

Let us imagine to ourselves an enclosure of lamp-black kept at a constant temperature, and containing two pieces of polished rock-salt similar to one another, except that the thickness of the one is greater than that of the other.

Now it is evident that since the thick piece absorbs more of the heat which falls upon it from the sides of the enclosure than the thin piece, it must likewise radiate more in order that it may always remain at the same temperature. Here then we have the fact of internal radiation in the case of rock-salt deduced as a theoretical consequence of the law of exchanges; experimentally it is found that a thick piece of rock-salt radiates very considerably more than a thin piece.

The fact of internal radiation being conceded, it is easy to see that the amount of heat which a particle radiates must be independent of its distance from the surface. For besides that this is the simplest hypothesis, the absorption, and consequently the radiation of two similar plates of rock-salt placed with their surfaces together, ought to be the same as from a single plate of double the thickness; and experiment shows that this is the case.

It being therefore supposed that the internal radiation of a particle is independent of its distance from the surface, let us imagine a row of particles A, B, C, D in the midst of a substance of constant temperature which extends indefinitely on all sides of them. There will be a certain stream of radiant heat constantly flowing past any such particle A to go in the direction AB.

Now, since the radiation is supposed to be the same for the different particles A, B, C, D, it follows that the absorption of the stream of heat by these particles must also be the same for each; and in order that this may be the case, it is necessary that the stream which impinges on one particle be the same in quantity and in quality as that which impinges upon another. This consideration leads us to a method of viewing internal radiation, which is wholly independent of the diathermanous or athermanous character of the body. For whatever be the absorption of a particle for any description of heat, its radiation must necessarily be precisely the same in order that the stream of heat in passing the particle may be just as much recruited by its radiation as it is reduced by its absorption; in other words, we may regard the substance through which the heat passes as perfectly diathermanous.

We gain another advantage by this method of viewing the subject: for, in the law which is expressed by saying that the absorption of a particle is equal to its radiation, and that for every description of heat, the word *description* is used to define and separate those rays of heat which are absorbed in different proportions by the same substance. Therefore in any problem connected with this subject we may suppose that a separate equilibrium holds for every such ray.

Now it is well known that rays of different wave-lengths are absorbed in different proportions by the same substance. We are therefore entitled to suppose that a separate equilibrium holds for each wave-length. The advantage of this is obvious in problems which admit of the application of optical principles. But we may go even further. For we know that in tourmaline, and in some other crystals cut parallel to the optic axis, the ordinary ray is more absorbed than the extraordinary; and the experiments of Prof. Kirchhoff and the author have shown that in tourmaline the ordinary ray is also radiated in excess. It thus appears that, in the case of crystals, we have not only a separate equilibrium for each wave-length, but for each of the two rays into which the incident ray is divided.

The following method of comparing together two streams of radiant heat has been adopted:—Consider a square unit of surface to be placed in the midst of a solid of indefinite thickness on all sides, and find the amount of radiant heat which passes across this square unit of surface in unit of time in directions very nearly perpendicular to the surface, and comprehending an exceedingly small solid angle $\delta\phi$. Call this heat $\mathrm{R}d\phi$, then R may be viewed as the intensity of the radiation in this direction.

Let us now suppose that we have a uniaxal crystal of indefinite thickness bounded by a plane surface, and that parallel to this surface, and separated from it by a vacuum, we have a surface of lamp-black, the whole being kept at a constant temperature.

Let us take a square unit of this surface, and consider the heat from the lamp-black which falls upon it through an exceedingly small solid angle in a direction not necessarily perpendicular to the surface. Part of this heat will be refracted into the interior of the crystal in two rays, the ordinary and the extraordinary. There will be thus two separate bundles of refracted rays, the solid angle comprised by the individual rays of the one being different from that comprised by the rays of the other; the inclination to the surface also being different for each bundle.

Now, on the principle of a separate equilibrium for each ray, these entering bundles of rays must respectively equal the rays of the same kind which emerge from the crystal in the same directions.

Hence if we know the radiation of lamp-black, and the direction in which the rays under consideration strike the surface of the crystal, as also the angle which the latter makes with the optic axis, it is conceivable that, by means of optical principles, joined to the fact of the equality between the entering and emerging bundles of rays, we may be enabled ultimately to ascertain the internal radiation through the crystal in different directions.

A little consideration, however, will show that this method of procedure presupposes a certain mutual adaptation to exist between the optical principles employed and the theory of exchanges. For it is evident that the expression for the internal radiation in any direction may be obtained by operating upon terminal surfaces bearing every possible inclination to the optic axis.

But the internal radiation, if the law of exchanges be true, is clearly independent of the position of this surface, which is indeed merely employed as an expedient. This is equivalent to saying that the constants which define the position of the bounding surface must ultimately disappear from the expression for the internal radiation.

The author then endeavours to show that such an adaptation does really exist, and that the expression for the internal radiation is independent of the position of the surface.

For the extraordinary ray, the internal radiation is found to be

$$R_e = \frac{Rr^4}{2m^4n^2},$$

where R is the radiation from lamp-black;

and for the ordinary, $$R_0 = \frac{R}{2n^2};$$

where n denotes the axial and m the equatorial radius of the ellipsoid into which the extraordinary ray will have spread in the crystal in the same time that *in vacuo* it would have spread into a sphere whose radius = unity; and lastly, r denotes the radius of this ellipsoid in the direction in which the internal radiation is measured.

The author concludes by remarking that the fundamental law, which is intimately connected with the theory of exchanges, and which renders an equilibrium of temperature possible in the case under consideration, seems to be the law of the equality between action and reaction in the impact of elastic bodies.

He also considers that the law which is expressed by saying "That the absorption of a particle is equal to its radiation, and that for every description of heat," expresses another law of action and reaction which holds when the motion which constitutes radiant heat is not conveyed from particle to particle without loss, or when the bodies under consideration are not perfectly elastic.

These two laws of action and reaction are viewed as supplementing each other, so as to render that equilibrium of temperature which is demanded by the theory of exchanges possible under all circumstances.

GEOLOGICAL SOCIETY.

[Continued from p. 244.]

January 22, 1862.—Sir R. I. Murchison, V.P.G.S., in the Chair.

The following communications were read:—

1. "On some Flint Arrow-heads (?) from near Baggy Point, North Devon." By N. Whitley, Esq.

Immediately beneath the surface-soil above the "raised beaches" of North Devon and Cornwall the author has observed broken flints; and even at the Scilly Isles such flints are found. At Croyde Bay, about halfway between Middle-Borough and Baggy Point, at the mouth of a small transverse valley, Mr. Whitley found them in considerable number, collecting about 200 specimens, of which about 10 per cent. of the splintered flints at this place have more or less of an arrow-head form, but they pass by insensible gradations from what appear to be perfect arrow-heads of human manufacture to such rough splinters as are evidently the result of natural causes. Hence the author suggested that great caution should be used in judging what flints have been naturally, and what have been artificially shaped.

2. "On some further Discoveries of Flint Implements in the Gravel near Bedford." By James Wyatt, Esq., F.G.S.

Since Mr. Prestwich described the occurrence of flint implements, near Bedford (Geol. Soc. Journ. No. 67, p. 366) Mr. Wyatt, Mr. Nall, the Rev. Mr. Hillier, and Mr. Berrill have added seven or eight to the list, from the gravel-pits at Cardington, Harrowden, Biddenham, and Kempston. Mr. J. G. Jeffreys, F.G.S., having examined Mr. Wyatt's further collections of Shells from the gravel-pits at Biddenham and Harrowden, has determined seventeen other species besides those noticed by Mr. Prestwich, and among these is *Hydrobia marginata* (from the Biddenham pit), which has not been found alive in this country. At Kempston, Mr. Wyatt has examined the sand beneath the gravel (which is destitute of shells), and at 3 feet in the sand (19 feet from the surface) he found *Helix, Succinea, Bithynia, Pupa, Planorbis,* &c. with a flint implement. The upper gravel contained several flint flakes.

3. "On a Hyæna-den at Wookey-Hole, near Wells, Somerset." By W. Boyd Dawkins, Esq., F.G.S.

In a ravine at the village of Wookey-Hole, on the southern flanks of the Mendips, and two miles N.W. of Wells, the River Axe flows out of the Wookey-Hole Cave by a canal cut in the rock. In cutting this passage, ten years ago, a cave, filled with ossiferous loam, was exposed and about 12 feet of its entrance cut away. In 1859 the author and Mr. Williamson began to explore it by digging away the red earth with which the cave was filled, and continued their operations in 1860 and 1861. They penetrated 34 feet into the cave; and here it bifurcates into two branches, one vertical (which was examined as far as practicable), and one to the right (left for further research). A lateral branch on the left, not far from the entrance, was also examined. The cave is hollowed out of the Dolomitic Conglomerate, from which have been derived the angular and water-worn stones scattered in the ossiferous cave-earth. Its greatest height is 9 feet, and the width 36 feet; it is contracted in the middle, and narrow towards the bifurcation. Remains of *Hyæna spelæa* (abundant), *Canis Vulpes, C. Lupus, Ursus spelæus, Equus* (abundant), *Rhinoceros tichorhinus, Rh. leptorhinus* (?), *Bos primigenius, Megaceros Hibernicus, C. Bucklandi, C. Guettardi, C. Tarandus* (?), *C. Dama* (?), and *Elephas primigenius* were met with; remains of *Felis spelæa* were found when the cave was first discovered. The following evidences of man were found by Messrs. Dawkins and Williamson in the red earth of the cave—chipped flints, flint-splinters, a spear-head of flint, chipped and shaped pieces of chert, and two bone arrow-heads; and the author argues that the conditions of the cave and its infilling prove that man was contemporaneous here with the extinct animals in the præ-glacial period (of Phillips), and that the cave was filled with its present contents slowly by the ordinary operations of nature, not by any violent cataclysm.

February 5, 1862.—Sir R. I. Murchison, V.P.G.S., in the Chair.

The following communications were read:—

1. "On some Volcanic Phenomena lately observed at Torre del Greco and Resina." By Signor Luigi Palmieri, Director of the Royal Observatory on Vesuvius. In letters addressed to H.M. Consul at Naples, and dated December 17th, 1861 and January 3rd, 1862.

The evolution of gases,—the outburst of springs of acidulous and hot water,—and particularly the upheaval of the ground at Torre del Greco to a height of 1·12 mètre above the sea-level, are mentioned in this communication.

2. "On the Recent Eruption of Vesuvius." By M. Pierre de Tchihatcheff.

M. Tchihatcheff's observations were made at Torre del Greco and Naples from December 8th to 25th. Near Torre del Greco several small craters (9–12) have been formed close to each other in an E.N.E.–W.S.W. line, at a distance of about 600 mètres E.S.E. of the crater of 1794, and either on a prolongation of the old fissure or on one parallel. The phenomena mentioned by Signor Palmieri were also described by M. Tchihatcheff in detail.

3. "On Isodiametric Lines as means of representing the Distribution of Sedimentary (clay and sandy, Strata) as distinguished from Calcareous Strata, with especial reference to the Carboniferous Rocks of Britain." By E. Hull, Esq., B.A., F.G.S.

The author, in the first place, made a comparison of argillaceo-arenaceous with calcareous deposits, as to their distribution, both in modern and in ancient seas, and stated that he objected to calcareous strata being regarded as sediments, in the strict sense of the word. After noticing the distribution of sediments in the Caribbean Sea, he referred to the relative distribution of limestones as compared with shales and sandstones in the Oolitic formations (comparing those of Yorkshire with those of Oxfordshire), in the Permian strata of England, and in the Lower Carboniferous strata of Belgium and Westphalia. After some observations on the nature of calcareous deposits, and on the contemporaneity of certain groups of deposits, dependent on the oscillatory movements of land and sea, the author described his plan of showing on maps the relative thicknesses of the two classes of strata under notice, by means of isodiametric or isometric lines.

Mr. Hull then proceeded to indicate the application of the isodiametric system of lines to the Carboniferous strata of the midland counties and north of England; showing that there is a south-easterly attenuation of the argillo-arenaceous strata, and a north-westerly attenuation of the calcareous strata. The existence, in the Carboniferous Period, of a barrier of land crossing the British area, immediately to the north of lat. 52°, was insisted upon; and, although this barrier was probably broken through (in South Warwickshire) in the latter portion of that period, yet it divided, in the author's opinion, the coal-area into a north and a south portion, the latter having a very different set of directions in the attenuation of its strata—the shales and sandstones thinning out eastward, the limestones in the contrary direction.

In conclusion, the author stated that, in his opinion, the source of the Carboniferous sediments was in the ancient North Atlantic Continent, for the existence of which Lyell, Godwin-Austen, and others have argued; and he inferred that the shores of this *Atlantis*, composed principally of granitoid or metamorphic rocks, were washed on the west side by a current running S.W., which drifted the sediment in that direction, and on the other by a current running S.E., which carried sediment over the submerged British area.

XLVII. *Intelligence and Miscellaneous Articles.*

ON THE PROBABLE CAUSE OF ELECTRICAL STORMS.

BY DR. J. P. JOULE, F.R.S.

THE very close correspondence between the theoretical rate of cooling in ascending, and the actual, indicates a rapid transmission of the atmosphere from above to below, and *vice versâ*, continually going on. We may believe that during thunder-storms this interchange goes on with much greater than ordinary rapidity. At a considerable distance from the thunder-cloud, where the atmosphere is free from cloud, the air descends, acquiring temperature according to the law of convective equilibrium in dry air. The air then traverses the ground towards the region where the storm is raging, acquiring moisture as it proceeds, but probably without much diminution of temperature, on account of the heated ground making up for the cold of evaporation. Arrived under the thunder-cloud, the air rises, losing temperature, but at a diminished rate, owing to the condensation of its vapour to form part of the immense cumulus cloud which overcasts the sky on these occasions. The upward current of air carries the cloud and incipient rain-drops upwards, but presently, in consequence of the increased capacity of the mass from the presence of a large quantity of water, the refrigeration of the air in consequence of its dilatation will be so far diminished as to prevent the condensation of fresh vapour, and ultimately to redissolve the upper portion of the cloud. This phenomenon, which has been noticed by Rankine in the cylinder of the steam-engine, will account for the defined outline of the upper edges of cumulus clouds. The upward current no doubt extends occasionally to regions below the freezing temperature. If cloud be carried with it, snow or hail will be formed, which, if sufficiently abundant, will pass through the cloud and fall to the ground before it is melted. Now the dry cold air in which the snow and hail are formed is a perfect insulator. Ice has also been proved, by Achard of Berlin, to be a non-conductor and an electric. Even water, in friction against an insulator, is known, from the experiments of Armstrong explained by himself and Faraday, to be able to produce powerful electric effects; and this fact has been suggested by Faraday to explain powerful electric effects in the atmosphere. Sturgeon has noted the remarkable development of electricity by hail-showers. Few heavy thunder-storms occur without the fall of hail. Hail, whether in summer or winter, is almost, if not invariably, accompained with lightning. In the presence of these facts it seems not unreasonable to consider the formation of hail as essential to great electrical storms, although, as has been pointed out by Prof. Thomson, very considerable electrical effects might be expected from the negatively charged air on the surface of the earth being drawn up into columns, and although, as the same philosopher has observed, every shower of rain gives the phenomena of a thunder-storm in miniature. The physical action of insulators and electrics in mutual friction must certainly produce very marked effects on the grand scale of nature. If we suppose that the falling hail is electrified by the air it meets, the electrification of the cloud into which the hail falls might thus be constantly increased until the balance between it and the inductively electrified earth is restored by a flash of lightning. If the hail is negatively electrified by the dry air with which it comes in contact, the latter will float off charged with positive electricity, which may account for the normal positive condition of the atmosphere in serene weather, as well as the electrification of the upper strata evidenced by the aurora borealis. The friction of wind has been supposed by Herschel to contribute to the intense electrification of the cloud which overhangs volcanoes during eruption.—*From the Proceedings of the Literary and Philosophical Society of Manchester,* March 18, 1862.

ON THE INFLUENCE OF HEAT ON PHOSPHORESCENCE.

BY M. O. FIEBIG.

The author has investigated the deportment of several phosphorescent bodies in reference to heat—whether phosphorescence could be developed by heat alone, without the substance having been previously submitted to the action of light. The sulphides of calcium, of barium, and of strontium were prepared by the method of M. Becquerel, and their phosphorescence confirmed. These substances, observed in darkness, ceased to be luminous at the expiration of a certain time, and were then subjected to the calorific action of a plate heated something below redness. Phosphorescence reappeared, but after a second disappearance it could not be reproduced by the same method. A fresh exposure of the substance to light rendered the substance again phosphorescent.

An analogous experiment was made with a fragment of green fluoride of calcium. According to M. Becquerel, this substance becomes phosphorescent under the action of heat until it has lost colour; but in this condition it has lost the property. A strong elevation of temperature developed at first an intense violet light in the fluoride of calcium; after having been cooled it was again heated, but to a less extent than at first; the fragment remained quite dark, although it had retained its colour, which was seen by exposing it to daylight. In a third case it was strongly heated until decrepitation commenced; phosphorescence again appeared, and when viewed by daylight it had lost its colour. Nevertheless, heated afresh it again became luminous. These experiments show that fluoride of calcium possesses the property of becoming phosphorescent under the action of heat after a previous insolation, and that this property remains after the loss of colour.

M. Fiebig has also investigated the influence of heat upon the phosphorescence of two liquids, æsculine and quinine. When a solution of the former is gradually heated, the blue tint at first becomes deeper, and tends towards violet; it then becomes paler, and at about 50 degrees it can scarcely be distinguished from the ordinary tint. On continuing to heat it, the tint diminishes in intensity, becoming of a pale green. In the case of a solution of quinine, the tint diminishes considerably in intensity when near the boiling-point. In both liquids, cooling reproduces the ordinary colour.—Poggendorff's *Annalen,* October 1861.

RESISTANCE TO THE CONDUCTION OF HEAT.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I am indebted to Professor Clausius of Zürich for having pointed out an oversight in some tables of coefficients of resistance to the conduction of heat which appeared some time ago in a work of mine 'On the Steam-engine and other Prime Movers.' Those coefficients were computed from data given by M. Peclet; but in the computation the difference of the French and British units of weight (the kilogramme and the pound) was neglected. I now beg leave to send you the annexed Tables of the corrected values of those coefficients. Their meaning will be best understood by the aid of the following formula.

Conceive two media, whose temperatures, in degrees of Fahrenheit, are respectively T′ (the higher), and T (the lower), to be separated from each other by a layer of any given substance, whose thickness, *in inches*, is denoted by x.

Let q denote the number of British units of heat (degrees of Fahrenheit) in a pound avoirdupois of water transmitted from the hotter to the colder medium, *per square foot of surface, per hour*.

Then the *resistance* of the layer means the following quantity:—

$$\frac{T'-T}{q}=\rho x+\frac{1}{A\{1+B(T'-T)\}},$$

the first term being the internal resistance, and the second the superficial resistance.

Values of the coefficient of internal resistance, ρ.

Gold, Platinum, Silver	·0016
Copper	·0018
Iron	·0043
Zinc	·0045
Lead	·0090
Marble	·0716
Brick	·1500

Values of A.

Water on both sides of the conducting layer.	8·8
Water on one side, air on the other:—	
Polished metallic surfaces..........	0·90
Dull metallic surfaces	1·58
Glass and varnished surfaces	1·31
Surfaces coated with lampblack	1·74

Values of B.

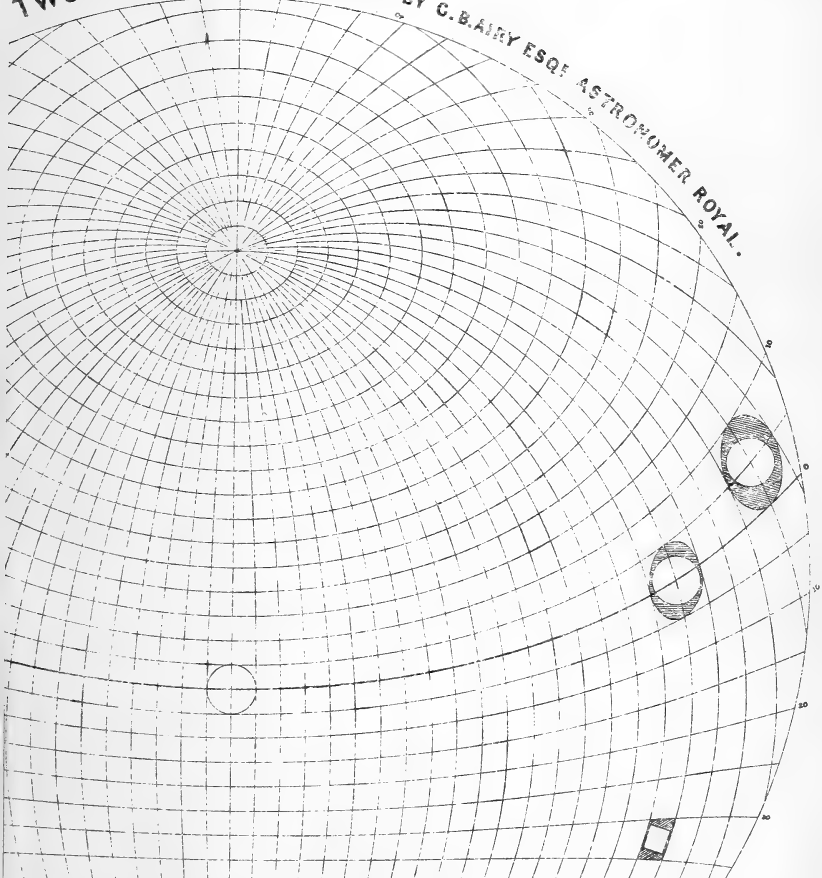
Water on both sides of the conducting layer.	0·0580
Water on one side, air on the other:—	
Polished metallic surfaces..........	0·0028
Rough and non-metallic surfaces....	0·0037

I am, Gentlemen,

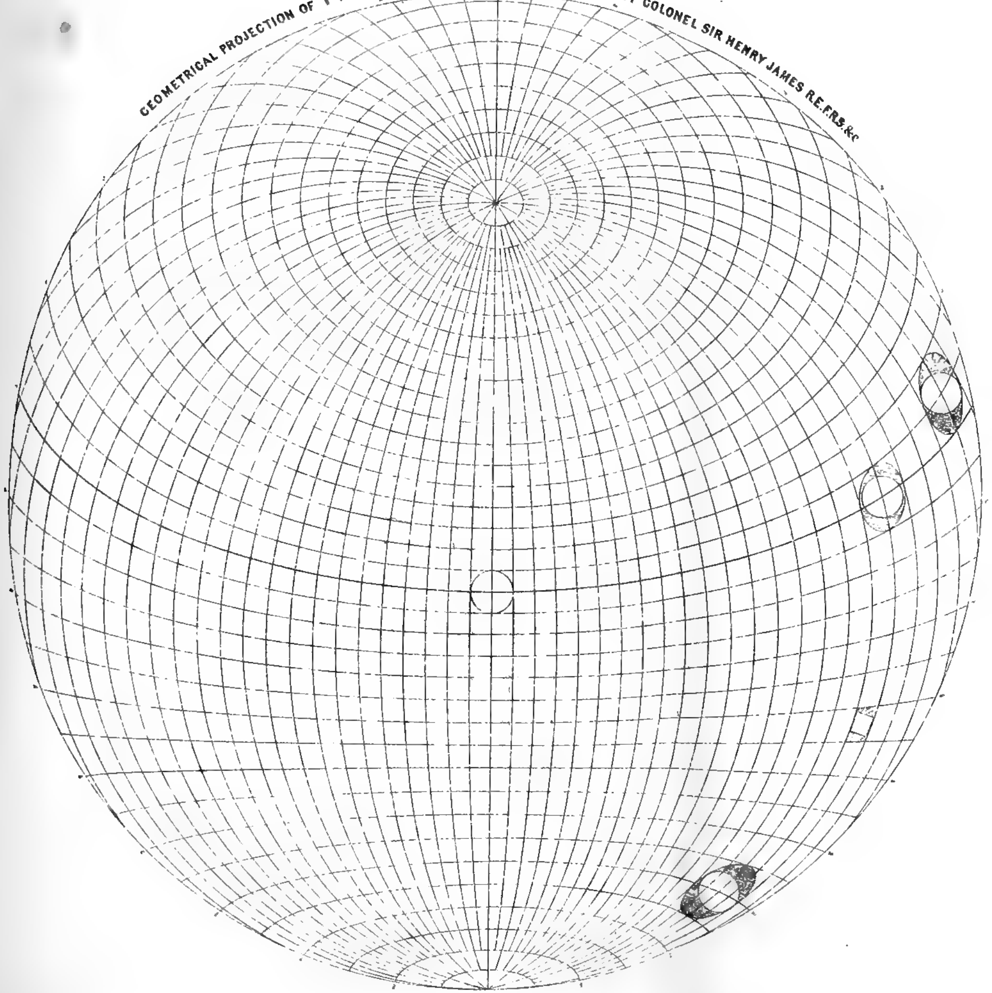
Your most obedient Servant,

Glasgow, March 17, 1862. W. J. MACQUORN RANKINE.

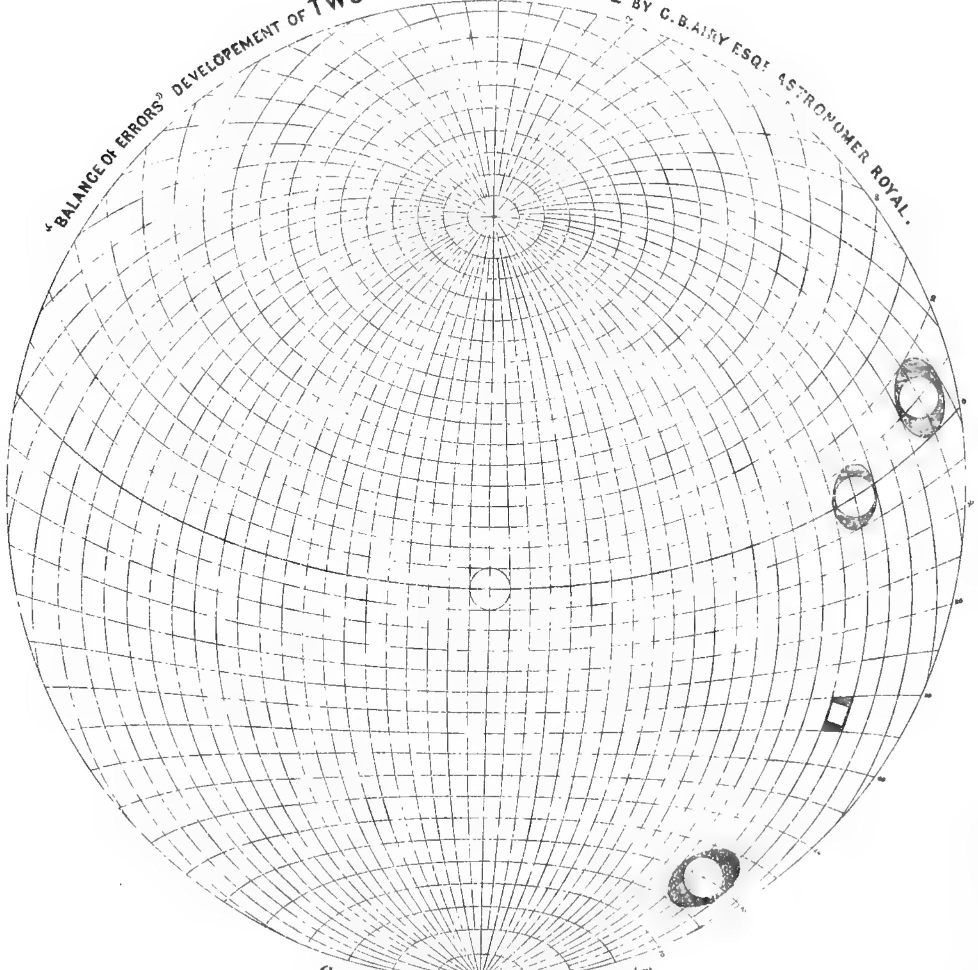
TWO-THIRDS OF THE SPHERE BY G. B. AIRY ESQ.R ASTRONOMER ROYAL.



Constructed from the corrected formulæ



GEOMETRICAL PROJECTION OF TWO-THIRDS OF THE SPHERE BY COLONEL SIR HENRY JAMES R.E. F.R.S. &c



"BALANCE OF ERRORS" DEVELOPEMENT OF TWO-THIRDS OF THE SPHERE BY G. B. AIRY ESQ.^E ASTRONOMER ROYAL.

Constructed from the corrected formulæ

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

MAY 1862.

XLVIII. *On some Apparatus for determining the Densities of Gases and Vapours.* *By* M. V. Regnault*.

[With a Plate.]

THE *density* of a gas or of a vapour at a temperature T and under a pressure H, is the relation between the weight P′ of a volume V of this gas or vapour, and the weight P of an equal volume of atmospheric air at the same temperature T, and under the same pressure H. If the gas or the vapour obeyed *exactly* the same laws of expansion and elasticity as atmospheric air, within the range of temperatures and pressures in which it retains the aëriform state, the density would be the same at all temperatures and under all pressures; it would constitute a specific character of the substance.

But experiment shows that this identity of laws does not exist even for the most permanent gases, for those which have not yet been liquefied. It shows further that the divergencies of the laws of expansion and compressibility are greater for vapours, and even that they often continue up to temperatures much higher than that at which the vapour would assume the liquid state under the same pressure. The density of a gas or of a vapour, as just defined, is therefore not represented by a constant number; it varies with the temperature and pressure, and these variations are often very considerable.

It is necessary in physics to define two kinds of densities for gases and vapours:—

1. The *real densities,* which vary with the temperature and pressure; they ought to be determined by numerous experiments, in which the temperatures and pressures are varied within considerable limits. These densities are represented by

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* Translated from the *Annales de Chimie et de Physique,* vol. lxiii. p. 45.

a function of the temperature and of the pressure, the numerical coefficients of which have been deduced from experiment; in other terms, the density is one of the coordinates of a surface, of which the two others represent the pressure and the temperature.

2. *Theoretical or limit densities.*—These are what would be found by operating on temperatures so high, and under pressures so feeble, that the gas or vapour would follow exactly the same laws of expansion and elasticity as atmospheric air under an increase of temperature or a diminution of pressure to which it is subjected. These *limit densities* obtain with all gases, and with all vapours when they are subjected to an extreme expansion, and if the temperature is sufficiently high. They are the only ones which could be of use in studying the composition of a compound gas expressed in volume relatively to the volumes of the simple gases of which it is formed.

Thus, when the densities of gases and vapours are determined simply with reference to the constitution of bodies, the limit density, or one nearly approaching it, ought to be sought. It is hence desirable that the apparatus with which densities are determined should be so constructed as to be easily used to ascertain whether if, starting from the temperature and pressure which prevailed in the apparatus during the experiment in which a density is to be deduced, the gas or vapour follows the same laws of expansion and elasticity as atmospheric air, for small variations of temperature and pressure to which it is subjected.

To satisfy these conditions in determining the density of a gas, and of that of the vapours of bodies which only boil at very high temperatures, I constructed some years ago the apparatus represented in figs. 12, 13, 14, Plate II. The receiver in which the gas or vapour is measured consists of a tube A B, 3 or 4 centimetres in diameter, terminating below in a tube B *c*, 2 centimetres in diameter, and above in a capillary tube A *b*, provided with a steel stopcock *r*. The lower tube is hermetically fixed in an apparatus of cast iron, *c d e f*. In the piece *c d e f* there is a second tubulure *e f*, in which the long open tube C D is fixed; *c d e f* is firmly screwed on an iron tripod M N P Q, which is furnished with levelling-screws V, V. A strong plate of sheet iron, *p q s*, the shape of which is seen in fig. 13, is fixed on *c d e f* by means of the screws *u*, *u* and of red-lead cement. Lastly, a semicylinder, *g h i*, of sheet lead, of which one side is a wrought-iron frame, E F G H (fig. 12), is fixed on the sheet-iron plate *x x x* by means of red-lead cement. The open face of the cylinder is closed by a pane of plate-glass, fixed by means of a second iron frame exactly like the first, fastened down by screws *z*, *z*, *z* (fig. 12). A plait of hemp, well stuffed with red lead, is interposed between the glass and the iron frames. This junction, which is made once for all, soon becomes hermetically tight.

The experiment is conducted in the following manner.

The sheet-lead casing with its transparent glass is raised from the sheet-iron base *p q s* after the screws *x, x, x* have been taken out. The tubes A B and C D are adjusted in their tubulures. Fig. 14, which represents a magnified vertical section of the piece *c d e f*, shows sufficiently the mode of adjustment. I will merely say that the tube B *c* or the tube C D is coated with a plait of hemp well stuffed with red-lead cement, and the plait is strongly screwed on the annular spaces *o o* by means of the screws K, L. The tubes A B, C D ought to be quite parallel. They are adjusted in a vertical position by moving the levelling-screws V, V.

I assume that the density of a gas is to be determined at various temperatures and under different pressures.

The stopcock R being in the position 3 (fig. 8), mercury is poured into the open tube C D, and a vacuum made by means of the air-pump which is fixed to the stopcock *r*. The vacuum is made several times, dry air being admitted each time. The object of this is to make the insides of the tube A B completely dry. The stopcock R is then gently turned into the position 1 (fig. 8), while the vacuum is still produced in the tube A B; the mercury from C D passes into the tube A B. The stopcock *r* is closed when the level of the mercury reaches the summit of the capillary tube A *b*. The tube *b m* is connected with a bell-jar full of the gas whose density is to be determined, or with the apparatus which disengages it in a state of purity; the stopcock *r* is opened, and mercury allowed to flow out from the tube A B by placing the stopcock R in the position 4 (fig. 8). The stopcock *r* is closed, and the stopcock R placed in the position 1 (fig. 8) as soon as enough gas has been introduced into the tube A B; the quantity varies according to the conditions of compression or of expansion under which the gas is to be studied.

The tubes A B and C D are divided into millimetres. In a preliminary experiment, the capacities of the tube A B corresponding to the divisions traced on the tube have been determined with great accuracy, by weighing the quantity of mercury which fills these capacities, and successively allowing them to run out by the stopcock R placed in the position 2 (fig. 8). The casing E F G H is now adjusted on the sheet iron *p q s*, and this case is filled with water at the temperature at which the experiment is to be made. This water is kept in continual motion by a stirrer, and its temperature is maintained quite uniform. By allowing mercury to flow out by the stopcock R, or by adding it to the tube C D, the same quantity of gas may be successively put under the pressures H, H′, H″, and the spaces V, V′, V″ noted which it occupies in the tube A B. The lowest pressure under which this apparatus could be worked would be that for which mercury would stop at D in the tube C D, and the highest would be that in which the mercury would stand at the top C of this tube. These limits might be indefinitely increased by connecting C D with a reservoir W, in which the air is either exhausted or compressed. The elastic force of the air in this reservoir is measured by a barometric pressure-gauge (fig. 15) when the air is expanded, or with an ordinary manometer when it is compressed. The mercury may also be kept at the same level in both tubes A B and C D.

These experiments may be repeated by raising the water in the bath successively to gradually higher temperatures, and keeping the temperature constant during each series of determinations. There are thus all the elements necessary for knowing the law of the compressibility of the gas for various temperatures.

The same experiments give all the data necessary for calculating the coefficient of expansion of gases under different pressures. The experiments may be made so as to determine the real expansion of the gas, the latter being always under the same pressure at different temperatures; or so as to measure the change which the elastic force of the gas undergoes for variations of temperature, its volume remaining the same.

To deduce from these experiments the density of a gas under different pressures and at different temperatures, its weight must be known. For this purpose different means may be employed, according to the chemical properties of the gas. The most general method consists in having a globe provided with a stopcock, which, by means of a capillary tube of platinum, silver, or copper, can be exactly fitted on the prolongation *b m* of the tube A B. Vacuum having been produced in this globe, the gas contained in the tube A B is passed into it, and the globe again weighed by the method of compensating-weights. The increase in the weight of the globe gives the weight of the gas. The gas can frequently be absorbed directly by chemical agents—for instance, carbonic acid, sulphurous acid, sulphuretted hydrogen, ammonia, &c. In this case the tube *b m* is connected with an apparatus containing the absorbing substance; the weight of the gas is indicated by the increase in weight of the absorbing substance. Lastly, in many cases the weight of gas may be determined by chemical analysis: thus, for the carburetted hydrogens and the various gases of organic chemistry, the gas from the tube A B is made to pass through an apparatus for organic analysis.

The same apparatus can be readily used for vapours. The liquid to be determined almost exactly fills a glass bulb, closed at the lamp, and the weight of which is known. The case E F G H being detached, the screw *c* (fig. 13) is loosened, and the tube A B removed. The bulb is placed in the tube A B, which is replaced in its original position and dried (as has been already described, p. 339), by exhausting and allowing dry air to enter. Lastly, the vacuum being continued, the stopcock R is placed in the position 1 (fig 8); the mercury completely fills the tube A B, and the stopcock *r* is closed when the mercury has reached it. The bath is replaced and filled with water, and its temperature raised by means of a lamp placed beneath it until the liquid by its expansion breaks the bulb. The mercury remains raised in the capillary tube A *b*, and hence neither the liquid nor its vapour come in contact with the stopcock *r*.

The water in the bath is gradually raised to various temperatures, and, by means of arrangements which I have already described, the volume is measured which the vapour occupies at different temperatures and under different pressures. The pressures may be made to vary from the lowest limits up to eight or ten atmospheres. We may thus determine at once, and within very extensive limits,—

1st. The laws of the compressibility of vapour at various temperatures.

2nd. Its coefficient of expansion at various degrees of the thermometric scale, the pressure remaining constant at different temperatures, while the volume of the vapour alone varies, the pressure varying within considerable limits.

3rd. The increase of the elasticity of the vapour in consequence of the increase of temperature, the volume remaining constant, and the original pressure varying within considerable limits.

4th. The real density of the vapour at different temperatures and pressures.

5th. The *limit density* of the vapour, which is that to which the real density constantly approximates when the pressure is diminished and the temperature raised.

The apparatus further gives the elastic forces of saturated vapours for various temperatures; for it is merely necessary to keep the pressure so that, while the temperatures gradually increase, liquid remains condensed on the surface of the mercury, though the vapour occupies part of the space of the tube A B.

Lastly, it may be used to measure the elastic forces of vapour, either saturated or not, in air, or in other gases, at various temperatures and pressures. For this purpose, the bulb having been introduced into the tube A B, air or the gas (dry) is allowed to enter by the stopcock *r*. The elastic force and the volume of the gas alone are measured at a known temperature T. The bulb is then broken, and a series of experiments made under varying temperature and pressure. From that, the elastic force of saturated vapour in gases for all cases in which liquid remains on the surface of the mercury, and the laws of the elasticity and expansion of the mixture of gases and of vapour at different degrees of saturation, may be deduced. Lastly, the elastic force of the vapour in the gas may be determined at the moment at which dew commences to be deposited in the tube A B. A large number of examples of these determinations will be found in vol. xxvi. of the *Mémoires de l'Académie.*

It often happens that the fragments of the broken bulb hinder the exact reading of the level of the mercury in the tube A B. This inconvenience is easily avoided by allowing the tube A B to terminate in a narrower part L, slightly spheroidal (fig. 16). The bulb is lowered to this cavity, and retained there by a small spiral of platinum; the remains of the bulb remain then almost entirely on the spiral.

We shall readily understand the advantages which this method presents over those hitherto used for determining vapours; for it furnishes at the same time a great number of other elements, a knowledge of which is necessary in order to know what use can be made of the density from the point of view of our chemical theories. It might be feared that it was only applicable to very volatile substances; for the temperature of the bath cannot much exceed 100°, even when the vessel is filled with a saline solution. But I must say that, for slightly volatile substances, which in general have high vapour-densities, it is especially interesting to determine their vapour-densities under very feeble pressures, because the *limit density* is thereby approximated to. Now it is always possible to realize these favourable conditions when the boiling-point does not exceed 200° under the ordinary atmospheric pressure.

Apparatus for determining the Vapour-density of Substances which boil at High Temperatures.

The very simple apparatus represented in figs. 10 and 11, Plate II., may be used for a small number of imperfectly volatile substances, the vapours of which do not readily alter in contact with the air. Two vessels, A, B, cast in iron of the same thickness, terminate in small tubes which are closed by bullets placed above. The capacities V and V′ of the two flasks are gauged, by ascertaining the weight of water which fills them. Mercury is poured into one of them, A; and in the other is placed the substance whose vapour-density is to be determined. The apparatus being thus arranged, is placed in a muffle heated to a high temperature. The substances soon begin to boil, expel the air, and escape by the tubes, which are very imperfectly closed by the ball. When the apparatus is of the same temperature as the muffle, it is withdrawn, and, after cooling, the weights of mercury and of the substance respectively in the flasks A and B are determined.

Let

P be the weight of mercury.

P′ that of the substance.

δ the density of mercury, compared with air under the pressure and at the temperature which prevailed in the muffle when the apparatus was withdrawn.

The density of vapour under the same conditions will be

$$x=\delta\frac{P'}{P}.$$

In my *Cours élémentaire de Chimie*, 5th edit. vol. iv. p. 66, I have given the arrangement of an apparatus analogous to that which M. Mitscherlich has employed for substances boiling at high temperatures; I simply endeavoured to obtain more equal temperatures for the air-thermometer and the vapour-tube by imparting a continual rotatory motion to the system of the two tubes in the muffle in which it is heated, and which has several metallic envelopes. This apparatus can be simplified and made more convenient now that the use of gas prevails in laboratories. It consists of three tubes of wrought iron closed at one end, and resembling gun-barrels; they are 50 centims. in length, and 20 millims. internal diameter. Fig 17 represents the longitudinal section of one of these tubes; A B is the part which is 50 centims. in length. On each of two of these tubes is screwed an additional piece B C of the same diameter, and on the second tube a narrower tube, C D. On the third tube, which is intended as a gas-thermometer, a single iron tube (fig. 18) B′ C′ is screwed, which is of almost capillary bore, and is terminated by a stopcock.

These three tubes, whose dimensions are quite similar, fit upon the same iron bar I I′, of which figs. 19 and 20 give a cross section, I. Fig. 19 shows by a section how the three iron tubes A are arranged in reference to the central bar I. The bar is longer than the iron tubes; it is firmly fixed on two cast-iron supports P P′ (fig. 21), arranged so that the bar is exactly on the notch. The three tubes are thus in a fixed position. This system of the three tubes is surrounded by a cylinder E F of copper or of sheet iron, which fits almost exactly, but so, however, that the cylinder may be made to rotate rapidly about a horizontal axis, I I′. A sheet-iron disc *a b c* (fig. 20), fitted on the central bar I I, and almost exactly filling the cylinder E F, forms the base of this cylinder, at a distance of a decimetre from the end A of the tubes A B. The other end of the cylinder is open; it corresponds to about the middle of the tube C B. This cylinder may be made to move rapidly about its axis by means of a rackwork, one of whose toothed wheels, F, is mounted at the end of the cylinder E F. A gas-furnace, like those used for organic analysis, serves to heat the cylinder to a strong red heat.

To make an experiment, the apparatus being arranged, a portion of the substance whose vapour-density is to be determined is introduced into each of the similar iron tubes, and the screw B put in its place. The air which fills the three tubes is first completely expelled; for this purpose pure dry hydrogen is allowed to enter by means of a capillary silver tube, which is introduced by the tube C D, until its open end touches the closed end A of the tube A B. When the air is expelled the silver tube is withdrawn, and by means of an india-rubber tube, C D, is connected with an apparatus which disengages hydrogen. In fine, the three iron tubes, including that which serves as gas-thermometer, remain during the rest of the experiment in connexion with an apparatus for disengaging hydrogen under the atmospheric pressure.

The gas-furnace is lighted, and its temperature raised as rapidly as possible; the substances converted into vapour expel the gas; the excess of vapour condenses in the half of the tube B C which is not contained in the heated metallic cylinder E F. In order to spread the heat uniformly over the three iron tubes, the heated metallic cylinder which surrounds the fixed system of tubes is continuously and rapidly turned.

The experiment is concluded by closing the stopcock of the gas-thermometer, extinguishing the lamps, and cooling the apparatus. The temperature of the gas-thermometer is determined by the method I have mentioned.

To ascertain the weight of vapour which filled the two other tubes at the moment of maximum temperature, the screws B are unfastened, and the substance which is condensed in the tube A B is determined by chemical methods.

The operation is much simpler when it is not attempted to determine the temperature by a gas-thermometer, but simply to seek the ratio of the density of the vapour of the substance to that of mercurial vapour, the two vapours being under the same circumstances of temperature and of pressure. It is simply necessary then to place mercury in one of the tubes A B, and to weigh the mercury which remains in the space A B after the experiment.

The volumes of the three tubes have been determined previously.

XLIX. *Note on the Correction for the Length of the Needle in Tangent-galvanometers.* *By* G. JOHNSTONE STONEY, *M.A., F.R.S., Secretary to the Queen's University in Ireland.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, Dublin, April 7, 1862.

PROFESSOR ZENGER, in the Supplement to the December Number of the Philosophical Magazine, which has just been published, couples my name in so pointed a manner with a formula which he criticizes, that I am compelled, though wholly averse from controversy, to ask you to be so good as to insert the following note in reply.

I am, Gentlemen,
Yours faithfully,
G. JOHNSTONE STONEY.

Until I read Professor Zenger's remarks*, I was under the impression that the formula

$$i = \mathrm{K} \tan\theta\left(1 + \frac{15}{4}\lambda^2 \sin^2\theta\right),$$

which is a necessary result of the known laws of electro-magnetism, had been, either in this or some equivalent form, long adopted by physicists as the expression which furnishes the correction arising from the length of the needle of a tangent-galvanometer of the usual pattern, on the hypothesis that λ (the *ratio* of the distance between the poles of the needle to the diameter of the circular current†) is sufficiently small to sanction our neglecting its fourth power.

Prof. Zenger does me too much honour in supposing me the author of this formula. I do not know by whom it was originally investigated; but it had been, as I mentioned‡ at the commencement of the paper which he criticizes, known to, and extended by, other writers before me. What I sought to contribute to our knowledge of the subject was an examination from which it appeared that *no alteration* of the formula becomes necessary when the ordinary tangent-galvanometer is *out of adjustment* in that slight degree likely to occur in practice; whereas the simpler formula for Gaugain's galvanometer requires, under similar circumstances, corrections which it would be difficult to apply.

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* Philosophical Magazine, Supplement for December 1861, p. 529.

† Professor Zenger seems to misunderstand this symbol. See his definition of λ on the top of page 530.

‡ Phil. Mag. February 1858, p. 135.

After quoting this formula, the Professor proceeds to deduce one which he proposes to substitute for it; but as his fundamental assumption, $p' = p \,.\, f(\delta)$, is at variance with the familiar fact that the action of the element of a current on a magnet depends on its inclination as well as its distance, it is not necessary to scrutinize another hypothesis inconsistent with that just stated, which he afterwards introduces*.

The formula thus obtained through a disregard of the laws ascertained by Ampère, is supported by experiments which appear equally independent of those due to Ohm. The *internal* resistance of an electromotor was varied by immersing the plates successively to different depths, and observations are recorded with the assumption that the intensity of the current changed in the same ratio, although there does not seem to have been any alteration made of the *external* resistance. I need not, then, contest other parts of the experiment, although I believe it would be difficult to render the method by which the change of internal resistance was estimated practically trustworthy; nor need I dwell on the peculiarity of testing the established formula, *expressly limited to cases in which the needle is sufficiently short to warrant our neglecting the fourth power of* λ, by making experiments with a galvanometer the needle of which had, to use Prof. Zenger's description of it, an "enormous length."

Under all these circumstances, it can scarcely be matter of surprise that no accordance was found between the observations and the established formula: and we seem compelled to regard the moderate agreement which was obtained between an erroneous formula and faulty experiments as a coincidence without scientific import.

Although the topic is quite unconnected with Prof. Zenger's remarks, whom the error seems to have escaped, I may be allowed to avail myself of this opportunity to correct a mistake in my paper, to which Prof. Curtis of Queen's College, Galway, was so good as to direct my attention some months ago. Having examined the corrections occasioned by a derangement from its intended position of the magnetic centre of the needle, regarded as the point round which the needle rotates, I endeavoured†, by a merely geometrical process, to include the error which a separation between the point of suspension and the magnetic centre would introduce. This was done under the mis-

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* Professor Zenger's equation,

$$S : S' = AN'^2 : AN^2$$

(see p. 530), is mathematically inconsistent with his former hypothesis. It is equally inconsistent with Ampère's law.

† Phil. Mag. February 1858, p. 138.

apprehension that the moment which arises is of a higher order of small quantities than those included in the rest of the investigation, whereas it is of the same order. Accordingly another term, with δ for its coefficient, and therefore of the second order, needs to be added to the general equation numbered (5) in my paper. This term is easily calculated: but, without being at the pains of going through the work, it is easy to see that it will behave exactly as the other small terms which have been included. In fact the new term arises from the difference of the action of the current on the two poles of the needle; and as this difference would gradually decrease to *nil* if the centre of the needle were moved from the position it occupies in Gaugain's galvanometer into the position it has in the ordinary galvanometer, it is plain *à priori* that the most considerable term of the series expressing the moment (which is of the second order of small quantities) vanishes as the needle approaches the latter position.

Hence no term of the second order needs to be added to formula (6), which has reference to the common galvanometer; but a new term of that order should be added to formula (7), which relates to Gaugain's instrument,—thus leaving the conclusion at which I arrived undisturbed, that, "though in Gaugain's galvanometer we get rid of the trouble of applying a correction for the length of the needle, it is necessary to attend carefully to the position of the needle in its cradle, and to the horizontal adjustments of the point of suspension, lest errors should creep in of which it would be impossible to make any exact estimate;" and that "in conducting investigations in which accuracy is a point of much importance, the ordinary form of tangent-galvanometer is to be preferred." (Phil. Mag. February 1858, p. 139.)

It may be well to add that the small moments arising from the other defects of adjustment which could exist (such as a slight *lateral* displacement of the axis of rotation from the line joining the poles, or a slight *dipping* of the needle, either when at the meridian, or from the state of equipoise ceasing when it deviates from the meridian) all yield to a precisely similar treatment, and all corroborate the same conclusion.

This remark may be extended to the effects of small deviations of the current-wire from the circular form, which therefore do not ever sensibly disturb the law of the common tangent-galvanometer, but might be such as would render Gaugain's instrument inaccurate.

L. *On the Position of Lievrite in the Mineral Series.* By E. J. Chapman, *Professor of Mineralogy and Geology in University College, Toronto, Canada West*.*

Much uncertainty still prevails regarding the true composition of Lievrite or Ilvaite. The earlier analyses of this mineral, those of Vauquelin and Collet-Descotils, made the substance essentially a silicate of sesquioxide of iron and lime. Stromeyer's analysis, which followed those of the above-named chemists, gave the iron, on the other hand, as protoxide. A subsequent examination by Von Kobell disclosed the presence of both oxides, as fully established by the later analyses of Rammelsberg and others. These analyses, the correctness of which it is impossible to doubt, do not lead, however, to any general formula; nor can any formula of a satisfactory character, in a mineralogical point of view, be obtained from them. It will be as well to quote their results before proceeding with our inquiry. A is Stromeyer's analysis, as calculated by Von Kobell; B, that of Rammelsberg; C, Wackernagel's; and D, Francke's,—all of Elba specimens. E is an analysis by E. Tobler, of a specimen from Herbornseelbach in Nassau. (*Ann. Chem. und Pharm.* vol. xcix. p. 122. Also Rammelsberg's *Mineral-Chemie*, p. 740.)

	A.	B.	C.	D.	E.
Silica	29·28	29·83	29·45	29·61	33·30
Sesquioxide of iron......	23·00	22·55	25·78	21·09	22·57
Protoxide of iron	31·90	32·40	28·60	32·71	24·02
Protoxide of manganese..	1·43	1·50	0·94	1·55	6·78
Lime	13·78	12·44	15·49	14·47	11·68
Alumina	0·61				
Water	1·27	1·60	....	....	1·12
	101·27	100·32	100·26	99·43	99·47

Rammelsberg deduces from the above the following oxygen-ratios, and calculates from these the formulæ given below, truly stating, however, at the outset, "es geht hieraus das wahre Verhältniss nicht mit Sicherheit hervor."

	RO.	R^2O^3.	SiO^2.		
A......	11·33	6·90	15·21 ..	=4·9 : 3 : 6·6=	9·8 : 6 : 13·2
B......	11·08	6·76	15·49 ..	=4·9 : 3 : 6·9=	9·8 : 6 : 13·8
C......	11·01	7·74	15·29 ..	=4·2 : 3 : 5·9=	8·4 : 6 : 11·8
D......	11·75	6·33	15·37 ..	=5·6 : 3 : 7·3=	11·2 : 6 : 14·6
E......	10·19	6·77	17·29 ..	=4·5 : 3 : 7·7=	9·0 : 6 : 15·4

I. $5(2RO, SiO^2)+2(Fe^2O^3, SiO^2)$. This requires the oxygen-ratio, 10 : 6 : 14.

II. $3RO, 2Fe^2O^3+6(RO, SiO^2)$. This assumes the sesquioxide to play an electro-negative part, and requires the oxygen-ratio 9 : 6 : 12.

III. $2(2RO, SiO^2)+Fe^2O^3, SiO^2$. This formula requires the oxygen-ratio 4 : 3 : 6.

IV. $9(2RO, SiO^2)+2(2Fe^2O^3, 3SiO^2)$. This exacts the oxygen-ratio 18 : 12 : 30.

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* Communicated by the Author.

Apart from this want of concordance, these formulæ do not serve to connect our mineral with other species of kindred character. In other words, they fail to present any satisfactory indication of the place which the Lievrite should occupy amongst the silicates generally. Reflecting upon this, it occurred to me that the true composition of the mineral might be arrived at by tracing out its mineralogical affinities. If this seem paradoxical, it must be remembered that purely mineralogical considerations have taken the initiative on more than one occasion in the solution of obscure questions connected with mineral chemistry. Whilst, for example, the various garnets, the different varieties of pyroxene, &c., were still kept apart by the chemist who adhered to chemistry alone, mineralogy insisted upon their union, and thus led the way to the recognition of isomorphism. If the relationship of Lievrite to a mineral of known composition can be clearly shown, a great assistance will at least be afforded towards the deduction of its true atomic character.

On comparing this mineral with other silicates, one cannot help being struck by the remarkable correspondence existing between it and chrysolite, at least as regards the so-called Fayalite and other iron-holding varieties of the latter. It is curious that this coincidence should hitherto have escaped attention. The gelatinization of the silica in acids, a very peculiar character in the case of anhydrous silicates, is exhibited by the two species in common. Their conditions of occurrence are also more or less identical; the form in each is trimetric, with axial relations in part corresponding; and each contains an unusually low average of silica. This amounts in hyalosiderite and Fayalite to about 30 per cent. The same also in Lievrite. Dana places the latter mineral (though doubtfully) in his ANDALUSITE GROUP, with Andalusite, topaz, and staurolite; but a collocation of this kind is an exceedingly forced one. Geological relations (an element in mineralogical classification of the highest importance, although hitherto strangely overlooked), composition, and general characters are all opposed to it. The form, it is true, is trimetric, with some remote analogy, as shown by Dana, to that of Andalusite; but since we find such opposite minerals as augite and borax, for example, exhibiting an identity of crystallization, no great stress can be placed on this character alone. There is an equal amount of crystallographic correspondence, moreover, between Lievrite and chrysolite, and in other respects the two present a close agreement. In chrysolite several vertical prisms are known. If we denote the protaxial prism by the symbol V, and make the macrodiagonal unity, these forms give for the brachydiagonal the following values:—0·4660 (=V; prism-angle, 130° 2′); 0·9484 (=V 2; prism-angle, 93° 3′); 1·397 (=V 3;

prism-angle, 71° 10′); 1·684 (= V 4; prism-angle, 61° 47′). In Lievrite, the two commonly occurring prisms give respectively 0·6840 (=V; prism-angle, 111° 12′) and 1·370 (=V 2; prism-angle, 72° 16′). To make these correspond with the chrysolite values, the first prism must bear the symbol $V\frac{3}{2}$, and the second V 3. The vertical axes stand to each other very nearly in the ratio of 5 to 4. But we need not attempt to push these analogies beyond their legitimate limits. The affinity in general characters and conditions of occurrence of Lievrite to Fayalite (and through this latter to chrysolite) cannot be overlooked if we take the entire relations of these substances into consideration. Single characters, in the determination of analogies, are necessarily useless.

Now the atomic constitution of the chrysolites is perfectly well established. The oxygen atoms in base and acid are equal, and the base consists of simple oxides only, the general formula being $2(RO), SiO^2$; or two atoms of monoxidized base + one atom of silica. In the normal chrysolites the base consists of magnesia; in the olivines, of magnesia and protoxide of iron; and in Fayalite (at least, essentially) of the latter oxide alone. In Lievrite, on the other hand, as shown above, both protoxide and sesquioxide of iron are present; and the oxygen-atoms of the silica do not correspond with those of the bases.

In attempting to reconcile these discrepancies, I called to recollection a fact that came under my notice some time ago. In examining a specimen of Lievrite that had been broken up into small pieces, I found, that certain fragments exerted a much more powerful influence on the magnet than others. Whilst some of the particles scarcely showed a trace of magnetism, others were magnetic in a marked degree. These latter, tested by the blowpipe, seemed almost free from silica; whilst the feebly-magnetic fragments gave very readily, with microcosmic salt, the well-known reaction of that substance. It occurred to me, therefore, that the analysed specimens of Lievrite might have contained a certain portion of magnetic iron ore,—a circumstance easily conceivable, if we call to mind the geological associations of our mineral. The nearly uniform proportions of the FeO and $Fe^2 O^3$ found in the various analyses seemed, it is true, opposed to this idea; but it appeared at least possible that the crystallized specimens might be able to take up a certain proportion, and no more, of the magnetic oxide. I calculated therefore, from the first four analyses given above, the mean composition of Lievrite, and reduced this, with the following results, to 100 parts:—

Silica	29·54	= 29·66
Sesquioxide of iron	23·08	= 23·25
Protoxide of iron	31·25	= 31·49
Protoxide of manganese	1·35	= 1·46
Lime	14·04	= 14·14
	99·26	100·00

Now 23·25 parts of $Fe^2 O^3$ require 10·46 parts of FeO to form magnetic iron ore ($=FeO, Fe^2 O^3$). This, deducted from 31·49, leaves 21·03 for the protoxide of iron present (on the above supposition) in the silicate. The analysis consequently stands as below :—

Silica	29·66	= 44·75
Protoxide of iron	21·03	= 31·72
Protoxide of manganese	1·46	= 2·20
Lime	14·14	= 21·33
	66·29	100·00

Calculating the oxygen-ratios of the above, we obtain—

SiO^2	23·24 = $1\frac{3}{4}$ = 7 = 14
FeO MnO CaO	13·59 = 1 = 4 = 8

Here, then, we have 8 atoms of RO to 14 atoms of SiO^2, in place of 2 to 1, as required by the chrysolite formula. If the oxygen-ratios in any of the above analyses had come out as 9 : 6 : 14, we might have written the formula

$$7(RO, SiO^2) + 2(FeO, Fe^2 O^3) ;$$

but the analyses do not yield these values. The view, therefore, as suggested above, that the peculiar composition of Lievrite may arise from the presence of magnetic iron ore, cannot be sustained.

But the formula of Lievrite may be brought to coincide with that of the chrysolite series by assuming the iron to have been originally present in the condition of FeO only. If this be assumed—and the assumption is in part warranted by the well-known fact that Lievrite is especially subject to alteration, the conversion of the FeO into $Fe^2 O^3$ still going on in many specimens—the difficulty is at once removed. Taking the mean composition as given above, and calculating the weight of FeO corresponding to the 23·25 per cent. of $Fe^2 O^3$, we obtain 20·92. Adding this to the FeO, and correcting the whole to 100 parts, the analysis reads as follows :—

		Oxygen.	
SiO^2 . . .	30·47		15·81
FeO . . .	53·66	11·91	16·33
MnO . . .	1·40	·31	
CaO . . .	14·47	4·11	

Although these values do not come out exactly equal, they lead evidently to the common chrysolite formula 2(RO), SiO^2. If we adopt, consequently, the assumption on which the above calculation is based, the Lievrite falls naturally into the mineralogical group to which it undoubtedly belongs; whereas on the other view, founded on the bare results of analysis, not only does the atomic constitution of the mineral remain uncertain, but its composition fails to harmonize with its physical characters and conditions. The suggestion, therefore, embodied in this brief notice may not be found altogether unworthy of consideration by those engaged in the study of mineral analogies.

LI. *On a Question in the Theory of Probabilities.*

By A. Cayley, *Esq.*[*]

It is, I think, very desirable to further consider the question in Probabilities proposed by Prof. Boole in the Cambridge and Dublin Mathematical Journal in the year 1851. The question was originally stated as follows:—"If an event E can only happen as a consequence of some one or more of certain causes $A_1, A_2 \ldots A_n$, and if generally c_i denote the probability of the cause A_i, and p_i the probability that if the cause A_i exist the event E will happen, then, the series of values $c_1, c_2 \ldots c_n$, $p_1, p_2 \ldots p_n$ being given, required the probability of the event E."

Considering only the causes A and B, the proposed question may be considered as being—

"If the event E can only happen as a consequence of one or both of the causes A and B; and if α be the probability of the existence of the cause A, p the probability that, the cause A existing, the event E will (whether or not as a consequence of A) happen; and in like manner if β be the probability of the existence of the cause B, q the probability that, the cause B existing, the event E will (whether or not as a consequence of B) happen: required the probability of the event E."

This, which is strictly equivalent to Prof. Boole's mode of stating the question, may for convenience be called the *Causation* statement. But his solution, presently to be spoken of, is rather a solution of what may be termed the *Concomitance* statement of

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[*] Communicated by the Author.

the question: viz., if for shortness we use AE to denote the compound event A and E, and so in other cases; and if we use also A′ to denote the non-occurrence of the event A, and so in other cases (of course (AE)′, which denotes the non-occurrence of the event AE, must not be confounded with A′E′, which would denote the non-occurrence of each of the events A, E), then the question is, "Given

$$\begin{aligned} \text{Prob. } A'B'E, &= 0, \\ \text{,, } \quad A \ , &= \alpha, \\ \text{,, } \quad AE, &= \alpha p, \\ \text{,, } \quad B \ , &= \beta, \\ \text{,, } \quad BE, &= \beta q; \end{aligned}$$

required the probability of E." To show that the two statements are really distinct questions, it may be observed that when A and B both exist, then, according to the causation statement, they may one or each of them act efficiently, and E may thus happen as an effect of one of them only, or as an effect of each of them; but, according to the concomitance statement, E cannot be attributed rather to one of the events A, B, than to the other of them, or to both of them.

The solution which I gave in the year 1854 (Phil. Mag. vol. vii. p. 259) refers to the causation statement of the question, and *assumes the independence of the two causes**; and on this assumption I believe it to be correct. And I remark, in passing, that in the strictest sense of the word *cause*, all causes are *ex vi termini* independent. The solution was as follows:—Let u be the required probability; λ the probability that A acting, it will act efficiently; μ the probability that B acting, it will act efficiently; then we have

$$\begin{aligned} u &= \lambda\alpha + \mu\beta - \lambda\mu\alpha\beta, \\ p &= \lambda + (1-\lambda)\mu\beta, \\ q &= \mu + (1-\mu)\lambda\alpha; \end{aligned}$$

and eliminating λ, μ from these equations, we have the required probability u.

As I did not further work out the solution, I omitted to state the relations of inequality presupposed among the data α, β, p, q, or to show how the sign of the quadratic radical in the resulting expression for u was to be fixed. The omissions in question were supplied by Dr. Dedekind, in his paper "Bemerkungen zu einer Aufgabe der Wahrscheinlichkeitsrechnung," *Crelle*, vol. l.

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* It is part of the assumption, that the causes do not combine to produce the effect: viz. if they both act, the effect is not produced unless one of them acts efficiently; they may or may not each of them act efficiently.

p. 268, 1855. In fact, writing for shortness $\alpha'=1-\alpha$, &c., we have

$$u-\alpha p=\beta\alpha'\mu,$$
$$u-\beta q=\alpha\beta'\lambda;$$

and thence

$$\beta'+\beta q-u=\beta'(1-\alpha\lambda),$$
$$\alpha'+\alpha q-u=\alpha'(1-\beta\mu),$$

which gives

$$(\alpha'-\alpha p-u)(\beta'+\beta q-u)=\alpha'\beta'(1-u);$$

that is,

$$u^2-u(\alpha'+\alpha p+\beta'+\beta q-\alpha'\beta')+(\alpha'+\alpha p)(\beta'+\beta q)-\alpha'\beta'=0;$$

or, what is the same thing,

$$u^2-u(1-\alpha\beta+\alpha p+\beta q)u+\alpha'\beta q+\alpha\beta' p+\alpha\beta pq=0;$$

and thence

$$u=\tfrac{1}{2}(1-\alpha\beta+\alpha p+\beta q-\rho),$$

where

$$\rho^2=(1-\alpha\beta+\alpha p+\beta q)^2-4(\alpha\beta' p+\alpha'\beta q+\alpha\beta pq),$$

which may also be written

$$\begin{aligned}\rho^2&=(\alpha'-\alpha\beta'+\alpha p-\beta q)^2+4\alpha\alpha'\beta' p',\\&=(\beta'-\beta\alpha'-\alpha p+\beta q)^2+4\beta\beta'\alpha' q',\\&=(1-\alpha\beta+\alpha p-\beta q)^2-4\alpha\beta'(p-\beta q),\\&=(1-\alpha\beta-\alpha p+\beta q)^2-4\alpha'\beta(q-\alpha p);\end{aligned}$$

and we then have

$$\beta\alpha'\mu=\tfrac{1}{2}(1-\alpha\beta-\alpha p+\beta q-\rho),$$
$$\beta'\alpha\lambda=\tfrac{1}{2}(1-\alpha\beta+\alpha p-\beta q-\rho).$$

As probabilities, α, β, p, q are all of them positive and less than unity (so that α', β', p', q' are all positive); u, λ, μ, which are also probabilities, must likewise be positive, and less than unity: and in order that this may be so, it is necessary and sufficient that

$$p\nless\beta q,\quad q\nless\alpha p,$$

and that ρ shall denote the *positive* square root of the above-mentioned value of ρ^2. The solution is therefore inapplicable, unless the data are such that

$$p\nless\beta q,\quad q\nless\alpha p.$$

It may be added that, the values of λ, μ being known, the solution gives the probabilities of all the compound events ABE &c.: thus

$$\begin{aligned}
\text{Prob. } A'B'E', &= \alpha'\beta', \\
\text{,, } AB'E, &= \alpha\beta'\lambda, \\
\text{,, } AB'E', &= \alpha\beta'\lambda', \\
\text{,, } A'BE, &= \alpha'\beta\mu, \\
\text{,, } A'BE', &= \alpha'\beta\mu', \\
\text{,, } ABE, &= \alpha\beta(\lambda+\mu-\lambda\mu), \\
\text{,, } ABE', &= \alpha\beta\lambda'\mu'.
\end{aligned}$$

It will be remembered that

$$\text{Prob. } A'B'E = 0.$$

Prof. Boole's solution, which is inconsistent with the foregoing one, is given in his well-known work, 'An Investigation of the Laws of Thought,' &c. Dublin, 1854, p. 321 *et seq.* Although given as a solution of the causation statement of the question, as already remarked, it seems to be (and I think Prof. Boole would say that it is) a solution of the concomitance statement of the question. It is certainly a most remarkable and suggestive one; I am strongly inclined to believe that it is correct; which of course does not interfere with the correctness of my solution, if the two really belong to distinct questions.

I reproduce Prof. Boole's solution, without attempting to explain (indeed I do not understand to my own satisfaction) the logical principles upon which it is based. It is conducted by means of the auxiliary quantities x, y, s, t, which are quantities replacing logical symbols originally represented by the same letters. I will designate these quantities, without attempting to explain the meaning of the term, as Boolian Probabilities, viz.

$$\begin{aligned}
\text{Boolian Prob. } A &= x, \\
\text{,, } AE &= s, \\
\text{,, } B &= y, \\
\text{,, } BE &= t;
\end{aligned}$$

and, as before, x', &c. are used to denote $1-x$, &c. The required probability of E is taken to be u.

The event A can, by the data of the question, only happen in concomitance as follows: viz. the concomitant events are

	Boolian Probs.
A . AE . B . BE	$x\,s\,y\,t$
A . AE . B′ (BE)′	$x\,s\,y'\,t'$
A . (AE)′ . (BE)′	$x\,s'\,t'$,

which may be analysed as follows: viz. if A and AE, then either B and BE or B′ and (BE)′; but if A and (AE)′, then (B or B′ but of necessity) (BE)′. And this being so, the sum

$$xsyt+xsy't'+xs't'$$

is assumed to be proportional to the given probability α of the event A; viz. the ratio in question is equal to that of the corresponding sum for all the possible events to unity.

The entire series of possible events is

	Boolian Probs.
$A\,.\,AE\,.\,B\,.\,BE$	$x\,s\,y\,t$
$A\,.\,AE\,.\,B'.(BE)'$	$x\,s\,y't'$
$A'.(AE)'.B\,.\,BE$	$x's'y\,t$
$(AE)'\,.\,(BE)'$	$s't'$,

which may be analysed as follows: viz. if AE and BE, then of necessity A and B; if AE and (BE)′, then of necessity A and B′; if (AE)′ and BE, then of necessity A′ and B; but if (AE)′ and (BE)′, then at pleasure A and B or A and B′, or A′ and B or A′ and B′: the sum proportional to unity therefore is

$$xsyt+xsy't'+x's'yt+s't'.$$

Now in the same manner as with α, dealing with the remaining given quantities αp, β, βq, and with the required quantity u, we have

$$\left.\begin{aligned}&\frac{xsyt+xsy't'+xs't'}{\alpha}\\ &=\frac{xsyt+x's'yt+s'yt'}{\beta}\\ &=\frac{xsyt+xsy't'}{\alpha p}\\ &=\frac{xsyt+x's'yt}{\beta q}\\ &=\frac{xsyt+xsy't'+x's'yt+s't'}{1}\\ &=\frac{xsyt+xsy't'+x's'yt}{u};\end{aligned}\right\} \quad . \quad . \quad (L)$$

each of which is also

$$\begin{aligned}&=\frac{x's'y't'}{A'B'E'}\\ &=\frac{xsy't'}{AB'E}\\ &=\frac{xs'y't'}{AB'E'}\\ &=\frac{x's'yt}{A'BE}\end{aligned}$$

$$=\frac{x's'yt'}{\mathrm{A'BE'}}$$

$$=\frac{xsyt}{\mathrm{ABE}}$$

$$=\frac{xs'yt'}{\mathrm{ABE'}};$$

where, to avoid multiplication of symbols, I have used A′B′E, &c. to denote the probabilities of the compound events A′B′E′, &c. if these probabilities should be sought for.

We have thus five equations to determine x, y, s, t, u; these equations give

$$\frac{x's'ty}{u-\alpha p}=\frac{xst'y'}{u-\beta q}=\frac{s't'}{1-u},$$

and

$$\frac{x's't'}{\alpha'+\alpha p-u}=\frac{s'y't'}{\beta'+\beta q-u}=\frac{stxy}{\alpha p+\beta q-u};$$

and we have thence

$$(u-\alpha p)(u-\beta q)(1-u)=(\alpha'+\alpha p-u)(\beta'+\beta q-u)(\alpha p+\beta q-u).$$

Putting, for shortness,

$$\alpha p=a,$$
$$\beta q=b,$$

$$\alpha'+\alpha p=f,$$
$$\beta'+\beta q=g,$$
$$\alpha p+\beta q=h,$$

the equation in u is

$$-(u-1)(u-a)(u-b)$$
$$+(u-f)(u-g)(u-h)=0;$$

viz. it is

$$u^2(1+a+b-f-g-h)-u(a+b+ab-fg-fh-gh)+ab-fgh=0;$$

or, since $h=a+b$, this is

$$u^2(1-f-g)-u\big(ab-fg+h(1-f-g)\big)+ab-fgh=0,$$

which is easily transformed into

$$u^2.(\alpha p'+\beta q'-1)$$
$$-u.\{(\alpha p'+\beta q'-1)(\alpha p+\beta q+1)+\alpha\beta(pq-p'q')\}$$
$$+\alpha\beta pq-(\alpha'+\alpha p)(\beta'+\beta q)(\alpha p+\beta q)=0.$$

And we then have

$$u=\frac{(\alpha p'+\beta q'-1)(\alpha p+\beta q+1)+\alpha\beta(pq-p'q')+\sigma}{2(\alpha p'+\beta q'-1)},$$

where

$$\sigma^2 = \{(\alpha p' + \beta q' - 1)(\alpha p + \beta q + 1) + \alpha\beta(pq - p'q')\}^2$$
$$- 4(\alpha p' + \beta q' - 1)\big(\alpha\beta pq - (\alpha' + \alpha p)(\beta' + \beta q)(\alpha p + \beta q)\big).$$

In order that u may represent the required probability, it is necessary and sufficient that it shall be

$$\nless \text{ each of } \alpha p,\ \beta q,$$
$$\ngtr \quad ,, \quad \alpha' + \alpha p,\ \ \beta' + \beta q,\ \ \alpha p + \beta q;$$

which implies that each of the three quantities is not less than each of the two quantities; or, what comes to the same thing, $\beta' + \beta q \nless \alpha p$, $\alpha' + \alpha p \nless \beta q$. So that the solution is only applicable if

$$\alpha' + \alpha p \nless \beta q, \quad \beta' + \beta q \nless \alpha p;$$

and these conditions being satisfied, σ must denote the *positive* square root of the above-mentioned value of σ^2.

The values of x, y, s, t are readily obtained in terms of u; the equations in fact give

$$x = \frac{\alpha p'}{u'}, \quad y = \frac{\beta q'}{u'}, \quad \frac{s}{s'} = \frac{u'(u - \beta q)}{\alpha p'(\beta' + \beta q - u)}, \quad \frac{t}{t'} = \frac{u'(u - \alpha p)}{\beta q'(\alpha' + \alpha p - u)},$$

where $u' = 1 - u$. And we thus have not only u, the probability of the event E, but the probabilities of all the compound events A'B'E', &c. I remark that the probabilities of the compound events A'B', A'B, AB', AB will *not* be $\alpha'\beta'$, $\alpha'\beta$, $\alpha\beta'$, $\alpha\beta$.

It is to be noticed that the conditions of applicability are different for the two solutions; this, however, does not remove the contradiction, as there are values of α, β, p, q which satisfy each of the two conditions.

An interesting particular case is when $p = 1$; that is, when A happening, E is certain to happen.

First for my solution. The conditions are satisfied (β, q being each less than unity) if only $q \nless \alpha$. The meaning of this is clear; for if B happen, then q (the probability of E) is equal to α (the probability of A) *plus* the probability that A not happening, E will happen in consequence of B; that is, q is at least equal to α.

And working out the particular case *ab initio*, we have

$$u = \lambda\alpha + \mu\beta - \lambda\mu\,\alpha\beta,$$

$$1 = \lambda + (1 - \lambda)\mu\beta,$$
$$q = \mu + (1 - \mu)\lambda\alpha,$$

where the second equation is $(1 - \lambda)(1 - \mu\beta) = 0$; that is, $1 - \lambda = 0$ or $\lambda = 1$ (assuming only that β, μ are not each of them equal to unity), *i. e.* A always acts efficiently. The third equation then gives $q=\mu+(1-\mu)\alpha$, or $\mu=\frac{q-\alpha}{\alpha'}$, and we then have

$$u=\alpha+\frac{q-\alpha}{\alpha'}\,\alpha'\beta\,;$$

that is,

$$u=\alpha\beta'+\beta q\,;$$

that is, the probability of E is the probability of AB′ plus the probability of BE.

Since $q \nless \alpha$, we cannot have $q=0$; in fact, the two equations $p=1$, $q=0$ (*i. e.* if A, then E always; if B, then E never) would imply that A and B could not happen together; which is contrary to the hypothesis that they are independent causes.

Next for Prof. Boole's solution: $p=1$, the conditions of applicability are satisfied if only $\beta'+\beta q \nless \alpha$; or, what is the same thing, $\alpha' \nless \beta q'$; viz. A′ may happen with BE′ or with BE, but BE′ can only happen with A′, that is, prob. A′ $\nless$ prob. BE′.

The equation in u written under the form

$$\begin{gathered}-(u-1)(u-a)(u-b)\\ +(u-f)(u-g)(u-h)=0,\end{gathered}$$

when $p=1$, and therefore $f=1$, gives immediately (since we cannot have $u=1$)

$$(u-a)(u-b)=(u-g)(u-h)\,;$$

that is,

$$u(g+h-a-b)=gh-ab,$$

and therefore

$$u=\frac{(\alpha+\beta q)(\beta'+\beta q)-\alpha\beta q}{\beta'+\beta q}=\frac{\alpha\beta'+\beta q(\beta'+\beta q)}{\beta'+\beta q}\,;$$

or finally,

$$u=\beta q+\frac{\alpha\beta'}{\beta'+\beta q}.$$

In the present case, if only $1 \nless \alpha+\beta$, $q=0$ is consistent with the condition of applicability; and it gives, as it ought to do, $u=\alpha$.

But q retaining its general value, we have

$$x=0,\quad \frac{s}{s'}=\infty \text{ (that is, } s=1),\quad \frac{x}{s'}=\frac{u-\beta q}{\beta'+\beta q-u},$$

$$y=\frac{\beta q'}{u'},\quad \frac{t}{t'}=\frac{u-\alpha}{\beta q'}\,;$$

or substituting for u its value,

$$x=0, \quad s=1, \quad \frac{x}{s'}=\frac{\alpha}{\beta'+\beta q-\alpha},$$

$$y=\frac{\beta q'(\beta'+\beta q)}{(1-\beta q)(\beta'+\beta q)-\alpha\beta'}, \quad \frac{t}{t'}=\frac{q(\beta'+\beta q-\alpha)}{\beta(\beta'+\beta q)},$$

which give x, s, y, t. It seems difficult to interpret the equations $x=0$, $s=1$.

An intermediate system of equations, putting therein $x=0$, $s=1$, and therefore $x'=1$, $s'=0$, is

$$\frac{ty}{u-\alpha}=\frac{\frac{x}{s'}t'y'}{u-\beta q}=\frac{t'}{u'}=\frac{t'y'}{\beta'+\beta q-u}=\frac{\frac{x}{s'}ty}{\alpha+\beta q-u'},$$

which in fact lead to the foregoing values of $u, x, s, \frac{x}{s'}, y$, and $\frac{t}{t'}$.

The probabilities of the compound events AB, &c. are in general as follows: viz.

Probs. of
AB :: to $xy(st+s't')$,
AB′ „ $xy't'$,
A′B „ $x's'y$,
A′B′ „ $x's'y't'$.

Or in the present case, dividing by s' and then writing $x=0, s=1$,

Probs. of
AB :: to $\frac{x}{s'}yt$,
AB′ „ $\frac{x}{s'}y't'$,
A′B „ y,
A′B′ „ $y't'$.

In particular, if $p=1$, $q=0$, then

$$x=0, \quad s=1, \quad \frac{x}{s'}=\frac{\alpha}{1-\alpha-\beta},$$

$$y=\frac{\beta}{1-\alpha}, \quad \frac{t}{t'}=0; \text{ that is, } y'=\frac{1-\alpha-\beta}{1-\alpha}, \quad t=0, \quad t'=1.$$

So that the probabilities of the four events are as

$$0:\frac{\alpha}{1-\alpha}:\frac{\beta}{1-\alpha}:\frac{1-\alpha-\beta}{1-\alpha};$$

that is, the probabilities of AB, AB′, A′B, A′B′ are

$$0, \quad \alpha, \quad \beta, \quad 1-\alpha-\beta$$

respectively. But this is an immediate consequence of the given values prob. $A=\alpha$, prob. $B=\beta$, and of the deduced equation prob. $AB=0$.

2 Stone Buildings, W.C.,
March 18, 1862.

The foregoing paper was submitted to Prof. Boole, who, in a letter dated March 26, 1862, writes:—

"The observations which have occurred to me after studying your paper are the following.

1st. "I think that your solution is correct under conditions partly expressed and partly implied. The one to which you direct attention is the assumed independence of the causes denoted by A and B. Now I am not sure that I can state precisely what the others are; but one at least appears to me to be the assumed independence of the events of which the probabilities according to your hypothesis are $\alpha\lambda$, $\beta\mu$. Assuming the independence of the causes as to *happening*, I do not think that you are entitled *on that ground* to assume their independence as to *acting*; because, to confine our observations to common experience, we often notice that states of things apparently independent as to their occurrence, may, when concurring, aid or hinder each other in such a manner that the one may be more or less likely to act 'efficiently' in the presence of the other than in its absence. I use the language of your own hypothesis of *efficient* action.

"2ndly. When I say that I think your solution correct under certain conditions, I ought to add that it appears to me that such conditions ought to be stated as part of the original data, and that they ought to be of such a kind that they can be established by experience in the same way as the other data are. For instance, if experience, as embodied in a sufficiently long series of statistical records, establish that

$$\text{Prob. } A=\alpha, \quad \text{Prob. } B=\beta,$$

the very same experience may, by establishing also that

$$\text{Prob. } AB=\alpha\beta,$$

whence in conjunction with the former it follows that

$$\text{Prob. } AB'=\alpha\beta', \quad \text{Prob. } A'B=\alpha'\beta, \quad \text{Prob. } A'B'=\alpha'\beta',$$

enable us to pronounce that A and B are in the long run, as to happening or not happening, in the position of mutually independent events.

"3rdly. I think it may be shown to demonstration, from the nature of the result, that the solution you have obtained does not apply simply and generally to the problem under the single modification of the assumption that A and B are independent. The completed data under this assumption are

$$\text{Prob. } A=\alpha,\quad \text{Prob. } B=\beta,\quad \text{Prob. } AB=\alpha\beta,$$
$$\text{Prob. } AE=\alpha p,\quad \text{Prob. } BE=\beta q.$$

You may deduce all these from your Table of probabilities of 'compound events' given in your paper. Now you may easily satisfy yourself that the sole necessary and sufficient conditions for the *consistency* of these data are the following:—

$$\left.\begin{array}{ll} (1) & \alpha p' + \beta q \geqq \alpha\beta. \\ (2) & \alpha p + \beta q' \geqq \alpha\beta. \\ (3) & \left\{\begin{array}{l} \alpha \\ \beta \\ p \\ q \end{array}\right\} \begin{array}{l} \leqq 1. \\ \\ \geqq 0. \end{array} \end{array}\right\} \quad \ldots\ldots \quad (M)$$

But your solution requires the following conditions to be satisfied, viz.

$$q-\alpha p \geqq 0,\quad p-\beta q \geqq 0,$$

together with the system (3). Now (1) and (2) are expressible in the form

$$\beta(q-\alpha p)+\alpha\beta' p' \geqq 0,$$
$$\alpha(p-\beta q)+\beta\alpha' q' \geqq 0.$$

From which you will see that your conditions are *narrower* than those which the data are really subject to. If your conditions are satisfied, the data will be consistent; but the converse of this proposition does not hold.

"4thly. You remark that my solution of the problem, in which the independence of A and B is not assumed, but in which the probabilities are otherwise the same as in yours, is only applicable when

$$\alpha' + \alpha p \geqq \beta q,\quad \beta' + \beta q \geqq \alpha p;$$

but you do not appear to have noticed that these are actually the conditions of *consistency in the data.* Unless these are satisfied, the data cannot possibly be furnished by experience.

"5thly. You remark that I have solved the problem under what you call the '*concomitance*' statement, and not the '*causation*' statement. I think that every problem stated in the 'causation' form admits, if capable of scientific treatment, of reduction to the 'concomitance' form. I admit it would have been better, in stating my problem, not to have employed the word 'cause' at all. But the introduction of the hypothesis of the independence of A and B does not affect the *nature* of the problem.

"6thly. The *x*, *s*, &c., about the interpretation of which you inquire, are the probabilities of ideal events in an ideal problem connected by a formal relation with the real one. I should fully concede that the auxiliary probabilities which are employed in my method always refer to an ideal problem; but it is one, the form of which, as given by the calculus of logic, is not arbitrary. Nor does its connexion with the real problem appear to me arbitrary. It involves an extension, but as it seems to me, a perfectly scientific extension, of the principles of the ordinary theory of probabilities. On this subject, however, I have but little to add to what I have said, Transactions of the Royal Society of Edinburgh, vol. xxi. part 4, 'On the Application of the Theory of Probabilities, &c.'

"7thly. The problem, as stated by me, and then modified by the simple introduction of the hypothesis of the independence of A and B, must admit of solution by my method; and that solution ought to impose no restriction beyond the conditions of possible experience noted in (M).

"I should be extremely glad if mathematicians would examine the analytical questions connected with the application of my method. There can, I think, after the partial proofs which I have given, exist no doubt that the conditions of applicability of the solutions are always identical with the conditions of consistency in the data, *i. e.* with what I have called, in the paper above referred to, the conditions of possible experience. The proof of the general proposition would involve the showing that a certain functional determinant consists solely of positive terms, with some connected theorems which appear to me to be of considerable analytical interest.

"8thly. I certainly think your paper deserving of publication. If you think proper to add any or the whole of my remarks, you can do so, with of course any comments of your own."

I remark upon Prof. Boole's observations:—

1st. I do assume that the causes A and B are absolutely independent of, and uninfluenced by each other; viz. not only the probability of A acting, but also the probability of its acting efficiently, are each of them the same whether B does not act, or acts inefficiently, or acts efficiently; and the like for B.

2ndly. I do assume that the same experience which establishes

$$\text{Prob. } A=\alpha, \quad \text{Prob. } B=\beta,$$

would in the long run establish

$$\text{Prob. } AB=\alpha\beta;$$

if it does not, *cadit quæstio*, the causes are not independent.

3rdly. I assume not only

$$\text{Prob. A}=\alpha,\quad \text{Prob. B}=\beta,\quad \text{Prob. AB}=\alpha\beta,$$

but also as 1st above stated; and I consider that, inasmuch as the result of the investigation is to show that the conditions $q-\alpha p \nless 0, p-\beta q \nless 0$ are necessary and sufficient conditions, it is also a result of the investigation that these are the conditions of *consistency among the data,* viz. the conditions in order that the data may be consistent with the above assumptions as to the independence of the causes. It is clear that since, as just stated, I do assume something beyond the last-mentioned three equations, the conditions of consistency *ought* to be *narrower* than those in Prof. Boole's 3rdly.

4thly. I had not overlooked, but I ought to have stated, that Prof. Boole's conditions were actually the conditions of consistency in the data.

5thly. I contend that the conception of A and B as *causes* does alter the *nature* of the problem. For when A and B are conceived of as causes, there is a definite notion of the efficient or inefficient action of A or B; and in particular when they both act, one of them, say A, may act inefficiently. But according to the concomitance statement, then either there is no such notion as that of the efficient or non-efficient happening of A or B (I believe this to be so), *or else* the only notion of efficient or inefficient happening is happening in concomitance or in non-concomitance with E; but in this view, if A, B, E all happen, then A and B each of them happens efficiently. The argument is to me conclusive as to the diversity of the two problems.

6thly. I do not in anywise assert, or even suppose, that the ideal problem is arbitrary, or that its connexion with the real problem is arbitrary. I simply do not know what the ideal problem is; I do not know the point of view, or the assumed mental state of knowledge or ignorance according to which x, y, s, t are the probabilities of A, B, AE, BE. It is to be borne in mind that x, y, s, t are, in Prof. Boole's solution, determined as numerical quantities included between the limits 0 and 1, *i. e.* as quantities which are or may be actual probabilities. What I desiderate is, that Prof. Boole should give for his auxiliary quantities x, y, s, t such an explanation of the meaning as I have given for my auxiliary quantities λ, μ. I do not find any such explanation in the memoir referred to.

7thly and 8thly. No remark is necessary.

March 29, 1862.

Prof. Boole, in his reply, dated April 2, writes, "No such explanation as you desiderate of the interpretation of the auxiliary quantities in my method of solution is possible; because they are not of the nature of additional data, and their introduction does not limit the problem as any hypotheses which are of that nature do. I do not see any difficulty whatever in the conception of the ideal problem."

We thus join issue as follows: Prof. Boole says that there is no difficulty in understanding, I say that I do not understand, the *rationale* of his solution.

It may be remarked that the question may be, not to find any actual probability whatever, but only to find a Boolian probability or probabilities. Thus the equations (L), p. 356, omitting the last member, which alone involves u, determine in terms of the data α, β, αp, βq the Boolian probabilities x, y, s, t of the events A, B, AE, BE.

In a subsequent hastily-written letter, Prof. Boole gives an explanation of the equations (L), which appears to me little more than a translation of these equations into ordinary language.

April 16, 1862.

LII. *Remarks on* Ampère's *Experiment on the Repulsion of a Rectilinear Electrical Current on itself.* *By* Mr. JAMES CROLL.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the Philosophical Magazine for February last, Professor van Breda of Haarlem* describes an experiment wherein, in order to show that the movement of the conductor in the experiment of Ampère is not due to the influence of angular currents, he suspends the conductor above the mercury, and allows the currents in the latter to pass underneath. A current which was found to be more than sufficient to move the conductor when floating upon the mercury, according to Ampère's experiment, was made to pass through the apparatus; but not the least motion in the suspended conductor could be observed, and he therefore concludes that the motion of the conductor in the experiment of Ampère cannot be due to the influence of angular currents.

It is remarkable how Prof. van Breda did not observe that a motion of translation in the case of his suspended conductor was absolutely impossible; for he allows his conductor to cross the currents in the mercury; and the consequence, of course, is, the currents in the mercury repel the one half of his conductor and attract the other half; for instance, they attract the perpendicular

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* In a note recently received from Prof. Logeman of Haarlem, we are informed that his name should have been associated with that of Prof. van Breda as joint author of the communication here referred to.—EDS.

parts, and repel the horizontal part D D. If Prof. van Breda were to cut his conductor asunder at the points where they intersect the currents in the mercury, and by some contrivance or other still allow the current to pass, he would find that the horizontal part would be instantly repelled, and the perpendicular parts instantly attracted; but when the whole is in one rigid piece, as in his apparatus, no motion can possibly take place.

Granting that the conductor is moved by the repulsion of the current on itself, still it certainly must be admitted that Ampère's experiment cannot prove the fact; for if we admit, in regard to his experiment, that the same motion under exactly the same conditions ought to take place from the influence of angular currents, then, when the motion does take place, we have no warrant to conclude that it is *not* due to angular currents, but to some other cause. The inference is the same whether the conductor be composed of solid copper or of fluid mercury; for the motion does not depend upon the nature of the conductor, but simply upon its position in relation to the impelling current.

If we reverse the direction of the points of the conductor, as Prof. van Breda has ingeniously done, the conditions of the experiment are entirely changed; for then Ampèrian repulsion and angular currents act in direct opposition to each other, and we are then at liberty to decide, from the direction of the motion which actually takes place, to which force the motion ought to be attributed.

I do not see how it follows, as Prof. van Breda and others seem to suppose, that, because two currents at all possible angles affect each other, they *must* do the same when they are both on the same straight line; because if they act only in *right* lines from their *sides*, no effect can possibly take place when they are both on the same straight line. In this case a *curved* line would be required to produce the effect.

The experiment of Prof. van Breda with the reversed conductor, and some others detailed by him, certainly prove that the molecules of a conductor, while the current is passing, do repel each other; but still this fact does not appear to settle the real question at issue; for it does not follow that, because the molecules of the conductor along which the current passes recede from each other, the contiguous parts of the current itself repel each other. Our conceptions of the nature of the electric current, as well as the facts from which these conceptions are derived, are in direct opposition to this conclusion.

Whether we view electricity as a substance or merely a dynamical affection of the ordinary molecules of matter—in fact whatever our views of its nature are, it will be admitted that the electric current is an effort of forces to regain a state of equilibrium.

The fact that a current is passing along a chain of molecules is a proof that these molecules are not statically in the same condition. This fact has been proved experimentally by Kohlrausch, in his investigations into the laws of Ohm. The differences in their statical condition are the immediate cause of the current; for the current is simply the discharge of the electricities of the contiguous molecules upon each other, so as to bring them under the same conditions. What, then, are the mutual tendencies of molecules thus differently charged? Is it to approach or to recede from each other? Prof. van Breda and others reply, facts show that they do recede from each other. Be it so; still we must allow that the electricities with which the separate molecules are charged do not recede. If the molecules themselves recede, the electricities approach; for it is the tendency of the electricities on the contiguous molecules to combine which produces the current. The contiguous sections of the *current* must therefore attract, not repel each other. It may be difficult to conceive how a mutual attraction of the contiguous sections of the current should result in a mutual repulsion in the contiguous molecules of the conductor carrying the current; but if such be the fact, this is enough.

The difficulty will, however, in a great measure disappear when we reflect that the molecules offer resistance to the transference which constitutes the current, the amount of the resistance depending upon the nature of the molecules, the conductibility of bodies for electricity being inversely as the amount of resistance offered by their molecules. Now if the current is produced by a tendency in the electricities on the contiguous molecules to combine, then it is quite natural to suppose that the molecules themselves would recede from each other in order to prevent combination. Their very endeavours to resist the transference of their electricities would cause them to recede from each other. If we suppose that the molecules are not simply passive, but active in their resistance (and we have every reason to believe that they are), then recession from each other, when the current is passing, is what might be anticipated.

The same conclusion will follow should we suppose that the passing of the current tends to produce a disturbance in the equilibrium of the molecules; for each molecule, in its endeavour to avoid disturbance in its statical condition caused by the presence of its neighbour, will recede from it. In fact recession is implied in the very idea of molecular resistance.

I am, Gentlemen,
Your most obedient Servant,
JAMES CROLL.

Glasgow, March 4, 1862.

LIII. *Liquid Diffusion applied to Analysis.* *By* Thomas Graham, *F.R.S., Master of the Mint.*

[Concluded from p. 306.]

5. *Dialysis of Organic Colloid Substances.*

Tannin.—The tannin employed was that extracted from gall-nuts by the ether process of Pelouze. A two per cent. solution of this substance, covering a surface of paper-parchment of the area of about $\frac{1}{100}$th of a square metre, or 15·6 square inches, to a depth of 10 millimetres, was diffused at 10° to 13° of temperature. The diffused matter amounted, in successive periods of twenty-four hours, to ·073, ·040, ·021, ·021, ·024 and ·024 gramme, derived from the two grammes in solution. Probably the earlier diffusates were increased by the presence of a little gallic acid, which, being a crystalloid, would no doubt be rapidly eliminated by diffusion. The latter observations indicate that tannin passes through a paper-parchment septum about 200 times less rapidly than chloride of sodium does, in similar circumstances as to temperature and strength of solution. The diffusates from the tannin solution gave a precipitate with gelatine, and therefore contained tannin unaltered. But the diffusates probably contained also throughout some products of decomposition of a crystalloid character.

To the low diffusibility of tannin may be ascribed the remarkably slow penetration of skins by that substance in the ordinary operation of tanning leather. Tannin appears to form compounds of much stability with certain other colloids, as tanno-gelatine, and the compound with albumen which appears to be the primary basis of the vegetable cell (Frémy).

Gum.—The diffusate obtained from a solution containing 2 grammes of gum-arabic, in experiments corresponding in their conditions with the experiments upon tannin just related, was ·013 gramme per day. The power of gum to penetrate the colloid septum appears, therefore, to be one-half less than that of tannin, and 400 times less than the diffusibility of chloride of sodium. Gum gave the same amount of diffusate with a mucus septum as with parchment-paper. When substances of the crystalloid class are mixed with the gum, the diffusion of the latter appears to be still further reduced, and may even be entirely extinguished. The separation of colloids from crystalloids by dialysis is, in consequence, generally more complete than might be expected from the relative diffusibility of the two classes of substances.

Vegetable gum, which Frémy has shown to be a gummate of lime, can be purified by a dialytic method, which may be found applicable with advantage in other cases. Oxalic acid, it is known, precipitates lime from the gum very imperfectly. Hydrochloric acid may be used to separate that base from a solution of gum placed upon the dialyser, with more effect. It is only necessary to add to a strong solution of gum 4 or 5 per cent. of hydrochloric acid, and to dialyse till the gum solution gives no precipitate with nitrate of silver. In an experiment made upon a 20 per cent. solution of gum, the ash was reduced to 0·1 per cent. of the gum in five days. The gummic acid possesses a sensible acid reaction, about equal to that of carbonic acid. This acid reaction was neutralized in 100 parts of gummic acid by 2·85 parts of potash. This amount of potash is very nearly equivalent to the lime originally present in the gum (1·72 lime, or 3·07 carbonate of lime, being equivalent to 2·89 potash). When the gummate of potash itself was dialysed without addition, the potash gradually diffused away, possibly in the state of carbonate, and left the gum again possessed of an acid reaction. Gummic acid, well dried at 100°, becomes insoluble in water, but swells up in that liquid, like gum-tragacanth. We appear to have here the pectous form of gummic acid.

It is worthy of inquiry whether such native gums as are insoluble in water are not the pectous form of soluble gum, rather than allotropic varieties of that substance. So also of the metagummic acid of Frémy, formed by the action of strong sulphuric acid on mucilage. This last substance is insoluble in water, but was found by Frémy to afford, when neutralized by lime and alkalies, a soluble gum undistinguishable from gum-arabic.

Gummic acid produces a remarkable compound with gelatine. When solutions of these two colloids are mixed, oily drops fall and form a nearly colourless jelly on standing. This jelly is very fusible, melting at 25°, or by the heat of the hand. The *gummate of gelatine* may be washed without decomposition, but is soluble to a certain extent in pure water, and still more so in a solution of gelatine. Prepared with gummic acid in excess, the compound, when dried at 100°, consisted of 100 parts gummic acid with 59 gelatine. The drops and the jelly contained 83·5 per cent. of water. Solution of gelatine is not precipitated by unpurified gum, nor by the gummate of potash.

Dextrine.—A two per cent. solution of dextrine, prepared from starch, was diffused in the same conditions as the preceding substances, but through a mucus septum. It gave in twenty-four hours ·034 gramme of diffusate from 2 grammes, or about three times more diffusate than was given by gum-arabic.

Caramel.—The dialytic examination of this substance adds to the accurate information on the subject lately supplied by M. A. Gélis*, and places caramel indisputably in the colloid class.

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* *Annales de Chimie, &c.*, sér. 3. t. lii. p. 352.

The crude caramel obtained by heating cane-sugar at 210°–220°, when placed on the dialyser, allows certain intermediate coloured substances (Caramelane and Caramelene of Gélis) to diffuse out with considerable facility, while the compound containing the largest proportion of carbon remains behind. The latter substance, as obtained by me, possessed five times the colouring-power of the original crude caramel, weight for weight. This highest soluble member of the caramel series may also be obtained, more quickly, by precipitation from its aqueous solution by means of alcohol. But I found it necessary to repeat the precipitation four times, or till the mass thrown down, from being plastic at first, became pulverulent. A solution containing 10 per cent. of the caramel so purified is gummy; and on standing, it formed a tremulous jelly entirely soluble in hot or cold water. Evaporated *in vacuo*, the solution dries up into a black shining mass, which is tough and elastic, while it still possesses a certain proportion of water, like gum containing some water. Once thoroughly dried at a low temperature, this soluble caramel may be heated, afterwards, to 120° and retain complete solubility. But if a solution of the same caramel be directly evaporated to dryness by the heat of a water-bath, the whole matter is rendered insoluble in hot or cold water. The soluble and insoluble caramel have the same composition, and appear to illustrate the usual double form of colloids. The proportion of carbon in the fluid caramel was found as high as 54·59 per cent., which comes nearer to $C^{24} H^{15} O^{15}$ (requiring C 55·17) than any other formula in which the oxygen and hydrogen are assumed to be present in the proportion of water. In the analysis by Gélis of his carameline, the proportion of carbon did not exceed 51·33 per cent., which does not apply to the present substance.

Fluid caramel is wholly tasteless, and appears to be neutral. It exhibits the same excessive sensibility to crystalloidal reagents which is witnessed in fluid silicic acid and alumina. The solution is precipitated or pectized by mere traces of any mineral acid, by alkaline sulphates, chloride of sodium, by most other salts, and by alcohol. The caramel then forms a brownish black pulverulent substance, insoluble in hot or cold water. The presence of sugar and of the intermediate brown substances protects fluid caramel in a remarkable way from the action of crystalloids, and accounts for the preceding properties not being observed in crude caramel. This colloid appears also to be precipitated by certain substances of its own class, such as peroxide of iron.

Pectous caramel may readily have its solubility restored. Placed in dilute potash, the caramel swells and appears gelatinous, and is dissolved on the application of heat. When this solution is dialysed, the potash is quickly reduced to the proportion of about 9 per cent., which forms a neutral compound. If an excess of acetic acid now be added, the whole potash is soon diffused away, and pure soluble caramel remains on the dialyser. Even carbonic acid will carry away the potash.

The extremely low diffusibility which has been assigned to caramel in former Tables, belongs to that substance as last described,—the brown intermediate substances which accompany it in crude caramel being considerably more diffusive, although they, again, are much less diffusive than any variety of crystallizable or uncrystallizable sugar. When the molasses of the cane-sugar are diffused, much the greater portion of the colouring-matters remains in the dialyser.

With the parchment-paper septum the fluid caramel appeared even less dialysable than gum, the diffusate in twenty-four hours from a 2 per cent. solution of the former being ·009 gramme only, while that of the latter was ·013. Caramel may be stated, approximately, to be 600 times less dialysable than chloride of sodium, and 200 times less so than sugar. Hence liquids coloured with caramel, such as porter and coffee, may be dialysed for a day with the passage of very little colouring-matter.

Before leaving caramel, the analogy may be referred to which the insoluble form of that substance presents to *coal*. Caramelization appears the first step in that direction—the beginning of a colloidal transformation to be consummated in the slow lapse of geological ages.

Albumen.—The purification of albumen is effected with much advantage upon the dialyser. The solution of egg-albumen is mixed freely with acetic acid and then dialysed. The earthy and alkaline salts are speedily got rid of, and in three or four days the albumen burns without leaving a trace of ash. Although the acetic acid used in the process appears to diffuse off entirely, albumen prepared in the manner described has a faint acid reaction. It also coagulates milk when mixed with the latter and heated. Albumen so prepared retains its constituent sulphur.

The passage through parchment-paper of pure albumen prepared by the unobjectionable process of M. Wurtz is so slow, that several days are required to produce a sensible result. Thus the diffusate from a solution of 2 grammes of albumen in 50 grammes of water was 0·052 gramme in eleven days, which gives 0·005 gramme in a single day. Albumen, then, appears to be about 2½ times less dialysable than gum, and 1000 times less so than chloride of sodium.

Even combination with an alkali does not appear to enable albumen to pass through the colloid septum. To half a gramme of pure albuminic acid dissolved in 50 grammes of water, ·05 gramme of hydrate of soda was added (one-tenth of the weight of the albumen), and the liquid was placed upon parchment-paper. No albumen could be discovered in the diffusate of several days, but it gave ·069 gramme of carbonate of soda, equivalent to ·053 gramme of hydrate of soda—that is, the whole soda originally added to the albumen. The separation of the soda from the albumen may possibly have been aided by the presence of carbonic acid in the water; but certainly the entire separation of the alkali from albumen by diffusion through a colloidal film is a remarkable fact. Hydrate of potash was found to diffuse away from albumen in the same manner.

A solution of *Emulsine* is precipitated by albuminic and gummic acids, but not by unpurified albumen or gum-arabic. The precipitates are white and opake, pulverulent, and not gelatinous. They are soluble in acetic acid.

A thin stratum of pure albumen coagulated by heat appears to intercept completely the passage of liquid albumen of the egg. Forty grammes of undiluted egg-albumen, representing 5·6 grammes of dry albumen, were placed on a dialyser of the small size, composed of two sheets of calico well-impregnated with albumen and coagulated by heat of steam, as in the albumenized osmometer*. After twelve days the volume of liquid within the instrument had increased to 117 grammes by osmose, while a diffusate had passed through the dialyser of 0·243 gramme, or 4·34 per cent. of the original dry albumen. This diffusate consisted of salts chiefly, with some organic matter, but no portion of the latter was coagulable by heat.

Neither gelatinous starch, animal gelatine dissolved in water, nor extract of flesh appears to be capable of diffusing through a colloid septum in a sensible degree, although salts and other crystallizable substances, which are mixed with the former, diffuse through the septum readily, and may thus be separated from the former substances.

6. *Separation of Arsenious Acid from Colloidal Liquids.*

Dialysis may be advantageously applied to the separation of arsenious acid and metallic salts from organic solutions in medico-legal inquiries. The process has the advantage of introducing no metallic substance or chemical reagent of any kind into the organic fluid. The arrangement for operating is also of the simplest nature.

The organic fluid is placed, to the depth of half an inch, on a dialyser formed of a hoop of gutta percha 10 or 12 inches in diameter, covered with parchment-paper (fig. 1, page 208). The

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* Philosophical Transactions, 1854, p. 189.

dialyser is then floated in a basin containing a volume of water about four times greater than the volume of organic fluid in the dialyser. The water of the basin is generally found to remain colourless after the lapse of twenty-four hours; and after being concentrated by evaporation, it admits of the application of the proper reagents to precipitate and remove a metal from solution. One-half to three-fourths of the crystalloidal and diffusible constituents of the organic fluid will generally be found in the water of the basin.

In the few illustrative experiments which follow, the 4-inch bulb dialyser, having an area of 16 square inches, or about $\frac{1}{100}$th part of a square metre, was generally made use of (figs. 3 & 4, pp. 291, 292). The volume of liquid placed in the bulb was 50 cubic centimetres, and accordingly covered the dialyser to a depth of 5 millimetres, or about 0·2 inch. The outer volume of water (in the jar) was not less than 1 litre, or twenty times the volume of the solution on the dialyser.

1. A solution of arsenious acid, in pure water, was first placed on the dialyser, the water containing 0·5 per cent. of arsenious acid, or 0·25 gramme of that substance, for twenty-four hours. The dialyser being then removed, the outer fluid was concentrated by heat, and then precipitated by sulphuretted hydrogen. It gave 0·300 gramme of tersulphide of arsenic, equivalent to 0·241 gramme of arsenious acid. It appears, then, that about 95 per cent. of the arsenious acid had diffused from the dialyser into the water-jar in twenty-four hours.

2. Water, with one-fourth of its volume of fluid egg-albumen and 0·25 gramme, or 0·5 per cent. of arsenious acid, was now placed on the dialyser as before. The diffusate gave, with sulphuretted hydrogen, after being acidulated with hydrochloric acid, 0·267 gramme of tersulphide of arsenic, equivalent to 0·214 gramme of arsenious acid.

3. The water contained 10 per cent. of gum-arabic and 1 per cent. arsenious acid, the latter amounting to 0·5 gramme. From the diffusate was derived 0·505 gramme of tersulphide of arsenic, equivalent to 0·406 gramme of arsenious acid. The dialyser still gave out arsenious acid when immersed for a second day in water. The outer fluid contained no gum.

It may be added that a similar 1 per cent. solution of arsenious acid, without the gum, gave a diffusate of 0·45 gramme arsenious acid in the same time, that is, nine-tenths of the whole acid.

4. A solution in hot water of 1 per cent. isinglass and 0·5 per cent. of arsenious acid (0·25 gramme), formed a jelly upon the dialyser on cooling. The diffusate from this jelly gave 0·260 tersulphide of arsenic, equivalent to 0·209 arsenious acid, with no gelatine. The escape of the arsenious acid appears then to

have been slightly retarded by the fixing of the gelatinous solution. This is probably due to the arrest of mechanical movement within the gelatinous stratum, and not to any sensible impediment offered by the jelly to diffusion.

In another experiment, similar to the last, but continued for four days instead of twenty-four hours, the tersulphide of arsenic weighed 0·320 gramme, equivalent to 0·257 arsenious acid.

5. A quantity of white of egg, amounting to 50 grammes, to which 0·01 gramme of arsenious acid in solution had been added, was coagulated by heat. The solid mass was then cut up into small pieces and placed on the dialyser, mixed with 50 grammes of water; after the usual period of twenty-four hours, the diffusate gave 0·01 gramme of tersulphide of arsenic, equivalent to 0·008 gramme arsenious acid. Here, of the mass upon the dialyser, the arsenious acid formed only $\frac{1}{10,000}$th part; yet four-fifths of it are recovered.

6. One hundred grammes of milk, charged with $\frac{1}{10,000}$th part of arsenious acid (0·01 gramme), and forming a stratum on the dialyser of 10 millimetres, gave a diffusate which yielded 0·010 tersulphide of arsenic, equivalent to 0·008 gramme of arsenious acid. The outer liquid was colourless, and gave no indication of caseine, but it contained, of course, the salts and the sugar of the milk.

7. The same experiment was repeated with sized writing-paper as the septum, applied to the same bulb. The result was a slight increase in the quantity of arsenious acid recovered.

It appears, then, that arsenious acid separates on the dialyser from gum, from gelatine, albumen, fluid or coagulated, and from caseine, and is obtained in a solution fit for the application of reagents.

8. Half a litre of dark-coloured porter, with 0·05 gramme of arsenious acid added ($\frac{1}{10,000}$th part of arsenious acid) was placed on a hoop dialyser, 8 inches in diameter, and the whole floated in an earthenware basin containing 2 or 3 litres of water. After twenty-four hours the latter fluid had acquired a slight tinge of yellow. It yielded, when concentrated and precipitated by sulphuretted hydrogen, upwards of one-half of the original arsenious acid in a fit state for examination.

9. In a similar experiment on 200 grammes of defibrinated blood charged with $\frac{1}{4000}$th part of arsenious acid (0·05 gramme), and placed on a similar dialyser to the last for twenty-four hours, the diffusate of arsenious acid was recovered with the same facility, and appeared to be equally considerable.

10. Animal intestines, charged with the usual minute proportion of arsenious acid, were cut into small pieces and digested in

water, about 32° C., for twenty-four hours. The whole was then thrown upon a dialyser for an equal time. Arsenious acid diffused out so free from colloidal matter that the action of reagents was not interfered with. A high temperature in digesting the intestines is quite unnecessary, and appeared indeed to increase the difficulty of diffusing out the arsenious acid afterwards.

The *tartrate of potash and antimony,* mixed in the small proportion of $\frac{1}{10,000}$th, with defibrinated blood and with milk, was separated by dialysis quite as effectually as the arsenious acid above.

Strychnine also was separated from organic fluids in the same manner, a small addition of hydrochloric acid being first made to the fluid on the dialyser.

Dialysis, then, appears of general application in the preparation of a liquid for examination by chemical tests, whether the poison looked for be mineral or organic. All soluble poisonous substances, whatever their origin, appear to be crystalloids, and accordingly pass through colloidal septa.

7. *Colloidal Condition of Matter.*

I may be allowed to advert again to the radical distinction assumed in this paper to exist between colloids and crystalloids in their intimate molecular constitution. Every physical and chemical property is characteristically modified in each class. They appear like different worlds of matter, and give occasion to a corresponding division of chemical science. The distinction between these kinds of matter is that subsisting between the material of a mineral and the material of an organized mass.

The colloidal character is not obliterated by liquefaction, and is therefore more than a modification of the physical condition of solid. Some colloids are soluble in water, as gelatine and gum-arabic; and some are insoluble, like gum-tragacanth. Some colloids, again, form solid compounds with water, as gelatine and gum-tragacanth, while others, like tannin, do not. In such points the colloids exhibit as great a diversity of property as the crystalloids. A certain parallelism is maintained between the two classes, notwithstanding their differences.

The phenomena of the solution of a salt or crystalloid probably all appear in the solution of a colloid, but greatly reduced in degree. The process becomes slow,—time, indeed, appearing essential to all colloidal changes. The change of temperature, usually occurring in the act of solution, becomes barely perceptible. The liquid is always sensibly gummy or viscous when concentrated. The colloid, although often dissolved in a large proportion by its solvent, is held in solution by a singularly feeble force. Hence colloids are generally displaced and precipitated by the addition to their solution of any substance from the other class. Of all the properties of liquid colloids, their slow diffusion in water, and their arrest by colloidal septa, are the most serviceable in distinguishing them from crystalloids. Colloids have feeble chemical reactions, but they exhibit at the same time a very general sensibility to liquid reagents, as has already been explained.

While soluble crystalloids are always highly sapid, soluble colloids are singularly insipid. It may be questioned whether a colloid, when tasted, ever reaches the sentient extremities of the nerves of the palate, as the latter are probably protected by a colloidal membrane, impermeable to soluble substances of the same physical constitution.

It has been observed that vegetable gum is not digested in the stomach. The coats of that organ dialyse the soluble food, absorbing crystalloids and rejecting all colloids. This action appears to be aided by the thick coating of mucus which usually lines the stomach.

The secretion of free hydrochloric acid during digestion—at times most abundant—appears to depend upon processes of which no distinct conception has been formed. But certain colloidal decompositions are equally inexplicable upon ordinary chemical views. To facilitate the separation of hydrochloric acid from the perchloride of iron, for instance, that salt is first rendered basic by the addition of peroxide of iron. The comparatively stable perchloride of iron is transformed, by such treatment, into a feebly-constituted colloidal hydrochlorate. The latter compound breaks up under the purely physical agency of diffusion, and divides on the dialyser into colloidal peroxide of iron and free hydrochloric acid. The superinduction of the colloidal condition may possibly form a stage in many analogous organic decompositions.

A tendency to spontaneous change, which is observed occasionally in crystalloids, appears to be general in the other class. The fluid colloid becomes pectous and insoluble by contact with certain other substances, without combining with these substances, and often under the influence of time alone. The pectizing substance appears to hasten merely an impending change. Even while fluid a colloid may alter sensibly, from colourless becoming opalescent; and while pectous the degree of hydration may become reduced from internal change. The gradual progress of alteration in the colloid effected by the agency of time, is an investigation yet to be entered upon.

The equivalent of a colloid appears to be always high, although the ratio between the elements of the substance may be simple.

Gummic acid, for instance, may be represented by $C^{12}H^{11}O^{11}$; but, judging from the small proportions of lime and potash which suffice to neutralize this acid, the true numbers of its formula must be several times greater. It is difficult to avoid associating the inertness of colloids with their high equivalents, particularly where the high number appears to be attained by the repetition of a smaller number. The inquiry suggests itself whether the colloid molecule may not be constituted by the grouping together of a number of smaller crystalloid molecules, and whether the basis of colloidality may not really be this composite character of the molecule.

With silicic acid, which can exist in combination both as a crystalloid and colloid, we have two series of compounds, silicates and cosilicates, the acid of the latter appearing to have an equivalent much greater (thirty-six times greater in one salt) than the acid of the former. The apparently small proportion of acid in a variety of metallic salts, such as certain red salts of iron, is accounted for by the high colloidal equivalent of their bases. The effect of such an insoluble colloid as prussian blue in carrying down small proportions of the precipitating salts, may admit of a similar explanation.

Gelatine appears to hold an important place as a colloidal base. This base unites with colloidal acids, giving a class of stable compounds, of which tanno-gelatine only appears to be hitherto known. Gelatine is precipitated entirely by a solution of metaphosphoric acid added drop by drop, 100 parts of gelatine uniting with 3·6 parts of the acid. The compound formed is a semitransparent, soft, elastic, and stringy solid mass, presenting a startling resemblance to animal fibrine. It will be an interesting inquiry whether metaphosphoric acid is a colloid, and enters into the compound described in that character, or is a crystalloid, as the small proportion and low equivalent of the acid would suggest. Gelatine is also precipitated by carbolic acid.

The hardness of the crystalloid, with its crystalline planes and angles, is replaced in the colloid by a degree of softness, with a more or less rounded outline. The water of crystallization is represented by the water of gelatination. The water in gelatinous hydrates is aptly described by M. Chevreul as retained by "capillary affinity," that is, by an attraction partaking both of the physical and chemical character. While it is here admitted that chemical affinity of the lowest degree may shade into capillary attraction, it is believed that the character of gelatinous hydration is as truly chemical as that of crystalline hydration. Combination of a colloid with water is feeble, it is true; but so is combination in general with the colloid. Notwithstanding this, anhydrous colloids can decompose certain crystalloid hydrates. The water in alcohol of greater strength than corresponds with the density 0·926, which represents the definite hydrate $C^4 H^6 O^2 + 6 HO$, is certainly in a state of chemical union. But alcohol so high as 0·906, contained in a close vessel, is concentrated in a notable degree by contact with dry mucus, gelatine, and gum, and sensibly even by dry parchment-paper. Dilute alcohol divided from the air of the atmosphere by a dry septum of mucus, gelatine, or gum, is also concentrated by evaporation, as in the well-known bladder experiment of Sömmering. The selective power is here apparent of the colloid for water, that fluid being separated from alcohol, and travelling through the colloidal septum by combination with successive molecules of the latter, till the outer surface is reached and evaporation takes place. The penetration in this manner of a colloid by a foreign substance may be taken as an illustration of the phenomena of cementation. Iron and other substances which soften under heat, may be supposed to assume at the same time a colloidal constitution. So it may be supposed does silica when fused into a glass by heat, and every other vitreous substance.

Gelatinous hydrates always exhibit a certain tendency to aggregation, as is seen in the jelly of hydrated silicic acid and of alumina. With some the jelly is also adhesive, as in glue and mucus; but, unless they be soluble in water, gelatinous hydrates, when once formed, are not in general adhesive; separated masses do not reunite when brought into contact. This want of adhesiveness is very remarkable in the gelose of Payen, which resembles gelatine so closely in other respects. Layers of a gelose solution, allowed to cool and gelatinize in succession in a diffusion-jar (p. 290), do not adhere together.

Ice itself presents colloidal characters at or near its melting-point, paradoxical although the statement may appear. When ice is formed at temperatures a few degrees under 0° C., it has a well-marked crystalline structure, as is seen in water frozen from a state of vapour, in the form of flakes of snow and hoar-frost, or in water frozen from dilute sulphuric acid, as observed by Mr. Faraday. But ice formed in contact with water at 0°, is a plain homogeneous mass with a vitreous fracture, exhibiting no facets or angles. This must appear singular when it is considered how favourable to crystallization are the circumstances in which a sheet of ice is slowly produced in the freezing of a lake or river. The continued extrication of latent heat by ice as it is cooled a few degrees below 0° C., observed by M. Person, appears also to indicate a molecular change subsequent to the first freezing. Further, ice, although exhibiting none of the viscous softness of pitch, has the elasticity and tendency to rend seen in colloids. In the properties last mentioned, ice presents a distant analogy to gum incompletely dried, to glue, or any other firm jelly. Ice further appears to be of the class of adhesive colloids. The redintegration (regelation of Faraday) of masses of melting ice, when placed in contact, has much of a colloid character. A colloidal view of the plasticity of ice demonstrated in the glacier-movement will readily develope itself.

A similar extreme departure from its normal condition appears to be presented by a colloid holding so high a place in its class as albumen. In the so-called blood-crystals of Funke, a soft and gelatinous albuminoid body is seen to assume a crystalline contour. Can any facts more strikingly illustrate the maxim that in nature there are no abrupt transitions, and that distinctions of class are never absolute?

8. *Osmose.*

Little has been said in the present paper respecting osmose, a subject closely connected with colloidal septa. It now appears to me that the water-movement in osmose is an affair of hydration and of dehydration in the substance of the membrane or other colloid septum, and that the diffusion of the saline solution placed within the osmometer has little or nothing to do with the osmotic result otherwise than as it affects the state of hydration of the septum.

Osmose is generally considerable, through membranous and other highly hydrated septa, with the solution of any colloid (gum, for instance) contained in the osmometer. Yet the diffusion outwards of the colloid is always minute, and may sometimes amount to nothing. Indeed, an insoluble colloid, such as gum-tragacanth, placed in powder within the osmometer, was found to indicate the rapid entrance of water to convert the gum into a bulky gelatinous hydrate. Here no outward or double movement is possible.

The degree of hydration of any gelatinous body is much affected by the liquid medium in which it is placed. This is very obvious in fibrine and animal membrane. Placed in pure water, such colloids are hydrated to a higher degree than they are in neutral saline solutions. Hence the equilibrium of hydration is different on the two sides of the membrane of an osmometer. The outer surface of the membrane being in contact with pure water tends to hydrate itself in a higher degree than the inner surface does, the latter surface being supposed to be in contact with a saline solution. When the full hydration of the outer surface extends through the thickness of the membrane and reaches the inner surface, it there receives a check. The degree of hydration is lowered, and water must be given up by the inner layer of the membrane, and it forms the osmose. The contact of the saline fluid is thus attended by a continuous catalysis of the gelatinous hydrate, by which it is resolved into a lower gelatinous hydrate and free water. The inner surface of the membrane of the osmometer contracts by contact with the saline solution, while the outer surface dilates by contact with pure water. Far from promoting this separation of water, the diffusion of the salt throughout the substance of the membrane appears to impede osmose, by equalizing the condition as to saline matter of the membrane through its whole thickness. The advantage which colloidal solutions have in inducing osmose, appears to depend in part upon the low diffusibility of such solutions, and their want of power to penetrate the colloidal septum.

The substances fibrine, albumen, and animal membrane swell greatly when immersed in water containing minute proportions of acid or of alkali, as is well known. On the other hand, when the proportion of acid or alkali is carried beyond a point peculiar to each substance, contraction of the colloid takes place. Such colloids as have been named acquire the power of combining with an increased proportion of water, and of forming superior gelatinous hydrates, in consequence of contact with dilute acid and alkaline reagents. Even parchment-paper is more elongated in an alkaline solution than in pure water. When so hydrated and dilated, the colloids present an extreme osmotic sensibility. Used as septa, they appear to assume or resign their water of gelatination under influences apparently the most feeble. It is not attempted to explain this varying hydration of colloids with the osmotic effects thence arising. Such phenomena belong to colloidal chemistry, where the prevailing changes in composition appear to be of the kind vaguely described as catalytic. To the future investigation of catalytic affinity, therefore, must we look for the further elucidation of osmose.

LIV. *On the Changes in the Apparent Size of the Moon.*

By Richard T. Lewis.

To Professor Tyndall.

1 Lowndes Terrace, Knightsbridge, S.W.,
March 25, 1862.

Sir,

THE enlarged appearance of the sun and moon at rising or setting has long been a subject of controversy; and although astronomers and others have proved by micrometrical measurements that the apparent diameter of those bodies is *actually* largest when they are on the meridian (since they are nearest to us then), the popular mind refuses to believe that the illusion is only mental, and hitherto no illustration has been found sufficiently satisfactory to convince those who are sceptical in the matter. The best illustration brought forward as yet, to my knowledge, was that which appeared some time since in 'Recreative Science,' in an article upon "The Rising and Setting of the Sun Physiologically considered." The writer had, one dark night, been gazing for some time upon the glowing furnace of a lime-burner, after which, on going out into the dark, he saw the luminous image of the fire in the air before him as a large circle; but happening to look up, this image apparently shrank up to about half its former diameter. This, however, does not appear to satisfy many persons; and since your most interesting lecture of the 16th ult. was the means of my discovering a far more forcible and complete illustration of this singular effect, I venture to trespass upon your time by detailing it to you. On my return home after that lecture I proceeded to make sundry experiments upon binocular vision, amongst which were the following:—

Fig. 1.

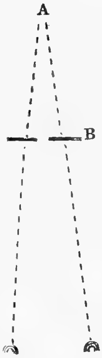
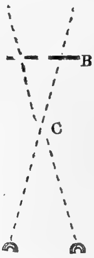


Fig. 2.



Centring the eyes upon a distant point A, I introduced a stereograph of the moon at such a distance from the eyes that the axis of each passed centrally through the picture taken for it; the result was a perfect and beautiful stereographic projection of the moon at A, apparently solid and spherical (as fig. 1). Then, without in any way altering the distance between the eye and the slide, I centred the eyes upon a point nearer to me than B, in such a way that the axis of the right eye passed through the centre of the left picture, the left also passing through the right: the result was a perfect view of the moon, but in this case *inside out,* as you termed it (the moon appearing quite concave), the image being seen *in the air at* C, and appearing in a most perceptible degree *smaller* than when in the former case I saw it apparently at A, nearly double the distance. Now as the distance between the real object and the eye was in both cases identically the same, the image upon the retina must have been of the same size in each experiment, the apparent enlargement of the object in one case and contraction in the other being purely mental. This singular effect may have been noticed by yourself before this; but in case it should not have come under your notice, I thought you would perhaps excuse the liberty I take in addressing this communication to you. Of course it would be quite out of place and equally unnecessary for me to explain the philosophy of the matter here, or the difficulties connected with first attempts to break the eye of the habit of focusing itself according to the amount of convergence; but for the delight with which I, as a young student of astronomy, made the above observation, as also for my future independence of all stereoscopes, I feel greatly and entirely indebted to the course of lectures upon light. In performing the second of the two experiments named, so perfect a perspective inversion is seen in the case of landscapes, that one feels for the moment endowed with the fabled optical powers of the lynx, men being seen most distinctly through brick walls, houses, or even hills.

Hoping, Sir, that you will excuse the liberty I have taken in thus addressing so long a communication to you,

Believe me to be, Sir,
Yours very respectfully,
RICHARD T. LEWIS.

LV. *On the Form and Distribution of the Land-tracts during the Secondary and Tertiary periods respectively; and on the effects upon Animal Life which great changes in Geographical Configuration have probably produced.* By SEARLES V. WOOD, *Jun.*

[Concluded from p. 282.]

SECTION 4.—*The effect produced by the Post-cretaceous Geographical Changes upon the Secondary Fauna.*

IF the foregoing inferences as to the respective geographical configurations of the secondary and post-cretaceous periods are well founded, the effect of the post-cretaceous changes upon the secondary fauna becomes more readily apparent.

An alignement of continent such as prevailed during the secondary periods from north to south, would, I conceive, necessarily have had the effect of assimilating the fauna of high and low latitudes in a great degree; a free and uninterrupted passage for currents of equatorial water along the coasts up into high latitudes, and the return currents from the poles, would have tended to the modification of the climate of the period, in the same way in which the western coasts of Europe and America are modified at the present day, and have produced that more equable rather than tropical character of climate which, it is now generally considered, characterized the prevalent climate of the secondary periods. The condition of the extreme lands of South America at the present time exemplifies the effect of the humidity produced by this north and south alignement of continent upon the capacity of what are considered the inhabitants of warm countries to support a very inclement climate. Here is found the fuchsia having perennial foliage, but which when transported to the dry but less inclement climate of England becomes deciduous. Here also the humming-bird finds food, enabling it to continue through rain and snow, while within 10 degrees of latitude to the southward the snow is perpetual at the waters' edge.

The climatal effects of the contrary configuration are shown in the Asiatic continent, where the extremes of heat and cold alternately prevail in much lower latitudes than the southern point of America, and where in the tropical and hypertropical countries there are substituted for these extremes the equally trying alternations of extreme aridity and excessive moisture. It is true that at the present day the effect of a trend of continent from north to south, where it still occurs, does not, much as it modifies the climate, present us with conditions analogous to those afforded by the secondary period, as we cannot suppose that at the extreme southern point of America reptiles such as existed during the oolitic age could still exist. Nor is this to be expected, as allowance has to be made for the effect on terrestrial climate produced by the numerous mountain chains of great elevation which have come into existence since the secondary period, to which in point of altitude we find no approximation among the mountain chains of that period; for even the Andes (which during that period would seem to have been a chain of volcanic islands) could not have had an altitude at all approaching that which it has at present, since the secondary deposits now occur in it at a considerable elevation. Allowance also should be made for the effect on the temperature of the ocean, by the increase in the depths corresponding to this increased altitude of the mountains*. Besides this, we are not to lose sight of the principle of the gradual refrigeration of climates in order of time, however much it may have been interfered with by the distribution of land at different periods.

The extension, however, of a great equatorial continent, such as prevailed (as I have attempted to show) during the intra-cretaceous and tertiary periods, must, I conceive, have brought into existence a state of things the most opposite from that which prevailed during the secondary period which it is conceivable for geographical changes to produce. Continents upon which occurred those extremes of heat and aridity, alternating with excessive moisture, that are caused by the conversion of the

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* See the views of M. Boué upon the heights and depths during geological periods, with his Table of them, in *Bull.* vol. xi. p. 62.

trade-winds into monsoons, which at the present day invariably occur in those continents that are washed on their southern border by tropical seas, would be no fit abode for terrestrial animals brought into existence under the opposite class of conditions, as were those which came into being during the long secondary period; and, as I shall attempt presently to show, the effect of the post-cretaceous changes was to raise into greater importance, during the tertiary period, those forms of terrestrial or fluviatile animals that are by their habits suited to sustain these altered conditions, and to destroy those which could support life only under the conditions of humidity and equable temperature prevailing when they came into existence.

Passing, however, for the present to the changes presented in marine animal life, we find one remarkable feature at the dawn of the tertiary period which appears to me to afford a clue to the entire change in marine vertebrate life that took place during the intra-cretaceous and tertiary interval; I allude to the disappearance of the tetrabranchiate family of Cephalopoda, with the sole exception of the Nautilus and Aturia, and to the preservation, and perhaps increase, of the dibranchiate Cephalopoda. We know that the Nautilus is a bottom feeder, and therefore *ex necessitate* a shore-follower; and there is reason to infer that the Ammonitidæ and other chambered Cephalopods of the newer secondary formations had similar habits: the abundance of these forms in those secondary formations which, like the lias, oolite, and cretaceous formations of England and Northern France, were deposited under littoral conditions, and in a partially land-locked gulf, supports this inference. We cannot suppose these chambered Cephalopods to have had habits in any way resembling the Dibranchiata, which at the present day are surpassed by no animal in their distribution over the ocean. This disappearance of the Ammonitidæ and preservation of the Nautilidæ, we may infer was due to the entire change which took place in the condition of the shores at the close of the cretaceous period; and this change was so complete, that such of the shore-followers as were unable to adapt themselves to it succumbed, while the others that adapted themselves to the change altered their specific characters altogether. The Nautilidæ having come into existence long prior to the introduction of the Ammonitidæ, and having also survived the destruction of the latter family, must have possessed in a remarkable degree a power of adapting themselves to altered conditions. It is evident also that ocean-rangers, such as the Dibranchiata, would be independent of those geographical changes; and these, again, are the forms which have been the most completely preserved, and which still exist as an important family*.

* The effect of geographical configuration upon marine life is shown

The disappearance of this great tetrabranchiate family affords, I think, the clue to the disappearance of the secondary marine saurians; while the development of the dibranchiate family has been commensurate with that of the Cetacean order, of some of which they form the food. The numerous family of Cestraciont fishes must have been mainly dependent upon a copious supply of mollusca for their food; and this most probably consisted mainly of the tetrabranchiate Cephalopoda, the means for the crushing of whose dense shells were afforded by the palate or tubercle teeth of these fishes. We may not unreasonably infer that the habits of fish feeding upon nearly stationary food, such as mollusca, would, unlike those of the Squalidæ, which now feed upon fish, have been sluggish. The marine Saurians, again, we may infer, procured their food from fish, and from those forms among them whose sluggish habits admitted of the easiest capture. Now the disappearance of the Tetrabranchiata, of the Cestracionts, and of the marine Saurians, was contemporaneous; and we can hardly refuse to admit that such a triple destruction must have arisen either from some common cause, or from these forms being successively dependent for existence upon each other. The habits of existing Squalidæ show that that order is not unsuited to a wide range remote from the shores, and thus independent of causes operating upon coast followers. The Squalidæ survived the post-cretaceous changes; but of the shore-followers existing at the cretaceous epoch, all the marine Sauria, all the Tetrabranchiata except the Nautilus and Aturia, and all the Cestracionts except the one-surviving Australian genus, perished between the cretaceous and the eocene epochs.

The changes of condition resulting from such an alteration in the geographical distribution as I have endeavoured to trace would, I conceive, by no means be confined to the marine fauna; its effects upon the Dinosaurian family by means of the extremes of aridity alternating with wet seasons, produced by monsoons parching up vegetation periodically, may be imagined if we reflect upon the prodigious amount of food required by the herbivorous forms of this order, and that the extinction of the herbivorous Dinosauria would involve that of the carnivorous. It is just those forms of Sauria which are suited to sustain these alternations of moisture and aridity which *did survive* the post-cretaceous changes, and still endure. The procœlian vertebrate form of Crocodile, which makes its appearance in the cretaceous deposits, has been throughout all the tertiary periods, and still is,

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notably in the case of the coral reefs, which occur almost exclusively on the eastern shores of the existing continents, although why this should be so we as yet know not.

the most important saurian; but the teleosaurian, or amphicœlian form of Crocodile, perished with the other marine Sauria with which it was by its structure of vertebra allied, whose habits it probably shared, and probably drew its sustenance from the same sources. The procœlian form, however, is and has been, through all tertiary periods at least, fluviatile or estuarine in its habits, and adapted by its vertebral column to sustain itself on land, and, inhabiting rivers, to draw its food from more than one source; this form also, by burying itself in mud, sustains periods of great drought by a species of hybernation, a habit partially shared by the peculiar reptilian form of fish (the Lepidosteus) which we find first appearing and in great abundance in the eocene formations.

These changes in animal life have been such as, reasoning *à priori,* one might predicate as likely to result from a change in geographical configuration from an alignement running north and south, producing a humid climate and an interchange of temperature between high and low latitudes, to an alignement from east to west with the land-tracts accumulated chiefly in low latitudes, producing a climate, not merely hot, but influenced by monsoons, which brought alternate seasons of moisture and aridity. Further, there is reason to infer that all the forms of terrestrial mammalia which are peculiarly adapted by migratory habits to obtain food in one region when, by the regular change of seasons, it has failed in another, of which the Ruminantia are the most striking example, have originated in that continent of which the Europeo-Asiatic one is a part, and which I have designated as the post-cretaceous continent.

The effect of the post-cretaceous changes upon reptile life, in extinguishing entirely several important orders and suborders, and nearly extinguishing others, was far greater than upon other forms of life. It is true that our knowledge of secondary warm-blooded life is as yet very limited; but none of the remains of that life hitherto obtained from secondary formations have been referred to any *order* not existing at the present day. It thus appears that the effect of the post-cretaceous changes upon animal life is commensurate with the degree in which that life is dependent upon climatal conditions. Reptilia at the present day are the most dependent upon climate, while Mollusca, Fish, and Mammalia are almost entirely independent of it; and the result of this dependence is, if the view put forward in the next section, of Australia being an isolated remnant of the secondary continents, be well founded, the state of change presented by the land and sea respectively of that country*.

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* Of the orders of Reptilia at present known from the cretaceous deposits, viz. Enaliosauria, Pterosauria, Dinosauria, Crocodilia, Lacertilia, and

SECTION 5.—*The preservation, at the present day, of isolated remnants of the Secondary Continents, and of the Secondary Fauna inhabiting them.*

I have endeavoured to show that a remnant of the continental tracts of the secondary period appears in the present Australian continent. Now it is an important fact that, with the possible exception of the Stereognathus, the nearest living affinities of the Mammalia yet discovered in secondary formations exist in Australia and its adjacent islands*, and in Madagascar. Many of the trifid footprints from the red sandstone of the Connecticut valley are admitted to be those of birds; and Sir Charles Lyell states† that in the impressions of the skin in some of them, Prof. Owen has recognized a resemblance to the skin of existing Struthionidæ. We cannot any longer, I think, hesitate to admit, notwithstanding the absence of osseous remains, that birds having affinities with the modern Struthionidæ and Dinornidæ existed in the triassic period. It is therefore significant to find that these modern Struthionidæ exist only (with the exception of Sumatra) in lands which, I have attempted to show, are remnants of the secondary continents; and that as regards all the other modern wingless birds except the Struthionidæ, *i. e.* the birds of Madagascar, the Mauritius and adjoining islands, and of New Zealand, they exist only in *isolated* remnants of those continents: and not less significant is it, that these forms of Mammalia and of modern wingless birds are associated with vegetable forms having the nearest affinities to the vegetation of the secondary and carboniferous periods—as witness the tree ferns and Cycadæ of Madagascar, Australia, and New Zealand, and the Araucariæ of various parts of the southern hemisphere; while in the Cestracion and Trigonia of the Australian shores are preserved the only living examples of those secondary genera.

Chelonia, only the last three appear to have survived the post-cretaceous changes; the other existing orders, Amphibia, Batrachia, and Ophidia, may, however, be expected to occur in the cretaceous formations. Of the cretaceous fish, not only did all the orders survive these changes, but the suborders and families also, and even about a fourth of the genera; while of the Mollusca, about a third of the genera (including the Cephalopoda) only perished.

* Not merely Tasmania, but New Guinea and Arroo, which have been shown by Mr. Wallace to possess a fauna entirely agreeing with Australia, and differing as entirely from the islands of the Indian Archipelago near to them. These two last-named islands appear to have been very recently severed from the ancient continent of Australia by the oscillations produced by the intensely active volcanic band of the Indian Archipelago.

† Man. Elem. Geol. 1851, p. 298.

These coincidences have, it is true, been explained by some on the ground that Australia, like the secondary lands, was adapted only for such forms of life, and that the animals most fit to inhabit it have been specially created there; but this explanation fails completely before our daily experience; and the explanation of their occurrence must be sought on other grounds. The only explanation which appears to me to be consistent with all the phenomena of the case is, that in these lands we possess remnants more or less isolated of the secondary continents. I by no means pretend that any such coincidences warrant a conclusion that this isolation has been in each case from the same period. On the contrary, while the Mammalia of Australia (among which occur the only known forms of the true Monotremata, the Ornithorhynchus and Echidna) and the birds of New Zealand, coupled with the predominance in the latter country of the fern tribe, conspire to show an isolation of those lands from a remote part of the secondary period, the fauna of the island of Madagascar, comprising as it does (and almost exclusively) the family of the Lemuridæ, points to an isolation of that country at a later part of the secondary period*. It will be seen whether or not these conjectures are well founded when future discoveries shall have made us better acquainted with the secondary Mammalia, and particularly the triassic forms, the latter of which, I anticipate, will be found to have their affinities rather with those of Australia than with those of Madagascar. I should advert also to the occurrence of the Dodo and its kindred in islands forming but the peaks of a submerged mountain chain connecting Madagascar with India. The organization of these birds was most unfavourable to migration; and the submergence of a country inhabited by them must necessarily have reduced the birds, first to insulated tracts surrounding the elevated ridges, and lastly to the mountain peaks themselves, which became small islands, as are Mauritius, Bourbon, and Rodriguez, in the midst of the Indian Ocean†.

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* See Lyell's 'Principles of Geology,' 1850, p. 610.

† These islands exhibit the very recent extinction of that volcanic action which has, as I conceive, reduced them from part of the mountain system of that ancient land which once united South Africa and Madagascar to India, into the condition of oceanic islands. See Maillard, *Bull.* vol. x. p. 499. These islands form, with the Cargados bank and the Chagos, Maldive, and Laccadive Archipelagos, a chain of submerged peaks parallel (as is Madagascar) with the eastern coast of Africa, and probably therefore part of the same geological systems which have imparted to the southern half of Africa and to Madagascar their geographical configuration—systems which, in Section 2, I have conjectured to be synchronous with the great known systems therein discussed—that governed the distribution of land during the secondary period, or at least then existing as part of the continents.

We find, further, that the only forms of wingless bird occurring in countries where the true Carnivora occur are those of the Struthionidæ; while, on the other hand, the lately exterminated forms of the Dodo and Pezophaps, the Dinornidæ and Notornis of New Zealand, and the wingless birds of Madagascar, have only been found to occur in lands where they have been secluded from these enemies, until, with the exception of the Apteryx, they perished at the hands of the most formidable of predaceous animals, Man. We are at no loss for the explanation why the Struthionidæ have sustained themselves in the face of these predaceous contemporaries; for the habits of wariness and swiftness of foot pertaining to this order of birds enable them successfully to escape the craft of man and the swiftness of his horse, and even the attacks of the lion. The gigantic birds of Madagascar and New Zealand have not yet been found to occur in Australia; but the presence of such gigantic forms of Carnivora in Australia as the Nototherium, may have been the cause of the extinction of these birds in that country, if indeed it were not man himself*. We see, further, that up to a certain period these gigantic birds survived upon the post-cretaceous continent, but hitherto they have not been found there in a deposit so recent as the period of the incoming of the true Carnivora. The Gastornis of the Paris Basin had, according to Prof. Owen, affinities with both the Notornis of New Zealand and with the Solitaire of the Indian Ocean, and it was perhaps almost as defenceless an animal; but we know of no more formidable Carnivora contemporaneous with that bird than the Hyænodon, and its allied Lophiodont carnivors.

Summary and Conclusion.

In the preceding sections I have endeavoured to deduce that the secondary continents, governed by the direction of the volcanic bands of the period, had an alignement trending nearly from north to south, and that this alignement had probably an important influence in the maintenance of the equable climates of the period; that while portions of these secondary continents have, we see, been incorporated into the post-cretaceous continents, other portions remain at the present day in a state of complete isolation, originating, however, at different dates; that an entire change in this alignement took place at the close of the cretaceous period, the east and west direction, in which the vol-

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* That they will be found, however, to have at one time existed there I feel little doubt; indeed their remains are stated to have occurred in caverns near Melbourne. See Blandowski, Trans. Phil. Soc. of Victoria, p. 55.

canic bands prevailed during the carboniferous epoch, being resumed, and that this direction has prevailed to the present time, being that generally of the mountain systems and other anticlinals which have an origin more recent than the cretaceous period, as well as of the chief volcanic bands now in activity other than the Andean and Rocky-Mountain band, which, as I have shown, is of ante-cretaceous origin; that this resumption of the volcanic direction of the carboniferous epoch by no means reproduced the geographical features of that palæozoic age (which were those of low-lying lands having an insular rather than a continental character), but brought into existence those stupendous upheavals of the earth's crust that have culminated at a very recent date in the formation of mountain chains incomparably exceeding in elevation, and consequently in their climatal effect, any of the upheavals of the secondary periods*; that this change produced, during the intra-cretaceous and tertiary interval, a vast continental extent of land uninterrupted by great mountain chains, extending from America on the west, to the Bay of Bengal on the east, if not perhaps to the centre of the South Pacific, and accumulated mainly in low latitudes; that the formation of such a continent introduced climates the most unlike those of the secondary period, and had its effect both on the condition of the seas near the land, and on that of the land itself; in the latter case by the introduction of alternations of aridity and moisture such as now occur on the southern shores of Asia; that the disappearance of the marine Saurians was consequent upon that of the Cestraciont fishes, the destruction of the latter having proceeded from the failure of the tetrabran-

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* The loftiest chain in the world (the Himalayah) has been formed *since* the *eocene* epoch, its area at that epoch having been comprised within the great nummulitic gulf, nummulites of a well-known European form (*N. Ramondi*) having been found in Cashmere 15,000 feet above the sea. See D'Archiac, *Bull.* vol. x. p. 380. *Animaux foss. de l'Inde*, p. 130.) The theory so long upheld, that the convulsions which the surface of the earth underwent in remote periods were on a far grander scale than those which have taken place during recent ages, is scarcely reconcileable with the fact that all the evidences which we have of mountain systems older than the tertiary (other than the Andes, whose elevation is due at least as much to tertiary as to older volcanic action) indicate not only a less entire disruption and inversion of strata than do many of the systems of tertiary origin, but they are altogether puny in point of elevation when compared with mountain chains whose grandest pinnacles are but the productions of a very late period (Himalayah, Ararat, Caucasus, Turco-Persian Mountains, Alps, &c.), and sink in comparison with such stupendous volcanoes as those of the Andes, of Teneriffe, of Timor, Hawaii, and the Antarctic Sea, and even with that of Etna. See the views of Ami Boué on the increasing heights of the mountains and depths of the seas in each successive geological epoch, and his Table of Heights and Depths, in *Bull.* vol. xi. p. 62.

chiate Cephalopoda which supplied their food; that the forms of Reptilia preserved or developed during this period were those (as the procœlian Crocodilia) which at the present day we find subsisting under these new and different conditions; that in several parts of the Southern Hemisphere there are still preserved to us remnants of the warm-blooded fauna of the secondary period in a state of isolation from different stages in that period; and that the disappearance of the wingless birds of the Trias, except the Struthionidæ, has taken place at the times when, and in the places where, they were exposed to the attacks of the Felinæ, or other carnivora of power and activity equal to the mastery of such powerful birds, or of man, and that the Struthionidæ alone, by their superior means of escape, have withstood these enemies.

The topics discussed in this paper are in harmony with Mr. Darwin's law of natural selection. The unequal rate at which some forms of mammalia have, when compared with others, changed their generic and specific characters, and even those of the suborders to which they belonged, as shown in the comparison of the change in the Opossum and Macacus with the true Ungulata, appears to be due to the greater influence which changing external conditions have upon some than upon other forms of life. The competition for existence, and the consequent elimination of new types of being, has had its maximum upon the Europeo-Asiatic continent; but the process has had its more limited parallel in the circumscribed regions formed by the isolated remnants of the secondary continents; for while the Lophiodontia, Solipedia, Ruminantia, Carnivora, and Pachydermata were eliminated in the former, a corresponding development of being in Australia, limited to the one order Marsupialia, took place in the introduction of the Macropi, Nototheria, Wombats, and other forms of Australian life, existing and extinct, analogous in their habits and powers with the characteristic mammalia of the Europeo-Asiatic continent, accompanied by a formidable carnivorous type, and in South America by the order Bruta and its allies.

The occurrence in Australia and its contiguous islands, and in Madagascar, of existing forms having the nearest affinities to the secondary Mammalia, and of the sole survivors of the once abundant Cestraciont fishes and mollusk Trigonia, appears to me susceptible of more rational explanation on the ground that in these regions we have preserved to us isolated tracts which once formed parts of the continents of the secondary period, in which a portion of the secondary inhabitants have, as it were, been imprisoned, than on the ground that the fauna of the secondary periods, and that of Australia, was and is respectively that most adapted to the conditions of habitat, and especially created with reference to the conditions under which it was to exist. We see the latter hypothesis fail when tested by the effect which the introduction of domestic animals, and of some of the wild ones which follow man in his migrations, has had in these regions—an effect so unmistakeable, that we cannot doubt but that the later introductions will eventually exterminate the indigenous population, and would have done so ages ago had the geographical conditions permitted the migration of the animals of the Europeo-Asiatic continent into these sequestered portions of an earlier land.

The preservation of the fauna of past geological periods in a state of isolation more or less complete has not, so far as I am aware, yet received much notice from geologists, doubtless from the reluctance hitherto exhibited by them to argue from any premises which involved the admission that all animated beings originated from common parents; but we now appear to be on the eve of a change in this sentiment, and of an admission that every organism has originated by parturition from one pre-existing, and not by creation out of an inorganic matrix, and that the organic world resembles an ever-branching tree, in which the orders, classes, subkingdoms, and even the animal and vegetable kingdoms themselves, are respectively connected by the lower or simpler types of each, rather than a chain or succession of types in which the lower grades of one group have originated out of the highest grades of that usually placed next below it. We may therefore look for less reluctance among geologists to approach a subject which is by no means confined to the more conspicuous examples which I have here sought to bring into notice. The preponderance of the orders Bruta and Edentata on the American continent, and the existence of the latter order in Africa and India, will, I think, one day be attributed to the isolation of portions of very ancient land in those places, from an epoch when those simpler forms of mammalia constituted the highest stage to which the animal kingdom had attained, and to the subsequent incorporation of those parts with newer land containing more advanced forms, permitting the reciprocal migration of their inhabitants. The preservation on various parts of the earth remote from each other of animals of kindred structure, limited now to almost a single species, and unfavourable to migration, such as the Proteus and Lepidosiren, or the few surviving forms of the Salamander, points also, in my mind, to a somewhat similar process commencing at a much more remote date, when the characteristics of these solitary survivors were those of the highest grades to which life had then attained,—a process, however, which has been modified by many subsequent occurrences, that have reduced the once great families to which these animals belonged to a few and scattered members*.

It has been demonstrated by Professor Owen, that in the earlier types of Vertebrata there were blended in one animal characters which have now become the peculiarities of suborders; and all the observations of naturalists conspire to show that as we recede in order of time, the confluence of types tends to a junction of branches with stems, and these again at remoter points with trunks conducting towards a root common to the animal and vegetable kingdoms. If, therefore, it can be shown with any degree of probability that there exist remnants of an earlier state of being, preserved in a state of complete isolation, which belong exclusively to one or more of the simpler forms, as is the case with the implacental mammalia of Australia, another link is added to the chain of evidence that all forms of life have originated, by a natural course of reproduction and slow variation, from one common root.

I would also lastly add that the unequal rate of change in some families of mammalia compared with others, and particularly the remarkable constancy of character exhibited by the Macacus since the eocene period, while entire suborders of Ungulata, which were coexistent with that genus, have disappeared, may serve to elucidate the antiquity of man, and seems to me to lead us to the presumption of a far greater antiquity for our race than has hitherto been accorded to it, reaching perhaps far back into the tertiary period.

Whether the inferences I have ventured to draw as to the alignement of the lands during the three periods into which geological formations are divided, and the preservation of portions of that land and of its inhabitants in an isolated condition, be or be not well founded, it will be my endeavour to collect all new facts bearing on the subject which the labours of geologists are constantly accumulating, and to collate them with impartiality, whether they support or militate against these inferences, and I hope at some future day to embody them in another paper.

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* This process of the isolation and subsequent incorporation of detached lands taking place at very remote dates may perhaps afford an explanation of the much debated and apparently anomalous case of the anthraxiferous beds of the Alps, where, alternating with beds containing remains of plants all belonging to palæozoic genera, and in many cases to well-known carboniferous species, occur beds with true liassic forms of mollusca. See S. Gras, *Bull.* vol. xii. p. 273.

LVI. *Note on the Occurrence of Flint Implements in the Drift.*

By BALFOUR STEWART, *A.M.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

WITHOUT pretending to be a geologist, I may yet be permitted to point out a general property of matter which may perhaps tend to modify the conclusions which some have derived from the occurrence of flint implements in the drift. Taking it for granted that such implements have been found in this deposit, and that they are the work of human hands, I do not yet feel prepared to allow the great antiquity of our race as a logical conclusion.

May it not be laid down as an axiom in physical science, that no substance whatever possesses a quality in such perfection as to exclude absolutely the opposite and antithetical quality?

In proof of this statement, it may be asserted that the most mobile liquids with which we are acquainted are yet viscous in some degree, and have therefore so far the properties of a solid; while, on the other hand, some of apparently the most solid bodies exhibit to some extent the properties of a liquid.

A very notable instance of this latter class is found in glacier ice, which, as Principal Forbes has shown, behaves like a somewhat viscous body, gradually moving down its bed, although it may be nearly two centuries before a particle finds its way from the top to the bottom of this river of ice.

May not the drift and superficial deposits which cover the surface of our globe be of this character—somewhat resembling mud, only very much more consistent, but yet not absolutely free from all traces of fluidity?

If this be allowed, it follows that bodies of some size placed in this deposit will in the course of ages find their way from the top to the bottom, if they possess a higher specific gravity than the drift in which they are placed. I think I am right in asserting that flint implements are of this nature; and it therefore becomes a question whether these may not have a slow downward secular motion in this deposit.

The consequence of such would be that, if merely judged by their position in the drift, we should ascribe to these implements a much greater age than they are really entitled to. This peculiar motion would, however, not take place in the case of human remains coeval with the flints, these remains being of small specific gravity; and we may thus perhaps explain the very remarkable fact, that while flint implements in abundance have been found in the drift, no human remains have as yet been discovered. I need hardly remark that the downward secular motion of the flints which is here supposed must be an exceedingly slow one, perhaps not more than a small fraction of an inch in a year. It would of course be very desirable to obtain experimental evidence of such a property of the drift; but this would be an exceedingly difficult and tedious task. In absence of such experiments, corroborative evidence may perhaps be derived from the fact of the disappearance of Roman villas which are afterwards exhumed. Coupling this testimony with that derived from the general character of the matter which surrounds us, it cannot be denied that such a property of the drift is at least possible; in which case we should, I think, be deterred from framing any theory regarding the great age of our race based upon the position of these implements, especially when such views are contradicted by strong evidence from another quarter.

I am, Gentlemen,
Your obedient Servant,
BALFOUR STEWART.

Kew Observatory,
April 16, 1862.

LVII. *Proceedings of Learned Societies.*

ROYAL INSTITUTION OF GREAT BRITAIN.

March 28, 1862.—John Peter Gassiot, Esq., F.R.S., Vice-President, in the Chair.

"AN Explanation of the Meteorological Telegraphy, and its Basis, now under trial at the Board of Trade." By Rear-Admiral FitzRoy, F.R.S.

The telegraphic communication of meteorological changes from distant stations to a central position, whence occasional warnings of impending storms might be given, which has been organized and tried by Government, had its origin at a Meeting of the British Association in 1859, at Aberdeen, under the presidency of that deeply lamented Prince whose short life was wholly devoted to the most useful objects. It was then resolved by their Council that application should be made to Her Majesty's Government for an organization and trial of a plan by which the approach of storms might be telegraphed to distant localities. At two meetings in Buckingham Palace, early the following year (1860), minutes were authorized on this subject, and correspondence ensued which resulted in establishing a telegraphic communication of meteorological facts between twenty home stations, besides foreign ones, and daily with Paris.

The Aberdeen Meeting had only just terminated, when public attention was startled by the loss of the 'Royal Charter.' It so happened that the storm which caused the destruction of that iron ship—notwithstanding power of steam additional to that of sails, and while a *sailing* ship, managed differently, was bearing its brunt uninjured within a few miles distant*—that storm, completely cyclonic, passed over the middle of England, and could be more fully investigated than *any* storm hitherto, because in every direction

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* 'The Cumming.'

observers happened to be ready, who recorded ample statical facts and many valuable dynamical results.

Advancing gradually, the first cautionary or storm-warning signals were made early in 1861, but on that occasion were unhappily disregarded in the Tyne; and on the following days awful losses of life were witnessed on the north-east coasts. From that time to the present similar warnings have been given there and elsewhere—with increasingly advantageous effects, it appears, if one may judge, in the first instance, by applications since sent officially from all the principal ports, and from the chief associations of Underwriters, in addition to Admiralty approval and the cooperation of the Coastguard.

In August 1861, the first *published* "forecasts" of weather were tried; and after *another* half-year had elapsed for gaining experience by varied tentative arrangements, the *present* system was established. Twenty reports are now received each morning (except Sundays), and ten each afternoon, besides five from the Continent. Double forecasts (*two* days in advance) are published, with the full Tables (on which they *chiefly* depend), and are sent to six *daily* papers, to one weekly, to Lloyds', to the Admiralty, and to the Horse Guards, besides the Board of Trade.

These forecasts add almost nothing to the pecuniary expense of the system, while their usefulness practically is said to be more and more recognized*. Warnings of storms arise out of them; and (scarcely enough considered) the satisfaction of knowing that no very bad weather is imminent, may be very great to a person about to cross the sea. Thus their negative evidence may be actually little less valuable than the positive.

Prophecies or predictions they are not: the term forecast is strictly applicable to such an *opinion* as is the result of a scientific combination and calculation, liable to be occasionally, though rarely, marred by an unexpected "downrush†" of southerly wind, or by a rapid electrical action not yet sufficiently indicated to our extremely limited sight and feeling. We shall know more and more by degrees. At present it is satisfactory to know that the measures practised daily in these proceedings do not depend solely on one *individual*. They are the results of facts exactly recorded, and deductions from their consideration, for which rules have been given. An assistant has been practised and is able to share their responsibility. Others are also advancing in the subject of dynamical meteorology.

Air-currents sometimes flow side by side, though in opposite directions, as "parallel streams," for hundreds or even thousands of miles. Sometimes they are more or less superposed, occasionally, indeed *frequently*, crossing at various angles; sometimes combining, and by the *composition* of their forces and *qualities* causing those

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* At a recent meeting of the shareholders of the Great Western Docks at Stonehouse, Plymouth, it was stated officially that "the deficiency (in revenue) is to be attributed chiefly to the absence of vessels requiring the use of the graving docks for the purpose of repairing the damages occasioned by storms and casualties at sea." (Feb. 24, 1862.)

† Herschel.

varieties of weather that are experienced as the wind veers more toward or from the equator or the nearest pole; and sometimes so antagonistic in their angular collision as to cause those large circling eddies or rotatory storms called cyclones (in modern parlance), which are really like the greater storms in all parts of the world, although they do *not* quite assimilate to those local whirlwinds, dust-storms, and other commotions of atmosphere which seem to be more *electrical* in their characteristics, if not in their origin.

Whenever a polar current prevails at any place, or is *approaching*, the air becomes heavy, and the barometer is high or rises. When the opposite (equatorial or tropical) prevails or approaches, the mercury is low or falls, because the air is, or is *becoming*, specifically lighter; and these changes take place *slowly*.

Whenever, from any causes—electrical, chemical, or simply mechanical—either current, or any combination of currents, ceases to press onward *without being opposed*, a *gradual* lightening of the atmosphere through a greater or less area, of hundreds or perhaps thousands of miles, occurs, not suddenly, but very gradually; and the barometer falls. There is less tension.

To restore equilibrium, the nearest *disposable* body of air (so to speak), or most moveable, advances first; but an impulse at the same time may be given to other and greater masses that, though later in arriving, may be stronger, last longer, and cause greater pressure mechanically as well as by combination. Air, like water, mingles very slowly, either from above or laterally.

Taking, with Dove, north-east and south-west (*true*) as the "wind-poles," all intermediate directions are found to be more or less assimilated to the characteristics of those extremes; while all the variations of pressure or *tension*, many of them caused by temperature, and all varieties of winds, may be clearly and directly traced to the operations of two constant principal currents, equatorial or tropical, and polar—our north-east and south-west.

Great distinction should be marked betwen those ever alternate and often conflicting main currents, tropical and polar, and the *local effects* of their union or antagonism, namely mixed winds, whether westerly or easterly, with occasional cyclones or circulating eddies, on a large or small scale.

Considering that the lower current does not ordinarily extend far upward (only a few thousand yards, or even feet), and that high land, mountains, especially *ranges* of mountains, alter and impede its progress, a variety of eddy winds, or, as it were, streams of wind, with local and apparently anomalous effects, must be frequently caused.

Electrical action, condensation of vapour into hail, snow, rain, or fog causing (*heat*), or its other changes, namely, evaporation, rarefaction, and expansion (absorbing heat, and therefore causing *cold*), immediately affect currents of air in a degree proportional to such influence, inducing horizontal motion.

The polar current always *advances* from the polar quarter, while *laterally* moving eastward (like a ship making lee-way), being pressed towards the east by the tropical flow which advances from the *south-*

westward, usually above and at an angle with the polar stream or current of air, often mixing with it, but at times *separately* penetrating downward, then sweeping and warming the earth's surface, uncombined with the polar current, even while feeling its approaching influence—and thus, as it were, forcing passages between streams of chilling polar air that at the same time are moving in opposite and nearly parallel directions.

Sometimes their opposition is so equal, and equilibrium is so complete, that a *calm* is the result, no sensible movement *horizontally* along the earth's surface being perceptible.

Self-registering barometers show the alterations in tension, or, so to speak, the *pulsations* on a large scale, of the atmosphere by hourly marks; and the diagram expresses to a practised observer what the "indicator card" of a steam cylinder shows to a skilful engineer, or a stethoscope to a physician.

Our own islands have very peculiar facilities for meteorological communication by telegraph between outlying stations on the sea-coast and a central place, all being at nearly the same level, and nearly all comparatively uninfluenced by mountain-ranges.

And now the results are, that having daily knowledge of weather (including ordinary facts of a meteorological nature) at the extreme limits and centre of our British Islands, we are warned of any *great change* taking place, the greater atmospherical changes being measured by days, rather than by hours. Only local changes, however violent they may be occasionally (and dangerous in proportion to their suddenness and violence), only such changes are unfelt at a distance, and do not influence great breadths, say hundreds of miles in area, of atmosphere.

Extensive changes, showing differences of pressure above or below the normal or mean level, amounting to an inch or thereabouts, are certain to be followed by a marked commotion of the elements in the course of a few days. If the fall has been sudden, or the rise very rapid, swift but brief will be the resulting elementary movement; if slow or gradual, time will elapse before the change, and the altered state of weather will take place more gradually, but last longer.

Notice may thus be obtained and given a few hours or a day, or even some days, before any important change in the weather actually occurs.

Having such knowledge, it obviously follows that telegraphic warning may be sent in any direction reached by the wires, and that occasionally, on the occurrence of very ominous signs, barometric and other, including always those of the heavens, such cautions may be given before storms as will tend to diminish the risks and loss of life so frequent on our exposed and tempestuous shores.

It has been proved also lately that storms, indeed all the greater circulations of atmosphere between the tropics and polar regions, have an eastward motion bodily while circulating around a centrical area. Within the tropics it is otherwise, or westward, till they *recurve*.

In answer to a question from the Royal Commissioners on Lights, Buoys, and Beacons, Sir John Herschel stated that "the most important meteorological communication which could be telegraphed would be information *just fresh received by telegraph* of a cyclone actually in progress at a great distance, and working its way towards the locality. There is no doubt that the progress of a cyclone may be telegraphed, and might secure many a ship from danger by fore-warning."

Successive or rather *consecutive* gyrations, circuits, or cyclones often affect one another, acting as temporary mutual checks, until a combination and joint action occurs—their union causing even *greater* effects, as may be seen even in water-currents, as well as in the atmosphere.

Between the tropics and the polar regions, or in temperate zones, the main currents are incessantly active, while more or less antagonistic, from the causes above mentioned; besides which, whenever considerable changes of temperature, development of electricity, heavy rain, or these in combination, cause temporary disturbance of atmospheric equilibrium (or a much altered *tension* of air), these grand agents of nature—the two great currents—speedily move by the *least-resisting lines* to restore equilibrium, or fill the comparative void. One current arrives probably or acts *sooner* than the other; but invariably collision occurs of some kind or degree, usually occasioning a circuit, a cyclonic or ellipsonic gyration, however little *noticed* when gentle or moderate in force.

As there must be resistance to moving air (or conflicting currents) to cause gyration, and as there are no such causes, on a large scale, near the equator, there are no storms (except local squalls) in very low latitudes.

It is at some distance (from about five to twenty degrees) from the equator that hurricanes are occasionally felt in their violence.

They originate in or near those hot and densely-clouded spaces sometimes spoken of as the "*cloud-ring*," where aggregated aqueous vapour is at times condensed into heavy rain (partly with vivid electrical action) and a comparative vacuum is suddenly caused, towards which air rushes from all sides. That which arrives from a higher latitude has a westwardly, that from a lower an eastwardly tendency, due to the earth's rotation and to the change of latitude, whence a chief cause of the cyclone's invariable rotation in one direction, as above explained.

The hurricane or cyclone is impelled to the *west* in *low* latitudes, because the tendency of *both* currents there is to the westward along the surface, although one, the tropical, is *much less so*, and becomes actually easterly *near* the tropic, after which its equatorial centrifugal force is more and more evident, while the *westwardly* tendency of the polar current diminishes; and therefore at that latitude hurricane cyclones cease to move westward (recurve), go then eastwardly, and toward the polar quarter.

Great and important changes of weather and wind are *preceded*, as well as accompanied, by notable alterations in the state of the atmosphere. Such changes, being indicated at *some* places sooner than at others around the British Islands, give frequent premonitions; and therefore great *differences* of pressure (or tension) shown by barometer, of temperature, of dryness, or moisture, and direction of wind should be considered as *signs of changes likely to occur soon.*

It will be observed, on any continued comparison of weather-reports, that during the stronger winds a far greater degree of uniformity and regularity is shown than during the prevalence of moderate or light breezes: and this should be remembered.

When neither of the greater and more extensive atmospheric currents is sweeping across the British Islands (currents of which the causes are remote, and on a large scale), the nature or character of our winds approaches, and is rather like, that of land and sea breezes in low latitudes, especially in summer.

Either the cooler sea-wind is drawn in over land heated by the summer sun, or cold air from frosty heights, snow-covered lands, or chilly valleys moves towards the sea, which is so *uniform in temperature* for many weeks together, changing so *slowly*, and but little in comparison with land during the year. These light *variables* may at such times be numerous, simultaneously, around the compass, on the various coasts of the British Islands.

Frequently it has been asked, "In this country, how much rise or fall of the glasses may foretell remarkable change or a dangerous storm?"

To which can now be replied, great changes or storms are *usually* shown by falls of barometer exceeding an inch, and by differences of temperature exceeding about fifteen degrees. A tenth of an inch an hour is a fall indicating a storm or very heavy rain. The more rapidly such changes occur, the more risk there is of dangerous atmospheric commotion.

As all barometric instruments often, if not usually, show what may be expected a day or even days in advance, rather than the weather of the present or next few hours, and as wind, or its *direction*, affects them much more than rain or snow, due allowance should always be made for days as well as for hours to come.

The general effect of storms is felt unequally in our islands, and less *inland* than on the coasts. Lord Wrottesley has shown, by the anemometer at his observatory in Staffordshire, that wind is diminished or checked by its passage over land. The mountain ranges of Wales and Scotland, rising two to four thousand feet above the ocean level, must have great power to alter the direction and probably the velocity of wind, independently of alterations caused by changes of temperature.

"It not unfrequently happens that a series of cyclones follow closely upon each other for several weeks, the preceding members of the series being often overtaken and interfered with by those succeeding. It is, however, important to remark that, amidst all the complexity necessarily occasioned by such combinations, the greater and more violent storms, and particularly that portion of them which is most dangerous and destructive, exhibit almost invariably the *simple* cyclonic character. It is thus with the 'Law of Storms' as with the 'Law of Gravitation;' the grand results of both are exceedingly simple, but the minor details become more and more complicated in proportion to their minuteness*."

Consecutive storms, at the meetings of main currents in zones of latitude at certain periods, have had appearances of continuity. The familiar instance of the 'Charles Heddle' has so often been adduced as proof of continuing circuitous action or gyration, that it may seem injudicious to doubt the *evidence*; but knowing how frequently circuits or cyclones succeed each other *rapidly*, and how unreliable are some of the earlier logs of events in a storm *written after its cessation*, especially respecting directions of wind and courses steered, when waves and storm-blasts were the guides, not the oscillating compass (if indeed *that* had not been washed away, as in the 'Charles Heddle's' case), it does not appear accordant to experience and enlarged acquaintance with the subject to imagine that such atmospheric eddies are *sui generis*, erratic, and so considerably independent as to cross a wide ocean.

When opposing currents meet, their masses must *continue* in motion a certain time, either rotating or ascending, or going onward horizontally in *combination*.

Masses of air, either of polar or tropical origin, so to speak *returning*, when driven back by stronger opposition, at first and for a certain time retain the characteristics of their peculiar and very different natures.

In our latitudes there is a continuous alternation of air-currents, each specifically different, and denoting approach by marked characteristics; and we have proved, by successive series of simultaneous statical observations over a wide range, embracing Scotland, Ireland, all England, and adjacent islands, that while these alternating or circuitously moving currents are thus incessantly passing, the whole body of atmosphere filling our temperate zone is moving gradually towards the east at an *average* rate of about five geographical miles an hour.

During strong westerly winds this eastward motion is greatly increased; and in easterly gales it is proportionally diminished, as measured by its passage along a horizontal surface of earth or ocean. Knowing these circumstances, and having accurate statical observations of these various currents at selected outlying stations, showing pressure (or tension), temperature, and relative dryness, with the direction and estimated horizontal force of wind at each place simultaneously, the dynamical consequences are already measurable approximately on geometrical principles; and, judging by the past, there appears to be reasonable ground for expectation that soon meteorological dynamics will be subjected to mathematical analysis and accurate formulas. The facts now weighed and measured mentally, in what may be correctly called "forecasting" weather, are the direction and force of *each* air-current or wind, reported telegraphically to the central station in London, from many distant

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* W. Stevenson, of Dunse, 1853.

stations, their respective tension and temperature, moisture or dryness, and their changes since former recent observations.

These show whether any or either movement or change is on the increase or decrease, whether a polar current is moving *laterally* off —passing from our stations towards Europe—or approaching us from the Atlantic, whether moving *direct* towards the south-westward with great velocity or with slow progress. If moving *fast* in the direction of its length, it will approach England more from the east, its speed *direct* being twenty to fifty or eighty miles an hour, while its *constant* lateral or easterly tendency (like a ship's leeway in a current), being only five miles an hour, is then insensible to us (though clearly deducible from other facts ascertained), and is that much in alteration of actual *direction,* as well as of what would *otherwise* be the velocity of the polar current.

With the opposite principal current, the equatorial or south-westerly, more briefly and correctly *tropical,* similar but opposite results occur: the direct motion from a south-*westerly* quarter is *accelerated,* sensibly to our perception, by *part* of the *eastward* constant (about five miles hourly); and therefore a body of air approaches us *sooner* (other things being equal) from the westward than it does from the eastward.

To seamen accustomed to navigate in ships making leeway, while in currents setting variously over the ground, such movements, complicated as they may appear, are familiar. Another important consideration is the disposal or progress of bodies of air united, or mixed, or contiguous to each other after their meeting, either directly opposed or at an angle on the earth's (or ocean's) surface. They do not vanish. They cannot go directly upwards—against gravitation; westward they cannot generally go when there is collision or meeting, because the momentum, elasticity, and extent of the tropical "anti-trade*" usually overpowers any direct polar current, or rises over it, and more or less affects the subordinate below, by the friction of its eastward pressure. Downward there is no exit; eastwardly (towards the east) the accumulating air must go; and this tendency, continued, causes the *varieties* of wind from the westward, being more or less mixed, more or less purely polar or tropical as either one prevails in combination.

After a body of air has passed and gone to some distance southward or northward, it may be stopped by an advancing and more powerful mass of atmosphere which is moving in a direction contrary to, or diagonally across, its line of force. If their appulse be gradual and gentle, only a check occurs, and the weaker body is pushed back until its special qualities, respecting temperature and moisture, are so masked by those of its opponent as to be almost obliterated. But if these currents meet with energy at very different temperatures and tension, rapid changes are noticed as the wind shifts, and circuitous eddies, storms, or cyclones occur.

Otherwise, when their meeting is, as first mentioned, *gradual,* there is the *return* of a portion of either current (which previously

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* Sir John Herschel's excellent term.

prevailed), either direct or deflected—deflected even through more than one quadrant of a circle—by its advancing opponent, and retaining for some considerable time its own previous characteristics.

Thus we have for short times cold dry winds from the south-west, instead of the usual warm and moist ones, or winds of this latter kind from the north instead of cold ones. The circuitous tendency of air in motion, and the numerous impediments to its horizontal progress, such as land, ranges of mountains, hills, or even cliffs, induce many a deviation from normal directions extremely puzzling to the student of this subject; but so retentive is air of its tension and temperature for a time, that, like currents in the ocean, each may be traced by its characteristics as long as within our island web of stations. When the polar current is driven back by a tropical advancing from a southerly direction gradually, their action united becomes south-easterly (from the south-eastward); and as the one or other prevails, the wind blows more from one side of east or from the other.

So retentive of temperature are oceanic currents, that when H.M.S. 'Nile' was going from Halifax to Bermuda, in May 1861, Admiral Milne found the temperature 70° at the bow while only 40° at the stern, as he entered the Gulf-stream.

Time is required to produce motion in the air horizontally; *time* is indispensable for its gradual cessation from movement. *Statical* effects are noticed at observatories, or by careful observers anywhere, hours or days before dynamical consequences occur.

The present daily forecasts or premonitions of weather are drawn up on the following arrangement. Districts are thus assumed:—

1. North Britain (including from the Moray Firth to the middle of Northumberland), along the coast.
2. Ireland, generally, around the coast.
3. Central (Wales to the Solway), coastwise.
4. East Coast (from Northumberland to the Thames).
5. South England (from the Thames round to Wales), by the coast.

As our space is very limited, and as *some* words are used in *different senses* by different persons, extreme care is taken in selecting those for such brief, general, and yet *sufficiently definite* sentences as will suit the purpose.

Such words as are on *published* scales of force or nature of wind and weather are *generally* understood, and therefore used in preference to others.

In saying on any day what the probable character of the weather will be tomorrow, or the day after, at the foot of a table showing its observed nature that very morning, a *limited* degree of information is offered for about two days in advance, which is as far as may be yet trusted generally, on an average, though at times a longer premonition *might* be given with sufficient accuracy to be of *occasional* use.

Minute or special details, such as showers at particular places, or merely local squalls, are avoided; but the general or average characteristics, those expected to be principally prevalent (with but few exceptions) the following day and the next after it, including the nights—not those of the weather actually *present*—are *cautiously* expressed after careful consideration.

It may now be seen, after many months' trial, whether tolerably correct forecasts of ordinary weather can be formed here *sooner* than at distant *isolated* places, where the published *general* Reports arrive a day or two later, and whether they are practically useful as conclusions available for the public.

Ordinary variations of *cloudiness*, or clear sky, or rain, of a *local* or only temporary character, are not noticed usually.

That a broad *general average* or *prevalence* is kept in view, referring to a day or more in *advance*, and to a *district* rather than only to *one* time or place, should be remembered.

The great practical difficulty is in separating the effect, on the mind, of *present* states of air, weather, and clouds, from abstract considerations of what may be *expected* on the morrow, or next following day.

When in doubt, distrusting the indications or inferences from them (duly considered on purely scientific principles, and checked by experience), the words "*Uncertain*" or "*Doubtful*" may be used without hesitation.

As meteorological instruments usually foretell important changes by at least a day, or much longer, we have to consider what wind and weather may be expected from the morning observations compared with those of the days immediately previous, as indicative of the morrow's weather, and of the day after, at *each* place—to take an *average* of those *expectations* for each district collectively *in groups*, and then to estimate dynamical effects.

Outline maps with moveable windmarkers, and cyclone-glasses or horns, are useful in forecasting weather; and full consideration should be given to the probable position, direction, extent, and degree of progress of that centrical area or node round which the principal currents usually circulate or turn as they meet and alter, combine with, or succeed one another.

Here dynamical considerations, with comprehensive comparisons of statical facts, are most important; and to treat them even approximately well, with such quick despatch as is requisite, demands aptitude and experience.

Those who are most concerned about approaching changes, who are going to sea, or on a journey, or a mere excursion,—those who have gardening, agricultural, or other out-door pursuits in view, may often derive useful *cautionary* notices from these published *expectations* of weather, although (from the nature of such subjects) they can be but *scanty* and imperfect under present circumstances.

Objection has been taken to such forecasts, because they cannot be always exactly correct for all places in one district. It is, however, considered by most persons that general comprehensive expressions in aid of local observers, who can form independent judgments from the Tables and *their own instruments* respecting their immediate vicinity, *though not so well for distant places*, may be very useful as well as interesting; while to an unprovided or otherwise uninformed person, an idea of the kind of weather thought *probable* cannot be otherwise than acceptable, provided that he is in no way *bound* to act in accordance with any such views against his own judgment.

Like the storm-signals, such notices should be merely *cautionary* —to denote anticipated disturbance *somewhere* over these islands— without being in the least degree compulsory, or interfering arbitrarily with the movements of vessels or individuals.

Certain it is, that although our conclusions may be incorrect, our judgment erroneous, the laws of nature and the signs afforded to man are invariably true. Accurate interpretation is the real deficiency.

Seamen know well the marked characteristics of the two great divisions of wind in all parts of the world, and do not care to calculate the *intermediate* changes or combinations to two or three points. They want to know the *quarter* whence a gale may be expected, whether northerly or southerly.

Every seaman will admit that, however useful, and therefore desirable, it would be to know exactly the *hour* of a storm's commencement, as our acquaintance with meteorology does not enable such times to be fixed, the next best thing is to have limits assigned for extra vigilance and due precaution, which limits *are* clearly stated in all the printed popular instructions to be from the *time* of hoisting the signal until *two* or *three days afterwards*.

But, say some, and justly, are ships to remain waiting to avoid a gale that, after all, may not happen? Are fishermen and coasters to wait idle and miss their opportunities? By no means. All that the cautionary signals imply is:—"Look out." "Be on your guard." "Notice your glasses and the signs of the weather." "The atmosphere is much disturbed."

Many remarkable cases have occurred which show the value of such warnings or cautionary notices. Some have been published in newspapers, and need not be repeated; others have been communicated only by private letters; and one or two of them may be mentioned now as instances.

Admiral Evans wrote that on one evening, after a warning had been given at Liverpool, such a sudden (though brief) storm swept over the Mersey as would have done much harm had not the harbourmaster made due preparations because of the signal.

A gentleman intending to cross the Irish Channel with an invalid lady was warned to *wait*, though the weather *then* looked beautiful in London. That night it blew a "hurricane" on the west of Ireland, and a gale in the Irish Sea which lasted the following day.

Three ships of war were lying in Plymouth Sound ready to sail for the West Indies. They waited two days, being cautioned, and then put to sea in the intervening lull (as it happened) between two gales, the first of which was blowing while they were detained; and the second was a violent cyclone that crossed France, the Netherlands, and Denmark, of which the northern semicircle swept our south and south-eastern coasts, but was *just avoided* by Her Majesty's squadron, or rather utilized by them, as they steered to the westward nearly before its easterly wind for some hours, and so passed out clear into the Atlantic.

Full warning was given along our eastern coasts of that storm in which the Prussian corvette 'Amazon' was totally lost; and so struck were the Prussian authorities by the facts of that period, taken in connexion with other known cases, that an official application was soon afterwards made to the Board of Trade for information, with the view of enabling a similar system to be organized in the Baltic, communicating if possible with England.

On the 12th of November, 1861, a warning was sent to Yarmouth in the afternoon. Being nearly dusk, and having then no night signals, nothing was done till next day, *after all* the fishing-boats had gone far out to sea, having started very early in the morning. That afternoon there was a storm; and to save their own lives, the fishermen were obliged to cut from and abandon some 40,000*l.* worth of nets and gear. Night signals might have saved that loss, and the imminent risk of many lives. Such means are ready now.

On Friday, the 7th of March, the warning-drum was hoisted all day at Plymouth. *Saturday* was so fine in *appearance* that the caution was not appreciated, and mackerel-boats went to a *distance*, as usual. That afternoon *another* signal was made—South cone under drum—to show that a heavy southerly gale was coming soon. It was a *beautiful* afternoon. No one anticipated the sequel, except those who, spider-like, could "feel along the lines." Before midnight there was a storm, which lasted much of the next day. One of the boats was lost with eight men. "A more *dangerous* gale had not been known," was written by an officer of experience and good judgment, in his letter to a friend.

Perhaps sufficient thought has not always been given to the consideration of mere pecuniary *loss* by wear and tear, risk, accident, delay, and demurrage caused by a gale at sea balanced, against the results of waiting for a tide or two, perhaps *once in two months*, when cautioned by a storm-signal.

But, be this as it may with coasters, short traders, or even screw-colliers, the question is entirely different with ordinary over-sea or foreign-going ships, especially when starting from a southern or from a western port. To such vessels a gale in the Channel, or even during the first day or two after clearing the land, must always be very prejudicial. Officers and men are mutually strange. Things are not in their places, often not secured, and the ship perhaps is untried at sea. Of course, however, these remarks are inapplicable to fine first-class ships, and to powerful well-managed steamers, independent of wind and weather, which start at fixed hours.

In conclusion, it appears that if due attention be paid on the coasts to cautionary signals, and at the central office to the telegraphed reports, no very dangerous storm need be anticipated without more or less notice of its approach being generally communicated around the British Islands, or to those coasts which are likely to be most affected by its greatest strength.

ROYAL SOCIETY.

[Continued from p. 331.]

May 2, 1861.—Major-General Sabine, R.A., Treasurer and Vice-President, in the Chair.

The following communication was read :—

"Note on Professor Faraday's Recent Experiments on 'Regelation.'" By Professor James Thomson, Queen's College, Belfast.

Some time ago*, Principal James D. Forbes showed that two slabs of ice, having each a face ground tolerably flat, and being both suspended in an atmosphere a little above the freezing-point upon a horizontal rod of glass passed through two holes in the plates of ice, so that the plates might hang vertically and in contact with one another, would unite gradually so as to adhere strongly together. This interesting experiment Principal Forbes adduced as being in opposition to the theory offered by me† of the plasticity of ice, and of the tendency of pieces of thawing ice to unite when placed in contact. He thought it showed that pressure was not essential to the union of the two pieces of ice. I pointed out, in reply‡, that the film of water between the two slabs, being held up against gravity by the capillary tension or contractile force of its free upper surface, and being distended besides against the atmospheric pressure, by the contractile force of its free surface round its whole perimeter—except for a very small space at bottom, from which water trickles away, or is on the point of trickling away,—exists under a pressure which, though increasing from above downwards, is everywhere, except at that little space near the bottom, less than atmospheric pressure :—that hence the two slabs are urged against one another by the excess of the external atmospheric pressure above the internal water pressure, and are thus pressed against one another by a force quite notable in amount ;—that, further, the film of water existing as it does, under less than atmospheric pressure, has its freezing-point raised in virtue of the reduced pressure ; and would therefore freeze even at the temperature of the surrounding ice, which I took to be the freezing-point for atmospheric pressure ; and would still more strongly be impelled to freeze by the joint action of this condition with the cold given out in the melting by pressure of the ice at the points of contact where the two slabs are urged against one another.

To this explanation of Principal Forbes's experiment I still adhere as mainly correct, though admitting of some further development and slight modification in reference to a point to which I shall have to make further allusion in what follows, and which seems to me to be as yet rather obscure :—the influence, namely, of the tension in the ice due to its own weight, which makes it *not* be subject in-

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* "On some Properties of Ice near its Melting-Point." By Prof. Forbes, Phil. Mag. vol. xvi. 1858, p. 544.

† Phil. Mag. S. 4. vol. xiv. p. 548, and British Association Reports, 1857.

‡ Phil. Mag. S. 4. vol. xix. p. 396.

ternally to simply atmospheric pressure:—and though I shall also, in what follows, point out some additional conditions, almost necessarily present in the experiment, which, under my general view of the plasticity of ice, would act in conjunction with those already adduced, and would increase the rapidity of the union.

Professor Faraday, holding it in view to remove the ground on which my explanation of Principal Forbes's experiment was founded, has contrived and carried out a set of new and very beautiful experiments from which the capillary action referred to has been completely eliminated, and he has still found the union of the ice to occur, and to increase with time, and has met with a curious additional phenomenon of "*flexible adhesion*"*. In these experiments, when two pieces of ice, rounded so as to be convex at their points where mutual contact is to be allowed, are placed in water, and are either anchored so as to be wholly under water, or are placed floating when so formed that they can touch one another only under water, and that, at the water surface, there shall be a wide space between them so that there shall be no capillary action drawing them together, he showed that the pieces of ice, in either of these cases, if brought gently into contact, will adhere together; unless indeed the movement bringing them into contact be so directed as to introduce forces capable of tearing them apart again by obliquity of action, by agitation of the water, or by other disturbances. He showed also that, if when the two pieces of ice have become attached at their point of contact, a slight force, such as may be given by one or two feathers, be applied, tending to separate them, at one side of their point of contact, they will roll round one another with a seemingly flexible adhesion; or that, if the point of a floating wedge-shaped piece of ice is brought under water against the side of another floating piece, it will stick to that piece like a leech. He showed that if the pieces be allowed to remain for a few moments in contact, their adhesion will become rigid, so that on a force being applied sufficient to break through the joint, the rupture will occur with a crackling noise, though the pieces may still continue to hold together, rolling on one another with the flexible adhesion. He made some other experiments nearly the same as these, but in which he showed the flexible and rigid adhesion to occur while there is constantly a decided tensile force applied externally tending to pull the pieces asunder instead of any external force tending to press them together. He thinks that the phenomena of flexible and rigid adhesion "under tension" go towards showing that pressure is not necessary to "regelation." He then gives his own idea of the flexible and rigid adhesion in the following words:—"Two convex surfaces of ice come together; the particles of water nearest to the place of contact, and therefore within the efficient sphere of action of those particles of ice which are on both sides of them, solidify; if the condition of things be left for a moment, that the heat evolved by the solidification may be conducted away and dispersed, more particles will solidify, and ultimately enough to form a fixed and rigid junction, which will

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* Phil. Mag. S. 4. vol. xxi. p. 146.

remain until a force sufficiently great to break through it is applied. But if the direction of the force resorted to can be relieved by any hinge-like motion at the point of contact, then I think that the union is broken up amongst the particles on the opening side of the angle, whilst the particles on the closing side come within the effectual regelation distance; regelation ensues there and the adhesion is maintained, though in an apparently flexible state. The flexibility appears to me to be due to a series of ruptures on one side of the centre of contact, and of adhesion on the other,—the regelation, which is dependent on the vicinity of the ice surfaces, being transferred as the place of efficient vicinity is changed. That the substance we are considering is as brittle as ice, does not make any difficulty to me in respect of the flexible adhesion; for if we suppose that the point of contact exists only at one particle, still the angular motion at that point must bring a second particle into contact (to suffer regelation) before separation could occur at the first; or if, as seems proved by the supervention of the rigid adhesion upon the flexible state, many particles are concerned at once, it is not possible that all these should be broken through by a force applied on one side of the place of adhesion, before particles on the opposite side should have the opportunity of regelation, and so of continuing the adhesion."

The interpretation thus put by Prof. Faraday on his experiments is not convincing to me; but, on the contrary, I think the experiments are in perfect accordance with my own theory, and tend to its confirmation. My view of the phenomena of these experiments is as follows:—The first contact of the two pieces of ice cannot occur without impact and consequent pressure; and, small as the total force may be, its intensity must be great, as the surface of contact must be little more than a geometrical point. This pressure produces union by the process of melting and regelation described by me in previous papers. On the application of the forces from the two feathers, at one side of the point of contact, tending to cause separation, the isthmus of ice formed by the union of the two pieces comes to act as a tie or fulcrum subject to tensile force; and consequently a corresponding pressure will occur at the side of the isthmus, far from the feathers; and that pressure will effect the union of the ice at the side where it occurs. The tensile force, it may readily be supposed, tends to preserve the isthmus, internally at least, in the state of ice, whatever may be its influence on the external molecules of the isthmus, and to solidify such water as, having occupied pores in the interior during previous compression, may now, by the linear tension or pull, be reduced in cubical pressure or hydrostatic pressure, because the melting-point of wet ice is raised by diminution of pressure of the water in contact with it*. The pull applied to the isthmus

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* How the *surface* of a bar of ice immersed in cold water, as distinguished from the *interior* of the bar, may in respect to tendency either to melt away, or to solidify to itself additional ice from the water, be influenced by the application of linear tension to the bar, I am not quite prepared to say positively. The application of tension, whether linear, superficial, or cubical (that is,

thus appears to put it out of the condition in which my theory has clearly indicated a cause of plasticity, and I presume makes it cease, or almost entirely cease, to be plastic. I believe no plastic yielding of ice to tension has been discovered by observation in any case, and I think there are theoretical reasons why ice should be expected to be very brittle in respect to tensile forces. The isthmus then being supposed devoid of plasticity at its extended side, ultimately breaks at that side, when the opening motion caused by the feathers has arrived at a sufficient amount to cause fracture, and the ice newly formed on the compressed side comes now to act as a tie instead of the part which has undergone disruption, and holds together the two pieces of ice, or serves as a fulcrum under tension to communicate a compressive force to the points of the two pieces of ice immediately beyond it; and so the rolling action with a constant union at the point of contact goes forward. It is to be observed that the leverage of the forces applied by the feathers is so great, compared with the distance from the fulcrum or tensile part of the isthmus, to the compressed part in process of formation at the other side, as that the compression may usually be considered almost equal to the tension:

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whether simply in one direction, or in two directions crossing one another, or in three directions crossing one another), to a piece of ice immersed in water at any given pressure, atmospheric for instance, is very distinct from the application of what might be called cubical tension, that is, diminution of hydrostatic pressure, to the surrounding water. In the former case the pressure of the water at the external surface of the ice will not be reduced by the application of the tension to the ice; though that of the water in the internal pores may, or probably in many of them must, be so; but in the second case, the diminution of cubical pressure in the external water effects the same diminution of pressure in the ice, and also in the water occupying pores in the ice. The theory and quantitative calculation which I originally gave (Transactions Roy. Soc. Edin. vol. xiv. part 5. 1849, and Cambridge and Dublin Math. Journ. Nov. 1850) of the effect of increase of pressure in lowering the freezing-point of water, and of course also of the effect of diminution of pressure in raising it, applied solely to effects of pressure communicated to the ice *through the water*, and therefore equal in all directions, and equally occurring in the ice and the water; but when changes of pressure in one or more directions are applied to the ice as distinguished from the water, the theory does not apply in any precise way to determine the conditions of the melting of the ice, or of its growth by the freezing of the adjacent water to its surface. There seems to me to be yet a field open for much additional theoretical and experimental investigation in this respect; but so far as I have applied the principle of the lowering of the freezing-point of water by pressure in developing or sketching out a theory of the plasticity of ice, I think I have done so correctly. I perceived that the application of pressures tending to change the form of the ice must necessarily produce volume-compression in some parts of the mass, accompanied by the occurrence of increased fluid pressure in the pores which might already exist in those parts, or which would arise in them as a consequence of the pressure; and this I thought was a sufficient basis on which to rest the theory, even without precise knowledge of all the varying influences on the melting or freezing of the ice or water, of all the possible varieties of pressures or stresses that could be applied to the ice, and of fluid pressure that could occur in the water contemporaneously with those stresses in the ice. Some additional developments of this part of the subject, which have occurred to me, may, I hope, form the subject of a future paper.

and the tension in the extended part cannot be of small intensity, being sufficient to break that side of the isthmus. In the experiments which gave flexible adhesion *seemingly* under tension, it is not to be admitted that tension was really the condition under which the ice existed at the places where the union was occurring. To apply a simple disruptive force to the whole isthmus of ice, it would be necessary to take very special precautions in order to arrange that the line of application of the disruptive forces should pass through the point of contact of the two pieces. If that were done, and the forces were gradually increased till the cohesive strength of the isthmus were overcome, it is clear that the two pieces of ice would separate altogether, and there would be no flexible adhesion; but the flexible adhesion, when it occurs, is essentially dependent on the existence of an intense pressure at the side of the isthmus remote from the line of the externally applied disruptive forces, or of the single force applied in some of the experiments to one only of the pieces, and resisted by the inertia of the other.

It is further to be observed that tremors and slight agitations to which the two pieces of ice united at their point of contact, may be subject, arising from undulations imparted to the water in which the ice is immersed, by manipulation of the experimenter,—from the tread of people on adjacent floors,—from the passage of vehicles on neighbouring streets,—from convective movements of the water,—and from other causes,—will be sources of power or energy operative in bringing about an increase of adhesion with time; that is to say, in changing gradually the flexible into the rigid adhesion.

It will now of course be obvious that the conditions involved in the explanation just offered of Prof. Faraday's experiments must also usually be present in the experiment of Principal Forbes. Their incidental occurrence, however, as additional causes increasing the rapidity of the union of the two slabs of ice, does not overthrow the particular explanation of Principal Forbes's experiment which I had offered as a perfect answer to the objection raised by that experiment against my general view of the plasticity of ice; and as indicating clearly and certainly the occurrence of all the conditions required for the union of the two pieces of ice under my theory. The contingent occurrence of the additional conditions now specially brought forward, was indeed from the first somewhat familiar to my mind, but was left out of the explanation as being unessential and not perhaps quite so clearly apparent. Their occurrence has, however, now become essential to the explanation of Prof. Faraday's new experiments:—and by it I consider these are shown not to militate against my general theory of the plasticity of ice, but to corroborate it strongly, and to confirm its application to the various observed cases of the union of two pieces of moist ice when placed in contact.

GEOLOGICAL SOCIETY.

[Continued from p. 333.]

February 26, 1862.—Prof. Ramsay, President, in the Chair.

The following communications were read:—

1. "On the Drift containing Arctic Shells in the neighbourhood of Wolverhampton." By the Rev. W. Lister, F.G.S.

In the parish of Bushbury, at the junction of the London and North-Western, the West-Midland, and the Stour-Valley Railways, is a gravel, with clay, sand, and pebbles, rolled Liassic fossils, flints, pieces of coal and of wood, and more or less fragmentary shells of the following species (as determined by J. G. Jeffreys, Esq., F.R.S., F.G.S.):—*Astarte arctica, Cardium echinatum, C. edule, Cyprina Islandica, Modiola modiolus, Tapes virginea, Tellina solidula, Venus striatula, Litorina squalida, Nassa reticulata, Purpura lapillus,* and *Turritella communis.* The *Astarte* and the *Litorina* are not now found living in our seas. Similar fossil shells have been also found by the author at Oxley Manor, half-a-mile to the N.W.; by Mr. G. E. Roberts at Acleton, eight miles to the S.W.; and by Mr. Beckett elsewhere. Liassic fossils have also been found in the gravel at Compton Holloway and at Wightwick (both in the parish of Tettenhall), and at Wolverhampton.

2. "On a Split Boulder in Little Cumbra, Western Isles." By James Smith, Esq., F.R.S., F.G.S.

The Islands of Great and Little Cumbra have (like the west coast of Scotland) a cliff and terrace, indicating an elevation of about 40 feet above the present level of the sea, and the removal of at least 100 feet of rock (sandstone and trap), the sea at its present level having worn away the rock to the extent of only a small fraction of an inch. The terrace on the Little Cumbra has been moreover ground down and scratched by ice-action, the striæ passing unobliterated under the present sea; and on the terrace lies a split boulder, such as are known to fall from glaciers, and which the author thinks must also in this case have fallen from an escarpment of ice.

3. "On the Ice-worn Rocks of Scotland." By T. F. Jamieson, F.G.S.

The author, first referring to the eroded surface of the rocks beneath the Drift-bed in Scotland, proceeded to show that the action of ice, and not that of torrents, could produce such markings, as he had observed in the bed of a mountain-stream in Argyllshire, down which had poured the torrent caused by the bursting of the reservoirs of the Crinan Canal. He then advanced reasons for considering that the erosion of the rocks in Scotland was due chiefly to land-ice and not to water-borne ice, bringing forward remarkable instances of ice-action on the glens and on the hill-sides at Loch Treig and Glen Spean, where moraines, blocs perchés, striæ, roches moutonnées, and boulders lifted above the parent rocks indicate a northern direction for the great ice-stream from Loch Treig to the

Spean, and then an eastern course on one hand up Loch Laggan, and a western, on the other, down the Spean. Up Glen Roy the ice had apparently passed north-eastwardly, over the watershed, towards the Spey. In Knapdale, Argyllshire, similar evidence is obtained of a great ice-stream passing over hill and dale, here falling into the Sound of Jura. The author referred to Rink's and Sutherland's observations on the continental ice of Greenland as affording a probable solution of these phenomena; and, objecting to the hypothesis either of floating ice and of debacles being sufficient to account for the conditions observed, he thought that land-ice, moving from central plateaux downwards and outwards, has effected the extensive erosions referred to, both in Scotland and other northern regions, at a time when the land was at a much higher level than at present. This must have been followed by a deep submergence, to account for the stratified and shell-bearing drift-beds.

March 5.—Sir P. G. Egerton, Vice-President, in the Chair.

The following communication was read:—

"On the Glacial Origin of certain Lakes in Switzerland, Scotland, Sweden, and North America." By A. C. Ramsay, F.R.S., President of the Geological Society.

The author first stated that in this memoir he proposed to extend his theory of the glacial origin of the smaller mountain-lakes of Wales and Switzerland (published in 'The Old Glaciers of North Wales') to those greater lakes of Switzerland which, like the tarns above alluded to, lie in true *rock-basins*. He then explained a map, compiled from those of Charpentier, Morlot, and Mortillet, showing the ancient extension of the great Alpine glaciers across the Lowlands of Switzerland to the Jura, also over the area that surrounds the Lake of Constance, and on the south into the plains of Piedmont and Lombardy. All the great lakes of Switzerland, and the lakes of Como, Lugano, and Maggiore, lie directly in the course of one or other of these great glaciers; and, as shown by the soundings, and the levels of the rocks at their mouths or in the river-beds below, each of these lakes, like the smaller tarns of the Todten See and the lake at the Grimsel, was shown to lie in a true rock-basin. He then considered the question of the denudation of the Alpine and Miocene areas of Switzerland, and showed that none of the lakes lie in *aboriginal undenuded synclinal hollows*. Next that they do not lie in areas of mere watery erosion. Neither running water nor the still water of lakes can scoop large hollow basins like those of the lakes, bounded on all sides by rocks. Running water may fill them up but cannot excavate them. He next contended that they do not lie in lines of gaping fracture. A glance shows this with respect to such lakes as those of Geneva, Neuchatel, and Constance; and, reasoning on the nature of the contortion of the strata of the Alps, he contended that, though fractures of the rocks must be common, they need not be gaping fractures. To produce such a mountain-chain, the strata are not *upheaved and stretched* so as to produce open cracks; on the contrary they are *compressed laterally and crumpled up* into smaller space; and the uppermost strata, that pressed heavily on the crumpled rocks now visible, would prevent the formation of wide open fractures below,—these upper strata, as in North Wales, having, over a great part of the area, been mostly or altogether removed by denudation. Next, lakes of the rock-basin kind do not lie each in an area of special subsidence. If so, for instance, we should require one for the Todten See, one for the Grimsel, one for the ancient lake of the Kirchet, several at the foot of the Siedelhorn, many hundreds close together in Sutherlandshire, and thousands in North America.

If then the lake-basins were formed by none of the above-named causes, the only other agent that has affected the country on a great scale is glacier ice. All the lakes lie directly in the courses of the ancient glaciers. The basin of the Lake of Geneva is 950 French feet deep near its eastern end, and was scooped out by the great glacier of the Rhone, the ice of which, from data supplied by Charpentier, was, as it issued from the valley, 3550 feet thick to the bottom of the lake. This great weight of ice ground out the hollow of the lake, which gradually shallows towards Geneva, where the glacier thinned and the grinding-power was lessened. Where the same glacier abutted on the Jura, the ice-current was arrested, and it flowed to the N.E. and S.W.; and where the ice was thickest and heaviest, above the Lake of Neuchatel, it ground out the hollow in which the lake lies.

The lakes of Thun and Brienz lie in the course of the great Aar glacier, those of Zug and the Four Cantons in that of Altorf, the Lake of Zurich lies in that of the Linth, the Lake of Constance in the course of the prodigious glacier of the Rhine Valleys, the numerous little rock-basin lakes near Ivrea in the line of the glacier of the Val d'Aosta, and those of Maggiore, Lugano, and Como in the courses of the two gigantic glacier-areas that drained the mountains between Monte Rosa and the Sondrio.

The sizes of the lakes and their depths were then shown to be, in several cases, proportional to the magnitude of the glaciers that ground out the basins in which they lie, and to the circumstance as to whether the pressure of ice was broadly diffused, or vertical as in narrow valleys.

Finally, it was shown that rock-basins holding lakes are always exceedingly numerous in and characteristic of *all countries that have been extensively glaciated.* Lakes are comparatively few in the southern half of North America; but immediately south and north of the great lakes and the St. Lawrence, the whole country is *moutonnée* and striated, and is also covered with a prodigious number of rock-basins holding water. The same is the case in the North of Scotland, the whole area of which has been *moulded by ice*; and east of the Scandinavian chain, in another intensely glaciated region, the country is covered by innumerable lakes.

LVIII. *Intelligence and Miscellaneous Articles.*

ON THE PHOSPHORESCENCE OF RAREFIED GASES.

BY M. MORREN.

1. PURE and dry oxygen, however rarefied, is never phosphorescent when traversed by the induction spark.

2. Any other gas, simple or compound, if rarefied when alone, never presents the phenomenon of phosphorescence.

3. A mixture of oxygen and nitrogen in the proportion of 37 per cent. of oxygen gives a phosphorescence, but it is feeble and very little durable.

4. It becomes more pronounced if to the preceding gaseous mixture a little vapour of monohydrated nitric acid is added.

5. The phosphorescence is splendid and permanent if to the above gaseous mixture a drop of Nordhausen sulphuric acid or a small quantity of anhydrous sulphuric acid is added.

6. The same result is attained by passing the induction spark through a rarefied mixture of the three gases, oxygen, nitrogen, and sulphurous acid, in the following proportions:—

Oxygen	200
Nitrogen	100
Sulphurous acid	150

7. In all cases the phosphorescence is produced by the successive decomposition and recomposition of a singular body well known to chemists, the crystallized body produced in the manufacture of sulphuric acid, and which has the formula $NO^3\,2SO^3$. When in the state of vapour and very rarefied, the spark traversing it separates it into two parts, NO^3 and $2SO^3$, which have only feeble affinities for each other. When the electricity ceases to pass, the elements NO^3 and $2SO^3$ cannot coexist in the vaporous state without recombining, especially in the presence of oxygen. During these molecular evolutions, and while the two parts of the compound are separated, the phosphorescence is kept up. Everything leads to the belief that it is anhydrous sulphuric acid, which, in its passage from the state of vapour to the solid state, is the seat of this luminous phenomenon.

8. Sulphuric acid is not the only acid which can produce this phenomenon. Nitric and probably other acids present it also. And it appears probable that there is a compound, analogous to the preceding, in which SO^3 is replaced by NO^5.

9. The compound $NO^3\,2SO^3$ can be made directly, under the influence of the apparatus described in the research of which this is an abstract.

10. In order to obtain Geissler's tube very strongly luminous and for a long time, nitrogen, and not carbonic acid, must be taken; for the latter, though luminous, has the inconvenience of easily decomposing. Quicksilver vapour must be mixed with it, by rarefying the air by the barometric vacuum, and not by the air-pump.

11. The spectra of the gases in this case, abstracting from the well-known lines of mercury, may be studied in day-light with great ease and accuracy. To obtain a spectrum accurate in every respect, the prism

need merely successively be placed at the minimum deviation for each ray of the spectrum, and a hollow prism used which is filled with sulphide of carbon, and closed by two parallel quartz plates. Flint-glass prisms have not always the same refracting power.

12. Lastly, with sufficient length, and in a barometric tube where the vacuum is made with the greatest care, the electric current can only be passed when the tension is very great; and in this case the prismatic analysis of the feeble light which passes shows that the electricity can only strike through long distances by detaching metallic particles from both electrodes, and, so to speak, forming for itself a bridge of material molecules.—*Comptes Rendus*, November 4, 1862.

ON THE SPECTRA OF PHOSPHORUS AND SULPHUR.

BY M. J.-M. SEGUIN.

Phosphorus and sulphur are volatilized in a current of hydrogen, and the sparks from a moderate-sized Ruhmkorff's machine passed through the mixture of gas and vapour. The electrodes consisted of fine platinum wires, covered with glass within a few millimetres of the end, and at a distance of a few millimetres from each other. They did not become red during the experiment. The spectrum was viewed through the prism with the naked eye, which gives the well-marked rays.

The vapour of phosphorus in hydrogen gives a red ray, an orange ray almost as visible as the red, two less well-marked green rays at the more refrangible end of the visible part of the green, and, at an interval comparatively dark, a bluish-green ray, then some blue or violet rays which are not easily distinguished. The orange ray, which is very bright, as well as the two feeble green rays, appear or disappear according as the receiver which contains the phosphorus is heated. They consequently belong to this substance. The red and bluish-green rays probably belong to hydrogen.

This experiment was confirmed in the main points by operating on phosphuretted hydrogen, and on a mixture of terchloride of phosphorus and hydrogen.

The spark in a mixture of sulphur vapour and hydrogen is of a bright blue, while it is of a pale rose in hydrogen alone. In the spectrum there is a red ray, and three distinct green rays almost equidistant from each other. The first, and often the second, appeared almost yellow from their lustre; the third is slightly less bright, and it extends a little on the side of the others; it seems to comprise several fine and near rays; there are also a bluish-green ray, two blue rays, and two violet rays, which form flutings in the most refrangible part of the spectrum. The three green rays are the most salient part of the spectrum of sulphur. Most of the blue and violet rays belong to this substance.

The spark was also observed in sulphuretted hydrogen and sulphurous acid. The three green rays showed with such precision that it was impossible to mistake their identity.—*Comptes Rendus*, December 30, 1862.

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[FOURTH SERIES.]

JUNE 1862.

LIX. *On the Conduction of Heat by Gases.* *By* R. Clausius*.

THE 108th volume of Poggendorff's *Annalen* (p. 153) contains a paper by M. Jochmann, in which several objections are urged against the hypothesis that the molecules of gaseous bodies are in a state of constantly changing progressive movement.

Jochmann says in the introduction, that he enters upon the discussion of this subject in answer to a challenge from me. In reply to a letter which he very kindly addressed to me, and wherein he expressed some doubts as to the truth of this hypothesis, I certainly did give it as my opinion that, if he believed he had any conclusive arguments to urge against it, he ought to publish them; indeed the subject seems to me one of such importance, that it is for the interest of science that it should be discussed from as many different points of view as possible. I have myself stated my views of the molecular condition of solid, liquid, and gaseous bodies, and of the change from one condition to another, only as hypotheses; and hitherto, in all my researches concerning the mechanical theory of heat, I have therefore carefully distinguished between such conclusions as rest upon certainly established fundamental principles, and which I regard as not to be overthrown, and those which depend upon the views in question. And with the same feeling I would gladly welcome any really trustworthy decision as to the justness of these views, whichever way it might be†.

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* Translated by G. C. Foster, B.A., from Poggendorff's *Annalen*, vol. cxv. p. 1 (January 1862).

† I may be allowed to take this opportunity of making a few remarks, which will be in place here, as to the first starting of this hypothesis concerning the molecular condition of gases.

When I published my views concerning the kind of motion which we call

I must, however, observe that Jochmann's paper does not come up to my expectations, inasmuch as I nowhere find in it strict proofs, but only opinions which are generally entertained, or are based upon analogies, and from which, in my judgment, no safe conclusions can be drawn. I intend, in the first place, to discuss a point which has been raised by Jochmann (and upon which great stress has been laid in other quarters also) as conclusive against this hypothesis, namely the *conduction of heat by gases*, and reserve to myself to return to other points at a future time. I will, however, at once remark that I consider the rest of Jochmann's objections as equally without foundation.

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heat, after the appearance of Krönig's memoir, I mentioned that, according to a communication I had received, the idea of the motion of the molecules of gaseous bodies had already been pronounced by Joule, and that Joule, again, had mentioned Herapath as having preceded himself. Somewhat later P. Du Bois Reymond pointed out that Dan. Bernoulli had expressed, and to a certain point worked out, the same view in his *Hydrodynamica*. Quite recently my attention has been called to a book edited by Prevost (*Deux Traités de Physique Mécanique*, publiés par Pierre Prevost, Genève et Paris, 1818), which contains two memoirs—one by G. L. Le Sage, edited after his death by Prevost, and one by Prevost himself, in which Le Sage's views are worked out further. In these memoirs, likewise, the idea is put forth and discussed, that the molecules of gases are in a state of progressive motion; and although they contain much, in reference to the way in which this motion is produced and sustained, greatly at variance with my own views, the manner in which the expansive force of gases is explained by means of it is essentially the same.

Le Sage quotes a series of authors who, previously to himself, had entertained similar ideas. At page 126 his words are:—"On trouve des vestiges de cette opinion sur la nature de l'air, et même de quelques autres fluides, dans divers auteurs qui m'ont précédé: Lucrèce, livre ii. vers 111–140. Gassendi, dans la 1^re^ section de sa *Physique*, au milieu du 8^e^ chapitre du 4^e^ livre, et au commencement du 4^e^ chapitre du 6^e^ livre. Boyle, dans ses *Nouvelles expériences physico-mécaniques sur la force élastique de l'air et sur ses effets*, ainsi que dans son *Traité sur la fluidité et la dureté*. Parent, dans *l'Histoire de l'Académie des Sciences de Paris*, pour 1708, à la suite des "Variations observées dans la règle Mariotte sur la dilatation de l'air." *Phoromie* de Herman, liv. ii. chap. 6. Dan. Bernoulli, dans la 10^e^ section de son *Hydrodynamique*. Enfin Dan. et Jean Bernoulli, dans une des pièces qui ont eu part au prix de l'Acad. des Sc. de Paris, en 1746."

It is scarcely needful for me to remark that I knew nothing of these earlier attempts at explaining the gaseous condition of bodies when I wrote my own memoir; otherwise I should not have neglected to mention them along with those of Krönig and Joule. Amid the large number of authors who are now quoted with reference to this subject (a number which might perhaps be still increased—some of whom, however, I venture to think, although I have not read the passages referred to in the earlier ones, expressed themselves very likely somewhat vaguely), it would be difficult to indicate with any certainty the one to whom the first suggestion of this hypothesis is to be ascribed, and all we can do is to determine how much each one has contributed to develope the vague idea into an admissible physical theory.

In answer to the objection that so great a mobility as I assume their molecules to possess must cause two gases which are in contact with each other to mix very quickly, I have shown in a former paper that the space moved through by each individual molecule must be exceedingly small. In reference to this, Jochmann says (p. 156), "Even if we consider this as disposing of the objection derived from the mixture of gases, it by no means disposes of the other, namely, that local variations in the temperature of a gas would be impossible, but that a uniform mean velocity must very soon be established throughout the mass. Seeing that the irregular motions of the gaseous atoms cannot be easily presented to the mind, let us make use of a simple analogy in order to assure ourselves that the two points are essentially different. Suppose a row of similar, perfectly elastic balls placed at equal distances from one another in a straight line. If a certain velocity be imparted to the first ball so as to cause it to strike the second centrally, it is true that, the movement propagating itself through the whole row, each ball will only alter its position as much as the distance between two balls; but the velocity imparted to the first will propagate itself through the whole series about as quickly as if the first ball had continued to move onward without encountering any obstruction."

Jochmann thus does not take the matter as it is into consideration, but gets over the difficulty which the consideration of it certainly does offer, by selecting as analogous a very much simpler case. This case is, however, so entirely different from the one it is supposed to represent, that no inference whatever can be drawn from the one respecting the other. If we wish to arrive at really reliable conclusions concerning this and other allied subjects, we must not be afraid of the somewhat troublesome consideration of the irregular motions. Of course this does not preclude the use here and there of assumptions which help us to avoid useless complication in calculations; but these assumptions ought always to be of such a kind that we see clearly that they cannot affect the result*.

The Philosophical Magazine for 1860 (vol. xix. p. 19, and vol. xx. p. 21) contains an interesting memoir by Prof. Maxwell,

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* Hoppe has also made the same objection, on essentially similar grounds, in two papers, the last of which (Pogg. *Ann.* vol. cx. p. 598) is a reply to a note published by myself. He says expressly, p. 603, "The cases in which two molecules that meet each other are unequal, or do not strike centrally, can plainly cause no alteration in the general result." The word *plainly* appears to me to be by no means in place here; on the contrary, I believe that the inaccuracy of the opinion which has been quoted will be made clearly evident by what follows. I leave the reader to form his own opinion of the other remarks occurring in Hoppe's reply, for I am unwilling to inflict upon the scientific public a mere dispute about words.

entitled "Illustrations of the Dynamical Theory of Gases," in which also the question of the conduction of heat is considered. In this memoir, which is remarkable for the elegance of its mathematical developments, the motion of small bodies is regarded from very general points of view, and many valuable results are arrived at in it; nevertheless I do not believe that its contents are correct in every point. I am more particularly of opinion that the author has treated the conduction of heat too incompletely; and although his formula differs but little from that which we shall deduce, important differences nevertheless occur in respect to other matters, to which I shall refer in their proper places, and which make it appear that the close agreement of the ultimate formula is merely accidental.

The general importance of the phenomenon of the conduction of heat, and the slight attempts that have hitherto been made to ascertain the real nature of the process upon which it depends, induce me to think that I shall be justified in submitting this process, and the entire condition of gaseous bodies by which it is accompanied, to a closer mathematical treatment upon the foundation of the hypothesis which I have hitherto advocated, and in thus endeavouring to deduce the laws of the conduction of heat by gases. I venture also at the same time to point out that the principles which will be followed in this investigation are capable of being applied, with certain modifications, to many other cases where the problem is to determine the internal processes going on in a quantity of gas, and that the developments which follow may lay claim in this respect to a more general significance than the problem treated in the first instance.

I. *Definition of the case to be considered.*

§ 1. We will suppose a quantity of gas between two parallel plane surfaces of infinite size, each of which is maintained at a constant temperature. If the temperature of one surface is higher than that of the other, a transference of heat from one surface to the other will take place, through the medium of the gas, by the continual passage of heat from the warmer surface into the gas, its advance from one layer to the next within the gas itself, and its being at last given up by the gas to the colder surface. As it is our object to consider here only that movement of heat which is caused by conduction, and not that which might be occasioned by currents of gas produced by the warmer portions being specifically lighter than the colder, we will suppose the action of gravity entirely excluded: this is approximately the case when the two surfaces are horizontal and the hotter is above, for then no currents can arise.

If both surfaces are kept for a considerable time at constant temperatures, a state of equilibrium is at length established in the gas, of such a kind that the temperature remains invariable at each point within it, but is different at different points—the heat being so distributed that, in any plane parallel to the two limiting surfaces, the temperature is the same at every point, but that it continually decreases according to a definite law in the direction from the warmer to the colder surface. A definite and constant flow of heat through the gas then takes place.

It is this stationary condition of the gas that we have to consider, and to endeavour to determine the amount of the flow of heat which goes on owing to the conductive property of the gas.

§ 2. We will suppose a straight line drawn between the two surfaces and perpendicular to them, and we will assume this as the axis of abscissæ: the temperature within the gas is then a function of the abscissa x; and if, in order to be able at once to form a definite conception, we assume that the first surface, where the abscissa has its smallest value, is the warmest, the temperature diminishes within the gas as the value of x increases. With the density of the gas the case is reversed, for in a state of equilibrium the density of the gas must be higher in proportion as the temperature of the gas is lower; it is therefore a function of x whose value increases with that of x.

We will assume at starting that the gaseous molecules fly about irregularly in all directions, and accordingly strike and rebound from each other, now in one place, now in another, and also that the velocity of their motion is greater the higher the temperature. Let us now suppose a plane cutting the space filled with gas, and parallel to the surfaces by which this space is bounded; then during a unit of time a great number of molecules will pass from the negative to the positive side of this plane, and *vice versâ*. The molecules which pass from the negative to the positive side have a greater average velocity than those which pass from the positive to the negative side, since, according to our assumption, the temperature is higher, and therefore the moving velocity of the molecules greater, on the negative side of the plane than on the positive side. The total *vis viva* which traverses the plane in a unit of time in the positive direction is therefore greater than that which traverses it in the negative direction; and if we strike out, as compensating each other, equal quantities which traverse it in opposite directions, we still obtain a certain excess of *vis viva* traversing the plane in the positive direction. *Vis viva* and heat being regarded as synonymous, the amount of *vis viva* thus passing through the plane constitutes the heat-stream mentioned in the last section, which we call conduction of heat, and which we have to consider in the sequel*.

II. *Behaviour of the molecules emitted from an infinitely thin stratum.*

§ 3. We will begin by considering somewhat more closely the nature of the motions of the individual molecules.

We will suppose two parallel planes to be placed perpendicularly to the axis of x and infinitely near to each other, so as to enclose an infinitely thin stratum. Since molecules are continually flying through this stratum in all directions, it must sometimes happen that two molecules strike each other within it and then rebound again. For the sake of shortness we will call these molecules, which, after having lost their previous motions by the impact, leave the stratum again with different motions, the *molecules emitted from the stratum*; and we will now fix our attention upon their motions.

These motions differ very much from each other; and we must distinguish between variations of two kinds, occasioned by two mutually independent causes, and therefore susceptible of being separately considered. The one kind consists of those irregular variations which always prevail in the molecular motions called heat, and which would therefore also occur if the gas were of uniform temperature and density throughout. They arise from various accidental inequalities accompanying the individual impacts: we will designate them *accidental* variations. The other kind of variations is caused by the circumstance of the gas not having an equal temperature and density throughout. These variations depend in a definite manner upon the laws which govern the differences of temperature and density existing in different parts of the gas: we will call them *normal* variations.

It is the latter which have especially to be considered in the conduction of heat, and we will therefore direct our attention first of all to them.

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* According to what is said above, we take account only, in considering conduction, of the heat which is inherent in the molecules themselves, and is communicated by one molecule to another solely by their impact. But besides this, each molecule radiates heat, which is transmitted by the æther, and is partially absorbed by other molecules on its way; so that there is thus also a transmission of heat from one molecule to another. The communication of heat in this way, in the case of bodies of such low radiating and absorbing powers as the gases, can, however, scarcely be reckoned as conduction, since the great distances which the rays of heat may traverse without being absorbed gives it an entirely different character. In any case, however, it is allowable to consider separately each of these two ways in which heat moves; and we shall accordingly in the sequel always speak of the *conduction of heat* in this sense.

The cause of their occurrence depends, in the case before us, upon the fact that when two molecules, coming from different sides, strike each other within the stratum, the molecule which comes from the warmer side has in general a greater velocity than the one which comes from the cooler side. The magnitude of this difference is determined by the distances from the stratum in question of the points at which the said molecules commenced their motions; and since the distances through which the molecules move between each two impacts are in general very small, this difference must also be very small, so that we can regard the mean value of this difference as a magnitude of the same order with the mean excursions (*Weglänge*) of the molecules. We must now try to determine what influence this difference, existing before the impacts, exerts upon the motions after the impacts.

§ 4. The behaviour of two impinging molecules is not in every respect the same as that of two elastic spheres; but we can nevertheless in many respects obtain a useful insight into the behaviour of molecules by starting from the consideration of elastic spheres. The mutual action of two impinging elastic spheres is very comprehensively treated by Maxwell in the memoir already mentioned. I will here only quote a few principles, which may, however, be considered as sufficiently well known without my doing so.

When two elastic spheres move with equal velocity in opposite directions, and with their centres in the same straight line, so that they strike each other centrally, they rebound from each other in such a manner that each sphere moves back with the same velocity in the direction of the point from which it came. But if the spheres move, before the impact, still in opposite directions, but with their centres in two parallel straight lines instead of in the same straight line, and so that the spheres consequently impinge excentrically, they rebound again with equal velocities, their centres again move in opposite directions in two parallel straight lines; but the direction of these straight lines is not the same as that of the straight lines in which the centres moved before the impact. The new direction depends upon the position on the two surfaces of the point of contact; and since the spheres may strike each other on an infinite number of different points of their surfaces, the rebound may also take place in an infinite number of different directions; and it can be easily shown that *each possible direction in space is equally likely for the motions of the spheres after the impact.*

Let it now be assumed, as a general case, that the two equal spheres move before the impact *with any velocities whatever and in any directions whatever.* We will decompose the motion of each sphere into two components. We will take as the first component the motion of the common centre of gravity of the two spheres; the second component must then be the motion of the two spheres in question relatively to their common centre of gravity. The former motion is equal and in the same direction for both spheres; the latter motion is equal and opposite for the two spheres. The former is not altered by the impact; the latter, on the other hand, is altered exactly in the same way as it would be if it existed alone and there were no common motion. In relation to it, what has already been said of the case of two spheres moving in parallel straight lines, and which assume various directions after impact, according to the point at which they strike each other, is applicable. It thus becomes evident how far the motions after impact, of molecules which impinge upon each other irregularly, are dependent upon their motions before impact, and how far they are independent of them. *The motion of each sphere consists of two components, the first of which is entirely determined, both as to magnitude and direction, by the motions before impact, and the second of which has also a determinate magnitude, but may have an infinite number of different directions, every direction in space being equally probable with every other*[*].

§ 5. In applying this result to the impacts which occur among the molecules, we may assume that here also only that portion of the motion possessed before impact by two impinging molecules remains unchanged in magnitude and direction which is common to both molecules, that is, the motion of their common centre of gravity; while the direction of the second component of their motions may be altered in so many ways that it may with equal probability assume any direction in space whatever.

Let us now consider the whole number of molecules which impinge upon each other in one unit of time within the infinitely thin stratum spoken of in § 3. The motions which they possess *before* the impact have already been discussed in § 3: all possible directions are represented among their motions; but the molecules coming from the warmer side have in general somewhat greater velocities than those which come from the colder side. Since, according to our assumption, the temperature diminishes as x increases, the warmer side is the negative side, that is, the one on which x has a smaller value than it has in the stratum: hence the molecules which pass from the negative to the positive

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[*] This result shows very plainly what a great departure it is from the real state of the case to regard, like Jochmann and Hoppe, in an approximate consideration of it, only central impact, since, instead of an infinite number of different directions, there is thus obtained only one determinate direction, and that one which is especially favourable to the transmission of *vis viva*.

side have in general greater velocities than those which pass from the positive to the negative side, so that, compounding the motions of all impinging molecules, we obtain a certain small momentum in the direction of positive x.

This common momentum remains unaltered by the impacts; but at the same time a complete change occurs in the directions of the motions, in so far that the molecules are impelled in all directions without distinction. If therefore the motion were, before the impacts, unequally distributed in the various directions (the number of molecules moving in certain directions being greater than the number moving in other directions, or their velocities being different), we must nevertheless assume that all these inequalities would be equalized by the impacts; and that, excepting the general motion in the direction of positive x, no distinction between the different directions would remain, but that all directions would be equally represented among the new motions.

It thus becomes easy to give a definite representation of the state of motion of the molecules emitted from the stratum, if, instead of regarding the velocities of the *separate* molecules, we content ourselves with knowing the *mean* velocity for each direction. First, let the molecules be conceived as moving equally in all directions, so that an equal number of molecules, and all with the same velocity, move in each direction, and then let a small component motion in the direction of positive x, equal for all the molecules, be conceived as added to all these motions. The directions and velocities of the motions will be thereby somewhat changed; and the system of motion so modified represents the motions of the molecules emitted from the stratum*.

§ 6. We can define this system of motion mathematically as follows.

Let the velocity possessed by all the molecules before the modification be A. The component velocity to be added to it in the direction of positive x can, according to what has been said above, be only a very small magnitude, of the same order as the mean excursions of the molecules. But as this latter is dependent on the density of the gas, it is not the same at every point of the quantity of gas under consideration; and it will therefore be convenient to substitute for this variable magnitude, in what follows, one which has a determinate value for each gas. For this purpose we will assume a certain condition as a *normal con-*

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* In the memoir quoted above (Phil. Mag. S. 4. vol. xx.), Maxwell, in determining the conduction of heat, has disregarded the circumstance that the molecules emitted from a stratum have an excess of positive momentum, but has tacitly assumed in his calculations that the molecules are emitted equally in all directions.

dition for each gas—for instance, where the gas is exposed to the pressure of one atmosphere, and its temperature throughout is zero (the freezing-point). We will call the mean length of excursion which corresponds to this condition of the gas, the *normal mean length of excursion* (*normale mittlere Weglänge*), and we will denote it by ϵ. We can then regard the component velocity already mentioned as a magnitude of the order of ϵ, and can accordingly denote it by $p\epsilon$.

We will now consider any molecule whatever whose direction forms the angle α with the axis of x. As in what follows we have generally only to consider the cosine of the angle which the direction of any molecule forms with the axis of x, we will for the sake of shortness call it the *cosine of the molecule*, and denote it by a single letter, which in the case before us shall be λ. If now the component velocity $p\epsilon$ in the direction of positive x be imparted to the molecule, its velocity and its cosine will be thereby changed, and we will denote the altered values which take the places of A and λ by U and μ. We have then for the determination of these two magnitudes the equations

$$\mathrm{U}\mu=\mathrm{A}\lambda+p\epsilon, \quad \ldots \ldots \quad (1)$$

$$\mathrm{U}^2=\mathrm{A}^2+2\lambda\mathrm{A}p\epsilon+p^2\epsilon^2. \quad \ldots \ldots \quad (2)$$

Substituting for $\lambda\mathrm{A}$ in the second equation the value $\mathrm{U}\mu-p\epsilon$ derived from the first equation, we get

$$\mathrm{U}^2=\mathrm{A}^2+2\mu\mathrm{U}p\epsilon-p^2\epsilon^2.$$

By solving this equation we obtain two values for U, one positive and one negative, of which it is evident that we must take the positive one: this is

$$\mathrm{U}=p\mu\epsilon+\sqrt{\mathrm{A}^2-p^2(1-\mu^2)\epsilon^2}. \quad \ldots \quad (3)$$

Denoting the particular value of U when $\mu=0$ by u, that is,

$$u=\sqrt{\mathrm{A}^2-p^2\epsilon^2}, \quad \ldots \ldots \quad (4)$$

the last equation becomes

$$\mathrm{U}=p\mu\epsilon+\sqrt{u^2+p^2\mu^2\epsilon^2}; \quad \ldots \ldots \quad (5)$$

and developing this expression according to $\mu\epsilon$, we get the following equation, which conveniently represents the dependence of the velocity U on the cosine μ,

$$\mathrm{U}=u+p\mu\epsilon+\frac{1}{2}\frac{p^2}{u}\mu^2\epsilon^2+\ldots \quad \ldots \ldots \quad (\mathrm{I.})$$

The magnitudes u and p which here occur may have different values in different strata, and are thus to be considered as functions of x.

With reference to the distribution of the molecules among the various directions of motion, it is easy to see that if the original system of motion were such that an equal number of atoms moved in each direction, this could no longer be the case in the modified system of motion, but that more molecules must move in the directions for which μ is positive than in those for which μ is negative.

In order to be able to express this modification, let us begin by considering the original system of motion, and let us determine the number of molecules whose directions form, with the axis of x, angles lying between α and $d\alpha$, the difference between these values being infinitely small. For this purpose let us imagine a spherical surface described with the radius 1; let the point, where it is cut by a straight line drawn through the centre in the direction of positive x, be the pole; and, with the pole for centre, and the arcs α and $\alpha+d\alpha$ for radii, let circles be drawn upon the spherical surface: these two circles will then enclose between them an infinitely narrow zone. The number of molecules, whose directions form with the axis of x angles between α and $\alpha+d\alpha$, will then be the same fraction of the entire number of molecules that the area of the surface of the described zone is of the entire area of the spherical surface, and will be represented by

$$\frac{2\pi \sin \alpha\, d\alpha}{4\pi}, \text{ or } \frac{1}{2} \sin \alpha\, d\alpha.$$

But since $\alpha d\alpha = -d \cos \alpha = -d\lambda$, we may also say that the number of molecules whose cosine lies between λ and $d\lambda$ is expressed as a fraction of the whole number by

$$\tfrac{1}{2}\, d\lambda.$$

To find a corresponding expression for the number of molecules in the modified system of motion whose cosine lies between μ and $\mu-d\mu$, we must modify the last expression by the addition of a factor which is dependent upon μ. Let this factor be H, when the new expression becomes

$$\tfrac{1}{2}\, \mathrm{H} d\mu.$$

The factor H may be determined as follows. Since the cosine λ is changed into μ by addition of the component velocity $p\epsilon$, and, similarly, the cosine $\lambda+d\lambda$ into $\mu+d\mu$, the same number which, before the modification, expressed the molecules whose cosine lay between λ and $\lambda+d\lambda$, will, after the modification, express those whose cosine lies between μ and $\mu+d\mu$. We may therefore put

$$\tfrac{1}{2}\, \mathrm{H} d\mu = \tfrac{1}{2}\, d\lambda,$$

whence

$$\mathrm{H} = \frac{d\lambda}{d\mu}. \quad \ldots \ldots \ldots \quad (6)$$

But by equation (1),

$$\lambda=\frac{\mathrm{U}\mu}{\mathrm{A}}-\frac{p\epsilon}{\mathrm{A}};$$

and A, p, and ϵ being independent of μ, we thus obtain

$$\mathrm{H}=\frac{1}{\mathrm{A}}\cdot\frac{d(\mathrm{U}\mu)}{d\mu}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Putting here for U the series given in (I.), and denoting the fraction $\frac{u}{\mathrm{A}}$ by h, we obtain

$$\mathrm{H}=h\left(1+2\frac{p}{u}\mu\epsilon+\frac{3}{2}\frac{p^2}{u^2}\mu^2\epsilon^2+\ldots\right). \quad . \quad . \quad (\mathrm{II.})$$

The factor h differs from 1 only by a quantity of the second order in relation to ϵ; and putting, according to equation (4), the value $\sqrt{u^2+p^2\epsilon^2}$ for A, we have

$$h=\frac{u}{\sqrt{u^2+p^2\epsilon^2}}=1-\frac{1}{2}\frac{p^2}{u^2}\epsilon^2+\ldots \quad . \quad . \quad (8)$$

The system of motion produced by adding the common component velocity $p\epsilon$ to the perfectly regular system, in which an equal number of molecules move in every direction, is completely defined by the equations (I.) and (II.)

§ 7. The system of motion so defined corresponds to the motions of the molecules emitted from a stratum, in case the *normal* variations only are regarded. To obtain the motions which actually exist, the *accidental* variations spoken of in § 3 must also be taken into account.

It is plainly impossible to do this by determining the motions of each individual molecule; but the rules of probabilities enable us to establish certain general principles for a *large number* of molecules. Maxwell has thus deduced a formula purporting to represent the manner in which the various existing velocities are distributed among the molecules. It is not, however, necessary for our present purpose to enter upon this; it is sufficient if it be granted that the accidental variations occur to an equal extent in all directions, and that therefore in a quantity of gas, whose temperature and density are uniform throughout, the same number of molecules move in every direction, and that the mean velocity in all directions is the same.

It is indeed easy to see in this case that the accidental variations cannot in any degree contribute to cause more *vis viva* to traverse a given plane in one direction than in the opposite direction, since, whatever may be their individual effects, their influence must be the same in both directions. We may therefore entirely disregard the accidental variations in deducing the general formula. They only come into account in the numerical calculation, since for this—if the velocities and the magnitudes dependent upon them, which are expressed in the formula by particular letters, have in reality various values—those mean values which correctly represent the values that really occur must be calculated; and for the calculation of these mean values, the manner in which the values are distributed must be known.

Reserving to ourselves to return again at the end to the latter point, we propose to ourselves now to determine the condition of the gas, and particularly the *vis viva* traversing a plane, starting from the assumption that the magnitudes U and H, determined by the equations (I.) and (II.), represent the real motions of the molecules emitted from a stratum.

III. *Behaviour of the molecules simultaneously existing in an infinitely thin stratum.*

§ 8. We will suppose two planes placed perpendicularly upon the axis of x, and with the abscissæ x and $x+dx$, whereby we obtain again, as in the foregoing section, an infinitely thin stratum; but we will now consider, not the molecules *emitted* from this stratum, but the molecules which *exist in it simultaneously.*

If the gas had the same temperature and density throughout, the motions would be such that an equal number of molecules would move in all directions, and that the velocities would be equal. But in the case before us, where the temperature and density are functions of x, this uniformity does not occur.

To determine the *velocities* of the molecules, let us choose any direction which makes with the axis of x an angle whose cosine is μ, and let us consider the molecules which move in this direction. Before such a molecule enters our infinitely thin stratum with the abscissa x, it has in general traversed a certain distance since its last impact. If this distance be called s, the abscissa of the point where the last impact occurred will be $x-\mu s$; which expression determines the velocity of the molecule, since, according to the assumptions made above, the velocity with which a molecule is impelled after an impact depends only upon the abscissa of the point of impact, and upon the direction of its motion. We have above denoted the velocity as a function of x and μ, by U, and we may accordingly in this case, in which a molecule is impelled from a point whose abscissa is $x-\mu s$, denote its velocity by V, and write

$$\mathrm{V}=\mathrm{U}-\frac{d\mathrm{U}}{dx}\mu s+\frac{1}{2}\frac{d^2\mathrm{U}}{dx^2}\mu^2 s^2-\ldots \quad . \quad . \quad . \quad . \quad (9)$$

The distance s is not the same for all the molecules in our stratum which have a determinate direction, so that their velocities are also somewhat unequal. We may hereafter denote the arithmetical mean of a magnitude whose value, in the particular cases which occur, is various, by making a horizontal stroke over the symbol which represents the particular values of the magnitude, so that $\overline{\mathrm{V}}$ shall represent the mean value of V, and $\overline{s}$ and $\overline{s^2}$ the mean values of s and s^2. We may then write

$$\overline{\mathrm{V}}=\mathrm{U}-\frac{d\mathrm{U}}{dx}\mu\overline{s}+\frac{1}{2}\frac{d^2\mathrm{U}}{dx^2}\mu^2\overline{s^2}-\ldots \quad . \quad . \quad . \quad (10)$$

In this expression it is to be observed that the magnitude $\overline{s^2}$ is not equivalent to $(\overline{s})^2$, but that it must be specially determined. Thence it also follows that the mean values of the powers V^2, V^3, &c. are not quite equal to the corresponding powers of the mean value $\overline{\mathrm{V}}$. We must, in fact, in order to obtain this mean value, start from the equation (9), and, after having squared it, cubed it, &c., then put the mean values for s, s^2, &c. We thus obtain

$$\left.\begin{aligned}\overline{\mathrm{V}^2}&=\mathrm{U}^2-2\mathrm{U}\frac{d\mathrm{U}}{dx}\mu\overline{s}+\left[\mathrm{U}\frac{d^2\mathrm{U}}{dx^2}+\left(\frac{d\mathrm{U}}{dx}\right)^2\right]\mu^2\overline{s^2}-\ldots,\\ \overline{\mathrm{V}^3}&=\mathrm{U}^3-3\mathrm{U}^2\frac{d\mathrm{U}}{dx}\mu\overline{s}+\left[\frac{3}{2}\mathrm{U}^2\frac{d^2\mathrm{U}}{dx^2}+3\mathrm{U}\left(\frac{d\mathrm{U}}{dx}\right)^2\right]\mu^2\overline{s^2}-\ldots,\\ \overline{\mathrm{V}^4}&=\&\mathrm{c}.\end{aligned}\right\}(11)$$

The differences between the magnitudes $\overline{\mathrm{V}^2}$, $\overline{\mathrm{V}^3}$, &c., and the magnitudes $(\overline{\mathrm{V}})^2$, $(\overline{\mathrm{V}})^3$, &c., which latter are obtained by squaring, cubing, &c., equation (10), occur, as will be seen, first in those terms which are of the second degree in relation to the length of the excursion s; and as these excursions are, on the average, very small quantities, the differences are also very small.

§ 9. It now becomes necessary to determine the values of $\overline{s}$ and $\overline{s^2}$ with greater exactness.

To this end, we will first examine the behaviour of these magnitudes when the temperature and density of the given quantity of gas are uniform throughout, and will afterwards superadd the modification due to the inequality of temperature and density.

Considering, then, all the molecules which are contained at any given time in a stratum of a gas whose temperature and density are everywhere the same, we ask ourselves, how great are the distances which the several molecules have traversed between their last impact and the moment at which we consider them. The likelihood that a molecule has traversed a distance lying between s and $s+ds$, between its last impact and the moment fixed upon, is just as great as the likelihood of its traversing an equal distance between this moment and its next impact; and the likelihood of the latter event can be easily expressed.

If, from a given moment of time, a large number of molecules be supposed to move through the gas with an equal velocity, their motion will cause each of them sooner or later to impinge upon other molecules; and if z denote the number of molecules which traverse the distance s without striking against other molecules, z must diminish according to a definite ratio as s increases. If we say that the probability of one molecule striking another while traversing the infinitely small distance ds is αds, then of the number z which have traversed the distance s without impediment, the number $z\alpha ds$ will be taken up during the next portion of their course ds, and the decrement of z will hence be represented by the equation

$$dz = -z\alpha ds;$$

whence it follows that, putting Z for the initial value of z when $s=0$,

$$z = Ze^{-\alpha s}.$$

This value being substituted for z in the product $z\alpha ds$, gives the expression

$$Ze^{-\alpha s}\alpha ds$$

for the number of molecules the length of whose excursions lies between s and $s+ds$.

In order now to obtain the mean length of all the excursions, it is only needful to multiply the last expression by s, then to integrate from $s=0$ to $s=\infty$, and to divide the integral by the whole number Z. This gives

$$\bar{s} = \int_0^\infty se^{-\alpha s}\alpha ds = \frac{1}{\alpha}. \quad . \quad . \quad . \quad . \quad . \quad (12)$$

This expression applies primarily to the mean length of the distances moved through by the molecules between the point of time in question and their next impact; but it can also be directly used for the distances the molecules have moved through between their last previous impact and the instant in question, for the distances before any given point of time must, on the average, be equal to the distances after it.

The same value, $\frac{1}{\alpha}$, is also obtained if we investigate the mean distances traversed between every two impacts during a given time. For if, instead of considering the motions of all the molecules between a given instant and their next impacts, we take a large number of impacts as our starting-point, and then follow the motions of the molecules until their next impacts, all the foregoing conclusions remain applicable to this case also without modification, and hence the value $\frac{1}{\alpha}$ given in (12) must also be the mean value of these distances*.

The mean value of $\overline{s^2}$ may be obtained in a way quite similar to the above if s^2 be used as the multiplier before integration instead of s, and the rest of the operation be conducted as before. We thus get

$$\overline{s^2}=\int_0^\infty s^2e^{-\alpha s}\alpha ds=\frac{2}{\alpha^2}. \quad . \quad . \quad . \quad . \quad . \quad (13)$$

Hence it follows that the two mean values $\bar{s}$ and $\overline{s^2}$ are related to each other as expressed by the equation

$$\overline{s^2}=2(\bar{s})^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

§ 10. We have now to investigate the modifications which these mean values undergo if the gas has not a uniform temperature and density throughout, but if its temperature and density are functions of x.

All the foregoing considerations remain applicable to the molecules whose motions, being perpendicular to the axis of x, do not cause any alteration in the value of their abscissæ. If, then, in order to distinguish those particular values of the general values which relate to this case, we attach to the letters concerned the index 0 (because in this case $\mu=0$), we may write

$$\bar{s}_0=\frac{1}{\alpha_0} \text{ and } \overline{s^2_0}=\frac{2}{\alpha^2_0}.$$

The quantity $\frac{1}{\alpha_0}$, which represents the mean length of excursion

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* It may perhaps appear surprising at first sight that the same value should be found for the distances traversed between the last impacts and a given moment of time, or between this moment and the next impacts, as for the entire distance traversed in the gas from one impact to the next during a given time. It must, however, be remembered that the mean value of all the distances traversed in the gas between every two impacts during a given time is not the same thing as the mean value which would be found by taking into consideration the distances which all the molecules, which at any given moment are simultaneously in one stratum, would traverse between their last previous and next following impacts. For the longer distances would be of more frequent occurrence in the latter case than in the former, since a molecule requires more time to move through a long distance than to move through a short one; and the probability is therefore greater that any given moment would occur during a longer than during a shorter distance, whereas in the former case all the distances traversed in the gas count equally. By making the calculation, it will be found that the latter supposition gives a mean value twice as great as the mean value given by the former. The value of $\bar{s}$, as determined above, is the half of this greater mean value.

for this particular case, is a magnitude of the same order as the *normal* mean length of excursion denoted by ϵ; and to indicate this, we will put

$$\frac{1}{\alpha_0}=c\epsilon, \quad \dots\dots\dots \quad (15)$$

whence we have

$$\left.\begin{aligned}\bar{s}_0&=c\epsilon,\\ \overline{s^2}_0&=2c^2\epsilon^2.\end{aligned}\right\} \quad \dots\dots\dots \quad (16)$$

The mean length of excursion is somewhat different for those molecules which do not move perpendicularly to the axis of x; we can express this by substituting, for the coefficients c and c^2 in the foregoing equations, magnitudes dependent on the direction. This dependence on the direction rests upon *two circumstances,* each of which may be considered separately.

The *first circumstance* is this—that a different temperature and density prevail at the points from which the molecules start, and in the strata through which they have to pass before they arrive at the stratum under consideration, from those which prevail in that stratum. If the cosine of the angle formed by a given direction of motion with the axis of x be denoted by μ, then the distance of a molecule whose excursion is s, from our infinitely thin stratum, is equal to μs. The differences of temperature and density existing at this distance can be represented, in the manner already known, by series which progress according to whole powers of μs. Now, since the modifications which the coefficients c and c^2 undergo owing to the differences of temperature and density must correspond to these differences themselves, we may conclude that the modified coefficients can be represented by similar series, containing, however, the proper mean values, instead of the particular values s, s^2, &c. We may accordingly write

$$\begin{aligned}\bar{s}&=\epsilon(c+a\mu\bar{s}+a'\mu^2\overline{s^2}+\dots),\\ \overline{s^2}&=2\epsilon^2(c^2+b\mu\bar{s}+\dots).\end{aligned}$$

By substituting for s and s^2 on the right of these equations the values which result from these same equations, we obtain series which progress according to powers of $\mu\epsilon$, and which, if we also substitute simple symbols for the complicated coefficients of the higher terms, may be written

$$\left.\begin{aligned}\bar{s}&=\epsilon(c+\mathrm{A}\mu\epsilon+\mathrm{A}'\mu^2\epsilon^2+\dots),\\ \overline{s^2}&=2\epsilon^2(c^2+\mathrm{B}\mu\epsilon+\dots).\end{aligned}\right\} \quad \dots \quad (17)$$

The *second circumstance* which has an influence on the mean length of excursion is, that the molecules do not move equally in all directions within each individual stratum considered separately, and that therefore the probability of one molecule striking another during the element *ds* of its excursion varies at the same place with the various directions which *ds* may possess. In order to bring this circumstance into calculation, let all the coefficients c, c^2, A, A′, B be again replaced by magnitudes which are dependent on the direction. Now we have already seen that the magnitudes U and H, which determine the unequal motions in various directions of the molecules emitted from any given stratum, vary only slightly with the variations of μ,—so, indeed, that they can be represented by series which progress according to powers of $\mu\epsilon$. It may be inferred hence that the coefficients, as modified for these unequal motions, can likewise be represented by similar series; so that we may substitute for c

$$c+c_1\mu\epsilon+c_2\mu^2\epsilon^2+\dots,$$

and so on for the other coefficients. By introducing these series into the equations (17) and arranging the expression according to $\mu\epsilon$, we again obtain for $\overline{s}$ and $\overline{s^2}$ series which progress according to powers of $\mu\epsilon$, and which differ from the former series only in the coefficients of the higher terms. If we denote these coefficients by new letters, the ultimate expression which we obtain by taking account of both circumstances, takes the form

$$\left.\begin{aligned}\overline{s}&=\epsilon(c+\mathrm{C}\mu\epsilon+\mathrm{C}'\mu^2\epsilon^2+\dots),\\ \overline{s^2}&=2\epsilon^2(c^2+\mathrm{D}\mu\epsilon+\dots)\end{aligned}\right\}\quad .\quad .\quad .\quad (18)$$

It may be further remarked that, of the coefficients of these series, only c will be used in the sequel; the higher terms, where they occur, are only added for the sake of greater completeness.

§ 11. These expressions for $\overline{s}$ and $\overline{s^2}$ must now be introduced into the equations (10) and (11) in § 8. If at the same time the series given in equation (I.) be substituted for U, we obtain for $\overline{\mathrm{V}}$, $\overline{\mathrm{V}^2}$, $\overline{\mathrm{V}^3}$, &c. series which progress according to powers of $\mu\epsilon$, and which, when a few new symbols are introduced, take the following forms:—

$$\left.\begin{aligned}\overline{\mathrm{V}}\;&=u+q\mu\epsilon+r^2\mu^2\epsilon^2+\dots,\\ \overline{\mathrm{V}^2}&=u^2+2uq\mu\epsilon+(2ur+q_1{}^2)\mu^2\epsilon^2+\dots,\\ \overline{\mathrm{V}^3}&=u^3+3u^2q\mu\epsilon+3(u^2r+uq_1{}^2)\mu^2\epsilon^2+\dots,\\ \overline{\mathrm{V}^4}&=\dots.\end{aligned}\right\}\quad .\quad (\mathrm{III.})$$

The letters q, $q_1{}^2$, and r here introduced have the following meanings:—

$$\left.\begin{aligned} q &= p - c\frac{du}{dx}, \\ q_1{}^2 &= q^2 + c^2\left(\frac{du}{dx}\right)^2, \\ r &= \frac{1}{2}\frac{p^2}{u} - c\frac{dp}{dx} - \mathrm{C}\frac{du}{dx} + c^2\frac{d^2u}{dx^2}. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (19)$$

§ 12. Having now determined the *velocities* of the molecules which exist simultaneously in a given stratum, it remains for us to investigate *the distribution of the motions of these molecules among the various directions.*

If the motions were directed equally towards all points, then, for the same reasons as those discussed in § 6, in treating of the molecules emitted from a stratum, the number of molecules whose cosine lay between μ and $\mu + d\mu$ would be represented as a fraction of the whole number present by $\frac{1}{2}d\mu$. In the case before us, however, where the motions are not equally divided among all the directions, but only among such directions as form the same angle with the axis of x, we will denote the number of molecules whose cosine lies between μ and $\mu + d\mu$ as a fraction of the whole number of molecules present by $\frac{1}{2}\mathrm{I}d\mu$, where I signifies a function of μ. Now it is easy to convince ourselves, by considerations similar to those contained in the foregoing sections, that the function I must be capable of expression by a series which progresses according to powers of $\mu\epsilon$, and it may therefore be written thus,

$$\mathrm{I} = i(1 + q'\mu\epsilon + r'\mu^2\epsilon^2 + \ldots), \quad . \quad . \quad . \quad . \quad (\mathrm{IV.})$$

where i, q', r', &c. are magnitudes independent of μ.

The magnitude i can be easily determined at once. If the expression $\frac{1}{2}\mathrm{I}d\mu$ be integrated from $\mu = -1$ to $\mu = +1$, this integration will include all the molecules present, and the value of the integral must therefore be 1. Working this out by putting for I the series just established, we get

$$1 = i(1 + \tfrac{1}{3}r'\epsilon^2 + \ldots),$$

and thence

$$i = 1 - \tfrac{1}{3}r'\epsilon^2 + \ldots \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

We will leave the other magnitudes q', r', &c., occurring in equation (IV.), for the present undetermined, as an opportunity will soon offer itself of determining them as far as is necessary.

[To be continued.]

LX. *On the general Differential Equations of Hydrodynamics.* *By* Professor Challis, *F.R.S.*[*]

1. THE propositions in hydrodynamics, the proofs of which I recently expressed the intention of bringing under review, are contained for the most part in communications to the Numbers of the Philosophical Magazine for January 1851, March 1851, December 1852, and February 1853. In the references that will be made to these communications, the meanings of the symbols will be supposed to be known; and in the present one the same symbols will be used, and in the same significations. The article "On the Principles of Hydrodynamics" in the Number for January 1851 contains definitions of two fundamental properties of a perfect fluid, and the proofs of six propositions founded on these properties, and on self-evident principles. The first five of the propositions need not be particularly dwelt upon, as the reasoning by which they are established is not new, and has been generally accepted. Respecting the fundamental properties, viz. that the parts of a fluid press mutually and against the surface of a solid, and that, if the fluidity be perfect, the parts are separable by an infinitely thin partition without assignable force, I will only remark that as they are obvious and distinctive, and rest on experimental evidence, they seem to be the most appropriate that can be thought of for the basis of mathematical reasoning applied to fluids. The proofs of Propositions I. and II. based upon them, the one demonstrating the law of pressure in the case of equilibrium, and the other the same law in case of motion, must be considered to be as exact, on the hypothesis of perfect fluidity, as are those proofs of propositions in statics and dynamics which rest on the hypothesis of the perfect rigidity of solids. Also the law of pressure is as strictly proved for fluid in motion as for fluid at rest.

2. Proposition VI., which has reference to a new general differential equation, will require more particular consideration, since it cannot be expected that such an equation will be admitted except upon ample evidence of the necessity for it, and of its truth. I propose, therefore, to devote this communication mainly to the discussion of the circumstances which render necessary a *third* general hydrodynamical equation, and of the process by which it may be investigated.

3. Before entering upon this inquiry, it will be proper to adduce the two commonly received hydrodynamical equations, and to state briefly the principles on which they rest. The first in order, the investigation of which is the solution of Prop. IV.,

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[*] Communicated by the Author.

is deduced, by means of D'Alembert's principle, from the general hydrostatical equation obtained as the solution of Prop. III., just as questions relating to the motions of solids are solved as statical questions by the intervention of the same principle. The following is the analytical expression of this equation in its most general form:

$$\frac{(dp)}{\rho}=\left(\mathrm{X}-\frac{d^2x}{dt^2}\right)dx+\left(\mathrm{Y}-\frac{d^2y}{dt^2}\right)dy+\left(\mathrm{Z}-\frac{d^2z}{dt^2}\right)dz. \quad . \quad (1)$$

It is here to be remarked that this equation, as well as the hydrostatical one on which it depends, was investigated with reference to a *single* elementary particle. But as the particle might be any one whatever of the mass of fluid considered, we may at once assert, with respect to the hydrostatical equation, that it applies to the whole of the mass. The same assertion cannot be made respecting the hydrodynamical equation (1), unless there be fulfilled certain conditions arising out of the distinctive character of the motion of fluids, according to which the particles move *inter se*, and continually change their relative positions. In fact, that equation has no application unless such motion be consistent with the principle of *constancy of mass.* This principle requires the investigation of a general equation, which shall express that each given element changes form and position by reason of the motion in a manner consistent with its remaining of the same mass in successive instants. The result of the investigation, which answers Prop. V., is the equation

$$\frac{d\rho}{dt}+\frac{d\,.\,\rho u}{dx}+\frac{d\,.\,\rho v}{dy}+\frac{d\,.\,\rho w}{dz}=0. \quad . \quad . \quad . \quad (2)$$

This, in case the fluid be incompressible, becomes

$$\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}=0.$$

4. Again, the movements of a fluid must be such as to satisfy the geometrical condition that the directions of the motion in each given element are normals to a continuous surface. It will not perhaps be denied that, unless this condition be satisfied, neither of the equations (1) and (2) has any application. But the necessity of obtaining a general differential equation to express the fulfilment of this condition has not been generally recognized. I propose, therefore, before proceeding to the investigation of such an equation, which, in fact, is the third general equation mentioned above, to give some account of what has been done with the other two, as this statement will serve to show the necessity for the third. First, I remark that the two equations have been applied to problems in which the motion is assumed to be in directions tending to or from a fixed point or a fixed

plane. But clearly in these cases the condition that the lines of motion are normals to continuous surfaces is satisfied, and the principle above enunciated as the foundation of a third general equation is consequently involved. For the solution of other problems, the differential function $udx+vdy+wdz$ is equated to $(d\phi)$, the differential with respect to coordinates of a new variable ϕ, so that $u=\frac{d\phi}{dx}$, $v=\frac{d\phi}{dy}$, and $w=\frac{d\phi}{dz}$. I am not aware that any problems have been attempted in which that supposition has not been actually or virtually made. But whence arises the necessity for a new variable, and what does the variable itself signify? Respecting the meaning of the variable, a very explicit answer can be given. For since in the expression $udx+vdy+wdz$ the differentials dx, dy, dz are independent and arbitrary, we may assume them to be such that that expression is equal to zero. It will then be seen that $(d\phi)=0$ is the differential equation of a surface which is everywhere cut at right angles by the directions of the lines of motion in the elements through which it passes. It is evident that there will be an unlimited number of such surfaces, the function ϕ being applicable at all times to all parts of the fluid. Thus the introduction of this variable is really a recognition of the principle that the lines of motion are subject to the above geometrical condition. The further step that I have taken is to regard this principle as necessary and fundamental, and to reason from it. According to this view, the substitution of $(d\phi)$ for $udx+vdy+wdz$ would be a *consequence* of that principle. The following considerations will, however, show that this substitution is not sufficiently general, and would unduly restrict the investigation of the laws of the motion of fluids.

5. It is known from analytical geometry that $udx+vdy+wdz=0$ would equally be the differential equation of a surface cutting at right angles the directions of the motion, if u, v, and w, instead of being equal, were respectively *proportional* to the partial differential coefficients with respect to x, y, and z of a function of x, y, z, and t, that is, λ and ψ being both unknown functions of x, y, z, and t, if

$$u=\lambda\frac{d\psi}{dx}, \quad v=\lambda\frac{d\psi}{dy}, \quad w=\lambda\frac{d\psi}{dz},$$

and consequently

$$\lambda(d\psi)=udx+vdy+wdz.$$

It is admitted that the right-hand side of the last equation is not an exact differential in every case of the motion of fluids; so that, although by substituting $(d\phi)$ for it a resulting differential equation involving only x, y, z, and t, with ϕ as the principal variable, might be found, this equation would not possess the requisite degree of generality. All this reasoning points to the conclusion that a third fundamental equation is necessary for eliminating the unknown function λ, and obtaining a resulting general differential equation in which the principal variable is ψ, and the other variables are x, y, z, and t. I proceed to the investigation of this third equation.

6. Preparatory to the investigation, it will be proper to take account of the following general dynamical circumstance. The accelerative forces which act on a given particle at any time are the extraneous forces X, Y, Z, and the force due to the pressure of the fluid, the components of which in the direction of the axes of coordinates are $\frac{dp}{\rho dx}$, $\frac{dp}{\rho dy}$, $\frac{dp}{\rho dz}$. Now these forces are by hypothesis *finite*, and consequently the direction of the motion of a given particle cannot alter *per saltum*, as it would require an infinite accelerative force to produce this effect in an indefinitely short time. Thus, although the course of a given particle cannot generally be expressed by means of algebraic functions of constant form, it must still be such that the tangents at any two consecutive points do not make a finite angle with each other. Hence also the directions of the surfaces of displacement which cut at right angles the lines of motion in a *given* element at two successive instants do not change *per saltum*.

7. This being premised, since the function ψ is, by the foregoing argument, applicable at all times to all parts of the fluid, the equation

$$(d\psi)=\frac{u}{\lambda}dx+\frac{v}{\lambda}dy+\frac{w}{\lambda}dz=0$$

is a general differential equation applicable to all the surfaces of displacement at all times. If therefore $(d\psi)=0$ be taken to be the differential equation of any one surface of displacement, the coordinates of which are x, y, z at the time t, and if $x+\delta x$, $y+\delta y$, $z+\delta z$, and $t+\delta t$ be substituted for these coordinates and for t respectively, that equation will still be satisfied if the new values of the coordinates apply to another surface of displacement at the time $t+\delta t$. But from what is argued above respecting successive surfaces of displacement of a given element, this will be the case if

$$\delta x=u\delta t,\quad \delta y=v\delta t,\quad \delta z=w\delta t,$$

that is if δx, δy, δz be the variations of the coordinates of any given element in the indefinitely small time δt. Now by the substitution of the new values ψ is changed to

$$\psi+\frac{d\psi}{dt}\delta t+\frac{d\psi}{dx}u\delta t+\frac{d\psi}{dy}v\delta t+\frac{d\psi}{dz}w\delta t,$$

which, by putting for u, v, and w their expressions above, becomes

$$\psi+\left\{\frac{d\psi}{dt}+\lambda\left(\frac{d\psi^2}{dx^2}+\frac{d\psi^2}{dy^2}+\frac{d\psi^2}{dz^2}\right)\right\}.\delta t.$$

Hence by the foregoing reasoning the differential of this quantity with respect to space-variables is equal to zero; that is,

$$(d\psi)+\left(d.\left\{\frac{d\psi}{dt}+\lambda\left(\frac{d\psi^2}{dx^2}+\frac{d\psi^2}{dy^2}+\frac{d\psi^2}{dz^2}\right)\right\}\right).\delta t=0.$$

But by the equation applicable to the first surface of displacement $(d\psi)=0$. Therefore also

$$\left(d.\left\{\frac{d\psi}{dt}+\lambda\left(\frac{d\psi^2}{dx^2}+\frac{d\psi^2}{dy^2}+\frac{d\psi^2}{dz^2}\right)\right\}\right)=0.$$

Now this cannot be the differential equation of a surface of which the variables x, y, z are the coordinates at the time t, because the equation $(d\psi)=0$, with which this one is not identical, is the differential equation of that surface. Consequently the equation can be satisfied only by supposing that

$$\frac{d\psi}{dt}+\lambda\left(\frac{d\psi^2}{dx^2}+\frac{d\psi^2}{dy^2}+\frac{d\psi^2}{dz^2}\right)=0. \quad . \quad . \quad . \quad (3)$$

It would be satisfied, it is true, if the same quantity were equated to a function of the time; but as such function may be supposed to be included in $\frac{d\psi}{dt}$, the above form of the equation is sufficiently general.

We have thus arrived at a third general hydrodynamical equation, by means of which the unknown function λ becomes determinable. It may be remarked that although, for the sake of the argument, $(d\psi)=0$ was supposed to be the differential equation of a particular surface of displacement, the generality of the reasoning is not thereby affected, because that surface might be any whatever. The course of the reasoning is, in fact, precisely analogous to that by which the second general equation is established, in the investigation of which the principle of constancy of mass is affirmed of a selected elementary particle. I consider the foregoing method of obtaining the equation (3) to be somewhat more complete than that given under Prop. VI. in the Philosophical Magazine for January 1851.

8. If $\frac{dx}{dt}$, $\frac{dy}{dt}$, $\frac{dz}{dt}$ be substituted for $\lambda\frac{d\psi}{dx}$, $\lambda\frac{d\psi}{dy}$, $\lambda\frac{d\psi}{dz}$ respectively in the equation (3), that equation will apply to a given particle, and the left-hand side will be the complete differential coefficient of ψ with respect to t. Thus we shall have

$$\left(\frac{d\psi}{dt}\right)=0, \text{ and } \psi=\mathrm{C}.$$

It is, however, to be observed that, as was before stated, ψ may be supposed to include a term which is a function of the time. For instance, if the motion be along straight lines drawn from a centre, and be a function of the distance from the centre, we shall have with respect to a given particle, $\psi=r-f(t)=C$, r being the distance of the particle from the centre at the time t, and the value of $f(t)$ depending on the given circumstances of its motion.

9. Assuming that the equations (1), (2), and (3) are necessary and sufficient for the determination of the motion of a perfect fluid, before applying them to that purpose three considerations of a general character, which it will be important to bear in mind, will now be stated. (1) The indications of the analysis are coextensive with the circumstances of the motion; so that there is no circumstance of the motion which has not its analytical expression, and no analytical circumstance, or result that is not *per se* impossible, which does not admit of interpretation by circumstances of the motion. (2) Any analytical result obtained without taking into account all the three equations, must admit of interpretation relative to the motion, although the application of such interpretation will be subject to limitations. (3) Analytical results which admit of interpretation relative to the motion prior to the consideration of particular disturbances, indicate circumstances of the motion which depend only on the quality of the fluid, and on necessary relations of its motion to time and space—such, for instance, as is the circumstance of the uniform propagation of motion in an elastic aëriform fluid. These three remarks will receive illustration as we proceed.

10. In the Philosophical Magazine for March 1851, I have obtained by two methods the following equation as the solution of Prop. VIII., viz.

$$\frac{d\rho}{dt}+\frac{d\,.\,\rho V}{dr}+\rho V\left(\frac{1}{r}+\frac{1}{r'}\right)=0. \quad . \quad . \quad . \quad . \quad (4)$$

This equation is deduced exclusively from the principle of constancy of mass combined with that which is the foundation of the third general equation, namely, that the directions of motion are normals to continuous surfaces. But that equation is not used in deducing it; and as it involves no consideration of pressure or accelerative force, it is wholly independent of the first general equation. In the Number of the Philosophical Magazine for November 1853, the equation (4) is employed in obtaining expressions for V and ρ under the following conditions. The motion is supposed to be central, and to be a function of the distance from the centre; and any three spherical surfaces of radii $r-\delta r$, r, and $r+\delta r$ being drawn, the quantity of fluid between the first and second surfaces, greater or less than that which would exist in the same space in the quiescent state of the fluid, is changed to the quantity between the second and third in the time δt such that $\delta r = a'\delta t$, a' being constant. The exact relation between V and ρ, and the laws of their variation resulting from these conditions, are found by the integration of equation (4) to be given by the equations

$$a'(\rho-1) = \frac{\mathrm{F}(r-a't+c)}{r^2} = \mathrm{V}\rho.$$

If $\rho = 1 + \sigma$, and σ be very small, we have very nearly

$$a'\sigma = \frac{\mathrm{F}(r-a't+c)}{r^2} = \mathrm{V}.$$

Now, if the supposed conditions were true, a *solitary* wave, either of condensation, or of rarefaction, and of constant breadth, might be propagated with the uniform rate a' from a centre; and the relation between the velocity and density, and the laws of their variation with the distance from the centre, would be correctly given by the above results. But when the problem of propagated motion from a centre is solved for small motions, after taking account of the first general equation, the results are quite different from those above. Consequently it must be concluded that the propagation of a solitary wave is not possible. This conclusion involves another, namely, that the variation of the condensation from point to point at a given time cannot be expressed by a *discontinuous* function; for if that were the case, the possibility of the uniform propagation of a solitary wave would necessarily follow. I mention these conclusions the rather because the progress of my hydrodynamical researches was for a long time retarded by the misconception, that results obtained from the equation (4) combined with the principle of discontinuity were necessarily true, and general in their application. The correction of this error is given in an article "On the Central Motion of an Elastic Fluid," contained in the Philosophical Magazine for January 1859. The principle of the discontinuity of the arbitrary functions can at least have no application prior to the consideration of particular cases of disturbance.

11. We may now proceed to the revision of Prop. VII. in the Philosophical Magazine for March 1851 (p. 232). The object of this proposition is to trace the consequences of assuming λ to be a function of ψ and t in the general equation (3), which is, in fact, to assume that $udx + vdy + wdz$ is integrable without a factor. Now this is a possible analytical circumstance of a general character, and, being such, it corresponds, according to the first of the three preliminary remarks in art. 9, to some general circumstance of the motion which is independent of particular

disturbances. It is also to be noticed that the equation (3), although it does not contain explicitly accelerative force, was deduced on the principle that the action of the forces is such that changes of position of the surfaces of displacement do not take place *per saltum*. Having made these statements, I might content myself with simply referring to the investigation given under Prop. VII., as I have seen no reason to call in question the exactness of the analysis there employed. Since, however, the proposition is an important one, the investigation will be repeated here in a condensed form. Representing by s any line drawn at a given instant constantly in the directions of the motions of the particles through which it passes, and terminating at the point xyz, and by V the velocity at that point, we shall have

$$\lambda^2\frac{d\psi^2}{dx^2}+\lambda^2\frac{d\psi^2}{dy^2}+\lambda^2\frac{d\psi^2}{dz^2}=\mathrm{V}^2=\lambda^2\frac{d\psi^2}{ds^2}.$$

Hence, substituting in the equation (3),

$$\frac{d\psi}{dt}+\lambda\frac{d\psi^2}{ds^2}=0.$$

Making, now, the supposition that λ is a function of ψ and t, the integration of this equation would give

$$\psi=f(s,\ t),$$

and consequently

$$(d\psi)=\frac{d\,.f(s,\ t)}{ds}(ds)=\frac{d\psi}{ds}(ds).$$

The variation $(d\psi)$ being supposed to be from point to point of a given surface of displacement, so that, as before, $(d\psi)=0$, we have the consequence

$$\frac{d\psi}{ds}(ds)=0.$$

But $\frac{d\psi}{ds}$, being proportional to the velocity V, does not vanish, and we must therefore have $(ds)=0$. Hence by integration $s=c_1$, which equation means that s does not vary in passing from point to point of a given surface of displacement. By taking any other surface of displacement we should similarly have $s=c_2$, and the difference between the two values of s would be constant. Thus there will be a constant interval between the two surfaces, which cannot be the case unless *the trajectory be a straight line and the motion be rectilinear.*

As this result has been reached without employing all the three general differential equations, it cannot be affirmed that rectilinear motion is general and necessary. According to the second of the remarks in art. 9, we may admit that the rectilinear motion may be modified in any manner by disturbing causes. But as the foregoing argument has been conducted prior to the consideration of particular disturbances, the inference to be drawn from the result is, that the integrability of $udx+vdy+wdz$ is the analytical exponent of rectilinear motion which takes place in the fluid *by the mutual action of its parts.* The inference so drawn is in accordance with the third remark in art. 9. It will be seen hereafter that this general inference explains *the rectilinear transmission of light.*

12. It will be proper here to meet an objection which possibly may be raised against the foregoing reasoning. If we suppose $udx+vdy+wdz$ to be integrable of itself, and substitute $(d\phi)$ for it, by applying to $(d\phi)$ the same process that was applied to $(d\psi)$ we should obtain the equation

$$\frac{d\phi}{dt}+\frac{d\phi^2}{dx^2}+\frac{d\phi^2}{dy^2}+\frac{d\phi^2}{dz^2}=0,$$

which is manifestly untrue. The answer to this objection is, that ψ is a function altogether different from ϕ, by reason of the function λ, which cannot be left out of account even when $udx+vdy+wdz$ is integrable without a factor. This may be proved as follows. Since by supposition $\lambda(d\psi)=(d\phi)$, we may put $\frac{d\,.\,\chi(\psi,\,t)}{d\psi}$ for λ, so that $\phi=\chi(\psi,\,t)$. Hence

$$\frac{d\phi}{dx}=\frac{d\,.\,\chi(\psi,\,t)}{d\psi}\cdot\frac{d\chi}{dx}=\lambda\frac{d\psi}{dx},\ \&c.$$

But at the same time

$$\frac{d\phi}{dt}=\frac{d\,.\,\chi(\psi,\,t)}{d\psi}\cdot\frac{d\psi}{dt}+\frac{d\,.\,\chi(\psi,\,t)}{dt}=\lambda\frac{d\psi}{dt}+\frac{d\,.\,\chi(\psi,\,t)}{dt},$$

the second term being the partial differential coefficient of $\chi(\psi,\,t)$ with respect to t. Consequently, by substituting in (3), the result is

$$\frac{d\phi}{dt}+\frac{d\phi^2}{dx^2}+\frac{d\phi^2}{dy^2}+\frac{d\phi^2}{dz^2}-\frac{d\,.\,\chi(\psi,\,t)}{dt}=0,$$

which equation, strictly deduced from (3) on the supposition that $udx+vdy+wdz$ is integrable, is different from that obtained above on the same supposition. This shows that it is not legitimate to apply to $(d\phi)$ the same reasoning as to $(d\psi)$.

13. From the last equation an inference of some importance may be drawn. There are cases of motion, as will appear hereafter, in which $udx+vdy+wdz$ is *approximately* an exact differential for the whole of the fluid and during the whole of the motion, and in the treatment of which the square of the velocity is neglected in comparison with $\frac{d\phi}{dt}$. If this be done in the above

equation, it reduces itself to

$$\frac{d\phi}{dt} - \frac{d\,.\,\chi(\psi, t)}{dt} = \lambda \frac{d\psi}{dt} = 0.$$

Hence by integration, the function ψ, which, as said before, may include a term which is an arbitrary function of the time, becomes approximately an arbitrary function of coordinates. Hence, while in those instances the motion, so far as it depends on the mutual action of the parts of the fluid, will still be rectilinear where $udx+vdy+wdz$ is strictly an exact differential, it cannot be shown to be such generally, although it may be gathered from that form of the function ψ that the lines of motion will approximately be straight lines.

The general mathematical theory of vibratory motion in an elastic fluid is reserved for treatment in my next communication.

Cambridge, April 22, 1862.

LXI. *Calculation of the Undulation of an Unstiffened Roadway in a Suspension Bridge as a heavy Train passes over it; and Remarks upon the effect of a suspended Iron Girder in deadening the Undulation.* *By* Archdeacon J. H. Pratt, *M.A.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

It is designed to build across the River Hooghly a bridge consisting of five or six spans, of 400 feet each, with suspension chains and suspended iron girders, to carry the railway from Calcutta to Delhi across the water. I have been asked by G. Turnbull, Esq., Chief Engineer E. I. Railway, for an opinion regarding the proposed structure upon theoretical grounds. The following calculations and remarks are the result, which I send to your Magazine as they may be useful to some of your readers.

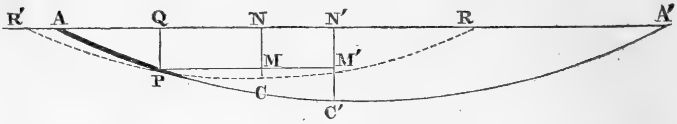
The data are as follows:—The span is 400 feet; the versed sine of the chains at the middle point is 33 feet 4 inches; the weight of the chains, including suspending rods, 200 tons; the weight of the girder and roadway 350 tons; the maximum moving load, when occupying the whole span, 400 tons; the depression of the girder at its middle point, when supported at its two ends, from its own weight alone, 0·7 foot, from the weight of the train 0·9 foot in addition, making 1·6 foot in all.

Calculation of the Undulation of an Unstiffened Roadway.

2. The train is supposed to be so long that both ends cannot be on the same span at the same instant. The weight of the train I suppose to be thrown into the chains, and the girder to

be perfectly flexible, in the present calculation. The effect of the train when in any position on the bridge will be found by considering the problem, To determine the position of equilibrium of a chain of given length suspended from two points in the same horizontal line, the chain consisting of two parts, of different thickness, but both uniform.

Fig. 1.



A P C′A′ is such a chain, of which A P is the thicker part and is the portion of a common catenary A P C R, and P A′ is the thinner part, and is a portion of the common catenary R′P C′ A′: C and C′ are the lowest points of these two catenaries; P Q, C N, C′N′ are vertical, P M M′ is horizontal. Let c and c' be the lengths of chain which measure by their weight the tensions at the two points C and C′ respectively, and β the ratio of the thickness of A P to that of P A′. Let

$$\mathrm{CM}=x,\quad \mathrm{MP}=y,\quad \mathrm{CN}=h,\quad \mathrm{NA}=k,\quad \mathrm{CP}=s,\quad \mathrm{CA}=l,$$
$$\mathrm{C'M'}=x',\quad \mathrm{M'P}=y',\quad \mathrm{C'N'}=h',\quad \mathrm{N'A}=k',\quad \mathrm{C'P}=s',\quad \mathrm{C'A'}=l',$$
$$\mathrm{AA'}=2b,\quad \mathrm{APA'}=2m,\quad \mathrm{AP}=t.$$

By the equations to the catenary we have

$$s^2+c^2=(x+c)^2. \quad . \quad (1) \qquad s'^2+c'^2=(x'+c')^2. \quad . \quad . \quad (5)$$
$$l^2+c^2=(h+c)^2. \quad . \quad (2) \qquad l'^2+c'^2=(h'+c')^2. \quad . \quad . \quad (6)$$
$$c\left(e^{\frac{k}{c}}+e^{-\frac{k}{c}}\right)=2(h+c). \quad (3) \qquad c'\left(e^{\frac{k'}{c'}}+e^{-\frac{k'}{c'}}\right)=2(h'+c'). \quad (7)$$
$$y=c\log_e\frac{s+\sqrt{s^2+c^2}}{c}. \quad (4) \qquad y'=c'\log_e\frac{s'+\sqrt{s'^2+c'^2}}{c'}. \quad (8)$$

The conditions of equilibrium require that the two curves should have a common point and tangent at P, and that the tensions there should be the same. These lead to the following equations:

$$h-x=h'-x', \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$
$$\frac{s}{c}=\frac{s'}{c'}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$
$$(x+c)\beta=x'+c'; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

also

$$s=l-t, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$
$$s'=2m-t-l'; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

and, since

$$AA' = AQ + QN' + N'A' = (AN - QN) + (QN' + N'A'),$$

$$\therefore\ 2b = c\log_e \frac{l + \sqrt{l^2 + c^2}}{s + \sqrt{s^2 + c^2}} + c'\log_e \frac{(s' + \sqrt{s'^2 + c'^2})(l' + \sqrt{l'^2 + c'^2})}{c'^2}. \quad (14)$$

Here are fourteen equations connecting the fifteen quantities ss' xx' cc' ll' hh' kk' yy' and t; each of the first fourteen can therefore be determined in terms of t, which measures the position of the train upon the bridge.

By (1), (5), and (11),

$$\beta^2(s^2 + c^2) = s'^2 + c'^2;$$

$\therefore$ by (10),

$$\beta s = s', \text{ also } \beta c = c'; \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

$\therefore$ by (12) and (13),

$$\beta(l - t) = 2m - t - l';$$

or by (2) and (6),

$$\sqrt{h'^2 + 2c'h'} = 2m + (\beta - 1)t - \beta\sqrt{h^2 + 2ch}; \quad . \quad . \quad (16)$$

$\therefore$ by (15),

$$h' + c' = \sqrt{\beta^2c^2 + \left(2m + (\beta - 1)t - \beta\sqrt{h^2 + 2ch}\right)^2}. \quad (17)$$

By (9), (1), (5), (12), (13), (15),

$$h + c - \sqrt{c^2 + (\sqrt{h^2 + 2ch} - t)^2} = h' + c' - \sqrt{\beta^2c^2 + \beta^2(\sqrt{h^2 + 2ch} - t)^2},$$

or, substituting from (17),

$$h + c + (\beta - 1)\sqrt{c^2 + (\sqrt{h^2 + 2ch} - t)^2}$$
$$= \sqrt{\beta^2c^2 + \left(2m + (\beta - 1)t - \beta\sqrt{h^2 + 2ch}\right)^2}.$$

h is a small quantity, and c a large one; I shall therefore expand these expressions in descending powers of c. This gives

$$h + c + (\beta - 1)\left\{c + h - t\sqrt{\frac{2h}{c}} + \frac{t^2}{2c} + \frac{3th}{4c}\sqrt{\frac{2h}{c}} - \frac{3t^2h}{2c^2}\right.$$
$$\left. + \frac{t^3}{2c^2}\sqrt{\frac{2h}{c}} - \frac{t^4}{8c^3}\right\}$$
$$= \beta\left\{c + h - r\sqrt{\frac{2h}{c}} + \frac{r^2}{2c} + \frac{3rh}{4c}\sqrt{\frac{2h}{c}} - \frac{3r^2h}{2c^2}\right.$$
$$\left. + \frac{r^3}{2c^2}\sqrt{\frac{2h}{c}} - \frac{r^4}{8c^3}\right\},$$

where

$$r = t + \frac{2m - t}{\beta};$$

$$\therefore\ 2m\sqrt{\frac{2h}{c}}=\big(\beta r^2-(\beta-1)t^2\big)\frac{1}{2c}+\frac{3h}{4c}\sqrt{\frac{2h}{c}}\big(\beta r-(\beta-1)t\big)$$

$$-\frac{3h}{4c^2}\big(\beta r^2-(\beta-1)t^2\big)+\frac{1}{2c^2}\big(\beta r^3-(\beta-1)t^3\big)$$

$$-\frac{1}{8c^3}\big(\beta r^4-(\beta-1)t^4\big).$$

Substitute first approximate values of h in small terms ;

$$\therefore\ \sqrt{2hc}=m\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)-\frac{m^3}{8c^2}(1+\mathrm{N}),$$

where $\mathrm{N}=$ some function of β, m, and t;

$$\therefore\ h=\frac{m^2}{2c}\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)^2-\frac{m^4}{8c^3}\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)(1+\mathrm{N});\quad(18)$$

$$\therefore\ (h+c)^2=c^2+m^2\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)^2-\frac{m^4}{4c^2}$$

$$\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)\left\{1+\mathrm{N}-\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)^3\right\};$$

$$\therefore\ \sqrt{h^2+2ch}=m\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)-\frac{m^3}{8c^2}$$

$$\left\{1+\mathrm{N}-\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)^3\right\};$$

$\therefore$ by (16),

$$h'^2+2c'h'+c'^2=\beta^2c^2+\left(2m+(\beta-1)t-\beta\sqrt{h^2+2ch}\right)^2$$

$$=\beta^2c^2+\beta^2m^2\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t^2}{4m^2}\right)\right)^2$$

$$+\frac{\beta^2m^4}{4c^2}\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t^2}{4m^2}\right)\right)\left\{1+\mathrm{N}-\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)^3\right\};$$

$$\therefore\ h'=\frac{\beta m^2}{2c}\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t^2}{4m^2}\right)\right)^2+\frac{\beta m^4}{8c^3}\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t^2}{4m^2}\right)\right)\Bigg\{(1+\mathrm{N})$$

$$-\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)^3-\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t^2}{4m^2}\right)\right)^3\Bigg\}.\quad .\quad(19)$$

From these values of h and h', by the help of (14) I shall obtain the value of c. That equation (14), by means of (1), (2), (15), (12), (6), becomes

$$\frac{2b}{c}=\log_e\left(1+\frac{h}{c}+\sqrt{\frac{h^2}{c^2}+\frac{2h}{c}}\right)+(\beta-1)\log_e\Bigg(\sqrt{\frac{h^2}{c^2}+\frac{2h}{c}}-\frac{t}{c}$$

$$+\sqrt{\left(\left(\sqrt{\frac{h^2}{c^2}+\frac{2h}{c}}\frac{t}{c}\right)^2+1\right)}\Bigg)+\beta\log_e\left(1+\frac{h'}{c'}+\sqrt{\frac{h'^2}{c'^2}+\frac{2h'}{c'}}\right).$$

The first term of this, after substituting for h and h' and expanding,

$$=\frac{m}{c}\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)-\frac{m^3}{8c^3}\left\{1+\mathrm{N}+3\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)^3\right\}.$$

The second term

$$=(\beta-1)\frac{m}{c}\left(1-\frac{t}{m}-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)-(\beta-1)\frac{m^3}{8c^3}\left\{1+\mathrm{N}\right.$$
$$\left.-\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)^3+4\left(1-\frac{t}{m}-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)^3\right\}.$$

The third term

$$=\beta\frac{m}{c}\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t^2}{4m^2}\right)\right)+\beta\frac{m^3}{8c^3}\left\{1+\mathrm{N}-\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)\right.$$
$$\left.-4\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t^2}{4m^2}\right)\right)^3\right\};$$

$$\therefore \frac{2b}{c}=\frac{2m}{c}-\frac{m^3}{2c^3}\left\{\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)^3\right.$$
$$\left.+(\beta-1)\left(1-\frac{t}{m}-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)^3+\beta\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t^2}{4m^2}\right)\right)^3\right\}.$$

Let C be the value of c when $\beta=1$, or no train is on the bridge; then

$$\frac{c^2}{\mathrm{C}^2}=\frac{1}{2}\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)^3+\frac{\beta-1}{2}\left(1-\frac{t}{m}-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)^3$$
$$+\frac{\beta}{2}\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t^2}{4m^2}\right)\right)^3.$$

Hence the values of h and h' are known; by (18), (19) they are

$$\left.\begin{aligned}h&=\frac{m^2}{2c}\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)^2,\\ h'&=\frac{\beta m^2}{2c}\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t^2}{4m^2}\right)\right)^2.\end{aligned}\right\}\quad\ldots\ldots\quad(20)$$

By (3),

$$c\left(2+\frac{k^2}{c^2}\right)=2(h+c);$$

$$\left.\begin{aligned}\therefore\ k&=\sqrt{2ch}=b\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right);\\ \text{so}\qquad k'&=\sqrt{2c'h'}=\beta b\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t^2}{4m^2}\right)\right).\end{aligned}\right\}\quad\ldots\quad(21)$$

m and b are the same to the degree of approximation to which it is necessary to go.

3. Having thus found the elements of the two variable catenaries, I will calculate the curve into which the roadway is thrown.

Fig. 2.

Let D N D′ be the undisturbed roadway, D S T D′ the undulation into which it is thrown when the train is on the road; DN=Y, NS=X the coordinates to any point in the part of the curved roadway corresponding with the thicker catenary. The depth of any point of that catenary below the fixed horizontal line A A′ (fig. 1), the horizontal ordinate from A, being Y, and therefore from the lowest point of the catenary being $k-\mathrm{Y}$, is

$$=h+c-\frac{c}{2}\left(e^{\frac{k-\mathrm{Y}}{c}}+e^{-\frac{k-\mathrm{Y}}{c}}\right)=h-\frac{(k-\mathrm{Y})^2}{2c}.$$

When $\beta=1$ and there is no train on the roadway, $c=\mathrm{C}$, $k=b$; let $h=\mathrm{H}$; then this expression becomes

$$\mathrm{H}-\frac{(b-\mathrm{Y})^2}{2\mathrm{C}}.$$

The excess of the first of these above the second is the depression of the roadway at the corresponding point, and therefore equals NS. Hence the equation to the curve of the roadway is

$$\mathrm{X}=h-\mathrm{H}-\frac{(k-\mathrm{Y})^2}{2c}+\frac{(b-\mathrm{Y})^2}{2c},$$

or

$$\left(\frac{\mathrm{Y}}{b}\right)^2\left(1-\frac{\mathrm{C}}{c}\right)-\frac{2\mathrm{Y}}{b}\left(1-\frac{k}{b}\frac{\mathrm{C}}{c}\right)=\frac{\mathrm{X}}{\mathrm{H}}$$

(since $k^2=2ch$ and $b^2=2\mathrm{CH}$ by the equation to the catenary, neglecting extremely small quantities), or

$$\left(\frac{\mathrm{Y}}{b}-\frac{\frac{k}{b}\frac{\mathrm{C}}{c}-1}{\frac{\mathrm{C}}{c}-1}\right)^2=\frac{1}{\frac{\mathrm{C}}{c}-1}\left(\frac{\left(\frac{k}{b}\frac{\mathrm{C}}{c}-1\right)^2}{\frac{\mathrm{C}}{c}-1}-\frac{\mathrm{X}}{\mathrm{H}}\right).$$

This is the equation to a parabola with the vertex at the lowest point: let N, D be the horizontal and downward vertical coordinates to the vertex,

$$\therefore\ \frac{\mathrm{N}}{b}=\frac{\frac{k}{b}\frac{\mathrm{C}}{c}-1}{\frac{\mathrm{C}}{c}-1}\quad\text{and}\quad\frac{\mathrm{D}}{\mathrm{H}}=\frac{\left(\frac{k}{b}\frac{\mathrm{C}}{c}-1\right)^2}{\frac{\mathrm{C}}{c}-1}.$$

By precisely similar reasoning it may be shown that, if Y is measured from the other end of the roadway and towards the left, the equation to the curve corresponding to the part on which the train is not situated is also a parabola, the equation being

$$\left(\frac{\mathrm{Y}}{b}-\frac{1-\frac{k'}{b}\frac{\mathrm{C}}{c'}}{1-\frac{\mathrm{C}}{c'}}\right)^2=\frac{1}{1-\frac{\mathrm{C}}{c'}}\left(\frac{\mathrm{X}}{\mathrm{H}}+\frac{\left(1-\frac{k'}{b}\frac{\mathrm{C}}{c'}\right)^2}{1-\frac{\mathrm{C}}{c'}}\right);$$

and the vertex is at the highest point of the curve, and its coordinates given by

$$\frac{\mathrm{N}'}{b}=\frac{1-\frac{k'}{b}\frac{\mathrm{C}}{c'}}{1-\frac{\mathrm{C}}{c'}}\quad\text{and}\quad\frac{\mathrm{E}}{\mathrm{H}}=\frac{\left(1-\frac{k'}{b}\frac{\mathrm{C}}{c'}\right)^2}{1-\frac{\mathrm{C}}{c'}}.$$

These two parabolas have a common point corresponding to the place where the end of the train is, and they touch each other at that point. For at this point $\mathrm{Y}=t$ in the first, and $=2b-t$ in the second parabola, and

$$\frac{\mathrm{X}}{\mathrm{H}}\text{ in the first }=\left(\frac{t}{b}\right)^2\left(1-\frac{\mathrm{C}}{c}\right)-\frac{2t}{b}\left(1-\frac{k}{b}\frac{\mathrm{C}}{c}\right)$$

$$=\left(\frac{t}{b}-1\right)^2-1-\frac{\mathrm{C}}{c}\frac{t^2-2tk}{b^2}$$

$$=\left(\frac{t}{b}-1\right)^2-1-\frac{\mathrm{C}}{\beta^2cb^2}\left\{\beta t^2-2\beta tb\left(1-\frac{\beta-1}{\beta}\left(1-\frac{t}{2m}\right)^2\right)\right\}\text{by (21)}$$

$$=\left(\frac{t}{b}-1\right)^2-1-\frac{\mathrm{C}}{c'b^2}\left\{t^2-2bt-(\beta-1)\left(1-\frac{t}{2b}\right)t^2\right\}$$

$$=\left\{\frac{2b-t}{b}-1\right\}^2-1-\frac{\mathrm{C}}{c'}\frac{(2b-t)^2-2(2b-t)k'}{b^2}\text{ by (21)}$$

$$=\frac{\mathrm{X}}{\mathrm{H}}\text{ in the second parabola, when }\mathrm{Y}=2b-t.$$

In the same manner it may be shown that at this same point

$$\frac{dX}{dY} \text{ in the first parabola} = -\frac{dX}{dY} \text{ in the second;}$$

that is, they have a common tangent at that point.

4. The formulæ thus deduced are ready for application to any particular example. In the Hooghly Bridge $b=200$ feet, $H=33$ feet 4 inches,

$$\beta = \frac{200+350+400 \text{ tons}}{200+350 \text{ tons}} = \frac{19}{11};$$

and the foregoing formulæ give the following numerical results for different positions of the moving load on the bridge:—

	Distance the train has passed along on the roadway, or *t*.									
	40 feet.	80 feet.	120 feet.	160 feet.	200 feet.	240 feet.	280 feet.	320 feet.	360 feet.	400 feet.
Greatest depression of roadway under the train, *in feet*	0·64	1·57	2·15	2·24	1·99	1·54	1·01	0·53	0·16	0
Horizontal distance of this depression	33	56	73	86	97	105	113	120	127	
Greatest elevation of roadway beyond the train ...	0·28	0·97	1·73	2·24	2·42	2·25	1·78	1·11	0·39	0
Horizontal distance of this elevation from the left pier.	271	275	279	287	297	309	324	342	367	

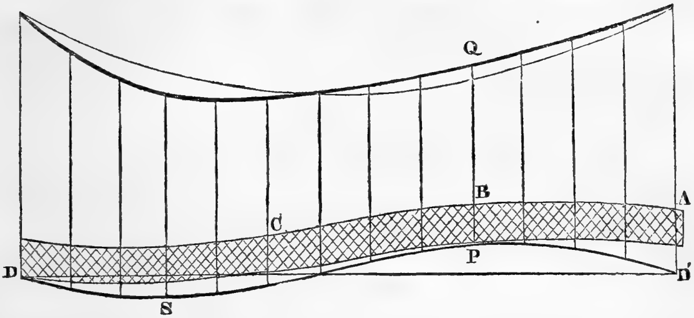
Hence the greatest depression occurs nearly about the time when the greatest elevation occurs, a little before it, that is, when the train is nearly halfway over the span; the distance between the lowest point of the roadway and the highest point at that time, measured along the roadway, is about 200 feet, or half the length of the span; and the greatest depression is somewhat behind the middle point of the train.

On the effect of the Iron Girder in checking the undulation of the Roadway.

5. Suppose it possible that the undulation described and calculated in the last paragraph can take place in the roadway and heave up the girder, as represented in the following diagram (fig. 3). More than half of the weight of the girder will be brought to bear upon the point P where the girder touches the roadway curve (which is the same as the curve into which the lowest ends of the suspending rods are thrown), unless this curve is of no greater curvature at P than that of the girder by its own weight. The length of the girder to be used in the Hooghly Bridge is

400 feet, its weight 350 tons, and its deflection when suspended at its two ends is 0·7 foot. It will have the same deflection as this at its two ends if suspended in the middle. Moreover, if a shorter girder of the same structure be used, the deflection will be less in proportion to the square of the length*. Thus take BC=AB: the curvature of the girder upheaved at P and resting on that point will be the same as that of a shorter girder AC resting on its middle point, and will therefore be less than 0·7 foot in the ratio of AC²: AD². Making use, then, of the Table deduced in the last paragraph, I obtain the following deflections of the girder at P for different positions of the train on the roadway:—

Fig. 3.



	Distance the train has passed along the roadway.									
	40 feet.	80 feet.	120 feet.	160 feet.	200 feet.	240 feet.	280 feet.	320 feet.	360 feet.	400 feet.
Distance of the greatest elevation of the roadway from the right-hand pier, *in feet*	129	125	121	112	103	91	74	58	33	0
Ratio of this to half-length of the roadway	0·64	0·62	0·60	0·56	0·51	0·45	0·37	0·29	0·16	0
Deflection of the girder AC	0·28	0·27	0·25	0·22	0·18	0·14	0·10	0·06	0·02	0

As the train, therefore, occupies these successive positions, the deflection of the girder at P, upheaved and subjected to its own weight, would be continually diminishing,—beginning at the first position with only 0·28 foot or 3·4 inches, lessening to 0·18 foot or 2·2 inches when the train is halfway across, and becoming

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* This is because the girder is a *lattice-girder*, in which the upper and lower bars sustain the weight by compression and extension. Were the girder a *beam* solid throughout, the deflection would vary as the cube of the length.

O when the whole train is on the roadway. This, in point of fact, will be the greatest curvature to which, not only the girder, but the roadway itself will be subjected in the several positions of the train. For the roadway will never be able to assume a curvature, as in the diagram, exceeding that of the girder. The instant the girder bore upon the point P, more than half the weight of the girder would be thrown upon the suspending rod QP. But the weight of the train could never communicate through the chain an upward tension in the rod PQ at all capable of sustaining this load. It is clear from this that the weight of the girder will keep down the undulation, and that the undulation of the roadway, in this arrangement of a suspended girder, can never exceed the curvature of which the girder itself is capable. The girder and roadway will therefore always remain in contact, and the position of the greatest point of elevation of the undulation shows where the upward pressure is greatest.

The greatest depression of the roadway and girder will be that corresponding to the deflection of a girder of the length DC. The following Table is constructed as the last was:—

	Distance the train has passed along the roadway.									
	40 feet.	80 feet.	120 feet.	160 feet.	200 feet.	240 feet.	280 feet.	320 feet.	360 feet.	400 feet.
Distance of greatest depression from the left-hand pier, *in feet*	33	56	73	86	97	105	113	120	127	
Ratio of this to half-length of the roadway	0·16	0·28	0·36	0·43	0·48	0·52	0·56	0·60	0·63	
Depression of the girder DC.	0·02	0·05	0·09	0·13	0·16	0·19	0·22	0·25	0·28	0·70

6. There are two cases which I will now consider when the train is in motion: (1) when the curvature of the roadway is continuous, without any sudden change; (2) when there is a slight break in the curvature.

(1) When the train is moving with a velocity v, the curved roadway is subject to additional pressure from centrifugal force: the pressure is increased by this effect beyond the simple weight of the train in the ratio of

$$g+\frac{v^2}{\text{radius of curvature}} : g \text{ (or gravity)}.$$

If h be the depression of the roadway when the train is in motion, k the depression when it is at rest, and $2n$ the length of DC the part depressed, then, taking the curve to be part of a circle,

$$\text{radius of curvature} = \frac{2n^2}{h}; \text{ and } \frac{h}{k} = 1 + \frac{v^2}{2n^2g}h;$$

$$\therefore h = \frac{k}{1 - \frac{v^2k}{2gn^2}}.$$

The value of k varies as n^2, as already stated, and equals 0·7 foot when $n=200$ feet: also $g=32$ feet, and $v=88$ feet for a velocity of 60 miles an hour; hence the denominator of h

$$=1-0{\cdot}0021\left(\frac{v}{88}\right)^2,$$

and the increase of h above k is therefore evanescent even for a velocity of 60 miles an hour.

If any part of the rail, from accident or bad structure, is more curved than I have supposed it to be, the radius of curvature of that part will be smaller, and the effect of centrifugal force proportionately greater.

(2) Next suppose that there is at some point of the rail a sudden change in the curvature, and that the tangents to the two parts of the curve at that point make a small angle θ with each other. The effect of this will be, that every time the wheels of the train come to that point an impulse or blow will be given to the roadway. The tendency of this blow will be suddenly to separate the roadway curve (which is the same as the curve in which the lowest points of the suspending rods lie) from the suspended girder, since the girder is not attached to the suspending rods. Thus the blow will bring into sudden action the downward pressure of the girder, which will consequently stifle the effect of the blow, and not suffer the roadway to be thrown into a greater undulation than the girder itself can assume under its own weight, as already described. No doubt a molecular tremor will run along the roadway, girder, and chain occasioned by the blow, but nothing more.

I will, however, consider what the effect would be if the girder, instead of lying unattached in the loops of the suspending rods, were attached to the roadway.

I must premise that if M be the mass of the girder, and a vertical blow be given to it at any point between its two ends on which it is supported, so as to cause that point to descend with a velocity u, then $\frac{2}{3}\mathrm{M}u$ is the whole momentum communicated to the beam. For let b' and b'' be the distances of the point of impulse from the left-hand and right-hand piers, $b'+b''=2b$; then the velocity communicated to a point at a horizontal distance y to the left or right of the point of impulse will be

$$u\left(1-\frac{y^2}{b'^2}\right) \text{ or } u\left(1-\frac{y^2}{b''^2}\right),$$

and therefore the momentum of an elementary section of the girder of width dy, being integrated, will give

$$\text{momentum of whole girder } =\frac{\mathrm{M}u}{2b}\left\{\int\left(1-\frac{y^2}{b'^2}\right)dy\right.$$
$$\left.+\int\left(1-\frac{y^2}{b''^2}\right)dy\right\}=\frac{2}{3}\mathrm{M}u, \text{ the limits being } 0,\ b', \text{ and } 0,\ b''.$$

If the train reaches the point of interruption with a velocity v, it moves on beyond that point with the diminished velocity $v\cos\theta$, and it impinges on the roadway with a velocity $v\sin\theta$. The momentum thus communicated to the girder will be $\mathrm{T}v\sin\theta$, T being the mass of the train;

$$\therefore\ \tfrac{2}{3}\mathrm{M}u=\mathrm{T}v\sin\theta, \quad \therefore\ u=\tfrac{12}{7}v\sin\theta.$$

I will now find the depression which a blow communicating a velocity u at the point of interruption will cause: I will take the worst position for the interruption, viz. the middle point. The girder unsupported by a chain will bend, as experiment shows, through 1·6 foot under its own weight, together with the weight of the maximum load, 400 tons. Let z be the additional depression which would be caused by a pressure P; then

$$\mathrm{P}=750\frac{z}{1\cdot 6}=470z \text{ tons nearly,}$$

and the mass of the girder set in motion is equivalent to

$$\tfrac{2}{3}+350 \text{ tons} \div g,$$
$$\therefore\ \frac{d^2z}{dt^2}=-\frac{3g\times 470z}{2\times 350}=-64z;$$
$$\therefore\ \left(\frac{dz}{dt}\right)^2=u^2-64z^2.$$

Hence if h be the extent of the vibration caused by the blow, $h=\frac{1}{8}u=\frac{3}{14}v\sin\theta$ feet. If v be 88 feet, or the velocity 60 miles an hour, $h=4$ inches, if $\theta=1°$, or for a sudden change of 1 in 57 in the gradient of the rail. The depression will be greater or less in proportion to the change in the gradient.

The time in which the girder, on receiving the blow, passes through its depression is $\frac{\pi''}{16}=\frac{1''}{5}$. Each carriage is 25 feet long, and has two pairs of wheels. Hence the interval between successive blows $=\frac{12\cdot5}{88}=\frac{1''}{7}$; and therefore a second blow will occur before the girder has begun to recover itself, and will therefore still further increase the deflection; a third blow and others also will follow before the girder has begun to recover its former position, and these effects being accumulated may become dangerous. The blows will grow feebler and feebler after the first, because each is produced by the impact of only that portion of the train which has not at the moment passed the point of interruption.

If the velocity of the train be 20 miles (the sudden change in the gradient still being 1°), the intervals of the blows will be about $\frac{2''}{5}$ instead of $\frac{1''}{7}$. The result will be that the second blow will occur just as the girder has sprung back to its original position with a momentum equal to that which the first blow gave to it, but in the upward direction; the second blow will therefore be counteracted by this. The third blow will then have its full effect; but being feebler than the first, will not produce so great a depression as before; and the successive results will be smaller and smaller.

The calculation in this paragraph is sufficient to show the great importance of the rail over the bridge being entirely free from every impediment or check of every kind, and the advantage of having the girder unattached to the roadway.

J. H. Pratt.

Calcutta, March 12, 1862.

LXII. *Some remarks on a Paper by* Dr. A. Matthiessen, *F.R.S., and* C. Vogt, *Ph.D., "On the Influence of Traces of Foreign Metals on the Electric Conducting Power of Mercury."* *By* Robert Sabine, *Esq.*[*]

In the above paper, published in the March Number of the Philosophical Magazine, Drs. Matthiessen and Vogt give six Tables, with data and calculations of their tests, of the conducting powers of amalgams of bismuth, lead, tin, zinc, gold, and silver, in different proportions.

Accompanying these Tables, however, no formula is given to indicate how the numbers in the seventh columns, headed "conducting powers calculated," are obtained.

Having recently had occasion, in the laboratory of Dr. Werner Siemens, at Berlin, to be occupied with inquiries on the electrical resistances of amalgams, I was interested in going

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[*] Communicated by the Author.

carefully through the above paper, and have arrived at the results which follow.

When, from the formula given by Dr. Siemens in his paper "Ueber Widerstandsmaasse und die Abhängigkeit des Leitungswiderstandes der Metalle von der Wärme*,"

$$\lambda = \frac{100\,\sigma\,(W - w)}{wsm} + 1$$

we develope the value of the conducting power of the amalgam, substitute volumes instead of weights divided by specific gravities, and set C = the specific conducting power of mercury instead of unity, we get the simple formula

$$A = B\frac{v}{100} + C\left(1 - \frac{v}{100}\right),$$

in which A is the conducting power of the amalgam, and B that of the dissolved metal.

With this formula (based on the supposition that the conducting power of an amalgam is equal to the sum of the conducting powers of two parallel wires of the metals composing it) it would seem that Drs. Matthiessen and Vogt have also reckoned the numbers given in their columns headed "conducting power calculated;" for, in so far as the first four Tables go, the results agree perfectly with the formula. In the remaining two Tables, however, Drs. Matthiessen and Vogt, for no assigned cause, forsake the formula by which their former results were calculated, and give us, in the "conducting powers calculated" of gold and of silver amalgams, only a tenth part of the value of the first member in the equation making up the value of A.

As the amount of this error (probably due to setting false indices before the mantissæ of their logarithms) is very considerable, I give in four instances the corrected numbers.

Amalgams.	*v*.	Conducting power.			Error per cent.
		Observed.	Calculated.	Re-calculated.	
Silver-mercury . . .	0·129	10·984	10·978	11·715	6·3
	1·280	11·566	11·581	18·878	38·6
Gold-mercury	0·007	10·913	10·913	10·944	0·3
	0·700	11·571	11·180	14·296	21·8

In his paper above referred to, Dr. Siemens expresses his opinion that the conducting power of a fluid metallic mixture is in proportion to the conducting powers of the two metals in their *fluid state* at the same temperature.

Drs. Matthiessen and Vogt have taken into their calculations of the conducting powers of their amalgams the conducting

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* Poggendorff's *Annalen*, vol. cxiii. p. 91.

powers of the various metals in their *solid states* in conjunction with that of fluid mercury.

As it is important to know which of these opinions is right, I have calculated the conducting powers of the foreign metals from all the tests in the given Tables, and set their means in column B of the following

Table of Conducting Powers of Metals in Amalgam.

Table.	Metal.	Conducting powers.	
		B (calculated).	Matthiessen.
I . .	Bismuth	37·25	7·91
II . .	Lead. .	61·31	52·64
III . .	Tin . . .	75·16	78·51
IV . .	Zinc . .	92·99	184·06
V . .	Gold . .	109·89	494·68
VI . .	Silver .	61·46	633·33

The comparisons are made with the gold-silver alloy of Dr. Matthiessen, the conducting power of which, at 0° C, is taken at 100.

The opinion of Dr. Siemens with regard to the conducting powers of dissolved metals is therefore corroborated qualitatively by these tests of the amalgams of gold, silver, tin, and zinc, the conductibilities of which are all less in a melted than in a solid state, but opposed by those of lead. The results of the tests of bismuth-amalgam is also qualitatively corroborative of Dr. Siemens's opinion, as the conductibility of melted bismuth is greater than that of mercury at the same temperature.

Dr. Siemens gave the calculated conductibility of fluid silver at 15° C. (the temperature of the amalgams he tested) from three different experiments, respectively 8·8, 9·3, and 7·8, compared with the conductibility of mercury as unit. These numbers do not differ so materially from 5·64, the value given in col. B reduced to the same standard, when we consider that the conducting power of solid silver, according to Dr. Matthiessen, is no less than 58·05, and according to Dr. Siemens 64·38.

The results of Dr. Siemens's calculations of the conducting power of fluid zinc at about 20° C., from three measurements with zinc-amalgams given in the same paper, are 11·2, 12·7, and 11·2 respectively. Corresponding results from my calculation of Drs. Matthiessen and Vogt's experiments (col. B), reduced to the same standard, is 8·5, while the conductibility of solid zinc is 16·9.

It is evident, therefore, that in no case are we entitled to take the conductibility of metals in their solid states into our calculations of fluid amalgam resistances.

It is not improbable that the metals combine with mercury in atomic proportions, and, in this event, that the resulting compounds are dissolved in the overplus of mercury.

Such a combination would undoubtedly modify the distances of the compound atoms, and hence also the conductibility of the mass. The conductibilities of tin, zinc, gold, and silver-amalgams from the tests in question show an expansion of the molecules, those of lead and bismuth a contraction, supposing that expansion of the molecules causes a decrement, and contraction an increment of conductibility.

In adopting the opinion of a chemical combination and subsequent solution, the very conditions prohibit the adoption of a common formula which shall express with exactitude, for every metal, its amalgam conductibility, unless that formula embody a term embracing the effect of the combination and solution on the atoms and density of the resulting compound, although the formula which combines the conductibilities of the metals in a fluid state, without being absolutely correct, is near enough with some metals, when the per-centage of foreign metal in the amalgam is not too great. And a more exact knowledge of the nature of the atoms and of the origin of electrical resistance is necessary before such a formula can be constructed.

In these considerations I have supposed the methods employed by Drs. Matthiessen and Vogt, as well as their measurements, to be correct.

It is doubtful, however, if the thermometer-tubes employed were not necessarily of so small a bore that *filtration* of poorer amalgam into the tube while thicker remained in the cups was not facilitated. Thus the known proportions of the mixture poured into the cups would not have given a correct idea of the contents of the amalgam in the tube, and the conducting powers of the richer amalgams would have appeared lower than they really were.

It is also questionable if Drs. Matthiessen and Vogt have not relied too much on the weights of the metals used in making up the amalgams instead of analysing the latter after each test; for it is well known that amalgams of the easily oxidizable metals change their proportions when in contact with the air by rapid oxidation of the foreign metals.

I diminished these sources of error by employing tubes of, at least, 2 millims. diameter, and by analysing the contents of each tube after each test, disregarding entirely the contents of the cups, which were made removeable.

94 Markgrafen Strasse, Berlin,
20th April, 1862.

LXIII. *On Collyrite, and a native Carbonate of Alumina and Lime. By* J. H. GLADSTONE, *Ph.D., F.R.S., and* G. GLADSTONE, *F.C.S.*[*]

AT Hove, near Brighton, is an old quarry in the upper chalk that presents some appearances of more than ordinary interest. Among these are the faults which have traversed the strata and broken the layers of flint, splitting them in every direction, and reducing them in some places almost to powder. Some of these fissures are filled up with a mineral whiter than the surrounding chalk, and perfectly distinct from it, which runs also along the dislocated layers of flint, and frequently imbeds the fragments.

This very white mineral occurs in rounded masses easily disintegrated in water. It is very soft, easily friable, with an earthy fracture, of low specific gravity, porous, and slightly hygroscopic. The external portions are frequently stained red with sesquioxide of iron. When examined chemically, it was found to consist mainly of hydrated silicate of alumina, perfectly decomposable by strong hydrochloric acid. There was also a varying amount of carbonic acid and of lime. The alumina was found to be free from phosphoric acid; nor was it mixed with glucina, a small quantity of which has been recently found so often to accompany this earth. The mineral, when strongly heated, gave off both the combined water and the carbonic acid.

For analysis the mineral was pounded and allowed to stand over sulphuric acid *in vacuo* till freed from all hygroscopic moisture. The determination of the different constituents was made in the usual manner.

A very soft, pure-looking specimen gave the following proportions:—

Silicic acid	14·49
Alumina	47·44
Carbonic acid	0·79
Lime	0·89
Water and loss . . .	36·39
	100·00

The carbonic acid and lime, being in very nearly equivalent proportions, may be assumed to have existed in combination; and as they form together only 1·68 per cent., they may be considered as no constituent part of the aluminous mineral.

Excluding them, the results of analysis are as given below

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[*] Communicated by the Authors.

in the first column. The second column gives the theoretical proportions calculated from the formula $SiO^2, 2Al^2O^3+9HO$.

It is perfectly clear that the silicic acid and the alumina are in this ratio, but the amount of water is rather low for 9 equivalents.

	I.	II.
Silicic acid	14·74	14·14
Alumina	48·25	48·02
Water	37·01	37·84
	100·00	100·00

The mineral agrees, both in physical characters and in chemical composition, with that which has been described under the name of Collyrite, and to which the formula $SiO^2, 2Al^2O^3+10HO$ has been attributed; but our specimen appears to have been purer, and to have given more accurate numbers for the disilicate of alumina than those analysed by previous observers.

But no two portions analysed gave exactly the same composition. Some had a larger amount of carbonate of lime: thus a piece which was considerably harder, and broke with a conchoidal fracture, was found to contain between 5 and 6 per cent. of lime-salt. Some had a much smaller amount of silica: thus a piece which very easily fell to powder, and had the specific gravity of 1·99, gave about the following proportions:—

Silicic acid	3·4
Alumina	60·5
Carbonate of lime	0·6
Water and loss	35·5
	100·0

But the most remarkable specimens were from another part of the quarry. They had the same physical characters as those already described; but quantitative analysis showed that they contained more carbonic acid than was necessary to saturate the lime, and that there was no other base present except the alumina. Now as bicarbonate of lime is soluble in water, and carbonate of alumina is unknown as a mineral species, and has seldom, if ever, been procured even in the laboratory, it seemed desirable not to depend on one analysis. A quantity of one specimen was therefore pounded up and several determinations were made of each constituent, and that by different processes, the mean results of which are given in the first column of the subjoined Table. Another specimen was reduced to powder, and exposed over sulphuric acid *in vacuo* for a whole month, by which it lost no carbonic acid, but apparently a little of its combined water. It gave the analysis in the second column. Other specimens yielded the numbers in columns III. and IV.

	I.	II.	III.	IV.
Silicic acid	6·22	5·87	5·41	5·30
Alumina	41·04	39·58	36·32	40·51
Carbonic acid . .	10·91	14·77	18·15	14·14
Lime	7·37	11·22	11·62	9·18
Water	33·16	28·56	29·16	30·87
Traces and loss .	1·30			
	100·00	100·00	100·66	100·00

Now in each of these cases the carbonic acid is far more than sufficient to neutralize the lime. Thus

7·37	parts lime neutralize	5·79	parts carb. acid, leaving	5·12
11·22	,, ,, ,,	8·81	,, ,, ,, ,,	5·96
11·62	,, ,, ,,	9·13	,, ,, ,, ,,	9·02
9·18	,, ,, ,,	7·20	,, ,, ,, ,,	6·94

In what way is this excess of carbonic acid combined? It never exceeds in amount that which would be required to form bicarbonate, but in three instances it nearly approaches that quantity, hence the lime might be conceived as existing as such; or it might be carbonate of alumina; or a double carbonate of lime and alumina; or collyrite in which part of the silicic acid is replaced by carbonic acid. But each of these suppositions has its difficulties. Bicarbonate of lime in a solid form is unknown; yet it is conceivable that alumina, by its remarkable power of withdrawing other substances from solution, might have enabled such a bicarbonate to exist in combination with itself. Of the existence of any carbonate of alumina we have as yet no proof, whether as a mineral or a production of the laboratory. A double carbonate of lime and alumina was purely hypothetical. The partial substitution of carbonic for silicic acid has not hitherto been recognized, that we are aware of, in mineralogical chemistry; and though the results of analysis of the first and second specimens given above would accord very well with that view, yet the third and fourth specimens show too much silicic acid, unless indeed we suppose that they were derived from a collyrite much richer in silica than those hitherto examined.

If in the above analyses we view the lime and carbonic acid as wholly in combination, and reject them as adventitious, the remaining mineral will have very nearly the same composition in the four specimens, and that composition will be that of the previous specimen, minus half its silica, or SiO^2, $4\,Al^2\,O^3$, $20\,HO$.

	I.	II.	III.	IV.	Theory.
Silicic acid .	7·8	8·0	7·8	7·3	7·7
Alumina . .	51·0	53·0	51·1	52·4	51·5
Water . . .	41·2	39·0	41·1	40·3	40·8
	100·0	100·0	100·0	100·0	100·0

In order, however, to solve if possible the question of the excess of carbonic acid, a portion of the fourth specimen was finely powdered, diffused through water, and exposed to a stream of carbonic acid; the gas dissolved out a little carbonate of lime, which was precipitated and found to amount to 0·8 per cent. It seemed incapable of dissolving out any more, leading therefore to the conclusion that the amount of carbonate of lime, existing as such in the mineral, is only a trace, and that the remainder is in some form of combination with the alumina. The powdered mineral, which had been acted on by a very large amount of the gas, was afterwards analysed, and found to contain very nearly all its original carbonic acid and lime.

Extending our inquiry we attempted to form an analogous compound artificially, and at once obtained a double carbonate of alumina and lime, in which the carbonic acid was to the lime in the ratio of three to one, and another where the ratio was similar to that in the mineral.

Another fact which bears on the state of combination of this excess of carbonic acid is the following:—If the mineral, after having been dried *in vacuo*, is exposed to a temperature of 100° C., it does not lose either water or carbonic acid; but if it be heated more strongly, though not even to incipient redness, it parts not only with the water and the excess of carbonic acid, but also with a portion of that required to neutralize the lime, and if to dull redness in a covered crucible, it parts with nearly the whole of its carbonic acid; yet a little remains which cannot be driven off, even if the temperature be greatly raised. Now it might be expected that simple carbonate of alumina or a carbonated collyrite would be decomposed at a low heat, or that bicarbonate of lime would be reduced to the common carbonate; but the easy expulsion of the remaining carbonic acid is not very compatible with either of these suppositions, and appears rather to point to a double carbonate which yields up all its carbonic acid more freely than carbonate of lime does. The small quantity of undecomposable carbonate of lime left may either have existed as such originally, or may have been formed during the decomposition of the double salt.

We are disposed, therefore, to regard this mineral as collyrite mixed with a varying amount of a hydrated double carbonate of alumina and lime. If it should bear a distinct name, it may be termed Hovite, from the place where it was first recognized.

On looking over published analyses of silicates, it did not appear that a carbonate had often been found entering into the composition of such minerals, yet there is a perfectly analogous instance on a smaller scale. Allophane is another hydrated silicate of alumina, and it occurs in a chalk-pit at Charlton, also in the upper chalk, and under circumstances almost identical with those under which we found the collyrite at Hove. In Mr. Dick's analysis of this[*], and in the analyses of four different specimens by Mr. Northcote[†], there was always found more carbonic acid than was required to saturate the lime. In none of these instances, however, did it exceed 1·31 per cent., and Mr. Northcote propounded no other view than that it existed as a bicarbonate.

On inquiring about the quarry at Charlton from which this mineral was obtained, we were informed by Mr. Church that the allophane was accompanied by a substance resembling our collyrite. Some of this was obtained, and the two following analyses were made of portions having rather different physical characters.

The first was more compact and more vitreous in its fracture than the specimens from Hove, and not so perfectly white. It more closely resembled the specimens of collyrite in the British Museum. On analysis it was found to be a silicate of alumina soluble in acids, with a little carbonate of lime. A portion which had been pounded and allowed to stand two days over sulphuric acid, lost 7·12 per cent. of water when heated to 100° C. The following was the analysis made:—

Silicic acid	16·58
Alumina	44·51
Water expelled above 100° C. and some Carbonic acid . .	34·17
Lime and loss	4·74
	100·00

This shows a somewhat larger proportion of silicic acid in comparison with alumina than any of the preceding analyses, and indicates that the mineral was intermediate between allophane and collyrite, in chemical composition as in physical pro-

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[*] See Professor Morris's paper, Quart. Journ. Geol. Soc. vol. xiii. p. 13.

[†] Phil. Mag. May 1857.

perties. The carbonic acid did not appear to be in excess of the lime.

The second portion was very white and soft; after being dried *in vacuo* it did not give off water when heated at 100° C. It dissolved readily in hydrochloric acid with formation of gelatinous silica.

The results of analysis were as follows:—

Silicic acid	4·31
Alumina	59·08
Water	35·03
Carbonate of lime, and loss	1·58
	100·00

This is almost identical with one of the specimens from Hove.

Altogether these hydrated silicates of alumina, many of which have been analysed and described under the names of allophane and collyrite, appear to form a series in which the silicic acid varies greatly in proportionate amount. They may be viewed as a hydrated silicate of definite composition, combined with indefinite amounts of the native hydrate of alumina, Al^2O^3, 3HO, Gibbsite. But what is this definite silicate? Collyrite, $2(Al^2O^3)SiO^2$, 9HO plus $6(Al^2O^3, 3HO)$, would give numbers almost identical with those of the last analysis recorded above; but collyrite itself might be viewed as allophane plus some equivalents of hydrate of alumina, and Mr. Northcote views allophane as a still higher silicate combined with different proportions of the hydrate. All these formulæ might also be expressed as Dr. Odling's ortho-silicate, $Al^4 SiO^4$, plus more or less hydrate of alumina, plus more or less water; but not one of these methods of expression appears to possess any such preponderating advantage as to lead to the conviction that it represents the true composition of the mineral under its various phases.

LXIV. *On the Allotropic States of Oxygen; and on Nitrification.*

By Professor C. F. Schönbein[*].

I HAVE been busily occupied with my favourite study, and have found out several new facts regarding the allotropic states of oxygen, their changeability one into another, and nitrification, and I am inclined to believe that the results obtained are not quite void of scientific interest.

After many fruitless attempts at isolating ozone from an "ozonide", I have at last succeeded in performing that exploit; and have also found out simple tests for distinguishing with the greatest ease ozone from its antipode, "antozone." As to the

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[*] Extracted from a Letter to Professor Faraday.

production of ozone by purely chemical means, the whole secret consists in dissolving pure manganate of potash in pure oil of vitriol and introducing into the green solution pure peroxide of barium, when ozone mixed with common oxygen will make its appearance, as you may easily perceive by your nose and other tests. By means of the ozone so prepared, I have rapidly oxidized silver at the temperature of —20° C., and by inhaling it produced a capital "catarrh."

Regarding nitrification, the most important fact I have discovered is the generation of nitrite of ammonia out of water and nitrogen, *i. e.* atmospheric air, which is certainly a most wonderful and wholly unexpected thing. To state the fact in the most general manner, it may be said that the salt mentioned is always produced if water be evaporated in contact with atmospheric air. This may be shown in a variety of ways. Let, for instance, a piece of clean linen drenched with distilled water dry in the open air, moisten it then with pure water, and you will find that the liquid wrung out of the linen and acidulated with dilute sulphuric acid (chemically pure) will strike a blue colour with starch-paste containing iodide of potassium,—by the by, the most delicate test for the nitrites. It is therefore a matter of course that shirts, handkerchiefs, table-cloths, in fact all linen, &c., must contain appreciable quantities of nitrite of ammonia; and if the chemistry of England be not entirely different from that of Switzerland, you will find the same thing at the Royal Institution. The purest water, suffered to evaporate spontaneously in the open air, will after some time have taken up enough nitrite of ammonia (continually being formed at the evaporating surface) to produce the nitrite reaction. If you make use of water holding a little potash, or any other alkali, in solution, the same result will be obtained, *i. e.* the nitrite of that base will be formed (of course in small quantity). The most convenient way of performing the experiment is to moisten a bit of filtering-paper with a dilute solution of chemically pure potash, &c., and to suspend it for twenty-four hours in the open air. On examining the paper it will be found to contain a perceptible quantity of a nitrite, which by a longer exposure of course increases. But you may still more rapidly convince yourself of the correctness of my statements, if you heat pure water to a temperature of 50° or 60° C. in a porcelain basin, and suspend over the evaporating surface bands of filtering-paper soaked with a weak solution of potash, soda, or the carbonates of these bases. Within a very short time (in ten minutes or so) there will be enough of the nitrite accumulated in the paper to produce the reactions of that salt. I enclose a bit of paper treated in that way for a couple of hours, and by laying it upon a watch-glass and pouring over it acidulated starch-paste containing iodide of potassium, you will perceive the effect produced. The fact which I have ascertained, that the purest water mixed with a little chemically pure sulphuric acid or potash and kept for some time evaporating in the open air at a temperature of 50° or 60° C. (the loss of the liquid being now and then restored) contains, in the first case, a perceptible quantity of ammonia, and, in the second case, of nitrous acid, may now be easily accounted for. You know that about eighteen months ago I found that, during the slow combustion of phosphorus in moist atmospheric air, very perceptible quantities of nitrite of ammonia are formed, and drew from that fact the inference that the salt is engendered by 3 equivalents of water combining directly with 2 equivalents of nitrogen. Now there is to me hardly any doubt that the production of that nitrite is due to the evaporation of water taking place about the phosphorus, whose temperature, in consequence of its burning state, proves to be higher than that of the surrounding medium, and the fact alluded to must therefore be considered only as a particular case of a general rule. The same remark applies to the formation of nitrite of ammonia which takes place during the rapid combustion of charcoal, &c. in atmospheric air. Combustion, as such, has, I believe, nothing to do with that formation. I must not omit to tell you that by means of a large copper still, properly heated, and taking care not to introduce too much water into the vessel at once, I can prepare in a very short time several pints of water with which the reactions of nitrite of ammonia may be produced in the most striking manner. I hope before long to have an opportunity of sending you some of this water.

I cannot finish my letter without saying a word or two about nitrification in general, a fact hitherto so much enveloped in obscurity. I think the matter is now clear enough. The evaporation of water is continually going on in the atmosphere, and along with it the generation of nitrite of ammonia. Now, this salt being put in contact with the alkaline bases or their carbonates, nitrites of potash and the other alkalies are formed, which afterwards become gradually oxidized into nitrates. In our rainy countries these salts are washed away almost as soon as formed, and carried into the springs, rivers, &c.; and there is therefore no accumulation of them as in the East Indies, &c.

That the formation of our nitrite out of water and nitrogen is a fact highly important for vegetation need hardly be stated. Indeed each plant, by continually evaporating water into the atmosphere, becomes a generator of nitrite of ammonia, preparing, if not all, at least part of its nitrogenous food, and the same thing takes place in the ground on which it stands. I am therefore inclined to think that our friend Liebig is right in asserting that no plant wants any artificial supply of ammonia, or of matters producing that compound, there being enough of it offered by natural means. Having communicated the results of my researches on the subjects mentioned above to the Academy of Munich, I hope they will soon be published.

[In relation to the peculiar circumstances under which oxygen and nitrogen combine, it may be worth while here to refer to the results obtained by Dr. Bence Jones (Phil. Trans. 1851, p. 407, &c.), where the direct union of these gases in all cases of combustion in air is described. Schönbein's results depend upon evaporation.—M. F.]

LXV. *Supplementary Remarks on* M. Hermite's *Argument relating to the Algebraical Resolution of Equations of the Fifth Degree. By* G. B. JERRARD*.

9. IN art. 8 of my "Remarks on M. Hermite's Argument†," I stated that his conclusion was such as to indicate that an error must somewhere have found its way into his calculus. The reasons in support of my statement, which are there only glanced at, I proceed to explain.

10. Putting his final result under the form

$$\frac{N}{\mathfrak{D}},$$

it is clear that N may be regarded as an integral function of the coefficients $A_1, A_2, \ldots A_5$, and such as not to involve any radicals except those characterized by the symbols $\sqrt{}$, $\sqrt[3]{}$; while $\mathfrak{D}$ may be supposed to be a rational as well as an integral function of the coefficients in question.

11. Let now

$$\frac{N_1}{\mathfrak{D}_1}$$

denote what $\frac{N}{\mathfrak{D}}$ becomes when we assign such values to $A_1, A_2, \ldots A_5$ that the equation in x shall be a solvible equation of the fifth degree, the expressions for whose roots shall involve irreducible radicals of the form $\sqrt[5]{z}$. How can this case be explained?

12. Here, you will say, $\mathfrak{D}_1=0$; so that we may obtain an independent solution into which quintic radicals shall enter. A

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* Communicated by the Author.

† See the Philosophical Magazine for last February.

very little reflection, however, will convince us that the question has a far wider scope than this answer would imply.

13. Doubtless $\mathfrak{D}_1$ ought to vanish; in other words, $\mathfrak{D}$ ought to be such as to become equal to zero for those particular values of $A_1, A_2, \ldots A_5$ which lead to a solvible case involving fifth roots. If, then, $\mathfrak{D}$ be so constituted as to apprise us of the existence of all such solvible cases—as it unquestionably ought to do—it must involve factors by the evanescence of which each expression for $\mathfrak{D}_1$ shall vanish. Accordingly $\mathfrak{D}$ must not be composed merely of a succession of terms of the form $0 \times A^\alpha B^\beta C^\gamma \ldots E^\epsilon$; A, B, C, .. E having the same meanings as $A_1, A_2, A_3, \ldots A_5$ respectively. Now this function $\mathfrak{D}$, in which A, B, C, .. E are all of them supposed to be arbitrary, cannot vanish unless—in opposition to what has been just stated—it be made up of terms of the form $0 \times A^\alpha B^\beta C^\gamma \ldots E^\epsilon$. I conclude therefore that $\mathfrak{D}$ *ought to be different from zero.*

14. Again, when I regard $\mathfrak{D}$ from another point of view as the denominator of an expression indicative of an impossibility, I am forced to come to the conclusion *that* $\mathfrak{D}$ *ought to be equal to zero.*

15. Reflecting on the incongruous properties which are thus seen to attach themselves to the function $\mathfrak{D}$, I am unable to accept the result

$$\frac{N}{\mathfrak{D}}$$

as free from error.

16. In my next paper I purpose to meet some objections urged by Mr. Cayley and Mr. Cockle against my proof, in the Philosophical Magazine for May 1861, of the impossibility of establishing a rational communication* between the function $f\iota f\iota^2 f\iota^3 f\iota^4$ and its fifth power.

March 1862.

LXVI. *Postscript to the Paper "On a Question in the Theory of Probabilities" in the May Number.* By A. Cayley, *Esq.*†

I UNACCOUNTABLY did not recall to myself Mr. H. Wilbraham's paper "On the Theory of Chances developed in Prof. Boole's 'Laws of Thought,'" Phil. Mag. vol. vii. pp. 465–476 (1854), which contains a most valuable discussion of the ques-

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* In addition, be it remembered, to the one

$$v=u^5,$$

which characterizes the case in question.

† Communicated by the Author.

tion. Using, as before, ABE, A′BE, &c. to denote the probabilities of the compound events ABE, A′BE, &c., Mr. Wilbraham in effect shows that in each of the two solutions the following equations are (as they obviously should be) satisfied, viz.

$$\left.\begin{array}{llllll}
\mathrm{ABE}+\mathrm{ABE'}+\mathrm{AB'E}+\mathrm{AB'E'} & +\mathrm{A'BE}+\mathrm{A'BE'}+\mathrm{A'B'E'} & & =1, \\
\mathrm{ABE}+\mathrm{ABE'}+\mathrm{AB'E}+\mathrm{AB'E'} & \ldots\ldots\ldots & & =\alpha, \\
\mathrm{ABE}+\mathrm{ABE'} \quad \ldots\ldots & +\mathrm{A'BE}+\mathrm{A'BE'} & \ldots & =\beta, \\
\mathrm{ABE} \quad \ldots \quad +\mathrm{AB'E} & \ldots\ldots\ldots & \ldots & =\alpha p, \\
\mathrm{ABE} \quad \ldots\ldots\ldots & +\mathrm{A'BE} & \ldots\ldots & =\beta q;
\end{array}\right\}(a)$$

but (besides these) that, on the one hand, Prof. Boole has the relations

$$\frac{\mathrm{ABE}}{\mathrm{A'BE}}=\frac{\mathrm{AB'E}}{\mathrm{A'B'E'}},\quad \frac{\mathrm{ABE'}}{\mathrm{A'BE'}}=\frac{\mathrm{AB'E'}}{\mathrm{A'B'E'}} \quad . \quad . \quad . \quad . \qquad (b)$$

which equations are consequently implicit assumptions in his theory, and which, with the equations (*a*), give his solution, and that, on the other hand, I have the relations

$$\frac{\mathrm{ABE}+\mathrm{ABE'}}{\mathrm{A'BE}+\mathrm{A'BE'}}=\frac{\mathrm{AB'E}+\mathrm{AB'E'}}{\mathrm{A'B'E}+\mathrm{A'B'E'}},\quad \frac{\mathrm{ABE'}}{\mathrm{A'BE'}}=\frac{\mathrm{AB'E'}}{\mathrm{A'B'E'}}. \qquad (c)$$

which are consequently implicit assumptions of mine, and which, with the equations (*a*), lead to my solution,—the signification of these two equations being that the events A, B are treated as independent (1) in the case where it is not observed whether E does or does not happen, (2) in the case where E does not happen.

The second of the equations (*b*) is the same as the second of the equations (*c*). But it is not easy to explain the first of the equations (*b*); indeed Mr. Wilbraham remarked that it appeared to him not only arbitrary but eminently anomalous. The peculiarity in its form is, that it does not, like the others, when ABE, &c. are considered as products, reduce itself to an identity; it seems to be a conclusion which, in support of his theory, Prof. Boole is bound to justify *à posteriori*.

Prof. B[illegible]le wishes me to mention that he has succeeded in obtaining a demonstration of the analytical theorem arising from his theory, referred to in his "Reply" in my paper.

2 Stone Buildings, W.C.,
May 7, 1862.

LXVII. *Chemical Notices from Foreign Journals.* *By* E. Atkinson, *Ph.D., F.C.S.*

[Continued from vol. xxii. p. 521.]

HÜBNER* has investigated several decompositions of chloride of acetyle. When this substance is enclosed in a sealed tube with pentachloride of phosphorus and heated for some time to 100°, and for a short time to 190°, an action takes place the result of which is that the tube contains nothing but liquid. On opening the tube, a stream of hydrochloric acid escapes; and on subsequently distilling the contents, a series of bodies is obtained, the first of which is terchloride of phosphorus, followed by a little oxychloride of phosphorus. The distillate which passed over at about 118° consisted of chloride of trichloracetyle, $\text{Ͼ}^2\,\text{Cl}^3\,\Theta\,\text{Cl}$, as was proved by converting this substance into trichloracetic ether. Besides this body, and the less highly chlorinated compounds, $\text{Ͼ}^2\,\text{H}^2\,\text{Cl}\,\Theta,\ \text{Cl}$ and $\text{C}^2\,\text{H}\,\text{Cl}^2\,\Theta,\ \text{Cl}$, Hübner considers that the bodies, $\text{Ͼ}^2\,\text{H}^3\,\text{Cl}^3$, $\text{Ͼ}^2\,\text{H}^2\,\text{Cl}^4$, $\text{Ͼ}^2\,\text{H}\,\text{Cl}^5$, and $\text{C}^2\,\text{Cl}^6$, are also probably formed.

Chloride of acetyle and cyanide of silver were enclosed together in a glass tube, and heated for a couple of hours to 100° to complete the reaction which was set up soon after the tube was sealed. On subsequently opening the tube, an odour of acetamide and hydrocyanic acid was perceived; and on distilling the contents, a body was obtained which was ultimately found to boil constantly at 93°. The analysis of this compound proved that it was the *cyanide of acetyle*, $\text{Ͼ}^2\,\text{H}^3\,\Theta\,\text{Ͼ}\,\text{N}$. This body is lighter than water, in which it dissolves with the formation of hydrocyanic and acetic acids.

When this cyanide of acetyle is placed in contact with hydrate of potash or sodium in closed vessels, it is transformed into an oil insoluble in water. Potash only acts when the mixture is agitated, but then with such a disengagement of heat as to require cooling down to prevent the evaporation of the cyanide of acetyle. This oil, when washed with water, solidifies to a divergent crystalline mass, especially when touched with a sharp point. Singularly enough it has exactly the same composition as the liquid cyanide of acetyle. It melts at 69° and boils at 170°; it remains liquid for some time at a moderate temperature, but then crystallizes in large plates. Boiled with potash it disengages ammonia.

The author is still engaged with the investigation of these compounds.

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* Liebig's *Annalen*, December 1861.

Sulphocyanic acid, $\left.\begin{matrix}Cy\\H\end{matrix}\right\}S$, can be regarded as sulphuretted hydrogen in which an equivalent of hydrogen is replaced by cyanogen. Linnemann has prepared the corresponding anhydride of sulphocyanic acid by the action of iodide of cyanogen on the sulphocyanide of silver.

$$\underset{\text{Sulphocyanide of silver.}}{AgCyS} + \underset{\text{Iodide of cyanogen.}}{ICy} = \underset{\text{Iodide of silver.}}{AgI} + \underset{\text{Anhydrous sulphocyanic acid.}}{Cy^2S}$$

The reaction is exceedingly regular; it takes place at a moderate temperature, and is best effected when an etherial solution of iodide of cyanogen is triturated at a gentle heat with the corresponding quantity of the silver-salt. The product is treated with bisulphide of carbon, by which the sulphide of cyanogen is dissolved out, and, on cooling, is obtained in transparent rhombic plates, or long thin laminæ. It sublimes at 30° or 40°, and is thereby obtained in small, highly refringent thin laminæ. It is heavier than bisulphide of carbon; it is dissolved by ether, alcohol, and water, and readily crystallizes from the hot supersaturated solution. It is decomposed by potash into cyanate and sulphocyanide of potassium; and with nascent hydrogen, sulphuretted hydrogen, and sulphide of potassium it yields the same products, namely hydrocyanic acid and sulphocyanic acid. It unites with two molecules of ammonia to form a sulphide of cyanammonium —a reaction quite analogous to that of sulphuretted hydrogen on ammonia, which yields sulphide of ammonium. Thus,

$$\left.\begin{matrix}Cy\\Cy\end{matrix}\right\}S+2NH^3=\left.\begin{matrix}NH^3Cy\\NH^3Cy\end{matrix}\right\}S,$$

$$\left.\begin{matrix}H\\H\end{matrix}\right\}S+2NH^3=\left.\begin{matrix}NH^3H\\NH^3H\end{matrix}\right\}S.$$

According to Lassaigne*, volatile *chloride of cyanogen* can be obtained by the action of chlorine on an aqueous solution of cyanide of potassium. A current of the gas is passed into a solution of one part of cyanide of potassium contained in a roomy flask which is kept cold with ice; in the cork of this flask is fixed a tube leading to a U-tube placed in a mixture of ice and salt: the greater part of the chloride of cyanogen condenses in this tube to colourless crystals.

Bromide of cyanogen may be formed in an analogous manner by adding bromine, which has been cooled to 0°, to a solution of cyanide of potassium in water contained in a retort, which is kept cold by means of ice. After a sufficient quantity of bromine has been added, the retort is stoppered and gently warmed,

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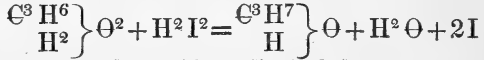
* Liebig's *Annalen* (Supplement), December 1861.

on which bromide of cyanogen is condensed in the neck in cubical and acicular crystals.

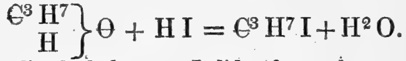
Bunsen*, by means of the spectrum-analysis, has established the presence of *lithion* in two meteorites—one which fell at Juvenas in France, in 1821, and the other at Parnella in Southern India, in 1857.

Lautemann has shown that, by the reducing action of hydriodic acid, lactic acid may be reduced to propionic acid†. Propylic glycol stands to propylic alcohol in the same relation as lactic acid to propionic acid; and by the action of hydriodic acid on propylic glycol, Wurtz‡ has shown that it is reduced to propylic alcohol. When the former substance was heated in the water-bath for some time with concentrated hydriodic acid, the mixture became dark from the separation of iodine; and on subsequent neutralization and distillation, a heavy liquid was obtained which proved to be iodide of propyle, Ꞓ^3H^7I. This had been formed in consequence of a double reaction—reduction of propylic glycol to propylic alcohol, and change of the latter to iodide of propyle in consequence of the further action of hydriodic acid.



$$\left.\begin{matrix}\text{Ꞓ}^3H^6\\H^2\end{matrix}\right\}\Theta^2+H^2I^2=\left.\begin{matrix}\text{Ꞓ}^3H^7\\H\end{matrix}\right\}\Theta+H^2\Theta+2I$$

Propylic glycol. Propylic alcohol.



$$\left.\begin{matrix}\text{Ꞓ}^3H^7\\H\end{matrix}\right\}\Theta+HI=\text{Ꞓ}^3H^7I+H^2O.$$

Propylic alcohol. Iodide of propyle.

This production of propylic alcohol differs from the result obtained by Lourenço, inasmuch as the above reaction is one of reduction, while Lourenço's reaction is a case of inverse substitution.

Wurtz has also obtained iodide of butyle, by a precisely analogous reaction with hydriodic acid and butylic glycol. Ethylic glycol, as Simpson has shown§, is converted by hydriodic acid into iodide of ethylene.

Boutlerow|| has described the synthetic formation of a saccharine substance. To a solution of dioxymethylene, lime-water was gradually added, and this solution evaporated in the water-bath and afterwards *in vacuo*, by which a syrupy residue was obtained containing crystals of formiate of lime. By treating this residue

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* Liebig's *Annalen*, November 1861.

† Phil. Mag. vol. xix. p. 384.

‡ *Ann. de Chim. et de Phys.*, vol. lxiii. p. 124.

§ Phil. Mag. vol. xix. p. 73.

|| Liebig's *Annalen*, December 1861.

with alcohol a saccharine substance was dissolved out, which Boutlerow calls *methylenitane.* It is uncrystallizable, and, heated on platinum-foil, emits the odour of burnt sugar. Its aqueous solution is feebly acid, and reduces a solution of tartrate of copper even in the cold. When heated with excess of butyric acid, it forms an oily compound, which is somewhat viscous at ordinary temperatures; it is decomposed by baryta-water with formation of butyrate of baryta. It was not possible to free it entirely from an inorganic substance, probably formiate of baryta, and the analyses were not very concordant. Boutlerow assumes provisionally that its formula is $\text{Ꞓ}^{7}\,\text{H}^{14}\,\text{Ɵ}^{6}$, and he thus expresses its formation:—

$$4\text{Ꞓ}^{2}\,\text{H}^{4}\,\text{Ɵ}^{2} \;=\; \text{Ꞓ}^{7}\,\text{H}^{14}\,\text{Ɵ}^{6} + \text{Ꞓ}\,\text{H}^{2}\,\text{Ɵ}^{2}.$$

Dioxymethylene. Methylenitane. Formic acid.

Boutlerow says, "It is the first example of the synthesis of a substance of a saccharine character, and from the simplest compounds of organic chemistry. Considering the whole series of changes through which ethylic alcohol passes, which can itself be formed from the elements it contains, it may be said that the first example of the complete synthesis of a saccharine substance is here met with."

The same chemist* has described a new mode of forming ethylene and its homologues. When iodideof methylene, $\text{C}^{2}\,\text{H}^{2}\,\text{I}^{2}$, was heated with copper and water in a closed tube, a gaseous mixture was formed containing carbonic acid, marsh-gas, and various hydrocarbons, about 85 per cent. of which were absorbed by bromine with the formation of an oily liquid. This proved to be a mixture of the bromides of the general formula $\text{Ꞓ}^{n}\,\text{H}^{n}\,\text{Br}^{2}$, the greater part of which consisted of bromide of ethylene, $\text{Ꞓ}^{2}\,\text{H}^{4}\,\text{Br}^{2}$.

Beilstein† has made a series of experiments on glyceric acid. When an aqueous solution of this acid was mixed with iodide of phosphorus a brisk reaction was set up, and hydriodic acid vapours disengaged; the mass in the retort, at first liquid, solidified on cooling to a white crystalline mass, which on recrystallization was found to consist of *iodopropionic acid,* $\text{Ꞓ}^{3}\,\text{H}^{5}\,\text{I}\,\text{Ɵ}^{2}$, the formation of which may be thus expressed:—

$$\text{Ꞓ}^{3}\,\text{H}^{6}\,\text{Ɵ}^{4} + \text{P}\,\text{I}^{2} \;=\; \text{Ꞓ}^{3}\,\text{H}^{5}\,\text{I}\,\text{Ɵ}^{2} + \text{H}\,\text{I} + \text{P}\,\text{Ɵ}^{2}.$$

Glyceric acid. Iodopropionic acid.

The group $\text{P}\,\text{Ɵ}^{2}$, which contains the elements of phosphorous and phosphoric acids, probably takes up water, and decomposes into these acids. Iodopropionic acid crystallizes from a hot saturated

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* Liebig's *Annalen,* December 1861. † Ibid. November 1861.

aqueous solution in pearly laminæ, and from a less concentrated solution in large glassy crystals. Its solution can be boiled without change, but its salts are decomposed by this process.

When the silver-salt was treated in this way, it was decomposed with formation of iodide of silver. The mother-liquor from this yielded a substance which crystallized in fine needles, was strongly acid, and evaporated without residue when heated on platinum-foil. The analysis of this compound proved that this was a substance having the same composition as lactic acid; but the investigation of the salts showed that it differed from this acid. It is still under investigation.

Beilstein also endeavoured, but without success, to form a bromolactic acid with a view to effecting its conversion into glyceric acid, by a reaction analogous to that by which chloropropionic acid is converted into lactic acid.

Caventou[*] has investigated some of the bromine substitution products of bromide of ethyle. Bromide of ethyle was enclosed in a sealed tube with excess of bromine and heated to about 170°. By repeated fractional distillations of the product, two distinct compounds were obtained: one, boiling at 110° to 112°, is monobrominated bromide of ethyle $(\text{Є}^2\,\text{H}^4\,\text{Br})\,\text{Br}$, and is isomeric with bromide of ethylene; the other, boiling at 187°, is $(\text{Є}^2\,\text{H}^3\,\text{Br}^2)\,\text{Br}$, and is not merely isomeric, but is identical in properties with Wurtz's brominated bromide of ethylene, $(\text{Є}^2\,\text{H}^3\,\text{Br})\,\text{Br}^2$. There is another compound of the formula $\text{Є}^2\,\text{H}^4\,\text{Br}^2$, bromide of ethylidene, obtained by the action of terbromide of phosphorus on aldehyde. Unlike, however, its isomers, bromide of ethylene and the body which Caventou has described above, it cannot be distilled without undergoing decomposition.

Caventou found that brominated bromide of ethyle is decomposed when heated with acetate of potassium for two days to 140°, forming bromide of potassium and acetate of glycol—an instance of the transformation of alcohol into a glycol compound.

Zwenger and Dronke[†] have discovered in the blossoms of the Acacia a new glucoside. It is prepared by treating the evaporated aqueous extract of the blossoms with alcohol; the new body, which they call *Robinine*, separates out from the concentrated alcoholic solution in crystals, which are pressed, dissolved in boiling water, and treated with neutral acetate of lead to remove foreign substances. The solution is then freed from lead by treatment with sulphuretted hydrogen; and, on the cooling

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[*] Liebig's *Annalen*, December 1861.

[†] Liebig's *Annalen* (Supplement), December 1861.

of the liquid, robinine separates out in yellowish crystals, and is purified by repeated recrystallizations from water.

It is a neutral substance, with a feeble astringent taste. Very soluble in hot water, it is slightly soluble in alcohol, and insoluble in ether. It melts at 195°, and solidifies to an amorphous mass. It is dissolved by alkaline solutions with a fine golden-yellow colour. It readily reduces an alkaline solution of oxide of copper, and also chloride of gold.

It is readily decomposed by boiling with dilute hydrochloric acid, with the separation of *quercetine*, while the solution contains sugar. Emulsine does not produce this change. This decomposition reveals a connexion between this new glucoside and quercitrine; both of them yield quercetine by decomposition, but differ in the nature of the saccharine substances which they contain as a copulate. This is the first instance of the kind in which such a difference occurs in the glucoside. Zwenger and Dronke have prepared quercetine by the decomposition of quercitrine, and found that this is quite identical with the body prepared from robinine.

Robinine, dried at 100°, has the formula $C^{50}H^{30}O^{32}$; dried in the air its formula is $C^{50}H^{41}O^{43}$, containing 11 equivalents of water of crystallization. The formula of quercitrine was found to be $C^{38}H^{18}O^{20}$.

The decomposition of robinine is expressed by the following equation:—

$$\underset{\text{Robinine.}}{C^{50}H^{30}O^{32}}+4HO=\underset{\text{Quercetine.}}{C^{26}H^{10}O^{12}}+\underset{\text{Sugar.}}{2(C^{12}H^{12}O^{12})}$$

and that of quercitrine thus:—

$$\underset{\text{Quercitrine.}}{C^{38}H^{18}O^{20}}+7HO=\underset{\text{Quercetine.}}{C^{26}H^{10}O^{12}}+\underset{\text{Sugar.}}{C^{12}H^{15}O^{15}}.$$

Quercitrine, in almost all its physical and chemical characteristics, is different from robinine—more especially in its difficult solubility in hot water, in its being precipitated by acetate of lead, and in its crystalline form; and the only similarity is the elimination of quercetine by both bodies when treated with acids.

The sugar from quercitrine forms yellowish crystals with a sweet taste, reduces alkaline solution of sulphate of copper even in the cold, and, mixed with yeast, passes into the spirituous fermentation. By oxidation with nitric acid it yields oxalic acid alone. The robinine sugar, on the contrary, does not crystallize; it however reduces oxide of copper in solution of potash even in the cold, and, mixed with yeast, passes into the alcoholic fermentation. Oxidized with nitric acid it yielded principally picric acid, and was thus different from quercitrine sugar.

From the great analogy which a number of glucosides present, in their products of decomposition, with robinine and quercitrine, it is probable that they are either identical with these, or like those analogous compounds of quercetine with different kinds of sugar. The authors are engaged on investigations in this direction.

Berthelot* observed that in an alcoholic solution of baryta which had been kept in a loosely-corked vessel for several years, there had been formed aldehyde resin, oxalic acid, and a small quantity of a peculiar volatile acid soluble in water. As far as this was investigated, its properties seemed to agree most closely with those of *acrylic acid*, $C^6 H^4 O^4$.

The formation of this acid from alcohol is readily understood, knowing that it is easily oxidized to acetic and formic acids, and that it thus can be conceived as being formed from ordinary aldehyde and formic acid,

$$C^6 H^4 O^4 = C^4 H^4 O^2 + C^2 H^2 O^4 - H^2 O^2,$$

just as cinnamic acid is formed from benzoic aldehyde and acetic acid.

LXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 411.]

May 16, 1861.—THE Croonian Lecture.—"On the Relations between Muscular Irritability, Cadaveric Rigidity, and Putrefaction." By C. E. Brown-Séquard, M.D., F.R.S.

May 30.—"On the Elimination of Urea and Urinary Water in their relation to the Period of the Day, Season, Exertion, Food, and other influences acting on the Cycle of the Year." By Dr. Edward Smith.

"On the Theory of the Polyedra." By the Rev. T. P. Kirkman, M.A., F.R.S. &c.

June 13.—Thomas Graham, Esq., Master of the Mint, Vice-President, in the Chair.

The following communications were read:—

"Notice of Recent Scientific Researches carried on Abroad." By the Foreign Secretary.

The following notice of his researches has been furnished to the Foreign Secretary by M. Schrauf.

"On the Determination of the Optical Constants of Crystallized Substances." (First and Second Series.) By Albert Schrauf (Vienna.)

In the two hitherto published series of these investigations, the data concerning the refractive and dispersive powers of twenty crystallized substances are communicated.

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* Liebig's *Annalen* (Supplement), June 1861.

Being persuaded that crystallo-physics, more than any other branch of physical science, is founded on quantitative calculation of absolute exactitude, I contrived to obtain first incontestable facts connected with the hitherto somewhat neglected phenomena of dispersion and refraction. Nearly 1000 substances have been crystallographically investigated, and about 200 have been made the object of optical researches; many of them, however, have remained unknown as to their dispersive and reflective powers, which, representing the quantitative and qualitative action of any substance on the propagation of light, are of absolute necessity for the construction of any sound theory.

It becomes every day a greater necessity to obtain, within these extensive dominions of human knowledge, a certain number of general views, subservient to the explanation and systematic arrangement of a great number of isolated facts, as only a small portion of the present investigation has led to the establishment of general laws. The great problem of crystallo-physics proposed for solution may be expressed by the question, *What is the causal connexion between chemical constitution and morphological and optical properties?* The phenomena of isomorphism, discovered by Prof. Mitscherlich, have indeed thrown considerable light on the mutual relation of chemical constitution and morphological properties; yet little, if anything, has been done to arrive at the solution of the problem in its general form.

As latterly several doubts have been expressed as to the possible existence of such a connexion, the purpose of my investigations shall be not only (as expressed in their title) to fill up deficiencies in the knowledge of facts, but also to propose several explanations indicating the real existence of such a connexion, and the necessity of making it an object of earnest research.

In the following paragraphs I intend only to mention some theorems whose solution is already achieved. Another series of my investigations, to be published subsequently, is to afford general demonstrations and applications of consequences in strict connexion with duly stated facts.

The most important of the theorems, as far as they may be simply enunciated, are—

§ 1. The calculation, graphic representation, and derivation of all the crystallographical and physical properties of the *rhombohedral system* are possible if three rectangular axes are assumed; the axis c coinciding with the principal rhombohedral axis, and the axes a and b with the diagonals of the prism of 60°.

§ 2. The following indices represent consequently the characteristic equations for the *symmetrical* crystallographic systems:—

Rectangular Axes.

Tesseral $a:b:c = 1:1:1$
Pyramidal $= 1:1:l$
Rhombohedral $= \sqrt{3}:1:l$
Prismatic $= h:1:l.$

§ 3. The optical axes of elasticity, coinciding with the diagonal of the prism of 60°, are nearly equal to each other, and (α and β being axes of elasticity and a and b crystallographical axes) if limit $\frac{a}{b}$ is supposed to be $=\sqrt{3}$, then $\alpha=\beta$.

§ 4. Whenever a prism of 60° is extant in the *prismatic* system, the first median line ("bissectrice de l'angle aigu") is perpendicular to its diagonal*.

§ 5. Whenever a number of prisms of 60° are extant (110, 011, 101), the first median line stands perpendicular to the diagonals of one of these prisms, and simultaneously to the plane of cleavage.

§ 6. The first median line is generally perpendicular to the diagonal of prisms, whose limit may be expressed by simpler proportions, as $1:\sqrt{2}:\sqrt{3}:\sqrt{5}:\sqrt{7}$.

§ 7. The *dispersion* of the optical axes in the prismatic system is dependent on the magnitude of the crystallographical axis, with which the *middle axis of elasticity* is coincident.

(A) If the crystallographical axes (d_{2m} being a crystallographical axis, with which coincides the *second median line* d_β, with this the *medial axis* of elasticity being coincident) are to each other as limit of the square roots of *odd* numbers, then for

$$d_\beta > d_{2m} \text{ is } \rho > v,$$
$$d_\beta < d_{2m} \text{ is } \rho < v.$$

(B) If the same axes are to each other as the square roots of *even* numbers to the *odd*, then the law of dispersion becomes the *reverse* of what it was under the first supposition (A).

§ 8. *Hemimorphous* substances are dependent on the *opposite* law as to the dispersion of their optic axes.

§ 9. Whenever, according to Cauchy, the index of refraction $\mu=A+\frac{B}{\lambda^2}$ (A being the coefficient of refraction and B the coefficient of dispersion), the relation between density D of the substances and the coefficients A and B is expressed by the following formulæ:

$$\int_{\lim D=0}^{D=x} 2A\,dA=\int_{\lim D=0}^{D=x} M\,dD,$$
$$\int_{\lim D=0}^{D=x} dB=\int_{\lim D=0}^{D=x} 2ND\,dD,$$

M and N remaining invariable quantities for every elementary substance. If M is made to signify *specific power of refraction,* and N *specific power of dispersion,* their values may be found out by means of the following equations:—

$$\frac{A^2-1}{D}=M,\quad \frac{B}{D^2}=N.$$

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* The totality of the substances belonging to the prismatic system, as far as they have been hitherto objects of optical investigation, may stand in proof and as exemplifications of the propositions enounced here.

§ 10. The consequence from § 9 is, that the density of the *ether* may be set in proportion to the density of the *substances*[*].

§ 11. Not the *elasticity*, but rather the *density* is subject to variation (Fresnel's theory).

§ 12. The consequence of § 9 is, that the *propagation* of *light* may be equally conceived as being independent of the *luminous ether*, and only in dependence on the *substantial molecules*.

§ 13. If Fresnel's formula is derived from the principle of conservation of *vis viva*, and $\frac{A^2-1}{D}$ is substituted, a formula similar to Cauchy's in structure is thus obtained.

§ 14. In consequence of § 9, it appears possible to calculate the *density* in the three dimensions of any crystal, and to bring this new moment in connexion with the rest of the physical properties.

§ 15. The *densities* being proportional to the *masses*, and these to the *distance r* of the *molecules*, the coefficient of *dispersion* must be subject to the general law of *gravitation*, and it would be admissible to write

$$\frac{B}{r^2}=N.$$

It must be remarked that, in consequence of the more or less relative mobility of the molecules, N must have a different value for different substances.

§ 16. The elementary substances, according to Boedecker's theory, not entering into mutual combination with the same density and correlation of atoms as they possess in the state of liberty, it may be admitted that they are probably compelled to form *double molecules*, or *semi-molecules*. In the second case of combination the *specific power* of *refraction* would be the *double*, in the *first* only the *half* of what it was in the elementary substance, when still chemically free.

§ 17. The indices of *refraction* of *chemical* combinations and mixtures may be calculated on the supposition enounced in § 16. Absolute exactitude may be obtained for any extensive groups, of which some members, and consequently the law of their chemical combination, are known[†].

The general law here is—Substances with nearly equal *powers* of *refraction* may combine without alteration in them. If these powers are very different, and far distant from each other, they tend to become homogeneous, so that the greater power of refraction is *halved*, and the lesser is *doubled* (see § 16).

§ 18. The coefficient of *dispersion* of combined substances[‡] seems

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[*] Calcareous spar, Arragonite; diamond, graphite, coal; water, ice; different varieties of topaz, beryl, apatite, &c.; and all the substances examined by Dale and Gladstone may serve as evidences and exemplifications of the above propositions.

[†] Evidence is afforded by Dulong's, Dale's, and Gladstone's determinations, also by *all* other, especially organic, combinations, on account of the simplicity of their formulæ; also by Deville's, Grailich's, Handl's, and Weiss's determinations concerning mixed substances.

[‡] Our knowledge of the coefficients of dispersion proper to chemical elements being still very defective, it is impossible to bring this proposition to full evidence. Sulphuret of carbon may serve for demonstration, and similar inferences may be deduced from investigations concerning mixtures.

also to be derivable from the principles stated in §§ 9 and 16; as B being dependent on d^2, must change proportionally to the square of molecular condensation (see § 16).

§ 19. In consequence of § 9, the *lines* of Fraunhofer (although the constancy of the lengths of undulation, to which they answer, remains unaltered), when refracting substances are condensed, must advance their *centre* toward the *violet* end, in analogy to the phenomena of *absorption*, observed by Weiss (Poggendorff's 'Annalen,' 1861). For this reason *absorption* must depend on the length of the undulation.

These propositions, and some others of similar nature which I succeeded in deriving, incomplete as they still may be in many respects, prove at all events that, even in this department of human knowledge, theoretical points of departure exist from which, by progressive investigation, the real connexion of facts may possibly be traced. My next purpose is to coordinate as much as possible whatever is at present known of facts, bringing them under general points of view as materials for a future scientific theory. I intend to publish by and by these investigations as completely as possible, in constant connexion with the phenomena which they are suited to explain, never leaving the secure foundation of experimental research.

Here I intend only to offer a brief abstract of the facts obtained through the first two series of investigations, published in the 'Proceedings' of the Vienna Imperial Academy (vols. xli. and xlii.).

These investigations took place in the Imperial Physical Institute of Vienna, and could only be effectually accomplished through the liberal and kind assistance I received from the Director of the Institute, from the Director of the Imperial Museum of Mineralogy, and from the Superintendent of the Chemical Laboratory of the Imperial Geological Institute.

In these results are comprised—the exponent of refraction, the calculated axial angle, the aperture of the cone of internal conical refraction, the measure of the apparent axial angle, and crystallographical investigations.

For brevity's sake I will only communicate the exponent of refraction for Fraunhofer's lines B and H, and the most important among the crystallographical determinations.

1. Diamond. C.

$\mu_B = 2{\cdot}46062$ $\mu_H = 2{\cdot}51425$

2. Mellite. $Al_2 O_3, 3C_4 O_3 + 18HO$.

$\omega_B = 1{\cdot}53450$ $\epsilon_B = 1{\cdot}50785$

$\omega_H = 1{\cdot}56113$ $\epsilon_H = 1{\cdot}52769$

3. Acetate of Ammonia and Oxide of Uranium.

$[AmO, \bar{A} + 2(Ur_2 O_3, \bar{A}) + 6HO.]$ Pyramidal.

$\omega = 1{\cdot}47538$ $\epsilon_B = 1{\cdot}48770$

$\omega_H = 1{\cdot}50687$ $\varepsilon_H = 1{\cdot}51974$

4. Chloride of Potassium and Cadmium. [$2\,KCl+Cd\,Cl$.]

Rhombohedral. $a:c=1:1{\cdot}6483$.

$\omega_B=1{\cdot}58409$ $\epsilon_B=1{\cdot}58420$
$\omega_H=1{\cdot}62083$ $\epsilon_H=1{\cdot}62100$

5. Chloride of Ammonium and Cadmium. [$2\,NH_4\,Cl+Cd\,Cl$.]

Rhombohedral. $a:c=1:1{\cdot}5704$.

$\omega_B=1{\cdot}59581$ $\epsilon_B=1{\cdot}59610$
$\omega_H=1{\cdot}64142$ $\epsilon_H=1{\cdot}64180$

6. Nitrate of Soda. NaO, NO_5.

Rhombohedral. $a:c=1:1{\cdot}1903$.

$\omega_B=1{\cdot}57933$ $\epsilon_B=1{\cdot}33456$
$\omega_H=1{\cdot}62598$ $\epsilon_H=1{\cdot}34395$

7. Nitrate of Potash. KO, NO_5.

$\alpha_B=1{\cdot}49939$ $\beta_B=1{\cdot}49881$ $\gamma_B=1{\cdot}33277$
$\alpha_H=1{\cdot}54045$ $\beta_H=1{\cdot}53848$ $\gamma_H=1{\cdot}34359$

8. Citric Acid. $3(C_4\,H_2\,O_4, HO)+HO$.

$\alpha_B=1{\cdot}50542$ $\beta_B=1{\cdot}49432$ $\gamma_B=1{\cdot}48964$
$\alpha_H=1{\cdot}52541$ $\beta_H=1{\cdot}51398$ $\gamma_H=1{\cdot}50978$

9. Sulphur. S. Prismatic. $a:b:c=1:0{\cdot}5264:0{\cdot}4279$.

$\alpha_B=2{\cdot}22145$ $\beta_B=2{\cdot}02098$ $\gamma_B=1{\cdot}93651$
$\alpha_H=2{\cdot}32967$ $\beta_H=2{\cdot}11721$ $\gamma_H=2{\cdot}01704$

10. Quartz. SiO_2 [for control].

$\omega_B=1{\cdot}54106$ $\varepsilon_B=1{\cdot}55012$
$\omega_H=1{\cdot}55806$ $\epsilon_H=1{\cdot}56758$

11. Anatase. TiO_2.

$\omega_B=2{\cdot}51118$ $\epsilon_B=2{\cdot}47596$
$\omega_H=2{\cdot}64967$ $\epsilon_H=2{\cdot}58062$

12. Apatite. From Tumilla.

$\omega_B=1{\cdot}63463$ $\epsilon_B=1{\cdot}63053$
$\omega_H=1{\cdot}65934$ $\epsilon_H=1{\cdot}65260$

13. Beryl. From Elba.

$\omega_B=1{\cdot}57028$ $\epsilon_B=1{\cdot}56540$
$\omega_H=1{\cdot}58884$ $\epsilon_H=1{\cdot}58261$

From Graõ Mogor, Brazil.

$\omega_B=1{\cdot}57762$ $\epsilon_B=1{\cdot}57148$
$\omega_H=1{\cdot}60321$ $\epsilon_H=1{\cdot}59542$

From Nertschinsk.

$\omega_B=1{\cdot}56630$ $\epsilon_B=1{\cdot}56165$
$\omega_H=1{\cdot}58818$ $\epsilon_H=1{\cdot}58393$

14\. Carbonate of Lead. Cerussite. PbO, CO_2.

$\alpha_B=2{\cdot}06131 \quad \beta_B=2{\cdot}05954 \quad \gamma_B=1{\cdot}79148$

$\alpha_H=2{\cdot}15614 \quad \beta_H=2{\cdot}15487 \quad \gamma_H=1{\cdot}86329$

15\. Formiate of Baryta. $BaO\ Fo\ O_3$.

Prismatic. $a:b:c=1:0{\cdot}8638:0{\cdot}7650$.

$\alpha_B=1{\cdot}63098 \quad \beta_B=1{\cdot}59181 \quad \gamma_B=1{\cdot}56788$

$\alpha_H=1{\cdot}66047 \quad \beta_H=1{\cdot}62176 \quad \gamma_H=1{\cdot}59643$

16\. Formiate of Lime. $CaO\ Fo\ O_3$.

Prismatic. $a:b:c=1:0{\cdot}7599:0{\cdot}4671$.

$\alpha_B=1{\cdot}57314 \quad \beta_B=1{\cdot}50997 \quad \gamma_B=1{\cdot}50669$

$\alpha_H=1{\cdot}59851 \quad \beta_H=1{\cdot}52971 \quad \gamma_H=1{\cdot}52577$

17\. Formiate of Strontia. $SrO, Fo\ O_3+2HO$.

Prismatic. $a:b:c=1:0{\cdot}9477:0{\cdot}8922$.

$\alpha_B=1{\cdot}53421 \quad \beta_B=1{\cdot}51743 \quad \gamma_B=1{\cdot}48057$

$\alpha_H=1{\cdot}55624 \quad \beta_H=1{\cdot}53769 \quad \gamma_H=1{\cdot}49899$

18\. Bimalate of Lime. $CaO, 2\bar{M}+9HO$.

Prismatic. $a:b:c=1:0{\cdot}9477:0{\cdot}8922$.

$\alpha_B=1{\cdot}54037 \quad \beta_B=1{\cdot}50293 \quad \gamma_B=1{\cdot}48873$

$\alpha_H=1{\cdot}56500 \quad \beta_H=1{\cdot}52564 \quad \gamma_H=1{\cdot}51192$

19\. Ferrocyanide of Potassium. $3KCy+Fe_2\,Cy_3$.

Prismatic. $a:b:c=1:0{\cdot}7725:0{\cdot}6220$.

$\alpha_B=1{\cdot}57586 \quad \beta_B=1{\cdot}56151 \quad \gamma_B=1{\cdot}55913$

[for D] $\alpha_D=1{\cdot}58306 \quad \beta_D=1{\cdot}56888 \quad \gamma_D=1{\cdot}56596$

20\. Asparagine. $HO, C_8\,H_7\,N_2\,O_5+2HO$.

Prismatic. $a:b:c=1:0{\cdot}8327:0{\cdot}4737$.

$\alpha_B=1{\cdot}61392 \quad \beta_B=1{\cdot}57517 \quad \gamma_B=1{\cdot}54380$

$\alpha_H=1{\cdot}64221 \quad \beta_H=1{\cdot}60194 \quad \gamma_H=1{\cdot}56538$

"Liquid Diffusion applied to Analysis." By Thomas Graham, Esq., F.R.S., Master of the Mint. (This paper is printed in full in the March, April, and May Numbers of this Magazine.)

"On some new Phenomena of Residuary Charge, and the Law of Exploding Distance of Electrical Accumulation on Coated Glass." By Sir W. Snow Harris, F.R.S.

A main object of this paper is to prove that residuary charge in the Leyden jar, subsequent to explosive discharge through an external interrupted circuit, as in the case of discharge by a Lane's electrometer, is not the result of a spreading of the charge upon the uncoated part of the glass, or of penetration within its substance, but arises from an undischarged portion of the accumulation left as it were behind, and still existing in precisely the same way and under the same conditions as the original charge.

The author introduces his subject with sundry observations on Lane's discharging electrometer, and the law of explosive discharge, and adverts to the fact recorded by Nicholson in the Royal Society's Transactions for 1789, that "although in moderate charges the exploding distance appears exactly, or very nearly, proportionate to the charge itself, yet for high intensity, the distance to which the charge is carried exceeds that proportion:" this the author finds to be the case generally, and quotes an experimental example showing the amount of deviation from Lane's law in that particular instance. He further shows, that in order to obtain explosive discharges at the increased distances agreeing with the calculated number of measures, the distances must be slightly increased by certain small quantities.

The probable sources of these differences are now adverted to, and the common objections to Lane's discharging electrometer considered. A new and improved form of this instrument is figured and described. One of its principal advantages is a means of changing the exploding points of the discharging balls, which are moveable on axial centres, so as to bring a new point of the circumference into play, should abrasion or any other defect arise in the existing exploding point. The author endeavours to show that the apparent irregularities so frequently observed in the striking distance of a charged electrical jar, do not arise from any defect in the quantity measure, or in the exploding electrometer when properly constructed, but are altogether dependent on some peculiar conditions of electrical accumulation on coated glass.

One remarkable peculiarity of the electrical jar, is a disposition to retain a portion of the charge notwithstanding explosive discharge has occurred through a discharging circuit; we do not discharge the whole accumulation; a portion is, as it were, left behind. The fact itself is undisputed, but the cause or theoretical explanation does not appear to have been very clearly comprehended.

The author here introduces some interesting quotations from certain unpublished manuscripts of Mr. Cavendish, who investigated so long since as the years 1771 and 1772, what he terms the "charges of plate glass and other electrical substances coated in the manner of Leyden phials." Mr. Cavendish found his experimental inquiries greatly embarrassed by the "spreading of the electricity" on the glass; it is, he says, faster on some kinds of glass than on others; besides the slow and gradual spreading, he observed an instantaneous spreading, visible in the dark, and extending to about ·07 of an inch beyond the edge of the coating upon glass ·2 of an inch thick, and about ·09 upon glass $\frac{1}{15}$th of an inch thick. Another source of inconvenience, observes Mr. Cavendish, arises from a certain amount of penetration of the charge into the substance of the glass itself, equal to about $\frac{7}{16}$ths of its thickness; the space, he says, within which the charge cannot penetrate is not above $\frac{1}{5}$th of the thickness, from which he concludes that the charge of a coated electric will be different in cases in which this penetration of the charge into the substance of the glass varies, and infers that different electrics are susceptible of different degrees of charge. He examined plates of glass of various kinds, as also of gum-lac, rosin, bees-wax, &c., and found the capacity of these substances for electrical charge different—phenomena recently explained by Faraday's fine discovery of Specific Inductive Capacity.

This last celebrated philosopher also recognizes the penetration or infiltration of electrical charge within the substance of coated and charged electrics, and attributes it to a certain amount of conducting power in the electrical substance. All substances, he infers, are conductors of electricity in a greater or less degree, and thus admit of infiltration of charge through their substance. In the case of charged electrics, the infiltrated electricity subsequently returns upon its path, and hence residuary charge.

The author has no disposition to question the experimental results arrived at by either of these eminent men, but is of opinion that they apply to a different case of electrical force than that of secondary and immediate discharge, supervening upon a primary discharge of an electrical jar through an external explosive circuit, which he thinks can neither be referred to any previous spreading of the charge upon the glass, or to any penetration of it into its substance, or to return action as described by Faraday. He has found that of 100 measures of accumulated charge on a jar with imperfect conducting coatings, no less than 75 measures, or three-fourths nearly of the whole accumulation, has been left behind after explosive discharge. In a jar coated with water full 14 measures out of 100, or about one-seventh, was left undischarged. He thinks it difficult to reconcile such an amount of residuary charge as this, with any spreading of the electricity on the glass, or any possible amount of penetration into its substance.

Although the deductions of Cavendish and Faraday may not be found to apply as solutions of the interesting problem of residuary charge, they still find their application in other cases, as in the case of the facts noticed by Nicholson already detailed. The intensity of explosive discharge may apparently become increased by a penetration of the exploding electricity into the air separating the balls of the discharging electrometer, in which case the measured distances of discharge, according to Cavendish, would, for given measured quantities of electricity, continually decrease, and discharge at the measured distances between the exploding balls would appear to happen prematurely. It is now shown by reference to a Table of experimental results, that at distances 1, 2, 3, 4, taken in tenths of an inch, with quantities of measured charge also as 1, 2, 3, 4, the actual distances of explosion are nearly as ·1, ·214, ·325, ·445. The author hence infers that, supposing the penetration of the first measures very small and not of much value, the penetration of the succeeding measures may be taken as ·014, ·025, ·045, that is ·007, ·0125, ·0225 upon each of the opposed exploding points, taking the surfaces of the exploding balls as curvilinear coatings to the intervening air.

If any considerable spreading of the charge upon the uncoated glass should arise, that, as remarked by Cavendish, would be equivalent to an increase of the coating, and hence the tension due to a given quantity of charge would be less. The effect would be greater on the first measured quantity than on succeeding quantities; hence for explosion at a first distance, an additional two or three measures might be required, which, as the spread upon the glass became satisfied, might not be requisite in the same proportion upon succeeding measured distances, in which case discharge would ensue with a less number of measures than calculation determines according to Lane's law, making it appear as if, according to Nicholson, "the intensity ran before the quantity."

Both Franklin and Nicholson have taken a sound practical and theoretical view of electrical accumulation on coated glass, which the author conceives to depend on a play of opposite electrical forces, either directly through the glass intermediate between the coatings, or through the medium of an external circuit, or both. He considers the terms "free" and "compensated," or "latent" electricity, perfectly admissible when correctly applied and limited by sound definition. All the accumulated charge, up to the exploding point, is evidently not sensible to the electrometer, and he thinks it convenient to distinguish between that portion of the charge of which the electrometer directly says nothing, and that portion to which its indications are more immediately referable, more especially as these two, or conjugate portions, have important relations to each other. Thus a double measured charge has twice the amount of free charge; and the free charge, as estimated by attractive force, is as the square of the accumulation. When the free charge explodes, the whole accumulation, or nearly all, goes with it, at least in common cases of metallic coated glass, and according to Nicholson carries it through distances proportionate to the charge itself: the terms "free" and "latent" electricity, or, as the French have it, "electricité dissimulée," may not be exact or admissible, if meant to imply a difference in kind or mode of action of electrical force, but they are by no means objectionable when denoting different amounts of the same force in this or that direction.

In considering the nature of electrical accumulation on coated electrics and the law of explosive discharge, we have to deal with a simple question of physical force taken in the abstract, and not with a theoretical electric fluid or fluids of high elasticity, subject to expansion or contraction, changes in thickness of stratum, tension, density, and the like. The terms "tension" and "intensity," so commonly applied to designate degrees of electrical force, are convenient and not inappropriate terms when legitimately applied and limited by definition. The term *intensity* is well adapted to express the attractive force of the charge in the direction of the electrometer, and which, in continually *increasing* according to a known law, terminates in explosion. The intensity or attractive force varies with the square of the charge. The term "tension" is more especially applicable to the constrained state of the dialectric particles sustaining the induction necessary to the charge, and is equivalent to the reactive force of the particles in an interrupted circuit of discharge to break down or reverse the polarized state of the dialectric medium impeding discharge, as between the exploding balls of the Lane's discharger: this is as the quantity of charge directly. In employing these terms, the author has not the least view to any specific changes in the quality or condition of the accumulated electricity, as relating to density, elasticity, and such like. Whether the tension and intensity of a charge, as evidenced by the electrometer, be great or little, he conceives that the nature of the force and its mode of operation remains the same. Viewing the process of electrical accumulation and discharge in the Leyden jar as the result of certain powers or forces operating either immediately through the glass or through an external circuit, or both, we may readily imagine that at the critical point at which the forces in the two directions become balanced, and at which point the equilibrium of charge is, at it were, overset on the side of the exterior circuit, then it is that residual charge ensues, either by a momentary revulsion of force between the coatings in the direction of the intervening glass, frequently causing fracture, or otherwise by a retention of some of the charge in that direction at the instant of explosion. Some instructive and important experiments by Mr. T. Howldy are here quoted in support of this conclusion, from the pages of the 'Philosophical Magazine' for the year 1815. A ruptured jar had the coatings removed from around the perforated part, so as to admit of the jar receiving a given amount of charge. When explosive discharge took place in the usual way, a spark was observed to pass at the same instant between the coatings through the perforation in the glass, evidently showing an exertion of force in that direction. This spark is entirely independent of the discharge in the circuit, the force of which remains the same as if no such perforation existed, as Priestley and other electricians, and Mr. Howldy himself, have fully demonstrated.

Considering the question of residual charge as bearing materially on our views of the nature of electrical force, the author seeks to investigate, by new forms and kinds of experiment, the relation of the residual quantity to the whole charge, whether accumulated on glass coated with very perfect conductors such as the metals, or otherwise with less perfect conductors, as water, or with imperfect conductors, such as paper, linen and the like. The instruments employed are now enumerated and commented on, and their experimental arrangement figured and described. They consist of the electrical or Leyden jar; Lane's improved electrometer; the hydrostatic electrometer as recently perfected; the thermo-electrometer; quantity or unit-measure; and battery charger and discharger. The following is the course which the author pursued in his inquiries, through the medium of these instruments.

The quantity of charge being given, its intensity is measured by the hydrostatic electrometer in terms of attractive force at a constant distance, suppose at distance 1 inch. This is first noted: the jar is now discharged through its exploding distance by completing the circuit through the Lane's discharger. The hydrostatic electrometer, being now made perfectly neutral, is again brought into connexion with the inner coating of the jar. The intensity or attractive force of the residuary or remaining charge is now noted, but as this force is necessarily small, it is taken with the attracting plates at a diminished distance from ·1 to ·3 of an inch or more, as the case may require, and subsequently reduced to the standard distance of one inch, taking the force to vary, as demonstrable by the electrometer, as $\frac{1}{D^2}$. This being determined, the relative quantities of electricity in the full charge and the residual charge will be as the square roots of the respective attractive forces or intensities; the total force, as also demonstrable by the instrument, being as the square of the accumulation. Let, for example, the quantity of charge communicated to the jar be 100 measures, and the attractive force, or intensity at distance one inch, be 144 degrees, and suppose intensity of residual force at the same distance $=\cdot 08$. In this case we have the simple proportion 100 measures : x measures $:: \sqrt{144} : \sqrt{\cdot 08} :: 12 : \cdot 283$ and quantity of residual electricity $=\frac{100 \times \cdot 283}{12}= 2\cdot 35$ measures nearly; so that of the original 100 measures of charge communicated to the jar, rather more than $\frac{1}{40}$th remains undischarged in this case.

The author here offers some explanatory observations on the relative dimensions and extent of coating of the unit of measure and the relative value of the measures quoted, and he thinks if electricians would agree to recognize a standard instrument of this description, it would be attended with very considerable advantage, as in the case of other standard instruments. The unit of measure he employs exposes about 9 square inches of coating; it is about 4 inches long, ·8 of an inch in diameter, and $\frac{1}{20}$th of an inch thick; distance of exploding balls ·05 of an inch. Similar observations were applied to the thermo-electrometer, the ball of which is 4 inches in diameter, and has a wire of platinum through it of ·01 of an inch in diameter. The dimensions of the attracting discs of the hydrostatic electrometer are also noted, which in these experiments were 4 inches in diameter; the suspended disc weighs 82 grains. The discs are carefully gilded; 5 degrees of the arc of measure represents a force of 1 grain, that is to say, a weight of 1 grain added to either side moves the index 5 degrees of the scale. Having offered these preliminary remarks, the author proceeds to the following experiments:—

Experiment 1.—Variable charges, amounting to 50, 100, 150 measures, were successively accumulated on different jars, exposing from two to six square feet of coating, and the residual charges due to each noted; these were found to be as the total charge. Thus the residual charge for 100 measures was in every case double that for 50 measures.

In a succeeding Table are noted—measured charge; exploding distance; intensity at distance 1 inch; residual measures and thermo-electric effect of discharge. It appears by this Table that residual charge is as the total charge; exploding distances, as the quantity or very nearly; intensities and thermo-electric effect of discharge as square of the quantity or number of measure accumulated.

The author finds that for every metal-coated jar, whether large or small, of thick or thin glass, exposing from 1·5 to 6 feet of coating, the residual charge or quantity left undischarged, varies between the limits of $\frac{1}{30}$th and $\frac{1}{50}$th of the total charge.

Experiment 2 investigates the effect of thickness of glass. Two jars, exposing 2·5 square feet of coating, were employed, their relative thickness being as 1 : 2, that is, $\frac{3}{20}$ths and $\frac{6}{20}$ths of an inch; 100 measures were accumulated and discharged at their respective exploding distances. The following results appeared:—exploding distance directly as thickness of glass; intensity or attractive force in direction of electrometer as square of the thickness; residuary charge in each case the same, being about $\frac{1}{45}$th part of the total charge; thermo-electric effect of discharge very nearly the same; so that whether discharged from thick glass or thin, under intensities of very different degrees the same quantity of electricity produces the same effect. The intensities in this case were as 4 : 1, yet the thermo-electric effect did not differ more than one or two degrees, one being 12°, the other 13°. The author finds, by numerous experiments on a series of jars, that the intensity indication has no influence on the force of discharge, the quantity discharged being the same. In a series of jars of different magnitudes, and in which the intensity of a given charge of 100 measures varied between the limits of 100 and 1000 degrees, there did not appear a difference of more than a few degrees amongst the whole; the effects varied between 8 and 11 degrees. Some little difference will generally arise in favour of electricity accumulated on a small area of coated glass; in consequence of the greater facility of discharge the accumulation has greater freedom of operation through the external circuit, as is shown by its greater effect on the electrometer.

A celebrated electrician, the late Mr. Brooke of Norwich, in a conference with Cuthbertson about the year 1800, stated that a Leyden jar coated with strips of metal $\frac{3}{4}$ths of an inch wide, leaving intervals of the same width between the strips, was equally efficient as a full coating in the ordinary way. Two equal and similar jars, about 1 foot in diameter and 19 inches high, were prepared accordingly; one fully coated to about 4 square feet, the other coated in strips to about 3·5 square feet. The author, although doubting this statement in all its generality, still considered an investigation of it, more especially coming from such men as Brooke and Cuthbertson, desirable, and as being calculated to throw further light on the phenomena of the Leyden jar.

A few preliminary experiments seemed to accord with Mr. Brooke's view; the exploding distance of the two jars with a given charge did not appear extremely different. The accumulated electricity spread upon the glass between the strips of metal, and thus enabled the partially coated jar to receive a larger accumulation, upon the principle stated by Cavendish, than was really due to its extent of actual coating. Mr. Brooke, in the then state of practical electricity, might have been therefore easily led to imagine that a partial coating such as he describes was sufficient. It is, however, shown in this paper that the cases of the two jars are widely different. As the spread of the electricity becomes satisfied, a less charge is required for explosion, and the tension of a given quantity increases. The following are the results of experiments with 100 measures similar to the preceding:—

Full coating.

Exploding distance	·15	Intensity 100° at 1 inch.
Residual measures	2·45	Therm. electric effect 8°.

Partial coating.

Mean exploding distance ..	·25	Intensity 160°.
Residual measures	4·97	Therm. electric effect 3°·5.

It is evident the two forms of coating are not equally efficient, the heating effect of discharge not being half as great in the partially coated jar, whilst the residual charge is twice as great. The experiment so far shows the spread of electricity on the uncoated glass to be a source of absorption of charge to a greater or less extent, and goes far to confirm the views of Mr. Cavendish, relative to the spreading of electricity on glass.

The phenomena of metal-coated jars having been so far examined, a similar course of experiment is followed with jars coated with less perfect conductors, commencing with water coatings. For this purpose a jar exposing nearly 5 square feet of coating was prepared with metal coating, and the results of a charge of 100 measures determined and noted as before; the metal coating being removed, the same jar had an equal extent of water applied to its opposite surface coating. The method of effecting this is described. The author states that it was so perfect as to shield the experiment from all interference of vapour from the water surface, so that the jar completely retained the charge without any dissipation, and in no sense differed in this respect from a metal-coated jar.

The results of this experiment are not a little remarkable. The exploding distance of the 100 measures, whether with the metal or with the water coating, did not materially differ, except in apparent force, being for the metal ·22, for the water ·2. The exploding spark from the water coating, instead of the sharp ringing sound attendant on the exploding spark from the metal coating, is weak and subdued, and is often like the sound of fired damp gunpowder. The intensity or attractive force is also in each case alike, or very nearly; being for the metal coating 144°, for the water 142°. The residuary charges differed considerably, being for the metal coating about 2·25 measures, or about $\frac{1}{45}$th part of the total charge; for the water coating 14·5 measures, or about the $\frac{1}{7}$th of the total charge. The residuary charge with a water coating is more than six times as great as with a metal coating. The thermo-electric effect with the metal coating was 10°, with the water coating nothing; 200 measures, or double the charge, had no effect on the thermo-electrometer.

In this experiment it does not appear requisite that both the coatings should be water; one coating may be metal, as in the first forms of the electrical jar. The author could not, at least, discover any material difference in the results, and concludes that if the first forms of the electrical jar with an internal coating of water had been continued, we should have had but small experience of the effects of artificial electrical discharge on metallic wires.

Imperfect conducting substances employed as coatings to the electrical jar have very similar but very exaggerated effects. With coatings of paper we have a striking example of retention of charge. A jar exposing 5·5 feet of coated glass, first coated with metal and subsequently with paper, gave the following results under a charge of 100 measures.

Exploding distances, as in the former case, nearly the same, being ·23 and ·25; attractive forces or intensity also nearly the same, being 158° and 160°; residual measures with the metal coating 2·5 measures, or about the $\frac{1}{40}$th of the total charge; with paper coating, in some experiments 80 measures, or about $\frac{8}{10}$ths of the total charge, so that the residual charges with metal and paper are as 1 : 32. Thermoelectric effect for metal coating 8°, for paper coating nothing. It appears from these and similar experiments, that the interposition of imperfect conductors between the coating and the glass of the Leyden jar must necessarily impair its efficiency, and change its electrical indications, especially when of any considerable thickness. Three turns of common linen interposed between the outer coating and the glass reduced the force of discharge from 11° to 6°, nearly one-half, whilst the residuary or retention of charge is considerably increased: this question, as bearing in some degree on the retention of charge by the electric cable, may not be undeserving of further investigation.

GEOLOGICAL SOCIETY.

[Continued from p. 414.]

March 19, 1862.—Prof. A. C. Ramsay, President, in the Chair.

The following communications were read:—

1. "On the Sandstones, and their associated deposits, in the Valley of the Eden, the Cumberland Plain, and the South-east of Dumfriesshire." By Prof. R. Harkness, F.R.S., F.G.S.

Having defined the area occupied by these sandstones, breccias, clays, and flagstones, and referred to the published memoirs in which some notices of these deposits have been given by Buckland, Sedgwick, Phillips, and Binney, the author described, 1st, a section near Kirkby-Stephen, across the vale of the Eden, where two breccias, separated by sandy clay-beds, underlie sandstones of considerable thickness; 2ndly, a section across Eden Vale from Great Ormside to Roman Fell, in which the breccias, associated with sandstones, form a mass 2000 feet thick, and are succeeded by thin sandstones, shales (with fossils), and thin limestone, altogether about 160 feet, and next by sandstones 700 feet thick. This is the typical section; the fossiliferous shales are regarded by Prof. Harkness as equivalent to the Permian Marl-slate of Durham; they contain (at Hilton Beck) remains of *Coniferæ*, *Neuropteris*, *Sphenopteris*, *Weissites* (?), *Caulerpites selaginoides* (?), *Cupressites Ullmani* (?), *Voltzia Phillipsii* (?), *Cyathocrinus ramosus,* and *Terebratula elongata.* The breccias and sandstone beneath, previously recognized as Permian, are here referred to the Rothliegende; and the sandstones above are regarded as belonging to the Trias. Detailed descriptions of the sandstones and breccias in the country between Great Ormside and Penrith were then given, and the gypseous character of the clays at Long Martin and Townsend noticed. In the section across the vale of the Eden from the west of Penrith to Hartside Fell, the Permian breccias, sandstone, and flags are nearly 5000 feet thick, but the clay series is poorly represented. North of Penrith the flagstones bear foot-marks (at Brownrigg) like those of Corncockle Muir. Mr. Harkness next described several sections of these Permian rocks in the western Westmoreland; and traced them to the other side of the Solway Firth, in Dumfriesshire (as described in former papers). Some remarks on the relations of the Permian beds of Cumberland and Westmoreland with those of St. Bee's Head, near Whitehaven, and those of Annandale and Nithdale, concluded the paper.

2. "On the Date of the Last Elevation of the Central Valley of Scotland." By Archibald Geikie, F.R.S.E., F.G.S.

After alluding to the position and nature of the raised beach which, at the height of from 20 to 30 feet above the present high-water-mark, fringes the coast-line of Scotland, the author proceeded to describe the works of art which had been found in it. From their occurrence in beds of elevated silt and sand, containing layers of marine shells, it was evident that the change of level had been effected since the commencement of the human period. The character of the remains likewise proved that the elevation could not be assigned to so ancient a time as the Stone Period of the archæologist. The canoes which had from time to time been exhumed from the upraised deposits of the Clyde at Glasgow clearly showed that, at the time when at least the more finished of them were in use, the natives of this part of Scotland were acquainted with the use of bronze, if not of iron. The remains found in the corresponding beds of the Forth estuary likewise indicated that there had been an upheaval long after the earlier races had settled in the country, and that the movement was subsequent to the employment of iron. From the Firth of Tay similar evidence was adduced to indicate an upheaval possibly as recent as the time of the Roman occupation. The author then cited several antiquaries who from a consideration of the present position of the Roman remains in Scotland had inferred a considerable change in the aspect of the coast-line since the earlier centuries of the Christian era. He pointed out also several circumstances in relation to these Roman relics, which tended to show a change of level, and he referred to the discovery of Roman pottery in a point of the raised beach at Leith. The conclusion to which the evidence led him was that since the first century of our era the central parts of Scotland, from the Clyde to the Forth and the Tay, had risen to a height of from 20 to 25 feet above their present level.

LXIX. *Intelligence and Miscellaneous Articles.*

NOTE ON THE ELECTRICITY DEVELOPED DURING EVAPORATION AND DURING EFFERVESCENCE FROM CHEMICAL ACTION. BY PROFESSOR TAIT AND J. A. WANKLYN, ESQ.

ONE of Professor W. Thomson's divided-ring electrometers having been recently procured for the Natural Philosophy collection in the University, we have made use of it in repeating and extending the experiments of Volta, Pouillet, and others, on the electricity produced during the evaporation of various bodies. In some cases our results agree with those already known, but in others we find effects differing totally in kind or degree from the accepted ones; and with some substances we find occasionally contradictory indications among our own results.

The electrometer is in every respect a far superior instrument to the gold-leaf electroscope, which (sometimes with the addition of a condenser) was used by former experimenters, and enables us to give our results in a form easily reducible to absolute measure. The charge of the instrument was such that, when the half-rings were respectively connected with the zinc and platinum of a single Grove's cell, the deflection observed amounted to about 5·8 scale divisions. This was found to be the most useful charge for the bulk of our experiments, but it was easily increased twenty or thirtyfold when we sought to verify any very delicate indications.

Our apparatus consisted of a platinum dish, placed on an insulating stand, and connected with the insulated half-ring. A lamp could be placed on the stand so as to heat the dish; and while this was going on, the indications of the electrometer gave us the atmospheric charge. The experiments were all conducted when the latter was very small; so that, although the sputtering of the fluids dropped on the hot plate may have prevented us from observing some slight effects, the large deflections we observed in many instances can have nothing to do with the electric state of the air of the room. With a different disposition, which enabled us to use a Bunsen lamp to heat the dish, we obtained the atmospheric potential by burning a little ether or alcohol on the dish itself when the lamp was removed.

We agree generally with previous experimenters, that during the continuance of the spheroidal state there is little, if any, perceptible disengagement of electricity. We also agree with the statement that the main effect is produced while the fizzing sound that accompanies the loss of the spheroidal state is heard, and that during the continuance of the mechanical action to which that sound is due the indications of the electrometer in general steadily increase. That the greater part of the electricity produced is due to friction is proved by the fact that when fluids are forcibly squirted upon the hot dish the electrical indications are very much increased, and that a concave surface gives far more powerful deflections than a convex one at the same temperature. The sputtering or violent boiling which succeeds the fizzing state shows little, if any, disengagement of electricity. The principal interest of the results which we have obtained is in the cases of iodine, bromine, and various other bodies which do not seem to have been before examined. We have as yet met with no discordance in our own results as far as *simple* bodies are concerned.

In giving the following numbers, we have not attempted any correction for the loss of electricity which is caused by the high temperature of the platinum dish.

Mean Electric Effects given by a few substances during the continuance of the fizzing sound which immediately follows the disappearance of the Spheroidal State, 5·8 *representing the Electromotive Force of a single Grove's Element.*

Bromine	+ 400
Iodine	+ 90*
Bromide of ethyle	+ but very small indeed, if any.
Iodide of methyle	In many experiments strong +, but in three cases pretty strong −.
Benzole	No effect.
Valerianic ether	No effect.
Common ether	Very slight and dubious effects.
Chloroform	− if plate very hot, + if colder.
Ammonia	− 200
Alcohol	− 10
Mercury	− 75
Chloride of sulphur	− 100
Water (distilled), containing only a trace of carbonic acid, which was too small to be detected by lime-water	− 80
Solutions in water of—	
Carbonate of potash (strong)	− 310
Caustic soda (strong)	− 40 †
Caustic soda (dilute)	− 25
Caustic potash (combustion strength)	+ 150
Nitric acid (strong)	+ 7·5
Nitric acid (1 in 4 of water)	− 35
Hydrochloric acid (strong)	− 160
Hydrochloric acid (weak)	− 50
Sulphuric acid (strong)	+ 15
Strong solution of Na Cl	− 400
Strong solution of KI	− 80
Strong solution of CuO, SO^3	−1000 ?
Solution of double oxalate of chromium and potash	Very trifling effect.
$Fe^2 Cl^3$, solution moderate	Negative effect.
Acetic acid (monohydrate)	+ 3
Acetic anhydride	− 9

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* This sample was in fine crystals. Far higher effects (also positive) were obtained from it in powder.

† This is a very difficult substance to experiment upon.

The sulphate-of-copper solution is by far the most remarkable that we have tried. The smallest globule, on leaving the spheroidal state, gave intense effects, sending the lamp-image entirely off the scale.

We have also commenced a set of experiments with a view to test the electricity developed during the brisk disengagement of a gas by chemical action, which was discovered eighty years ago by Volta. In some of these experiments it was observed that when the gases were disengaged with considerable effervescence, and in a mass of large bubbles foaming over the platinum crucible in which the experiment was conducted, the bursting of each bubble was attended by a simultaneous increase of deflection in the electrometer. These experiments are as yet exceedingly imperfect, but they seem, like the preceding, to indicate friction as a main cause of the observed results. The effects on the electrometer are by no means so uniform, either as to kind or quantity of electricity, as those given by evaporation.

Electricity developed during Effervescence.

$Zn+HCl$	-750
$Zn+NO^5 HO$	$+175$. In another trial -120.
MnO^2+HCl..........	-150
CaO, CO^2+HCl..........	Trifling effects.
NaO, SO^2+HCl..........	At first a small negative deflection, finally $+50$.
$NaCl+SO^3 HO$	$+10$

—*From the Proceedings of the Royal Society of Edinburgh*, February 1862.

POSTSCRIPT TO PAPER "ON CHINESE ASTRONOMICAL EPOCHS."

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

In my paper "On Chinese Astronomical Epochs" which you published in your Number for January last, I substituted in a formula the English measure of 12 inches for the foot, whereas I ought to have used 10 only, the French measure. In consequence of this, the 0·25 at the top of p. 3 ought to be changed into 0·30. The effect of this is that the range of time within which the Chinese observations may have been made is 45 centuries, instead of 37½; and therefore the uncertainty arising from the ill-defined termination of the shadow of the gnomon is still greater than I make it in that paper. I shall be much obliged by your inserting this letter in your next Number after receiving it.

I take this opportunity of asking you to make two more corrections; viz.

Page 3, last line but one, *for* Nos. 4 and 5 *read* Nos. 5 and 6.
— 6, line 12 from the bottom, *for* Nos. 2 and 4 *read* No. 4.

I am, Gentlemen,
Yours faithfully,
J. H. PRATT.

Calcutta, April 1, 1862.

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

SUPPLEMENT TO VOL. XXIII. FOURTH SERIES.

LXX. *An Account of Observations on Solar Radiation.*
By JOHN JAMES WATERSTON, *Esq.*[*]

[With a Plate.]

§ 1. IN March of last year I submitted to the Society some computations with reference to the sun's heat, and suggesting a mode of deducing the potential temperature of its radiating surface. This last summer I have endeavoured to put this method to the proof by a series of observations on solar radiation, supplemented with experiments on the rate of cooling of thermometers in air and *in vacuo* with different kinds of radiating surfaces,—also by another series, applying the method of deducing the potential temperature of a radiating surface to predicate the temperature of one that is maintained at a constant *known* temperature.

The success of these last mentioned, so far as they have as yet been carried, encourages me to lay before the Society the observations on solar radiation, with an account of the method employed to obtain the results, and of the mode of reducing them to a vacuum.

§ 2. It will be remarked, on inspecting the Chart in which the observations are projected[†], that a simple law of atmospheric absorption is indicated, which, if confirmed by other similar observations in different climates, would perhaps lead to more exact ideas of the influence of the atmosphere on the sun's rays. Unfortunately the best part of the summer had passed before I could begin to observe, and there was almost constant interruption with clouds and unsettled weather. In a tropical station,

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[*] From the Monthly Notices of the Royal Astronomical Society with Notes; communicated by the Author.

[†] This chart is omitted, and another substituted (fig. 3, Plate V.), showing the observations of August 21, also of August 6 and July 28, projected in the same way to a larger scale. Each observation is represented by a point, the coordinates of which are the reciprocal of the radiation and secant of apparent zenith distance of the sun's centre.

where the sun rules in a cloudless sky, the presumed law might soon be put to the test, and the heating-power of the sun's rays before entering the atmosphere ascertained with precision. Having determined this for the earth's mean distance from the sun, its value for any other planetary distance may be deduced by the law of the inverse square.

§ 3. When a thermometer is exposed to the sun with its bulb blackened, it is presumed to absorb all the heat that impinges on a plane surface equal to the transverse section of the bulb; it rises and is maintained at a certain temperature; and when this balanced condition is attained, we can with certainty assert that the amount that issues from the bulb is precisely equal to the quantity that enters. The elevation of its temperature above surrounding bodies due to the sun's radiant power (which is denoted by the symbol r) would be an *exact measure* of that power if no heat issued from it except by radiation, and if the rate at which heat was emitted from it increased exactly in proportion to r. Now I find that if the bulb of a thermometer is enclosed in a vacuum, the walls of which are brass coated with lampblack, the rate at which it cools is *exactly* proportional to the value of r, and this rate has exactly the same value whether the glass bulb is uncoated or coated with lampblack*. When enclosed in air, the rate of cooling increases faster than r. The mode of measuring the rate in both cases, and of reducing the values of r observed in air to what they would be in a vacuum, are described at the end of this paper.

§ 4. The instrument employed was designed so that the thermometer exposed to the sun's rays should radiate against an enclosing metallic surface coated with lampblack, and so that the temperature of that surface should always be known.

Fig. 1, Plate V., with the description that accompanies it, gives the details. The rays of the sun were admitted, to strike upon the bulb of the thermometer X, through a hole but little larger than its diameter, and were entirely screened off the brass tube, against the blackened inner surface of which the radiation of the bulb took place. The thickness of the sides of the brass tube was $\frac{1}{4}$ inch; and the thermometer Y, that indicated its temperature, was lodged in a hole cut in the upper side. The

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* This unexpected result is confirmed by repeated observations. While the emitting-power of a mercurial thermometer is thus the same, coated or uncoated, the absorbing-power of the uncoated surface is only six-tenths of the same coated with lampblack. This apparent anomaly is no doubt caused by the incident rays of heat, after passing through the glass, being reflected from the surface of the mercury, because I find that, with spirit-thermometers (in which the spirit is largely impregnated with red vegetable matter), the uncoated reservoir absorbs radiant heat exactly the same as when coated with lampblack.

circumference of the bulb touched the brass, and its upper side was enclosed with cork, while the lower was exposed to the air within the tube, but was untouched by the rays of the sun that passed through. The internal diameter of the tube was 0·9 inch, and length 6 inches. The bulb of the solar thermometer was 0·42 inch in diameter, spherical in shape, and fixed in the centre, as shown in the figure. Its shadow was an easy guide in moving the tube in altitude and azimuth to keep pace with the sun. It is difficult, if not impossible, to demonstrate that the thermometer Y shows the exact temperature of the inner surface of the tube. It was subjected to three tests.

1. The instrument being out of the sun's rays, and X and Y showing the same temperature, it was removed to a place where the atmospheric temperature was 10 degrees lower. Both thermometers descended, and showed a difference equal to about one-tenth the amount they had to fall to arrive at the atmospheric temperature, Y being so much in advance of X.

2. A bat's-wing flame of gas was brought within 3 inches fronting the middle of the tube; both X and Y rose together, keeping pace exactly.

3. While taking observations, the heat absorbed by X from the sun, and again emitted from it and transferred to the tube, gradually raised its temperature until a maximum was obtained. Now, comparing X and Y while both are rising, and after having obtained their maximum, a difference of 0°·3 was remarked; and this difference, no doubt, affected isolated observations when this maximum was not attained in consequence of interruption by clouds passing, when it was usual to heat the solar thermometer artificially to near the stationary point, in order to save time, the great inconvenience of the apparatus in this climate being the slowness with which r obtained its final value. An arrangement with a differential air-thermometer would, no doubt, be preferable in this respect, but the absolute value of the degrees indicated does not seem capable of being exactly determined*.

The thermometers were carefully graduated and compared by myself, and the divisions between two fixed points, 60° and 100° (which included all the observed temperatures), were drawn as nearly equal as a good ivory scale and magnifying lens would admit. The length of a degree on the scale of Y was about ·05 inch, and upon X ·067 inch: with practised eye it was easy to read off the temperature to $\frac{1}{20}$th of a degree with lens; but such accuracy was unattainable for other reasons, and chiefly

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* Another mode, which is perhaps the least liable to uncertainty, is to have the surface against which the sun-thermometer radiates maintained at 212° by surrounding it with the steam from boiling water: only one thermometer would be required in this arrangement.

a sensible difference was caused by the varying amount of the stem that was under the influence of the sun's rays as it moved.

§ 5. The observations taken on the morning of the 21st of August, continuously during $2\frac{1}{2}$ hours of uninterrupted sunshine, were graphically equalized, the curve drawn, and ordinates measured off at every 20 minutes. This was the only opportunity that occurred of continuous observation between such favourable limits of altitude as to indicate the direction of a line with some precision.

§ 6. In the Table of observations given in the Appendix*, the date and apparent time are given in the first two columns. The timepiece was regulated daily by the one o'clock signal-gun. The third column contains the values of r, the observed difference between thermometers X and Y. The film of talc that was interposed between the sun and X was found to reflect $\frac{9}{50}$ths of the incident rays. This ratio was determined by observations taken with the film off during calm weather. The value of r without the film to r with the film on was as 1·18 to 1·00, the sun's power not sensibly varying during the interval. This proportion was maintained at low values of r, and even when the source of radiation was a gas-flame. The fourth column contains the observed values of r increased in this ratio. The fifth column contains the corrections required to reduce the values in the third to a vacuum. The correction is taken from a scale that was constructed by means of an empirical formula derived from observations on the cooling of X, as detailed in the Appendix. The sixth column is the final value of r as it would appear in a naked vacuum, that is, a vacuum without any interposed transparent solid between the sun and the bulb of the thermometer. The numbers in this column represent the quantity of heat-force supplied from the sun to the bulb of the thermometer in a constant element of time, or the quantity that emanates from the bulb in a unit of time.

§ 7. The experiments on the cooling of the thermometer X in a vacuum show that, from $r=30°$ to $r=15°$, the time of cooling was 294 beats of a time-piece, of which $77\frac{1}{2}$ were equal to 60 seconds; also from $r=15°$ to $r=7\frac{1}{2}°$ the time was the same, and generally from $r=2m$ to $r=m$ the elapsed time is the constant 294, which thus represents the logarithm of 2 in the logarithmic curve of which the ordinates are r, and the abscissæ the time of cooling, t. The equation of the curve being

$$c \log \frac{r_1}{r_0} = t_1 - t_0,$$

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* The observations commenced on July 28, and continued for one month, at times when the sky was sufficiently free of clouds. A selection from this Table is given at the end of this paper.

in which $c \log 2 = 294$, or to reduce to seconds,

$$c \log 2 = 294 \times \frac{60}{77\frac{1}{2}},$$

and $c = 756\cdot1$. Let $t_1 - t_0 = \delta t$, then $\log \frac{r_1}{r_0} = \frac{1}{\mu} \cdot \frac{\delta r}{r}$, in which μ=hyp. log of 10; hence

$$\frac{c}{\mu} \cdot \frac{\delta r}{r} = \delta t \qquad [\log \frac{c}{\mu} = 2\cdot51636].$$

From this we may compute the quantity of heat supplied to a unit of surface by the sun in a unit of time corresponding to any value of r. As an example, suppose $r = 10°$ and $\delta t = 1$ second, then $\delta r = \frac{10\mu}{c} = 0°\cdot030453$, or $3°\cdot0453$ in 100 seconds, is the rate at which the sun communicates heat to a thermometer whose bulb is a sphere 0·42 inch in diameter, when $r = 10°$.

Suppose the glass of the bulb to be $\frac{1}{50}$th of an inch thick, there would be ·0108 cubic inch glass and ·0287 cubic inch mercury heated 3°·045 in 100 seconds. If $r = 20°$, the same heating would take place in 50 seconds, and so on.

To reduce this to thickness of ice melted in 1 minute, we have

Specific heat of mercury ·033, and of glass ·177.

Specific gravity of mercury 13·5, and of glass 2·9.

·0108 cubic inch glass equal in weight to ·0313 cubic inch water.

·0287 cubic inch mercury equal in weight to ·387 cubic inch water.

·0108 cubic inch glass raised 3°·045 takes as much heat as ·0313 cubic inch water raised 0°·54.

·0287 cubic inch mercury raised 3°·045 takes as much heat as ·387 cubic inch water raised 0°·101.

·0313 cubic inch water raised 0°·54 takes as much heat as is required to raise 1 cubic inch 0°·0169.

·387 cubic inch water raised 0°·101 takes as much heat as is required to raise 1 cubic inch 0°·0391.

The entire bulb of the thermometer thus raised 3°·045 is thus equal to 1 cubic inch of water raised ·0169 + ·0391 = 0°·056.

Now the transverse section of bulb is 0·138 square inch; and since specific gravity of ice is 0·93, and it requires 140° to melt ice, we have $140 \times 0\cdot738 \times 0\cdot93 \times x = 3\cdot045$; hence $x = 0\cdot00312$ inch, the thickness of ice melted by the sun in 100 seconds, when $r = 10°$. This is equivalent to 0·001872 inch in 1 minute.

With $r=20°$ the thickness would be double this amount, and so on. Thus the presumed extra atmospheric value of r being 67° gives 0·0124 inch thickness melted per minute.

From June to December the amount may be expected to vary $\frac{1}{15}$th, corresponding to alteration of sun's distance. In Herschel's 'Meteorology' the probable thickness is stated to be ·0109 inch.

§ 8. If the law indicated by straight lines on the chart is true, it would require extremely accurate observations to give the extra atmospheric constant of solar radiation with precision*. From a single observation made in Bombay some years ago, I am disposed to believe it may exceed 67° considerably.

§ 9. The mode of approaching the law of absorption is as follows:—Project the values of r as ordinates to the secants of zenith distances as abscissæ: the resulting curve is evidently hyperbolic in character. If it is the conic hyperbola, the reciprocals of the ordinates laid off to the same abscissæ should range in a straight line. The obvious plan is therefore to lay off the reciprocals of r in this way, and see how far their range agrees with the straight; and if it differs, the character of the divergence might lead us to the true function that expresses the natural law, if it was not very complicated, and if the condition of the atmosphere did not vary so rapidly as to obscure it.

The observations, though taken under unfavourable conditions, favour the simple hyperbola†.

§ 10. It will be remarked, on inspecting the Chart, that the value of r at the same altitude of the sun diminishes with the declination as the season advances. If continuous observations were possible for a few hours each day, when the altitude of the sun ranged between 15° and 45°, we might expect to see the projection of the equalized observations range each day in a different line; but these lines ought all to converge on nearly the same point in the ordinate at the zero of the secant scale, if the law holds good.

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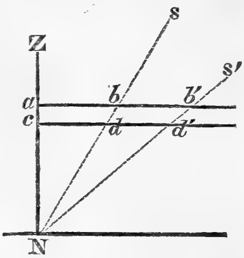
* That is in this climate, where r is comparatively small, and the trend of consecutive observations slopes from a part of the chart, where the scale of r is large, to where it is small. In a tropical station, such as Bombay, where even during the winter months the value of r for the same altitude is double what it is at our summer solstice, the trend must incline much less to the axis, and consequently errors of observation will be but slightly magnified in the value given to R, the extra atmospheric value of r.

† The equation of which is r cosec altitude $=k$ a constant, hence cosec altitude $\propto \frac{1}{r}$; and thus the reciprocals of r laid off as ordinates to the secants of zenith distances may be expected to range in a straight line. See Chart, fig 3.

Fig. 5.



Let N, fig. 5, be the position of the observer, Z his zenith, and N S the direction of the sun. Draw parallel lines *a b*, *c d*, &c.; now *a c* : *b d* :: rad. : sec. sun's zenith distance; so that the thickness of each stratum varies as the secant; and if the physical condition of the stratum do not alter between two observations*, we may take the secant as the representative of the collective thickness of the absorbing medium traversed by the sun's rays, except at such low altitudes when the curvature of the earth as well as refraction may be expected to introduce uncertainty. The minimum value of the secant is radius; but we may imagine the sun's rays to pass through a similarly constituted atmosphere in which the thickness of the same layers proportionally diminishes from unity or radius to zero. The reciprocal of r diminishes for values below radius at the same rate as for values above radius, attains at zero the extra atmospheric limit, which, in all climates and seasons, ought to be determined by the inverse square of our planet's distance from the sun in its orbit, and should not vary beyond $\frac{1}{30}$th of its mean value.

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* But the very absorption of the sun's rays must promote change in physical condition. Is the law for a constant physical condition thereby masked? I believe that during an interval of one or two hours it is not sensibly disturbed, for the following reason: the absorption of the sun's heat by the aqueous particles in the atmosphere would tend to diminish their number, and thus augment r beyond the value assigned by the law for the condition constant. The later of the observations taken in the forenoon of August 21, projected in fig. 3, may thus be suspected to give too high values for r; and if so, the lower points on the left-hand side would require to be removed higher up, and the trend of the points continued towards the vertical axis would intersect it at a point which would give a value to R less than 67°. But this is impossible, because in India r exceeds this amount. The trend must therefore be the other way; and as the scale of r diminishes rapidly downwards, and as R is certainly a constant quantity in all climates, a very slight depression of the trend (the dotted line in fig. 3), such as may be due to fault of observation, would intersect the axis at a point giving a sufficiently probable value of R. In short, R is a fixed point in the axis towards which the projected points of a consecutive series of observations, wherever taken, must trend, whether by straight or curve, if the physical condition of the atmosphere is undisturbed. The line of the consecutive series of the 21st is straight, and points, within moderate limits of inaccuracy, to a probable value of R. On the other hand, if the effect of the sun's rays was to *diminish* the diathermity of the atmosphere, and the observations were quite accurate, the points would be conformable, and indicate a disturbance of physical condition.

Let m_0, m_1 be the secants at which the radiation is r_0, r_1, we have, according to the projection $\frac{m_0 - m_1}{\frac{1}{r_0} - \frac{1}{r_1}} = k$, a constant quantity so long as the physical condition of the atmosphere remains constant; and to find R, the extra atmospheric value of r, we have

$$m_0 - m_1 : \frac{1}{r_0} - \frac{1}{r_1} :: m_1 : \frac{1}{r_1} - \frac{1}{\mathrm{R}}.$$

Hence $m = \frac{k}{r} - \frac{k}{\mathrm{R}}$ and $\frac{1}{\mathrm{R}} = \frac{1}{r} - \frac{m}{k}$. Since R is constant, we may put $\frac{k}{\mathrm{R}} = e$, so that $r = \frac{k}{m+e}$. This expresses the presumed law of absorption or interception.

The essential nature of this law is seen by studying the proportionate differential of r,

$$-\delta r = \frac{k\delta m}{(m+e)^2} \text{ and } \frac{-\delta r}{r} = \frac{\delta m}{m+e} = \delta m \frac{r}{k}; \quad \text{hence } \frac{-\delta r}{\delta m} \propto r^2.$$

Thus the sun's rays, in passing through a constant element of the thickness of the atmospheric medium, loses a proportionate amount of its power that is not constant, but that diminishes in the simple ratio of that power.

As an example, suppose with $r = 30°$, the value diminishes 1° in passing through 1 mile; it would only lose $\frac{1}{4}$° in passing through the same mile if $r = 15°$, and $\frac{1}{900}$th of a degree if $r = 1°$. We might thus expect, when the atmosphere is clear, it does not intercept any sensible proportion of the heat radiated from the earth's surface into space.

§ 11. Compare the value of r with one sun and with two; the supply from each, supposed equal, doubles the value of r, which, measured at the extremities of the mile nearest and furthest from them, shows that for the same element of the thickness of the medium the proportionate decrement of r is constant. Let α represent the angular space occupied by the sun's disk, and t the potential temperature of its radiating surface, then $t\alpha$ represents the supply of heat by radiation from it upon a unit surface, and is measured by r; so that if αt becomes $2\alpha t$, r becomes $2r$. Now the factor 2 may have reference to α, the magnitude of the sun's disk; or it may have reference to t, its temperature. The fluctuating value of r from change of altitude or climate represents a fluctuating potential value of αt; but α being constant, the change is similar to what would take place above the atmosphere by a change in t alone. At different parts of the earth's orbit the value of α changes; so that with

t constant and α variable the proportionate absorption represented by $\frac{\delta r}{r\delta m}$ is a constant quantity; but so far as r depends on t, the value $\frac{\delta r}{r\delta m}$ increases with t, and the causal relation may be expressed as follows:—

The heat-pulse travels, carrying with it an intensity that it borrows from the temperature of its source, and encounters a deflecting or absorbing power in passing through a constant element of the atmospheric medium that is exactly proportional to that intensity.

It would be simpler if the resistance was uniform—if the proportion of force absorbed was constant; but the observations do not admit of the possibility of this. The curve traced out by the coordinates, r and secant zenith distance, would in that case be no longer the conic hyperbola, but the logarithmic curve*.

§ 12. At 6 o'clock in the evening of the 31st of July, while making an observation, an extensive shower of thin rain took place overhead and westward towards the sun, without sensibly obscuring its light or affecting its image when examined through a telescope: the value of r descended immediately from 15° to 13°. The single observation I took in India, compared with those taken at the same altitude in this country, indicates that the value of r is there fully double what it is here, while the quantity of vapour held in suspension estimated from the dew-point is certainly greater. It would seem probable, therefore, that the absorbing power of the atmosphere depends on the watery particles contained in it, not upon the aqueous vapour dissolved in it.

§ 13. Referring to the method of computing the sun's potential temperature, described in the 'Proceedings' of the Society for March 1860, and employing the same rule with R, the extra atmospheric value of r equal to 70° at earth's mean distance, we arrive at 12,880,000° as the potential temperature of its radiating surface†.

If we expose the flame of a bat's-wing jet to one ball of a differential thermometer, the effect is the same whether the broad side or the narrow side of the flame is presented, as I have found on trial. Now the potential temperature being equal to the product of r by the reciprocal of the angular space occupied by the flame, it is in the one case about five times

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* And the points on the Chart ranging in a curve convex towards the axis, and leading by its trend to an inadmissibly low value of R.

† By a typographical error, x, the potential temperature of the radiating surface of the sun, was represented to be 918,000° instead of 9,180,000°.

greater than in the other. In the same way we might compute the potential temperature of an angular space occupied by many thousand flames placed one behind the other, extending in a line from the observer, and probably we should find it cumulative in the ratio of the number of flames.

From observations I have made on gas-flames with the radiation-meter, fig. 1, it would seem to require about 4000 bat's-wing flames ranged behind each other to give a potential equal to that of the sun.

If the upper radiating matter of the sun is in any degree transparent or permeable to radiation from lower strata, it is obvious that the *actual* temperature may thus be much below the *potential*.

26 Royal Crescent, Edinburgh,
November 25, 1861.

EXPLANATION OF THE FIGURES IN PLATE V.

Fig. 1. T U B E is a square tube of brass, mounted with motion in altitude upon an upright, R, fixed into a round slab of lead. The inner surface of this tube is blackened, and at each end, at *l* and *c*, a film of transparent talc was stuck on to prevent the wind from moving the air within the tube.

H D, D H a double screen made of cardboard and cork, coated on both sides with tinfoil, and fitted to slip on the extremity of the tube presented towards the sun.

m, the hole in the centre of screen, about $\frac{1}{10}$th inch greater diameter than the bulb of the solar thermometer, X.

The talc film *l* was also coated with tinfoil, except the central circle.

X, the thermometer in sun with spherical bulb fixed in a cork that fitted the hole L, L in top of brass tube.

Y, the thermometer in the shade fixed in the hole N, N, with cork and soft wax as shown.

Z, a thermometer applied to outer surface of tube.

Fig. 2 is a transverse section of vacuum-bath, employed to ascertain the rate of cooling of the solar thermometer X in air and *in vacuo*.

It consists of a cylindrical vessel of brass, coated internally with lamp-black; the lid L is ground to the upper edge of the cylinder, and in its centre is a stuffing-box, S, with Indian-rubber collar, through which the stem of the thermometer is passed, as shown in the figure; C is a stopcock, upon which N, the nozzle of a flexible tube communicating with an air-pump, is ground air-tight. H is a wooden handle for removing the apparatus to and from the water-bath without touching the metal.

Fig. 3 is a Chart of the observations taken in July 28, August 6 and 21. Each observation is represented by a point, the ordinate of which is the reciprocal of the radiation potential, and the abscissa of which is the secant of the apparent zenith distance of the sun's centre.

Fig. 4 is a duplex scale, showing the correction to be applied to the radiation potential as observed in air to what it would be if *in vacuo*. It conforms to the observations on cooling in air and *in vacuo* given in the Appendix, and was computed from the empirical formula therein described.

The correction in degrees and tenths is found on the right-hand side of the line opposite the value of ra entered on the left-hand side. It has to be added to r_a.

Appendix, describing the Method employed to discover the Influence of the Air in the Cooling of the Sun-thermometer X, *and of ascertaining the Correction required to be applied to Observations of* r, *so as to reduce them to a vacuum.*

The apparatus employed was the vacuum-bath represented in fig. 2. Plate V. (see explanation of figures). With a plentiful supply of lard to the stuffing-box and ground surfaces, a good vacuum could be maintained in it for a day unimpaired.

The time was measured by the beats of a clock: to register the number of these at each degree as the mercury of the thermometer descended, a scale of equal parts was prepared extending to 1000, and with distinguishing marks at each 5, 10, 50, and 100. Then with a pencil in the right hand over the scale, and a magnifying glass in the left over the scale of the thermometer, I counted the beats; and when the mercury came to the line of a degree, made a mark on the scale of equal parts opposite the number of beats, and at the same time continued to count on; *e. g.* if 57 was the number when the mercury came to a line, a pencil-mark was made at 57 on the scale of equal parts, and the counting went on, 58, 59, &c., until the mercury came to the next line.

Thus not a beat was lost from beginning to end, and the accuracy was only limited by the accuracy of the divisions on the scale of the thermometer. Indeed, this method is a severe test to the equality of the divisions, because the reciprocal of the differences in the number of the beats for each degree, if laid off as ordinates to the total number of beats, ought to range in a straight line, and any saw-like irregularities indicate inaccuracy in the divisions of the scale of the thermometer. To heat the bulb of the thermometer, a funnel was placed over the small flame of a Bunsen; then holding the plate (having the thermometer fixed in its place) by means of the stopcock, the bulb was brought over the top of the funnel until the mercury had risen to near the top of the scale. The plate was then quickly placed on the cylinder, communication made with the air-pump, and the air exhausted from the cylinder by twenty strokes, the capacity of the pump being about one-third that of the cylindrical vessel or vacuum-bath. The vessel thus exhausted was placed in a water-bath, the temperature of which was ascertained at the beginning and end, the difference seldom amounting to one-tenth of a degree.

The following Table exhibits two series of observations on the cooling of the sun-thermometer X in the vacuum, and in air, taken while the water-bath remained steady at 48°. This basal temperature being at an exact degree, enables the rate of cooling to be studied easily without fractional parts or interpolation :—

Temp. X.	Beats in vacuum.	r.	Beats in air.	Temp. X.	Beats in vacuum.	r.	Beats in air.
90°	...	42°	0	90°	...	42°	
85	0	37	36	65	323	17	270
4	12	36		4	348	16	291
3	24	35		3	377	15	312
2	36	34		2	406	14	336
1	48	33		1	437	13	360
80	60½	32	78	60	470	12	385
9	73	31		9	510	11	414
8	86	30		8	549	10	448
7	100½	29		7	594	9	484
6	115	28		6	643	8	527
75	130½	27	129	55	704	7	576
4	145½	26		4	765	6	630
3	161	25		3	843	5	695
2	179½	24		2	...	4	780
1	197	23		1	...	3	
70	214	22	191	50	...	2	
9	235	21		9	...	1	
8	255	20		48	...	0	
7	276	19					
6	298	18					

Thus, 53 being the last observation of the vacuum-cooling, corresponds with $r=5°$; then

At $r = 5°$ we have 843 beats
$r = 10$ „ 549 „
$r = 20$ „ 255 „
$r = 40$ „ − 32 „
Difference 294
„ 294 Mean Difference 291·7
„ 287

Again, beginning with $r=7°$,

At $r = 7°$ we have 704 beats
$r = 14$ „ 406 „
$r = 28$ „ 115 „
Difference 298
„ 291 Mean Difference 294½

And beginning with $r=9°$,

At $r = 9°$ we have 594 beats
$r = 18$ „ 298 „
$r = 36$ „ 12 „
Difference 296
„ 286 Mean Difference 291

The numbers in the column of differences ought to be the same if the law of cooling in a vacuum is perfectly true, if the vacuum is complete, and if the graduation of the thermometer is correct. The difference between them is so small that the result must, I think, be deemed satisfactory.

Let us study the same differences with the cooling in air:—

At $r=5^{\circ}$	we have	695 beats	1st Difference 247	
$r=10$	,,	448 ,,	,, 228	2nd Difference 19
$r=20$	,,	220 ,,	,, 206	,, 22
$r=40$	,,	14 ,,		
$r=7$	we have	576 beats	1st Difference 240	
$r=14$	,,	336 ,,	,, 217	2nd Difference 23
$r=28$	,,	119 ,,		
$r=9$	we have	484 beats	1st Difference 230	
$r=18$	,,	254 ,,	,, 210	2nd Difference 20
$r=36$	,,	44 ,,		

Thus it appears that in air the cooling takes place in a ratio greater than r, the first difference of the times diminishing and the second difference slightly increasing between 247 and 210. The limiting value of the first difference must be 294 when $r=0$, and 294 minus the first difference increases nearly as $\sqrt{r}$. An empirical formula constructed in conformity with this ratio cannot differ much from the observations*.

Let Δ represent 1st difference, and g a constant,

$$\Delta=294-g\sqrt{r}$$

and

$$g=\frac{294-247}{\sqrt{7\frac{1}{2}}}=17\cdot162=\frac{294-210}{\sqrt{27}}=\&\text{c. (nearly)}.$$

---

* That is, within the above limits of the value of r; but since it makes $\Delta_a=0$ when $r=294$, it is not to be trusted in the upper part of the scale. Another empirical formula of better promise may be constructed on the hypothesis that the second difference is constant. If we lay off the first difference as ordinate to the mean proportional between the values of r to which it belongs, the curve traced out would be the logarithmic curve if the second difference were constant. Now it has every appearance of being so. The mean of the four is 21, the extremes being 19 and 23, and the irregularities are evidently casual. The equation derived from this is $\Delta_a=217+\frac{20}{\log 2}(\log 20-\log r_a)$, the second difference being assumed 20. The following Table is computed from this value of Δ_a:—

$r_a+p=r_v$.	$r_a+p=r_v$.
$5+0\cdot7=5\cdot7$	$50+27\cdot4=77\cdot4$
$10+2\cdot5=12\cdot5$	$60+35\cdot4=95\cdot4$
$15+4\cdot6=19\cdot6$	$70+43\cdot9=113\cdot9$
$20+7\cdot1=27\cdot1$	$80+53\cdot2=133\cdot2$
$30+13=43\cdot0$	$90+63\cdot2=153\cdot2$
$40+19\cdot8=59\cdot8$	$100+73\cdot6=173\cdot6$

The influence of the air in conveying away heat thus increases in a much higher proportion than r. Further experiments are required to test this at the higher parts of the scale, also to determine whether the ratio of r_a to r_v is not affected by the size of the spherical reservoir. The actual time of cooling must augment with the diameter of the sphere, but the ratio of the times in air and *in vacuo* is probably not affected by the diameter.

Δ represents the logarithm of 2; so that, c being a constant, we have $\log 2 = \frac{\Delta}{c}$. In the curve that represents the cooling in air, we may assume a small arc of it to coincide with a true logarithmic curve, or the curve of cooling in a vacuum, and we have to find the value of r, the ordinate of the true logarithmic curve at the given point.

The logarithmic curve is defined by the equation

$$c \log \frac{r_1}{r_0} = t_1 - t_0 = \frac{\Delta}{\log 2} \log \frac{r_1}{r_0}.$$

Let

$$r_0 = (r_1 - 0^\circ 1).$$

To find the value of r_v corresponding to a given r_a (r_v vacuum, r_a air), we require to compute the value of $t_1 - t_0$, employing $\Delta_a = 294 - g\sqrt{r}$ in the equation

$$\frac{\Delta_a}{\log 2} \log \frac{r_a}{(r_a - 0^\circ \cdot 1)} = t_1 - t_0;$$

then, with this value of $t_1 - t_0$, and with $\Delta_v = 294$, find the value of r_v in the equation

$$t_1 - t_0 = \frac{\Delta_v}{\log 2} \cdot \log \frac{r_v}{(r_v - 0^\circ \cdot 1)}.$$

The direct equation is

$$\Delta_a \{\log r_a - \log (r_a - 0^\circ \cdot 1)\} = \Delta_v \{\log r_v - \log (r_v - 0^\circ \cdot 1)\}$$

and

$$\frac{\Delta_a}{\Delta_v} = 1 - \frac{g}{294} \sqrt{r_a} = 1 - \frac{\sqrt{r_a}}{17 \cdot 131}.$$

Hence r_v may be ascertained by inspecting the differences of a table of logarithms; and it was from these that the Scale, fig. 4. Plate V., was constructed for reducing the values of r taken in air to what they would be if taken in a vacuum, where the emission of heat was by radiation alone.

The cooling of the sun-thermometer in air when fixed in its place in the tube, as in fig. 1, was found to be exactly the same as when fixed in the cylinder, fig. 2, unexhausted.

A chemical thermometer with cylindrical reservoir was tried in the vacuum-bath, and the cooling was found to take place exactly in the logarithmic curve. It is difficult to adjust the vacuum-bath in time to observe a high value of r, but good observations were obtained from $r = 190^\circ$ downwards; so there is little doubt that the law of cooling by radiation is general and independent of the shape of the cooling body. I purpose extending these observations with different surfaces. One result is interesting as showing the perfect reciprocity of the radiation, viz. a gilt bulb radiating against a blackened metallic surface loses heat at the same rate as a blackened bulb against a bright metallic surface, the rate being slower than when both are blackened.

Observations on Solar Radiation.

	Apparent time. Latitude, 55° 58′ N. Longitude, 0h. 12m. 44s. W.	Observed values of *r*.	Ditto, corrected for talc film.	Correction to reduce to a vacuum.	Value of *r* reduced to a vacuum.	Reciprocal of ditto.	Apparent altitude of sun's centre.	Secant of apparent zenith distance.	Remarks.
		ro.	*ra*.	*p*.	*rv*.	$\frac{1}{rv}$.	App. alt.		
1861.	h m								
July 28.	4 25 P.M.	17°·5	20°·6	+7°·4	28°·0	·0357	28° 49′	2·075	
	4 36	16·5	19·5	6·8	26·3	·0380	27 19	2·183	
	4 56	15·5	18·3	6·2	24·5	·0408	24 30	2·411	
	5 26	14·0	16·5	5·2	21·7	·0461	20 19	2·880	
	6 18	13·2	15·5	4·7	20·2	·0495	13 11	4·385	
Aug. 6.	8 7 A.M.	16·2	19·1	6·6	25·7	·0389	31 17	1·926	
	9 8	18·0	21·2	7·7	28·9	·0346	42 21	1·484	
	9 32	18·3	21·6	8·0	29·6	·0338	45 12	1·409	
13.	7 27 P.M.	2·2	2·6	0·3	2·9	·3448	0 53	...	Sunset
16.	5 16 A.M.	4·4	5·2	0·8	6·0	·1667	5 29	10·465	Sun rises perfectly clear of clouds.
	9 39	14·6	17·2	5·6	22·8	·0439	39 51	1·561	
	11 3	15·0	17·7	5·8	23·5	·0425	46 20	1·382	
20.	6 21 16 P.M.	6·6	7·7	+1·4	9·1	·1099	7 21	7·817	Consecutive series not equalized.
	6 23 16	6·5	7·6	1·4	9·0	·1111	7 4	8·128	
	6 24 31	6·4	7·5	1·4	8·9	·1124	6 55	8·304	
	6 26 16	6·1	7·2	1·3	8·5	·1177	6 41	8·592	
	6 27 16	6·0	7·1	1·2	8·3	·1205	6 33	8·767	
	6 28 36	6·1	7·2	1·3	8·5	·1177	6 22	9·018	
	6 31 0	5·7	6·7	1·1	7·8	·1282	6 2	9·514	
	6 37 16	4·8	5·7	0·8	6·5	·1538	5 12	11·034	
	6 39 0	4·7	5·6	0·8	6·4	·1562	4 59	11·512	
21.	7 20 A.M.	9·8	11·6	2·9	14·5	·0690	19 0	3·071	Consecutive series graphically projected and equalized.
	7 40	10·7	12·6	3·3	15·9	·0629	21 45	2·698	
	8 0	11·6	13·7	3·8	17·5	·0571	24 29	2·413	
	8 20	12·3	14·5	4·2	18·7	·0535	27 9	2·191	
	8 40	13·0	15·3	4·6	19·9	·0502	29 45	2·015	
	9 0	13·5	15·9	4·9	20·8	·0481	32 14	1·874	
	9 20	13·8	16·2	5·0	21·2	·0472	34 37	1·760	
	9 40	14·1	16·6	5·3	21·9	·0457	36 50	1·668	

[The observations taken on seven other days between July 28 and August 28 are here omitted.]

LXXI. *On the Conduction of Heat by Gases.* *By* R. Clausius.

[Concluded from p. 435.]

IV. *Behaviour of the Molecules which traverse a given Plane in a Unit of Time.*

§ 13. We will direct our attention to any plane situated perpendicularly to the axis of x, and to the molecules which traverse this plane. Let us take, for instance, the plane whose abscissa is x, and which is therefore the first limiting plane of the infinitely thin stratum that we have been considering in § § 8 *et seq.;* we can then draw, from the behaviour of the molecules existing simultaneously in the stratum, definite conclusions as to the behaviour of those which traverse our plane during a given time.

Let us suppose a part of the plane, equal in size to a unit of surface, to be divided off from the rest. The cubic capacity of the portion of the stratum corresponding to this extent of surface will then be represented by dx, if dx is the thickness of the stratum; and we will denote the number of molecules which exist simultaneously in this space by $\mathrm{N}dx$, where N is a very large number dependent upon the density of the gas at the place in question. These $\mathrm{N}dx$ molecules move in all possible directions, and the number of them whose cosine lies between μ and $\mu+d\mu$ is, according to § 12, the fraction $\frac{1}{2}\mathrm{I}d\mu$ of the entire number, and is therefore perfectly represented by the product,

$$\tfrac{1}{2}\mathrm{NI}dxd\mu.$$

In order from this expression, which refers to the molecules simultaneously existing in the stratum, to deduce the number of molecules which traverse the stratum in a unit of time, and which therefore must also traverse the plane in question, we must take into consideration the time which each molecule requires in order to traverse the stratum from one limiting plane to the other. For a molecule with the cosine μ, the distance to be traversed from one plane to the other is, disregarding its sign, equal to $\frac{dx}{\mu}$; and the time required to traverse this distance is equal to $\frac{dx}{\mu\mathrm{V}}$, if V denotes the velocity. We will assume provisionally that all molecules whose cosine lies between μ and $\mu+d\mu$ have the same velocity, and therefore require the same time for traversing the stratum; the number of molecules which exist simultaneously in the stratum will then bear the same proportion to the number which traverse the stratum in a unit of time as this small space of time bears to a unit of time; we must therefore divide the former number by the small time in order to obtain the latter.

To apply this to the case before us, we must divide the magnitude $\frac{1}{2}\mathrm{NI}dx d\mu$ by $\frac{dx}{\mu \mathrm{V}}$, and we thus obtain for *the number of molecules which traverse our unit of surface during a unit of time, in such directions that their cosines lie between μ and $\mu + d\mu$*, the expression

$$\tfrac{1}{2}\mathrm{NIV}\mu d\mu.$$

It must, however, be further remarked that the difference of sign in this expression, resulting from the circumstance that the cosine μ may be either positive or negative, corresponds to an essential difference in the manner of traversing the stratum. If μ is positive, the molecules pass through the plane from the negative to the positive side; if μ is negative, they traverse in the contrary direction.

§ 14. Before extending the expression just arrived at, which refers only to an infinitely small interval of the cosine μ, and presupposes equal velocities, we will first deduce two other corresponding expressions.

If we denote by m the mass of a molecule moving with the velocity V, its momentum is $m\mathrm{V}$, and the product $m\mathrm{V}\mu$ represents that component of the momentum which falls in the direction of x, so that a positive value of this product corresponds to the case in which the component falls in the direction of positive x. We will accordingly call the product shortly *the positive momentum* of the molecule. Hence the collective *positive momentum* of the above mentioned $\frac{1}{2}\mathrm{NIV}\mu d\mu$ molecules which traverse our plane will be represented by

$$\tfrac{1}{2}m\mathrm{NIV}^2\mu^2 d\mu.$$

Further, the *vis viva* of a molecule whose mass is m, and whose velocity is V, will be represented by $\frac{1}{2}m\mathrm{V}^2$. If, in addition to the motion of translation with the velocity V, the constituents of the molecule have also a rotatory or a vibratory motion, the collective *vis viva* exceeds that product. I have spoken of these additional motions, which may occur independently of the motion of translation, in my memoir "On the kind of Motion which we call Heat" (Phil. Mag. August 1857, p. 108), and have pointed out that, for a given kind of molecules, a constant relation must on an average prevail between the various simultaneously occurring motions, in such sort that the *vis viva* of the motion of translation forms a constant aliquot part of the total *vis viva*. We will accordingly denote the mean value of the total *vis viva* of a molecule by $\frac{1}{2}km\mathrm{V}^2$, where k is a factor whose value

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* In my earlier paper, quoted in the text, I have shown how this factor may be calculated by aid of the two specific heats. For such simple gases

is constant for each individual kind of gas*. In accordance herewith, we obtain for the *vis viva* of the $\frac{1}{2}\mathrm{NIV}\mu d\mu$ molecules which traverse our plane the expression

$$\tfrac{1}{4}km\mathrm{NIV}^3\mu d\mu.$$

§ 15. In order now so to transform the expressions arrived at in the last three sections that they may also remain applicable when the velocities of the individual molecules are not equal, we only require to substitute for the values V, V^2, and V^3 the mean values $\overline{\mathrm{V}}$, $\overline{\mathrm{V}^2}$, and $\overline{\mathrm{V}^3}$. In order further to extend the expression, which at present has reference only to an infinitely small interval of the cosine μ, to all the molecules which traverse the plane, we must also integrate it from $\mu=-1$ to $\mu=+1$. In addition, we will, for the sake of uniformity, multiply the first expression also by m, so that, instead of the number, it may denote the mass of the molecules. If, then, for the sake of shortness, we introduce the following signs, E the *mass*, F the *positive momentum*, and G the *vis viva*, which pass in the positive direction through the superficial unit of our plane in a unit of time, we obtain the following equations:—

$$\left.\begin{aligned}\mathrm{E}&=\frac{1}{2}m\mathrm{N}\int_{-1}^{+1}\mathrm{I}\overline{\mathrm{V}}\mu d\mu,\\ \mathrm{F}&=\frac{1}{2}m\mathrm{N}\int_{-1}^{+1}\mathrm{I}\overline{\mathrm{V}^2}\mu^2 d\mu,\\ \mathrm{G}&=\frac{1}{4}mkm\mathrm{N}\int_{-1}^{+1}\mathrm{I}\overline{\mathrm{V}^3}\mu d\mu.\end{aligned}\right\}\quad\dots\dots\quad(\mathrm{V.})$$

The last of these three quantities represents the *conduction of heat* which occurs in the gas; it is therefore the determination of this quantity which will be principally treated of in what follows. The two other quantities must also be taken into consideration, because, as we shall immediately see, they aid in the determination of this one.

By substituting in the three equations the series given in (III.) and (IV.) for $\overline{\mathrm{V}}$, $\overline{\mathrm{V}^2}$, $\overline{\mathrm{V}^3}$, and I, and performing the integration, we get

$$\left.\begin{aligned}\mathrm{E}&=\tfrac{1}{3}m\mathrm{N}(q+uq')\epsilon+\mathrm{X}\epsilon^3,\\ \mathrm{F}&=\tfrac{1}{3}m\mathrm{N}u^2+\mathrm{X}_1\epsilon^2,\\ \mathrm{G}&=\tfrac{1}{6}km\mathrm{N}u^2(3q+uq')\epsilon+\mathrm{X}_2\epsilon^3.\end{aligned}\right\}\quad\dots\quad(\mathrm{VI.})$$

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as exhibit no irregularity in regard to their volume, and for compound gases which are formed without condensation, the value of this factor is approximately $\frac{1}{0{\cdot}6315}=1{\cdot}584$. For gases whose formation is attended with diminution of volume it is larger.

The terms $X\epsilon^3$, $X_1\epsilon^2$, and $X_2\epsilon^3$, in which the factors X, X_1, and X_2 denote functions of x which are left undetermined, are only added in order to indicate of what degree are the terms which would be obtained by carrying out the calculation still further. It will be seen that in all three equations the second term is two degrees higher than the first; and if we content ourselves with such an approximation in our results that we neglect magnitudes of the order ϵ^2 in comparison with unity, which we may do without hesitation, seeing that ϵ is a very small quantity, we may entirely disregard these additional indeterminate terms in the developments which follow.

On considering the degree of the first and important term, it may perhaps appear surprising that the magnitude F is of no degree in respect of ϵ, whereas E and G are of the first degree. This, however, becomes intelligible when we remember that the momentum behaves differently in regard to its sign from the mass or *vis viva*. The momentum of a molecule which traverses the plane in the negative direction is in itself negative; but as it must again receive a negative sign in consequence of its passage in the negative direction, it thereby becomes positive again; so that the positive and negative passages are not in this case, as in the other two, to be subtracted from, but added to, each other.

§ 16. In reference to the magnitudes E, F, and G, the assumption that the gas is in a *state of rest* enables us to deduce at once the following propositions:—

1. *The mass of gas which traverses the plane must be equal to* 0. For, since the whole quantity of gas is contained between two fixed surfaces, if any gas passed through an intermediate plane in either direction, the density must increase at one side of the plane and diminish at the other, which would be in contradiction of the presupposed conditions.

2. *The positive momentum which traverses our plane in a unit of time must be independent of the situation of the plane, and therefore constant in regard to* x. For if we suppose a stratum bounded by any two parallel planes, the momentum which enters the stratum through one plane must be equal to the momentum which passes out of it through the other, for otherwise the momentum present in the stratum must vary; this, however, would be in contradiction of the stationary state which is one of our conditions.

3. *The* vis viva *which traverses the plane in a unit of time must be constant in regard to* x, for the same reason as that given in the case of the positive momentum.

We can therefore establish the following three equations of condition:—

$$\left.\begin{aligned}E&=0,\\ F&=\text{ constant quantity},\\ G&=\text{ constant quantity},\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (21)$$

which we will now apply to the expressions already arrived at for E, F, and G.

The *first* equation gives, if we neglect the term $X\epsilon^3$,

$$q+uq'=0,$$

which determines the ratio between the coefficients q and q', namely,

$$q'=-\frac{q}{u}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

Hence equation (IV.) takes the following form, if we at the same time introduce into it the value of i given in (20):—

$$I=1-\frac{q}{u}\mu\epsilon+r'(\mu^2-\tfrac{1}{3})\epsilon^2+\ldots \quad . \quad . \quad (\text{VII.})$$

The *second* of the foregoing equations (VI.), neglecting $X_1\epsilon^2$, gives

$$Nu^2=\text{const.} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

N determines the density of the gas at the point in question, and u^2 is proportional to its absolute temperature; whence it follows that the product of the density into the absolute temperature, or, what comes to the same thing, that the *pressure* must be the same throughout the whole mass of gas—a result which might also have been assumed as self-evident at starting.

Finally, touching the magnitude G. On applying to the *last* of the equations (VI.) the equation (22), and neglecting the term $X_2\epsilon^3$, it becomes

$$G=\tfrac{1}{3}kmNu^2q\epsilon. \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{VIII.})$$

But since, according to what precedes, Nu^2 is a constant quantity, and k, m, and ϵ are essentially constants, it follows that, if G is to be, as in fact it must be, constant,

$$q=\text{constant quantity}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

In order to determine the conduction of heat without considering the magnitude ϵ, which I have discussed in my former paper, it now only remains to determine this one constant quantity q.

V. *Relation between the molecules existing simultaneously in a given stratum and those emitted from the same stratum.*

§ 17. In order to find how many molecules are emitted from a stratum, we must know how great is the likelihood that a molecule, while traversing the stratum, will strike against another molecule; for it is the molecules which strike each other, and, after the rebound, leave the stratum with altered directions and velocities, which we have agreed to call the *molecules emitted from the stratum.*

We will call the probability of one molecule striking another while traversing the infinitely small space ds, as we have done in § 9, αds; and our business is now to make a closer approximation to the value of α.

In my former memoir* I have determined the value of α for the case of a molecule moving in a space containing very many other molecules in a state of rest, and there I found

$$\alpha=\frac{\pi\rho^2}{\lambda^3},$$

where ρ is the radius of the sphere of action of a molecule, in the sense indicated in the paper quoted, and λ is the interval which would exist between every pair of neighbouring molecules if, instead of the irregular distribution of them which occurs in reality, the molecules had a regular cubical arrangement (that is, if the entire space were divided up into small cubical spaces, and the centres of the molecules were at the corners of the cubes). Instead of the magnitude λ, we may likewise introduce N, the number of the molecules existing in a unit of space. There must indeed be as many molecules in a unit of space as there are such cubical spaces with the side λ contained in it; hence we have $N=\frac{1}{\lambda^3}$, whereby the last equation is transformed into

$$\alpha=\pi\rho^2 N. \qquad (25)$$

This expression for α admits of being easily modified so as to be likewise applicable to the case in which the other molecules are in motion instead of being at rest.

If we denote the likelihood that there is of the molecule under consideration striking another during the element of time dt, by adt, and regard ds as the space traversed during the time dt, we get

$$adt=\alpha ds; \qquad (26)$$

or, putting v in place of $\frac{ds}{dt}$, that is to say, the velocity of the molecule in question,

$$a=\alpha v. \qquad (27)$$

Substituting here for α its value as deduced in (25), we have

$$a=\pi\rho^2 Nv. \qquad (28)$$

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* Phil. Mag. S. 4. vol. xvii. p. 87.

If we now suppose that, instead of being at rest, the other molecules are all moving with a common velocity in a given direction, the likelihood of the molecule in question striking another during the time dt will plainly be represented by the same formula, if we substitute, for the *absolute* velocity, v, of this molecule, its velocity *relatively* to the other molecules. Let V be the common velocity of the other molecules, ϕ the angle which the direction of their motion makes with the direction of the molecule under consideration, and R the relative velocity; then

$$R=\sqrt{V^2+v^2-2Vv\cos\phi}, \quad . \quad . \quad . \quad . \quad (29)$$

and with this value we can put

$$a=\pi\rho^2 NR. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (30)$$

Finally, let us suppose that the other molecules move, not all in the same direction, but in various directions, and with velocities which are not necessarily equal to each other; in this case the velocities of our molecule, relatively to the several other molecules, will be various, and we must use in the equation the *mean value* of the relative velocities. Denoting this mean value by $\overline{\overline{R}}$*, the equation for a becomes

$$a=\pi\rho^2 N\overline{\overline{R}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (31)$$

and thence we obtain as a result of (27), the following equation for α:—

$$\alpha=\pi\rho^2 N\frac{\overline{\overline{R}}}{v}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (32)$$

§ 18. We have now to determine the *mean relative velocity* of a given molecule moving in our stratum, as compared with all the other molecules simultaneously existing therein.

The velocity of the given molecule relatively to another given molecule, whose direction forms the angle ϕ with its own direction, and whose velocity is V, is determined by equation (29). If we now consider all the molecules which are moving in the same direction, their velocities, as we have already seen in § 8, are not exactly equal to each other; and hence the velocities of the given molecule relatively to them are also somewhat unequal. We will accordingly, in the first place, introduce a mean relative velocity, denoted by $\overline{R}$, for each separate direction.

In order to be able to present in a tangible form the considerations which further regard the various directions in which the movements take place, we will, as before, imagine a spherical surface described with the radius 1, and regard the various direc-

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* The reason for putting here two horizontal strokes over the letter R, instead of only one, as in former cases, will become evident immediately.

tions as being drawn from its centre, so that every point on the surface of the sphere represents a direction. If the molecules moved equally in all directions, the number whose directions would fall within an element $d\omega$ of the spherical surface would have the same ratio to the whole number of molecules as the size of that element to the surface of the entire sphere; hence it would be represented, as a fraction of the whole number of molecules present, by $\frac{d\omega}{4\pi}$. In the present case, in which the molecules do not move equally in all directions, this expression must undergo a modification, and one of such a kind that, according to the notation adopted in § 12, the number of molecules whose directions fall within the superficial element $d\omega$ will be represented, as a fraction of the entire number of molecules, by $\mathrm{I}\frac{d\omega}{4\pi}$.

If $\overline{\mathrm{R}}$ represents the mean velocity of the given molecule relatively to those molecules whose directions fall within the element $d\omega$, and $\overline{\overline{\mathrm{R}}}$ its mean velocity relatively to all the molecules present, the following equation will serve to determine the latter quantity:—

$$\overline{\overline{\mathrm{R}}}=\int\frac{d\omega}{4\pi}\mathrm{I}\overline{\mathrm{R}}. \qquad (33)$$

The integration must here be extended to the whole spherical surface; and this integral we will now proceed to develope.

§ 19. According to equation (29),

$$\mathrm{R}=\sqrt{\mathrm{V}^2+v^2-2\mathrm{V}v\cos\phi},$$

to which we will give the following slightly modified form:

$$\mathrm{R}=\sqrt{2}\sqrt{\mathrm{V}v}\sqrt{1-\cos\phi+\frac{(\mathrm{V}-v)^2}{2\mathrm{V}v}}. \qquad (34)$$

The velocity V of any molecule existing in the stratum differs, as we have already seen, only so slightly from the velocity u of the molecules which move perpendicularly to the axis of x, that the difference is a magnitude of the same order as ϵ. If we now assume that the velocity of the given molecule denoted by v likewise only differs from u by a magnitude of the same order, the difference $\mathrm{V}-v$ must also be a magnitude of the order of ϵ; and hence the term $\frac{(\mathrm{V}-v)^2}{2\mathrm{V}v}$, which occurs in the last root, must be a magnitude of the order of ϵ^2. By integrating this term nothing but another term of the same order can be obtained:

accordingly, if we neglect terms of the second or higher degrees in the expression sought for for $\overline{\overline{R}}$, we may disregard the quantity $\frac{(V-v)^2}{2Vv}$, whereby the calculation becomes very much simplified.

The equation for R then becomes

$$R=\sqrt{2}\sqrt{1-\cos\phi}\sqrt{Vv}. \quad . \quad . \quad . \quad . \quad (35)$$

But, by equation (9),

$$V=U-\frac{dU}{dx}\mu s+\ldots;$$

so that, by developing the foregoing equation according to s as far as the term of the first degree, it is transformed into

$$R=\sqrt{2}\sqrt{1-\cos\phi}\sqrt{Uv}\left(1-\frac{1}{2}\frac{1}{U}\frac{dU}{dx}\mu s\right).$$

And substituting for s in this expression the mean value $\bar{s}$, we obtain instead of R the mean value $\overline{R}$: thus

$$\overline{R}=\sqrt{2}\sqrt{1-\cos\phi}\sqrt{Uv}\left(1-\frac{1}{2}\frac{1}{U}\frac{dU}{dx}\mu\bar{s}\right). \quad . \quad (36)$$

In place of U and $\bar{s}$ we will now put the values given for them in (1) and (18), which, neglecting terms of the second and higher degrees, are as follows:—

$$U=u+p\mu\epsilon,$$
$$\bar{s}=c\epsilon;$$

at the same time we will put

$$v=u+\delta, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (37)$$

where δ may denote any magnitude of the order of ϵ. Then we get

$$\overline{R}=\sqrt{2}\sqrt{1-\cos\phi}\left[u+\frac{1}{2}\delta+\frac{1}{2}\left(p-c\frac{du}{dx}\right)\mu\epsilon\right];$$

or, substituting q for $p-c\frac{du}{dx}$, as we have done already in (19),

$$\overline{R}=\sqrt{2}\sqrt{1-\cos\phi}(u+\tfrac{1}{2}\delta+\tfrac{1}{2}q\mu\epsilon). \quad . \quad . \quad . \quad . \quad (38)$$

This expression must be multiplied by I, which is represented, according to (VII.), disregarding the higher terms, by $1-\frac{q}{u}\mu\epsilon$.

Thus we have

$$I\overline{R}=\sqrt{2}\sqrt{1-\cos\phi}\,(u+\tfrac{1}{2}\delta-\tfrac{1}{2}q\mu\epsilon)\text{[*]}, \quad . \quad . \quad . \quad (39)$$

which product must be introduced into (33) and the integration then carried out.

For this purpose we require to know what ratio $\cos\phi$ bears to the cosine denoted by μ. We have used μ to stand for the cosine of the angle formed by the moving direction of any molecule whatever with the axis of x, and ϕ to stand for the angle contained between the direction of this molecule and that of the given molecule. Further, let the angle which the moving direction of the given molecule forms with the axis of x be η, and the angle between two planes passing through the moving direction of the given molecule, and containing respectively the angles ϕ and η, be ψ. Then

$$\mu=\cos\eta\cos\phi+\sin\eta\sin\phi\cos\psi. \quad . \quad . \quad . \quad (40)$$

The superficial element $d\omega$ may at the same time be represented by $\sin\phi d\phi d\psi$. The equation (33) thus becomes

$$\overline{\overline{R}}=\frac{\sqrt{2}}{4\pi}\int\!\!\int d\phi d\psi\,\sin\phi\,\sqrt{1-\cos\phi}\,[u+\tfrac{1}{2}\delta$$
$$-\tfrac{1}{2}q(\cos\eta\cos\phi+\sin\eta\sin\phi\cos\psi)\epsilon], \quad . \quad . \quad (41)$$

where the integration according to ψ must be carried out from 0 to 2π, and that according to ϕ from 0 to π.

By performing this integration we get

$$\overline{\overline{R}}=\tfrac{4}{3}(u+\tfrac{1}{2}\delta+\tfrac{1}{10}q\cos\eta\,.\,\epsilon)\text{[†]}. \quad . \quad . \quad . \quad (\text{IX.})$$

§ 20. We must introduce this expression for $\overline{\overline{R}}$ into the equations (31) and (32), in order to obtain the values of a and α.

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[*] [Thus in the original: probably a misprint for

$$I\overline{R}=\sqrt{2}\sqrt{1-\cos\phi}\left(u+\frac{1}{2}\delta-\frac{1}{2}q\mu\epsilon\right)-\left[\sqrt{2}\sqrt{1-\cos\phi}\left(u+\frac{1}{2}\delta-\frac{1}{2}q\mu\epsilon\right)\right]\frac{q}{u}\mu\epsilon.$$

—G. C. F.]

[†] For the sake of greater clearness, I have omitted in the above calculations all terms containing any power of ϵ higher than the first. I will, however, here give the result of the more extended calculation in which terms containing the second power of ϵ are also included; namely,

$$\overline{\overline{R}}=\frac{4}{3}\left\{u+\frac{1}{2}\delta+\frac{1}{10}q\cos\eta\epsilon+\frac{1}{4}\frac{\delta^2}{u}-\frac{1}{5}q\cos\eta\frac{\delta\epsilon}{u}\right.$$
$$\left.+\frac{1}{5\cdot7}\left[-6q^2+2q_1^2+6ru+\frac{1}{3}r'u^2+\left(\frac{1}{2}q^2+\frac{11}{4}q_1^2-\frac{1}{2}ru-r'u^2\right)\cos^2\eta\right]\frac{\epsilon^2}{u}\right\}.$$

Thereby, substituting also $u+\delta$ for v, we get

$$a=\frac{4}{3}\pi\rho^2\text{N}\left(u+\frac{1}{2}\delta+\frac{1}{10}q\cos\eta\epsilon\right), \quad . \quad . \quad . \quad (42)$$

$$\alpha=\frac{4}{3}\pi\rho^2\text{N}\left(1-\frac{1}{2}\frac{\delta}{u}+\frac{1}{10}q\cos\eta\frac{\epsilon}{u}\right). \quad . \quad . \quad (43)$$

The unknown quantity ρ can still be eliminated from these expressions. For assuming, as a particular case, that the given molecule, as well as all the other molecules present, has the velocity u, we have $\delta=0$ and $q=0$; whence it results that

$$\alpha=\tfrac{4}{3}\pi\rho^2\text{N}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (44)$$

Further, according to § 9, the fraction $\frac{1}{\alpha}$ represents the mean length of excursion between any two impacts, whence we obtain for the mean length of excursion the following expression:—

$$\frac{3}{4}\frac{1}{\pi\rho^2\text{N}}*.$$

In order to render the signification of this expression still more special, so that it may represent the *normal* mean length of excursion, which we have denoted by ϵ, we only require to substitute for N, which signifies the number of molecules contained in a unit of volume, the particular value which corresponds to the normal condition of the gas. Distinguishing this value by N_0, we obtain

$$\epsilon=\frac{3}{4}\frac{1}{\pi\rho^2\text{N}_0}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (45)$$

Eliminating ρ^2 from the above expressions by means of the equation, they become

$$a=\frac{\text{N}}{\text{N}_0\epsilon}\left(u+\frac{1}{2}\delta+\frac{1}{10}q\cos\eta\epsilon\right), \quad . \quad . \quad . \quad (\text{X.})$$

$$\alpha=\frac{\text{N}}{\text{N}_0\epsilon}\left(1-\frac{1}{2}\frac{\delta}{u}+\frac{1}{10}q\cos\eta\frac{\epsilon}{u}\right). \quad . \quad . \quad . \quad (\text{XI.})$$

We see from these expressions that the quantities a and α are dependent on the velocity and direction of motion of the given molecule, and further, since N and u are functions of x, that they are dependent upon the position of the particular stratum in which we consider the motion†.

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[*] I have already given this value for the mean length of excursion, in the case in which all the velocities are equal, in my former paper (Pogg. *Ann.* vol. cxv. p. 249), but without the details of calculation.

[†] Maxwell has not in his calculations sufficiently attended to the dependence of the quantity α on various circumstances, inasmuch as he treats

By help of these expressions it will now be easy to make the calculations necessary for our purpose.

§ 21. We will try to determine how many molecules strike each other with our infinitely thin stratum during a unit of time, and how great is the collective momentum of these molecules.

The probability that one molecule moving within the stratum will meet another during the element of time dt, will be represented by adt, if we put for $\cos\eta$ and δ, in the expression for a, the values corresponding to the direction and velocity of the molecule in question. If, therefore, we want to determine the number of molecules, out of a given large number of molecules, which will strike each other during the time dt, we only need to multiply the whole number of molecules by adt, employing the mean value of a, if it is not the same for all the molecules. Considering now the molecules simultaneously existing in a portion of our stratum corresponding to a superficial unit, let us direct our attention, in the first instance, to those whose cosine lies between μ and $\mu+d\mu$. The number of these is $\frac{1}{2}\mathrm{NI}d\mu dx$; and multiplying this expression by $\overline{a}dt$, where $\overline{a}$ denotes the mean value of a for these molecules, the product $\frac{1}{2}\mathrm{NI}\overline{a}d\mu dx dt$ represents, according to what has been said above, the number of them which will strike each other during the time dt. Integrating the last expression according to μ from -1 to $+1$, we obtain the total number of molecules which will strike one another within the stratum during the time dt. We now only require to divide this expression by dt, in order to obtain the total number of molecules which will strike one another within the stratum during a unit of time. Calling this number $\mathrm{M}dx$, we have

$$\mathrm{M}=\tfrac{1}{2}\mathrm{N}\int_{-1}^{+1}\mathrm{I}\overline{a}d\mu. \quad . \quad . \quad . \quad . \quad . \quad (46)$$

The quantity I, which occurs here, we already know to be $=1-\frac{q}{u}\mu\epsilon$. To obtain the value of $\overline{a}$, we must put μ for $\cos\eta$ in equation (X.), and $\mathrm{V}-u$ for δ, since V denotes the velocity of a molecule. But as all the molecules for which the cosine μ has the same value have not the same velocity, we must make use of the mean value $\overline{\mathrm{V}}$ in order to obtain the mean value $\overline{a}$. According to (III.), $\overline{\mathrm{V}}=u+q\mu\epsilon+\ldots$, and, disregarding higher

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the motions of the molecules emitted from an infinitely thin stratum as though the value of α were the same for all and invariable. It accidentally happens that the effect of this oversight is in the opposite direction to that mentioned at § 5; so that the two partially compensate each other, at least so far as regards the calculation of the conduction of heat.

powers of ϵ, we thus obtain for the mean value of δ the quantity $q\mu\epsilon$. Introducing this value in equation (X.), we obtain

$$\bar{a}=\frac{N}{N_0\epsilon}\left(\mu+\frac{3}{5}q\mu\epsilon\right); \quad . \quad . \quad . \quad . \quad . \quad (47)$$

and accordingly equation (46) becomes

$$M=\frac{1}{2}\frac{N^2}{N_0\epsilon}\int_{-1}^{+1}\left(u-\frac{2}{5}q\mu\epsilon\right)d\mu;$$

and performing the integration, we get

$$M=\frac{N^2u}{N_0\epsilon}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (48)$$

The total positive momentum of the molecules which strike each other within the stratum during a unit of time may be arrived at in a corresponding manner. The positive momentum of a molecule whose velocity is V and whose cosine is μ, is $m\mu V$, and hence we have to make use of the product $m\mu Va$, instead of the quantity a; but here again we have to determine the mean value of Va, just as previously we had to determine the mean value of a. The expression for the momentum sought is therefore

$$\frac{1}{2}dxmN\int_{-1}^{+1}I\overline{Va}\mu d\mu.$$

If, as before, we substitute their values for I and $\overline{Va}$ in this expression, it becomes

$$\frac{1}{2}dxm\frac{N^2}{N_0\epsilon}\int_{-1}^{+1}\left(u^2+\frac{3}{5}uq\mu\epsilon\right)\mu d\mu,$$

whence we get, by performing the integration,

$$\frac{1}{5}dxm\frac{N^2}{N_0}uq,$$

for which, by applying equation (48), we may also write

$$\tfrac{1}{5}dxmMq\epsilon.$$

§ 22. This last expression may serve us for the determination of the constant quantity q.

The molecules which *impinge within the stratum* are also those which, after impact, *are emitted from the stratum*, and the collective momentum which these molecules possessed before impinging must remain the same afterwards. Now the positive momentum of the molecules emitted from the stratum can be easily expressed according to the method of representation previously adopted. For we have seen that the motions of these molecules may be expressed by assuming at first motions taking place equally in all directions, and then supposing a small additional component velocity in the direction of positive x, which we represented by $p\epsilon$, to be imparted to all the molecules. It follows thence that, if $\mathrm{M}dx$ denotes the number of molecules emitted in a unit of time, their collective positive momentum will be expressed by

$$dxm\mathrm{M}p\epsilon.$$

Comparing this expression with that previously arrived at, we have

$$dxm\mathrm{M}p\epsilon=\tfrac{1}{5}dxm\mathrm{M}q\epsilon,$$

and hence

$$p=\tfrac{1}{5}q. \quad \dots\dots\dots \quad (49)$$

Having obtained this result, let us return to equation (19), which is as follows:—

$$q=p-c\frac{du}{dx},$$

and by means of the foregoing equation may be transformed into

$$q=-\frac{5}{4}c\frac{du}{dx}. \quad \dots\dots\dots \quad (50)$$

The magnitude c which here occurs may be deduced from what has gone before in the following manner. According to equation (15),

$$\frac{1}{\alpha_0}=c\epsilon,$$

where α_0 denotes the particular value possessed by α in the case of those molecules which move perpendicularly to the axis of x,—a value which is obtained by putting δ and $\cos\eta$ equal to 0 in equation (XI.), namely,

$$\alpha_0=\frac{\mathrm{N}}{\mathrm{N}_0\epsilon}.$$

This value, introduced into the foregoing equation, gives us

$$c=\frac{\mathrm{N}_0}{\mathrm{N}}. \quad \dots\dots\dots \quad (51)$$

Hence equation (50) becomes

$$q=-\frac{5}{4}\frac{\mathrm{N}_0}{\mathrm{N}}\frac{du}{dx}*. \quad \dots\dots \quad \text{XII.})$$

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* If the calculations are worked out further than they have been above, by taking account, that is, throughout of the next higher power of ϵ, it will be found that the expressions deduced above, for the number and momentum of the molecules which impinge within the stratum, have such a degree of accuracy that only a quantity of the order of ϵ^2, as compared with unity, has been disregarded throughout.

The determination of the coefficient $q\epsilon$ gives at the same time, as a consequence of equation (22), also the value of q'. The series in equations (III.) and (IV.), which express the kind of motion of the molecules existing simultaneously in a stratum, are therefore known as far as is necessary for our purpose; that is, in each series, besides the term which is independent of ϵ, that one which contains its first power is known*.

VI. *Final Conclusions.*

§ 23. Having in the preceding pages ascertained the value of the necessary coefficients, we may now proceed to draw conclusions from the equations that have been established, as to the condition of the gas and the conduction of heat taking place within it.

In § 16 we found that q must be a constant quantity; and if, instead of q, we put its value, we may accordingly write

$$\frac{1}{N}\frac{du}{dx}=\text{constant}.$$

The same section further teaches us that

$$Nu^2=\text{const.};$$

and by multiplying these two equations, we have

$$u^2\frac{du}{dx}=\text{const.} \quad \ldots\ldots\ldots \quad (52)$$

But, since the quantity u^2 is proportional to the absolute temperature T, we may put

$$u=\text{const.}\sqrt{T},$$

and hence the last equation becomes

$$\sqrt{T}\frac{dT}{dx}=\text{const.} \quad \ldots\ldots\ldots \quad (53)$$

By integrating this equation, an equation of the following form

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* In the terms of the second degree we meet with the quantities q_1^2, r, and r', which can be determined in the same way as q by carrying out the calculations further. Without here dwelling upon this extension of the calculations, which presents no difficulty whatever in regard to principle, I will merely quote the values of these quantities which are so arrived at: namely,

$$q_1^2=\frac{41}{25}q^2,$$

$$r=-\frac{31}{50}\frac{q^2}{u},$$

$$r'=\frac{266}{17\cdot 25}\frac{q^2}{u^2}.$$

is obtained,

$$T^{\frac{3}{2}}=Cx+C_1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (54)$$

where C and C_1 are constants.

The quantity of gas enclosed betweeen two surfaces of given temperatures does not, therefore, as might perhaps be supposed at first glance, assume such a condition that the temperature is a linear function of the abscissa; but the alteration of temperature from one limiting surface to the other takes place according to a somewhat more complicated law, inasmuch as the power $T^{\frac{3}{2}}$ is represented by a linear function of the abscissa.

When the constants C and C_1 in equation (54) are determined by aid of the given temperatures of the limiting surfaces, the temperature can be calculated for every other point of the gas. And since, further, the product of temperature into density must remain constant within the gas if the density be given for any one point, it can be calculated from the temperature for every other point. Accordingly, the condition of the gas is fully known so far as regards temperature, density, and pressure.

§ 24. By introducing into equation (VIII.) the value that has been found for q, we obtain the following equation for G, the conduction of heat within the gas:

$$G=-\frac{5}{12}kmN_0u^2\frac{du}{dx}\epsilon^{*}. \quad . \quad . \quad . \quad (XIII.)$$

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* Maxwell (Phil. Mag. S. 4. vol. xx. p. 32) gives the following expression for the *vis viva* which passes in the positive direction through a superficial unit of a plane perpendicular to the axis of x during a unit of time,

$$G=-\frac{1}{3}\frac{d}{dx}\left(\frac{1}{2}kmu^2Nul\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (A)$$

where l denotes the mean length of excursion of the molecules which corresponds to the density of the gas at the place under consideration. Substituting for l its value

$$l=\frac{N_0}{N}\epsilon,$$

we have

$$G=-\frac{1}{3}\frac{d}{dx}\left(\frac{1}{2}kmN_0u^3\epsilon\right)=-\frac{1}{2}kmN_0u^2\frac{du}{dx}\epsilon.$$

This expression differs from that given above only by containing $\frac{1}{2}$ in place of $\frac{5}{12}$. But if we trace the way in which Maxwell arrives at equation (A), we shall find that this near accordance of his result with mine is only apparent.

Denoting the mass of gas which passes in a positive direction through the unit of surface during a unit of time by E, Maxwell establishes the

For the sake of greater convenience we will still somewhat alter the form of this equation. If we denote the velocity of the molecules in the normal condition of the gas by u_0, and the absolute temperature by T_0, we have

$$\frac{u^2}{u_0^2}=\frac{T}{T_0},$$

and thence

$$u=\frac{u_0}{\sqrt{T_0}}\sqrt{T}. \quad \ldots\ldots \quad (55)$$

The foregoing equation thus becomes

$$G=-\frac{5}{24}\frac{kmN_0u_0^3\epsilon}{T_0}\sqrt{\frac{T}{T_0}}\frac{dT}{dx}. \quad \ldots \quad (56)$$

If we assume the freezing-point as the temperature of the gas in its normal condition, $T_0=273$ nearly, and $T=273+t$, where t

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following equation (*loc. cit.* p. 23),

$$E=-\frac{1}{3}\frac{d}{dx}(mNul). \quad \ldots\ldots\ldots \quad (B)$$

Then, in order to obtain the *vis viva* which traverses the plane instead of the mass, he simply substitutes in this equation the *vis viva* of a molecule, $\frac{1}{2}kmu^2$, for the *mass* of a molecule m, and so obtains equation (A). If we now consider equation (B) more closely, and substitute there also its value $\frac{N_0}{N}\epsilon$ for l, we get

$$E=-\frac{1}{3}\frac{d}{dx}(mN_0u\epsilon)=-\frac{1}{3}mN_0\frac{du}{dx}\epsilon.$$

This equation proclaims that, if the temperature of the gas varies in the direction of x so that $\frac{du}{dx}$ has an appreciable value, a progressive movement of the mass in the direction of x must take place, inasmuch as more molecules pass through the plane in one direction than in the other. It is therefore contradictory of the supposition which we must make when we speak of the conduction of heat; for we understand by conduction of heat a progressive movement of the heat *without a progressive movement of the mass.*

Independently, therefore, of the question whether equation (B) is admissible or not, we are forced to one of the following conclusions: in establishing his equations, Maxwell either had in view a state of things quite different from what we presuppose in speaking of conduction of heat, namely, such a state that the gas has a progressive movement in a particular direction, in which case his equation (A) does not express what we understand by conduction of heat, and what is expressed by my equation (XIII.), but *a motion of heat accompanied, and partly occasioned, by a motion of mass*; or else he really intended to represent the condition in which a movement of heat takes place unaccompanied by a movement of mass, in which case the equation (B) is wrong, and the equation (A) deduced from it is only approximately correct because two errors have partially neutralized each other.

denotes the temperature reckoned from the freezing-point. If we further represent the coefficient of expansion of the permanent gases, namely $\frac{1}{273}$, by α, as is usually done, we can write

$$G = -\frac{5}{24}\frac{kmN_0 u_0^3 \epsilon}{273}\sqrt{1+\alpha t}\,\frac{dt}{dx}. \quad . \quad . \quad (\text{XIV.})$$

Lastly, if we introduce here the symbol K with the value

$$K = \frac{5}{24}\frac{kmN_0 u_0^3 \epsilon}{273}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{XV.})$$

our equation reads

$$G = -K\sqrt{1+\alpha t}\,\frac{dt}{dx}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{XVI.})$$

§ 25. The factor K contains only magnitudes which relate to the normal condition of the gas, and is therefore only a constant dependent on the nature of the gas under consideration. Accordingly, the form of the last equation enables us at once to draw two general conclusions.

First. *For a given value of* $\frac{dt}{dx}$, *the conduction of heat increases with the temperature which the gas has at the place under consideration.* This increase takes place in the same ratio as the increase in the velocity of sound by rise of temperature, namely, proportionally to the quantity $\sqrt{1+\alpha t}$.

Secondly. *The conduction of heat is not affected by the pressure to which the gas is exposed.* This is explained by the circumstance that, although the number of molecules which can convey the heat is greater in a gas which is rendered more dense by increased pressure, the distances traversed by the individual molecules are smaller. This latter conclusion might lead to absurdity if it were assumed to be applicable to the gas under every conceivable condition of compression or expansion. It must, however, be borne in mind that there are obvious limits to the application of it to conditions of the gas which depart very much from the mean condition: on the one hand, the gas must not be so much compressed as to produce a too great departure from the laws of permanent gases which have been taken as the foundation for the whole course of reasoning; and on the other hand, it must not be so much expanded that the mean length of excursion of the molecules becomes so great that its higher powers cannot be disregarded.

§ 26. It will be necessary, for the numerical calculation of the above formula, to return once more to the point mentioned in § 7, namely, the *accidental* variations of the velocity of the molecules which occur even when the temperature and density of the gas is uniform throughout.

Accordingly we must not attribute to the quantity u (which occurs in the formulæ for the motions of the molecules, and represents their velocity for the case where no variations of temperature and density occur) a fixed value applicable in the case of all the molecules, but different values which vary in many ways from one molecule to another. The same thing holds also for other magnitudes which are dependent on the velocity of the molecules,—*e. g.* for the length of excursion s, which we meet with in §§ 8 *et seq.*, and whose value must be on the average somewhat greater in the case of molecules whose velocity is greater, than in the case of those that have a less velocity. We have then now to find mean values for these quantities, so far as they occur in the formulæ, which must be determined in such a way that by their employment the values of the formulæ remain the same as those which would be obtained by taking into calculation the actual velocity of each molecule.

In order to be able rightly to calculate these mean values, we must know the law which regulates the various velocities which occur. As I have already stated above, such a law was established by Maxwell, and it might perhaps be employed for calculating the mean values*. I prefer, however, not to discuss this subject here, as a few remarks concerning this law would be required which would lead us too far at present; and I feel the more justified in leaving this point, since the numerical value of ϵ is so imperfectly known that the accurate numerical calculation of a formula in which it occurs is not possible. I will therefore content myself, in the calculation of the conduction of heat, with

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* I must here remark that this calculation would not be quite so simple as might perhaps appear at first sight. For a point must be attended to which has already been remarked upon in a similar connexion above, namely, that the mean value of a power of u is not the same thing as the corresponding power of the mean value of u; and the same thing is true for other quantities which depend upon u, or for products into which such quantities enter. The consideration of the following series of expressions, for example (a horizontal stroke being used, as before, to denote the mean values),

$$\overline{u^2},\quad (\overline{u})^2,\quad \overline{au^2}+(1-a)(\overline{u})^2,\quad \frac{\overline{u^{n+2}}}{\overline{u^n}},\ \&c.,$$

plainly shows that if all the values of u which occur in them were equal, they would take the common form u^2; whereas if the value of u is not everywhere the same, they are not equivalent to each other. If, therefore, in any formula which is deduced upon the supposition of u having always the same value, u^2 should occur, we cannot be at once certain which of the mean values indicated above ought to be taken, but, in order to decide, we must trace the whole development of the formula.

employing in the above formula, which is deduced without considering the accidental variations, a mean value for the velocity which is easily arrived at, and which, though not strictly accurate, may still be regarded as sufficiently so, considering the uncertainty which still prevails in regard to the value of ϵ.

§ 27. We will employ that mean value of u which gives the same *vis viva* as the velocities which actually occur. This value may be obtained by taking the arithmetical mean of the squares of the velocities, and extracting therefrom the square root.

In this case the product $\frac{1}{2}kN_0mu_0^2$ has a simple meaning. It represents, namely, the *vis viva*, or, in other words, the quantity of heat contained in a unit of volume of the gas in its normal condition. If γ stands for the specific heat of a unit of volume of the gas, the volume being kept constant, γT_0 will represent this quantity of heat; or if the freezing-point be taken as the normal temperature T_0, it will be represented approximately by $\gamma . 273$; whereby equation (XV.) becomes

$$K=\tfrac{5}{12}\gamma u_0\epsilon; \quad \dots\dots \quad (57)$$

and if γ be expressed in common heat-units, the conduction of heat is also expressed in common heat-units by employing this formula. The magnitude u_0 may be deduced as follows from the formula which I formerly* established for the moving velocity of the molecules,

$$u_0=\frac{485^m}{\sqrt{\sigma}}, \quad \dots\dots \quad (58)$$

where σ denotes the specific gravity of the gas in question compared with atmospheric air. The foregoing equation is thus transformed into

$$K=202{\cdot}1\frac{\gamma}{\sqrt{\sigma}}\epsilon. \quad \dots\dots \quad (XVII.)$$

For the simple permanent gases, and such compound gases as suffer no contraction on the combination of their elements, the specific heat γ is the same as for atmospheric air; and if a cubic metre, which contains 1·2932 kilog. atmospheric air in the normal condition, be taken as our unit of volume,

$$\gamma=0{\cdot}1686 . 1{\cdot}2932=0{\cdot}21803. \quad \dots \quad (59)$$

By employing this value, we get for the gases mentioned

$$K=\frac{44{\cdot}06}{\sqrt{\sigma}}\epsilon. \quad \dots\dots \quad (XVIII.)$$

Hence for the three simple permanent gases and for atmospheric air, which must be treated as a simple gas in relation to the

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* Phil. Mag. S. 4. vol. xvii. p. 124.

conduction of heat, we obtain the following values for K; namely,

For atmospheric air . . .	$44{\cdot}06\,.\,\epsilon$
For oxygen	$41{\cdot}90\,.\,\epsilon$
For nitrogen	$44{\cdot}71\,.\,\epsilon$
For hydrogen	$167{\cdot}49\,.\,\epsilon$

The complete numerical determination of these values requires that the factor ϵ should be known. A direct theoretical calculation of this quantity, according to the principles developed above, is not possible, because for this it is necessary to know the radius of the sphere of action ρ. We must therefore make use of other data for the determination of ϵ. Maxwell has calculated the mean length of excursion of the molecules from the result of experiments on the friction of air in motion and on the diffusion of gases, and in both cases has arrived at figures which do not differ much from

$$\frac{1}{400{,}000}\text{ English inch, or }\frac{1}{16{,}000{,}000}\text{ metre.}$$

Without giving any opinion here as to the degree of confidence which may be placed in this number, I am nevertheless of opinion that we may employ it to give us an approximate idea of the kind of magnitudes with which we have to do. Putting this value into the above equation, we get for atmospheric air

$$K=\frac{44}{16{,}000{,}000}=\frac{11}{4{,}000{,}000}. \quad . \quad . \quad . \quad (60)$$

This quantity denotes the quantity of heat, expressed in common heat-units, which would traverse a plane of one square metre during one second, if $\frac{dt}{dx}$ were equal to -1; that is, if the temperature decreased in the direction of the axis of abscissæ near the point under consideration in such wise that, if a similar decrease took place throughout a greater length, the temperature would diminish 1° C. in the length of 1 metre.

§ 28. In order to compare this conducting-power for heat with that of the metals, we may make use of a result observed by Peclet, who found, by measurement of the quantity of heat which passed through a plate of lead, that, if a large mass of lead were placed under such circumstances that a diminution of temperature of 1° C. took place in a thickness of 1 metre, a quantity of heat equal to 14 heat-units would then pass through a surface of 1 metre square in one hour*. To compare this number with that found for air, we must multiply the latter by the number of seconds contained in an hour, it having been

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* *Traité de la Chaleur*, vol. i. p. 391.

calculated for 1 second; we thus get

$$\frac{11\times3600}{4{,}000{,}000}=\frac{1}{100}.$$

This calculation leads therefore to a conducting-power for heat which is 1400 times smaller than that of lead*.

If this number can but lay claim to a small degree of accuracy, so that it can only be regarded as an approximation to the truth, we may at least regard it as proved that the conducting-power of gases for heat, which can be theoretically deduced from the hypotheses respecting the molecular motions of gases which lie at the foundation of this memoir, is much less than that of the metals—a result which entirely accords with observation. The objection that this hypothesis involved so rapid a distribution of heat that local differences of temperature within the gas are impossible, is accordingly completely without foundation. We may even quote, as a fresh argument in favour of this hypothesis, the very phenomenon which has been urged with such particular emphasis against it.

§ 29. The expressions which have been found for K admit, further, of an approximate comparison with each other of the various gases in respect of their conducting-power for heat.

In the expression (XVII.) the specific gravity σ is sufficiently well known; and the specific heat γ can be approximately calculated from the experiments of Regnault. If we consider apart the simple gases and such compound gases as possess the same volume as their constituents before combination, we may, as has been said, assume that γ is the same for all; and the expression for K thus assumes the form (XVIII.), which contains nothing but the fraction $\frac{\epsilon}{\sqrt{\sigma}}$ in addition to the numerical factor.

The quantity ϵ, the mean length of excursion of the molecules, is not necessarily equal for different gases, and we do not know what proportion the lengths of excursion in various gases hold to each other. Nevertheless there is no obvious reason to

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* Maxwell arrived at quite a different result, namely, that atmospheric air conducts ten million times worse than copper. This was, however, caused only by the occurrence of two oversights in his numerical calculations. In the first place, instead of Peclet's numbers, which express the conducting-power of the metals in French measures, he employs numbers calculated from them by Rankine (Manual of the Steam-engine, p. 259) in order to express the conducting-power in English measures. These numbers, however, are not quite correct; they still require to be multiplied by 0·4536, the ratio of the English pound to the kilogramme, in order to make them correspond with Peclet's numbers. Maxwell has further employed the numbers which relate to one hour as the unit of time as though they were calculated for one second.

assume that it is shorter for light gases than for heavy gases; for it is inversely proportional to the radius of the sphere of action, and it would be difficult to assign a greater sphere of action to the lighter than to the heavier molecules. If, accordingly, ϵ is not smaller for the lighter gases, the fraction $\frac{\epsilon}{\sqrt{\sigma}}$, and therefore the conducting-power for heat, must be greater in this case than in the case of the heavier gases.

This result accords perfectly with the results of observation hitherto obtained, and especially with those of the beautiful investigations by Magnus, in which he avoided the currents of gas which, in the experiments of Dulong and Petit, existed at the same time as the conduction of heat properly so called. From these experiments we see very plainly that the lightest gas (hydrogen) conducts considerably better than the other gases.

§ 30. We may sum up as follows the conclusions at which we have arrived.

1. Gases conduct heat considerably worse than the metals. An approximate calculation based upon the mean length of excursion of the molecules, as deduced by Maxwell, gives, for the conducting-power of atmospheric air near the freezing-point, a number which is 1400 times smaller than that which represents the conducting-power of lead.
2. The conducting-power for heat depends on the temperature of the gas, and increases in the same ratio as the velocity of sound.
3. The conducting-power for heat is, within certain limits, independent of the pressure to which the gas is subjected.
4. Light gases conduct heat better than heavy gases. Hydrogen must therefore conduct heat considerably better than any other gas.

Zürich, October 1861.

LXXII. *Contributions to the Metallurgy of Lead.* By William Baker, *Associate of the Government School of Mines, F.C.S.*[*]

IT was shown, in a paper published in the 'Chemical Gazette,' October 1, 1856, that Pattinson's process of concentrating silver in lead by crystallization accomplished at the same time a separation of copper, the latter being found in the larger proportion in the fluid lead which had been drained from the crystals. It was supposed therefore that this method offered a valuable means of preparing an inferior quality of lead for those purposes in the arts for which a metal of the greatest purity attainable is required.

[*] Communicated by the Author.

It might seem to the uninitiated that the lead then operated upon was already sufficiently pure for all practical purposes, the quantity of copper it contained amounting to only 0·0154 per cent., or 5 oz. 0 dwt. 14 grs. per ton. Neglecting the small quantities of iron, sulphur, and silver, we will confine our attention to the copper, which is the most objectionable impurity that has to be removed.

For a long time certain "brands" of lead have been preferred to all others for such purposes as making white lead and glass-maker's red lead. We may instance the lead made from the Snailbeach Mines in Shropshire, which has enjoyed a reputation for making good red lead for glass-makers. The best selected lead of the Northumberland district has been also much sought after by the manufacturers of white lead. Happily we are now emancipating ourselves from the fashion of ascribing peculiarities in the smelted metal to the "nature of the ore." This term is perhaps useful enough in the mouth of the practical smelter when explaining how the charge is worked in the furnace, but in all cases it may be translated into more precise language by a due methodical and scientific inquiry. As the most remains to be known about iron of all the common metals, it is exceedingly probable the "nature of the ore" will have for some time to come to account for the nature of the pigs introduced.

A very small quantity of oxide of copper in the red lead is found to impart a bluish shade to flint-glass. In some cases it can hardly be pronounced blue, but at least the glass is wanting in the pure watery lustre which is the perfection of "cristal." It is almost incredible that so small a proportion as 3 oz. per ton, or 0·009 per cent., should impart an undesirable tint; yet the evidence upon this point is conclusive. Some facts in reference to white-lead-making will, however, quite corroborate this statement. In making carbonate of lead by the method of fermenting tanners' bark and acetic-acid vapour, the corroded or converted lead often presents a delicate pink tint. Close observation has shown me that this is invariably connected with the presence of copper. Where the air has had more free access, the pink colour disappears, or is replaced by a far more delicate blue, indicating the passage from suboxide of copper (Cu^2O) to protoxide of copper (CuO). Finally, pure lead specially prepared, which gave repeatedly pure-white corrosions on receiving the addition of a very small proportion of copper, exhibited the characteristic pink tint when again submitted to the action of the corrosive vapours. Providing the lead is otherwise pure, a proportion of only 2¼ ozs. per ton, or 0·0071 per cent. of copper, is sufficient to produce a delicate but decided pink hue. Should antimony or sulphur be present, the colour is somewhat masked and a dull-coloured white lead is produced. That the presence of iron has nothing whatever to do with this appearance, is manifest from the fact that the lead containing only an inappreciable trace of copper which gave pure white corrosions, contained quite as much iron as the specimens which afforded pink corrosions.

Proceeding upon the results given above, Derbyshire lead, which, when properly smelted, contains from 2 ozs. to 5 ozs. of copper per ton, was crystallized three or four times and produced remarkably pure lead. Numerous analyses have confirmed the fact that, in dealing with a metal containing up to 5 ozs. or perhaps 7 ozs. per ton, the copper is always concentrated along with the silver. But a most remarkable fact was discovered upon applying this method of purification to lead containing above 10 ozs. of copper per ton.

Five tons of lead, containing 0·0774 per cent. of copper, or 25 ozs. per ton, were submitted to Pattinson's process. At the fourth operation the following was the distribution of copper in the charge:—

Crystals	0·0574	per cent. copper
Fluid lead drained from the crystals .	0·0526	„ „

At the sixth crystallization,—

Crystals	0·0642	per cent. copper
Fluid lead	0·0570	„ „

proving that no concentration of copper in the fluid portion had taken place. The lead, which was otherwise soft and fit for all ordinary purposes, such as rolling into sheets and making pipes, possessed a surface unmistakeably different from that of the purest lead, the most marked difference being a somewhat irregular depression or crumpling in a line along the direction of the length of the mould. Other experiments with lead containing various proportions of copper have showed that when the quantity is above a certain limit, which can only be more accurately defined when a larger number of analyses have been made, the crystallization process cannot be economically employed. It still remains, however, a useful adjunct to refining-operations, when the lead operated upon has been smelted from ore carefully selected to exclude the more coppery kinds. Lead-smelters, besides, might do much for the purity of lead if their charges were worked in such a manner as that, with good ore, by keeping the temperature as low as possible, even at the sacrifice of the yield of metal, most of the copper would go into the slags. These would yield equally good common lead; and the reputation of the smelter would be so much increased by the higher degree of purity of the selected lead. It is to be feared that, in the attempt to get the utmost out of the ores at the "*first*" operation, they have often deteriorated from the quality of metal produced in former times.

LXXIII. *On an Air-Thermometer used as a Pyrometer in measuring High Temperatures.* *By* M. V. Regnault*.

[With a Plate.]

THE pyrometers which from time to time have been proposed for the measurement of high temperatures in industrial furnaces, have not hitherto received any important application. Those which depend upon the expansion or on the increase in elastic force of air confined in a closed vessel, are apparatus both difficult and expensive to construct, and can only be used by observers well practised in delicate manipulations.

The pyrometers whose indications are based on the apparent expansions of two metals, or on that of a metal as compared with a rod of baked clay or porcelain, which is supposed to be unalterable, could in no case be used other than as *pyroscopes*, to see if the same furnace has been raised to the same temperature in successive operations. But it is difficult to graduate these instruments, and even to compare them with a normal instrument so as to be able to transform their indications into degrees of our thermometric scale. Lastly, the same instrument undergoes, when exposed to high temperatures, permanent alterations which affect the scale and prevent any comparison of its indications.

Wedgewood's pyrometer, which depends on the contractions which the same clay undergoes at different temperatures, can also only be used as a pyroscope; but it is still more defective in principle. The contraction which the same clay undergoes for the same increase of temperature depends on the degree of compression to which it has been exposed in the crude state, on the more or less rapid increase of temperature, on the more or less prolonged action of heat.

I proposed in 1846 (*Mémoires de l'Académie des Sciences*, vol. xxi. p. 267) an apparatus easy of manipulation, by which the temperature of any part of a furnace may at any given moment be obtained with sufficient accuracy. The apparatus consists of a kind of flask A, fig. 5, Plate II., either cylindrical or spherical, of from $\frac{1}{2}$ to 1 litre in capacity, and which may be either of cast or wrought iron, of platinum or of porcelain; the mouth *a b* is closed by a plate *c d* containing an aperture *o*. From 15 to 20

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* Translated from the *Annales de Chimie et de Physique*, September 1861.

grms. of mercury are added to this flask, which is then placed in the furnace in the part the temperature of which is to be known. The mercury soon boils, its vapour expels the air, which escapes by the orifice *o*; the excess of mercurial vapour emerges by the same issue. When the apparatus has acquired the temperature of the furnace, the plate *c d* is taken out in order to close the orifice *o*, the flask is withdrawn and made to cool rapidly. The mercury which remains in the flask is weighed; it is removed by the addition of water and agitation. The mercury can be weighed directly; or if it contains impurities, it is dissolved in acid and estimated as precipitate.

The neck may also have the form of fig. 6. The tubulure then terminates in a hollow conical part, and in its orifice is a ball B of the same material as the flask. This does not close it hermetically, but it prevents the currents of air of the furnace from disturbing the mercurial vapour in the interior of the flask.

Let

V be the capacity in cubic centimetres of the flask at 0° as given by the weight of water which fills the flask;

k be the coefficient of cubical expansion of the substance of which it is made;

H be the height of the barometer at the moment at which it is withdrawn from the furnace;

h the difference between the pressure in the interior of the furnace and of the surrounding atmosphere. This difference may often be neglected, but it can readily be determined by means of a water manometer.

H_0 the height $H-h$ reduced to 0°;

δ the *theoretical* density of mercurial vapour compared with that of air under the same circumstances of temperature and pressure; that is, the density of this vapour taken at temperatures starting from which it no longer changes; in other terms, higher than that, starting from which, mercurial vapour and air follow the same laws of expansibility and compression. It is, moreover, clear that it is only above this temperature that the pyrometer can give exact temperatures.

p the weight of the mercury which remains in the flask at the end of the experiment.

The weight of mercurial vapour which fills the flask at the maximum temperature x is

$$V \cdot \frac{1+kx}{1+\alpha x} \cdot 0{\cdot}0012932 \cdot \delta \frac{H_0}{760};$$

we have then

$$V \cdot \frac{1+kx}{1+\alpha x}\ 0{\cdot}0012932 \cdot \delta \frac{H_0}{760} = p,$$

from which

$$\frac{1+kx}{1+\alpha x}=\frac{760}{\mathrm{V}.0{\cdot}0012932.\delta}\cdot\frac{p}{\mathrm{H}}=\mathrm{M}\frac{p}{\mathrm{H}},$$

M being a quantity which is constant for the same flask.

From which is obtained

$$x=\frac{1-\mathrm{M}\frac{p}{\mathrm{H}}}{\mathrm{M}\alpha\frac{p}{\mathrm{H}}-k},$$

$$\alpha=0{\cdot}00367.$$

The method here described may be used in many cases, but it requires that the introduction and removal of the apparatus from the furnace takes place without detriment to the operation. I have devised another apparatus, which has the advantage over this of always remaining in one place, and of serving as often as necessary to measure the variable temperatures of the same furnace. I proposed to employ it in determining the temperatures at which enamels and the different kinds of painting on porcelain are baked at the Imperial Manufactory of Sèvres.

The apparatus consists of a tube, A B, fig. 7, of wrought iron, the length of which varies according to the extent of the space whose mean temperature is to be determined. Its internal diameter varies from 2 to 5 centimetres; it is more when the tube is somewhat long. This tube is closed at both ends by iron discs which are screwed and braced, each of which is provided with an iron capillary tube *a b*, *c d* passing through the side E E′ of the furnace. To construct these tubes, a very soft and frequently reheated soft iron cylinder, perforated by a longitudinal aperture 3 or 4 millims. in diameter, is drawn out in the drawing-frame. Each of these capillary tubes terminates outside the furnace in a three-way stopcock, R R′. By means of the stopcock R, the large tube A B can be successively connected with either of the two tubulures *e* and *f*. By the stopcock R′ the same tube communicates with either of the tubulures *g*, *h*. The metallic tubulure *h* is soldered to the end of a copper tube C, filled with oxide of copper.

When the temperature of the furnaces is to be determined at a given moment, the stopcocks R and R′ are placed in the positions represented by fig. 7; by means of a caoutchouc tube, the tubulure *f* is connected with an apparatus which furnishes a constant supply of dried and purified hydrogen; the hydrogen expels the air of the tube A B through the tubulure *g*, which remains open. The disengagement of hydrogen is continued until the air is completely expelled: any oxide which might exist on the inner side of the tube A B will be reduced to the metallic state.

The stopcock R is then turned into the position 2 (fig. 8), the hydrogen-apparatus is detached from the tubulure *f*, and the tubulure *e* is connected with an apparatus which disengages, when convenient, dry air with a velocity which can be regulated. This apparatus, represented by fig. 9, consists of a tubulated flask V, into which water passes from a higher reservoir X, through a leaden tube *a b*, provided with a stopcock *r*, by which the flow of water can be regulated. The air of the flask V traverses a tube *c d*, filled with pumice saturated with strong sulphuric acid. When the stopcock r' is closed, the air in the flask V is under a higher pressure than that of the atmosphere, for it supports in addition the pressure of the column of water *a b*.

Thus, when the temperature of the furnace is to be determined, the tube A B is filled with hydrogen under the pressure of the atmosphere H_0, and at the unknown temperature x. Further, the copper tube C, filled with oxide of copper, is heated to redness by a row of gas-burners; lastly, its second tubulure, *i*, communicates with a U-tube, filled with pumice saturated with sulphuric acid, and which has been previously weighed. The stopcock R is in the position 2 (fig. 8), and the stopcock R′ in the position 3 (fig. 8). The stopcock R′ is opened for a moment to expel the hydrogen of the tubulure *e f*; R′ is then placed in the position 1 (fig. 8), finally in the position 3 (fig. 8). The stopcock r' of the air-vessel is then carefully opened (fig. 9); dry air passes slowly into the large tube A B; expels from it the hydrogen, which partially burns, and drives the remainder over the hot oxide of copper, where it burns completely; the water arising from this combustion condenses in the sulphuric acid tube S. The current of air is continued until the hydrogen and aqueous vapour are completely expelled from the tube A B. The reduced oxide of copper reoxidizes in this current of air.

Let

V be the capacity of the apparatus at 0° in cubic centimetres.

δ the density of hydrogen as compared with air.

α the coefficient of expansion of this gas.

k the coefficient of cubical expansion of the metal of which the tube A B is made.

P the weight of water collected in the tube S.

We shall have

$$V\frac{1+kx}{1+\alpha x}\,.\,\delta\,.\,0{\cdot}0012932\,.\,\frac{H}{760}=P\frac{12{\cdot}50}{112{\cdot}50}.$$

In a preliminary experiment, made in the same apparatus before the tube A B had been arranged in the furnace, A B was surrounded by melting ice; and, working exactly in the manner described above, a determination was made of the weight P′ of water furnished by the hydrogen which filled the apparatus at 0°, and under the pressure H′. There was obtained thus:

$$V.\delta.0{\cdot}0012932\frac{H'}{760}=P'\frac{12{\cdot}50}{112{\cdot}50}.$$

Dividing the first of these equations by the second, we get

$$\frac{1+kx}{1+\alpha x}=\frac{P}{H}\cdot\frac{H'}{P'}.$$

$\frac{H'}{P'}$ is given by experiment with melting ice; it remains the same for all determinations of high temperatures; I represent it by M; I have then

$$\frac{1+kx}{1+\alpha x}=M\frac{H'}{P'},$$

whence

$$x=\frac{1-M\frac{H'}{P'}}{M\frac{H'}{P'}\alpha-k}.$$

The determination of a temperature by this method requires very little time, and the apparatus is ready for the next experiment. There is therefore every facility for studying the ascending or descending course of the temperature of furnaces.

LXXIV. *On the Oxidation of Gaseous Hydrocarbon-compounds contained in the Atmosphere.* By H. Karsten[*].

MY experiments on the oxidation of organic substances containing carbon, published in Poggendorff's *Annalen*, vol. cix. p. 346, proved that these bodies combine, at the ordinary temperature, with the oxygen of the air to form carbonic acid and water, and that the presence of nitrogenous substances, which chemists have been hitherto accustomed to think requisite in order to set up the process of decay, is of no consequence, since even pure carbon is oxidized to carbonic acid by exposure to the air at the common temperature, just as it is at higher temperatures, only more slowly.

These experiments further showed that organic hydrocarbon-compounds are also oxidized under water, with formation of carbonic acid, if air has sufficient access to them, more rapidly than in the dry state. When under these circumstances the access of air is insufficient, they rot, that is, they yield, besides

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[*] Translated by G. C. Foster, B.A., from Poggendorff's *Annalen*, for 1862.

carbonic acid, gaseous hydrocarbons and other volatile products, as yet for the most part only imperfectly known. When oxygen is completely excluded, they remain unchanged under water[*].

After the completion of my former investigation, it still remained to determine the manner in which the gaseous hydrocarbons produced by the process of putrefaction, and the other gaseous and solid organic bodies (odorous substances, &c.) which are also present in the air, behave when they are diffused through the atmosphere—a problem of the greatest importance in relation to the vital process of the animal organism.

The results of my former experiments rendered it probable that these hydrocarbons would be acted on by contact with free oxygen in the same way as others, but it still seemed desirable to confirm this supposition by experiment.

Two methods presented themselves for the attainment of this object, which, as supplementary to each other, required both to be carried out.

In the *first* place, atmospheric air which still contains volatile hydrocarbons, when passed through a series of vessels filled alternately with air and with lime-water, must give up carbonic acid to the lime-water until all the hydrocarbons contained in it are oxidized; and the quantity of carbonic acid deposited in the first vessel must be greater than the quantity deposited in those which follow, if the intervening spaces filled with air are of equal size.

In the *second* place, if atmospheric air laden with hydrocarbons is heated to redness, it must deposit carbonic acid only in the first vessel containing lime-water or potash, and, when thus freed from carbonic acid, it ought not to deposit anything in the succeeding vessels. Experiments had already been made according to the first of these two methods by Messrs. C. W. Eliot and Frank H. Storer; but as these chemists had in view the solution of another problem, and as I did not know whether in their experiments, which were carried out very carefully in other respects, the contact of the air operated upon with the organic substances, such as cork and caoutchouc, used for connecting the various parts of the apparatus was avoided (a circumstance which must necessarily have affected the result), I repeated the experiments myself.

In doing so I made use of a series of vessels similar to one another, one of which is represented in the accompanying figure, connected together in such a way that the air in passing through

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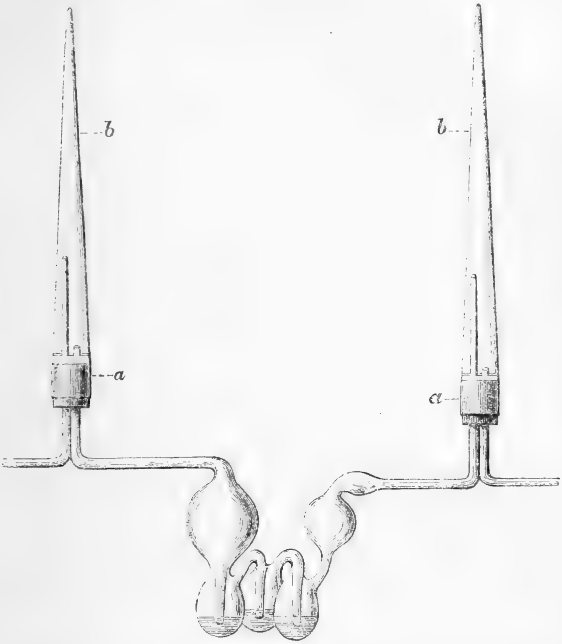
[*] During the course of the experiments here published, the evolution of carburetted hydrogen and carbonic oxide gases by living plants was observed by Boussingault (*Compt. Rend.* Nov. 1861); and the presence of a large quantity of solid organic matter in the air, which remains suspended when the air is in motion, was pointed out by Pasteur (*Annales de Chimie et de Physique,* 3rd series, vol. lxiv. p. 24, January 1862).

them never came in contact with organic matter. This was effected by covering the corks *a* with a layer of mercury of sufficient thickness.



The mercury was introduced through the drawn-out point of the tube *b*, by means of another tube drawn out to a still longer and finer point, and serving as a funnel.

Before filling the apparatus with lime-water the air contained in it was replaced by air free from carbonic acid, and in like manner the tubes *b*, which serve to connect the different vessels containing lime-water, were filled with air deprived of carbonic acid immediately after being placed on the corks and before the mercury was poured in. After the introduction of the mercury, air free from carbonic acid was again passed into them, in order to displace any common air which might have got in during the pouring in of the mercury, and they were then immediately closed by quickly melting off the drawn-out points.

In order to exclude any possibility of the leakage inwards of common air at any accidental orifice, the air experimented upon was not drawn, but pressed, through the apparatus*.

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* The whole apparatus was, moreover, found to be perfectly air-tight before beginning the experiment, and no leakage arose during the course of it.

In the first experiment, that of leading through the apparatus atmospheric air which had not been heated to redness, the supposition with which I started required that a certain amount of carbonate of lime should be deposited in each vessel, but especially in the first, and, further, that the vessel following a large tube of 200 cubic centims. capacity, which was substituted for one of the small tubes *b*, should contain more carbonate of lime at the end of the experiment than the vessel immediately preceding it.

120 litres of air were pressed through the apparatus, a single bubble at a time, so slowly that about 5 litres went through in twelve hours.

The air used in the experiment was freed from carbonic acid by being passed through three vessels containing hydrate of potash before it came in contact with the lime-water.

As I have already pointed out*, the white opake precipitate of carbonate of lime does not form (especially in the cold) on the sides of the glass tube where it dips into the lime-water, if only a very small quantity of carbonic acid is contained in the gas which bubbles through the liquid, but only crystals of hydrated carbonate of lime, which collect at the bottom and against the sides of the glass bulbs together with a little gelatinous-looking hydrate of lime. And since the air employed in this case was very strongly dried by being previously passed through a concentrated solution of potash, it carried away water from the saturated solution of caustic lime, and so occasioned the precipitation of a certain quantity of hydrate of lime.

When 60 litres of air had been passed through the apparatus, the first tube of the lime-water-vessel immediately following the large tube filled with air showed already the well-known deposit of carbonate of lime, whereas all the other tubes in the apparatus remained free from it even at the end of the experiment. This vessel also contained a larger quantity of crystallized carbonate of lime than the others, in which, however, were distinct (though not weighable) traces of it, recognizable by the evolution of gas bubbles when a few drops of hydrochloric acid were brought in contact with it after the lime-water had been removed by a stream of air free from carbonic acid.

The result of this one experiment is not alone sufficient to prove the accuracy of the supposition that a continual oxidation of compounds of carbon and hydrogen goes on in the atmosphere. It might be urged that the potash solution was not sufficient to keep back the whole of the carbonic acid already existing in the air, or even that it is altogether impossible to free air completely from carbonic acid by means of lime-water or of solution of

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* Poggendorff's *Annalen*, vol. cix. p. 349.

potash. The latter opinion has in fact been maintained by very able chemists, for example, by Messrs. Eliot and Storer; and it was with a view of proving the accuracy of it that they undertook the experiments which they have published in the 'Proceedings of the American Academy of Arts and Sciences' for September 1860.

As has been already stated, these chemists do not appear to have observed the precautions necessary for keeping the air from contact with organic matter during its passage through the apparatus, so that the result of their experiments is affected by a twofold source of error*. The first of these sources of error was avoided in my experiments by disposing the apparatus in the manner already described. The presence of the second was demonstrated by my second experiment, which consisted in passing the air, before allowing it to enter the apparatus where it was washed with potash and lime-water, through a red-hot platinum tube, 1 metre long and 15 centimetres wide, filled with oxide of copper, so as to convert any volatile compounds of carbon and hydrogen which might be contained in it into carbonic acid and water.

In this experiment, as in the first, the heated air to be experimented upon was passed through three bulb-apparatus, such as that shown in the figure, containing potash before it came in contact with the lime-water. The quantity of air passed through the apparatus was also, as before, 120 litres, and the rate of passage about 5 litres in twelve hours.

At the end of this experiment all the vessels containing lime-water were found to be perfectly unchanged, except that the quantity of liquid in them was somewhat smaller, and hence a little gelatinous hydrate of lime was deposited in them, but no

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* During the passage of atmospheric air through a caoutchouc tube, the formation of carbonic acid by the oxidation of the caoutchouc takes place to a very considerable extent. I placed at the end of the apparatus above described a piece of so-called *vulcanized* india-rubber tubing, 1 foot in length, and of the diameter of a raven's quill, which was connected with a bulb-apparatus filled with lime-water, wherein the air, which entered the india-rubber tube free from carbonic acid, was again washed. At the conclusion of the experiment above described, the whole surface of the bulb-apparatus, with which the lime-water came in contact, was covered with crystals of carbonate of lime. In order to form an approximate idea of the quantity of carbonic acid formed in the caoutchouc tube, I allowed a very slow stream of air, deprived of carbonic acid, to flow during fourteen weeks through a caoutchouc tube 3·2 metres in length, and 4·7 millimetres internal diameter, the air as it issued being made to pass through a weighed quantity of solution of potash, and then through a weighed chloride-of-calcium tube as described in Poggendorff's *Annalen*, vol. cix. p. 349. At the end of the experiment the potash apparatus and chloride-of-calcium tube had together increased in weight by 0·1166 gramme.

crystals. On moistening them with hydrochloric acid, with the precautions already indicated, the closest observation could not detect any evolution of gas; evidently no formation of carbonate of lime had taken place.

This experiment proves, in the *first* place, that a solution of hydrate of potash is sufficient to absorb completely the carbonic acid contained in the air; and *secondly*, that air which has been carefully and sufficiently heated to redness, and then passed through caustic potash, is and remains free from carbonic acid; while, on the other hand, air that has not been heated to redness, when passed through the same apparatus and through the same quantity of potash, still yields to potash a distinctly perceptible quantity of carbonic acid, which must therefore have been formed during the passage of the air through the apparatus.

In conclusion, I cannot omit publicly acknowledging my obligation to Dr. Finkener for the willing aid he afforded me in putting together, in Prof. H. Rose's laboratory, the complicated apparatus required for these experiments.

LXXV. *Further Researches on the Auroræ Boreales, and the Phenomena which attend them.* *By* M. A. DE LA RIVE*.

THE object which I have in view in this new investigation is to show that the theory which I have advanced of the phenomenon of auroræ boreales is remarkably confirmed by observations made during the last few years,—especially by those of Mr. Walker on the currents exhibited by telegraphic wires, notwithstanding that this learned observer has deduced from them conclusions unfavourable to this theory.

I will in the first place call attention to two fundamental points which have been confirmed by observation, and which may now be regarded as definitively established in science.

The first of these points is the coincidence of the occurrence of auroræ boreales and auroræ australes; it has been established by numerous observations made in the two hemispheres, particularly at Hobart Town in the southern hemisphere, and at Christiania in the northern.

The second important point, likewise definitively established in science, is that the phenomenon of aurora borealis and australis is an atmospheric phenomenon. Father Secchi and several other distinguished scientific men were of this opinion, which had already been expressed by Arago, and the truth of which I have endeavoured to demonstrate in my former researches, and is confirmed by recent investigations.

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* Extract, communicated by the Author, of a memoir read before la Société de Physique et d'Histoire Naturelle de Genève on the 6th of February, 1862.

Not insisting here on the discussion and study of these investigations, from which it results that the aurora is an electro-atmospheric phenomenon, I will simply call attention to the fact that it is satisfactorily accounted for by admitting, in conformity with the data furnished by direct observation, that, the waters of the ocean being continually charged with positive electricity, the vapours which arise from them act as a conductor of this electricity as far as the upper strata of the atmosphere, where, carried towards the polar regions by the trade-winds, they form as it were a positive envelope to the earth, which itself remains charged with negative electricity. But the earth and the highly rarefied air of the elevated atmospheric regions being perfect conductors, they may be regarded as forming the two conducting-plates of a condenser, of which the insulating stratum is the inferior portion of the atmosphere. The two antagonistic electricities must then necessarily be condensed by their mutual influence in those portions of the atmosphere and of the earth to which they are the nearest, consequently in the regions near the poles, and there neutralize themselves in the form of discharges more or less frequent as soon as their tension reaches the limit which it cannot exceed. These discharges should take place almost simultaneously at the two poles, since the earth being a perfect conductor, the electric tension should be nearly the same at each; there can only be differences in the intensity of the discharges in one region and the other, and from one instant to another in the same region, since the resistance of the stratum of air which separates the two electricities must constantly vary from sundry causes. It is evident, too, that the neutralization of the opposite electricities would not be effected instantaneously, but, considering the low conducting-power of the medium through which it takes place, by successive discharges more or less continuous and variable in intensity.

These principles admitted, I endeavoured to produce artificially in all its details, and under all the attendant circumstances, the phenomenon of the aurora. I have already published the results which I obtained some time since respecting the influence of strong electro-magnets on luminous electric discharges in highly rarefied air—an influence which explains that of terrestrial magnetism on the aurora. The magnetic and electric phenomena which attend their appearance may likewise be produced artificially. The first, as is well known, consist of an augmentation of westerly deflection, followed and occasionally preceded by a much weaker and much less durable easterly deflection. The second, the electric phenomena, manifest themselves by the presence of currents, frequently very intense, in the telegraphic wires. Accurate observations made by Mr. Walker in England,

and by Mr. Loomis in America, show that these currents vary every moment during the appearance of an aurora, not only in intensity, but also in direction, flowing sometimes from N. to S., sometimes from S. to N. But bearing in mind that the currents developed in the telegraphic wires are currents derived from large sheets of metal implanted in moist earth, it will be seen that, the plates being speedily polarized under the chemical action of the current passing through them, they must develope in the wire which unites them an opposite current as soon as that whence they derived their polarization ceases or simply diminishes in intensity. In fact it has been universally remarked that the light of auroræ exhibits a very variable intensity and continual oscillations. Moreover, the discharges which take place simultaneously at the two terrestrial poles, and which constitute the auroræ boreales and australes, must, by the influence of variable and local circumstances, be alternately stronger at one pole than at the other, and even momentarily cease at one pole whilst in action at the other.

The phenomenon occurs thus: the negative electricity with which the earth is charged, arriving at a certain degree of tension, discharges itself in the atmosphere of the polar regions, where it meets the positive accumulated there by the trade-winds. There result on the earth two currents directed from the poles to the equator, the direction being that of the positive electricity; there is especially a current directed from the N. to the S. in the northern hemisphere. But if the discharge takes place at one pole only, the south pole for instance, there is in the northern hemisphere, instead of a current directed from the N. to the S., a current directed from the S. to the N., but weaker. It results from this that the deflection of the magnetic needle, which under the influence of the first current was west in the northern hemisphere, in conformity with laws of electrodynamics, becomes east; hence also the currents exhibited by the electric wires are directed from the S. to the N. instead of from the N. to the S. This cause, added to the secondary polarity which the plates had acquired in transmitting the current directed from the N. to the S., must produce a current almost as strong as the latter.

But in reality it is only rarely that the discharges cease at one pole to take place exclusively at the other; it is rather in difference of intensity that these variations manifest themselves: the same results, however, ensue both as regards the magnetic needle and the telegraphic wires, only they are less pronounced, and accompanied by numerous oscillations.

I have succeeded in verifying by experiment all these results; I have especially convinced myself, by passing the discharge of a Ruhmkorff apparatus, which has traversed highly rarefied air in order to produce the appearance of the aurora, through a slightly saline solution, and by producing, by means of two copper plates immersed in this solution, a derived current, that these plates acquire secondary polarities which give rise to an inverse current almost as strong as the derived, compensating by its duration what it may lack in intensity. As regards the magnetic disturbances, they are very easy to reproduce by suspending above and very near a surface of mercury placed in the circuit of the same discharge a sewing-needle, the lightest obtainable, very strongly magnetized: the extent and direction of its deviations show that it obeys all the variations of intensity and of direction of the discharge.

The better to realize this reproduction of the natural phenomenon in its entirety and in detail, I have had an apparatus constructed composed of a wooden sphere of from 30 to 35 centimetres in diameter, which represents the earth. At each extremity of one of the diameters of this sphere is fixed a cylinder of soft iron of from 3 to 4 centimetres in diameter, and from 5 to 6 long. The two cylinders repose each, in the portion nearest to the sphere, on a rod of soft iron to which they are solidly united by strong screws; the two rods being vertical, serve as a support to the cylinders and to the sphere, which has thus a horizontal axis terminated by two soft-iron cylinders, which may be magnetized by placing the two vertical supports respectively on the two poles of an electro-magnet, or by surrounding them with a coil traversed by a strong current. An excellent representation is thus obtained of the earth with the two magnetic poles.

The cylinders of soft iron covered with a non-conducting coating, except at their extremity, are each surrounded with a wide glass tube of which they occupy the axis, ending in the middle of this axis. The tubes have a diameter of about 10 centimetres, and a length of about 15; they are hermetically closed by two metallic disks, of which one is traversed by the soft-iron cylinder, and the other bears, by means of two metal branches covered with a non-conducting varnish, a ring, of which the diameter is a trifle less than that of the tube, and of which the centre coincides with the end of the soft-iron cylinder, whilst its plane is perpendicular to the axis of the cylinder, and consequently to that of the tube. The ring itself presents a bright metallic surface, and its outer edge is about half a centimetre from the inner surface of the glass tube. Stopcocks fastened to the disks which close the tubes externally, admit of producing a vacuum, or of introducing at pleasure gases or vapours in greater or smaller quantities.

When operating with this apparatus, two broad bands of blotting-paper are placed on the wooden sphere, one of which entirely surrounds its equator, and the other, which crosses the first, extends from one pole to the other in such a manner that its extremities are respectively in contact with the iron cylinders. On the last-mentioned band, small plates of copper, of from 1 to 2 centimetres square, are placed, on both sides of the equatorial band; these are to be fixed on by small screws of the same metal, which penetrate the wood of the sphere, and to be placed equidistant from each other on the same meridian. A metallic communication may be established between them by means of a galvanometer wire, which is placed at 10 or 12 metres distance, so as not to be directly influenced by the electro-magnets. The apparatus thus arranged, moisten the blotting-paper bands with salt water; to keep these bands in the necessary state of humidity, it will be sufficient to dip the two ends of the equatorial band in a saline solution contained in a small metal capsule fixed, by means of a rod fastened in the wooden sphere, a little below the lower part of the sphere. The metal capsule is connected with the negative electrode of a Ruhmkorff apparatus, of which the positive electrode is put in communication, by means of a bifurcate conductor, with the metallic disks whence proceed the rings placed in the interior of the tubes. As soon as the Ruhmkorff apparatus is put in action, the discharge is seen, if care has been taken to make a sufficient vacuum in the interior of the tubes, issuing in the shape of a luminous jet between the end of the soft-iron cylinder and the ring; but it is sometimes in one tube and sometimes in the other that the jet bursts, and rarely in both together, notwithstanding that care has been taken to place the two media in condition apparently identical, and particularly to have exactly the same vacuum in both by connecting, during this operation, the interior of one of the tubes with that of the other.

Immediately the soft irons are magnetized, the jet spreads and assumes a rotatory motion, the direction of which depends upon that of the magnetization and of the direction of the discharge. This latter circumstance influences not only the direction of the rotation, but gives rise to other curious changes. Thus, if the direction be altered so that the positive electricity is received by the soft iron, and the negative by the ring, the jet will be observed to spread round the magnetized end of the soft iron so as to form a true ring 1 to 2 centimetres in thickness, of a beautiful rosy red, with a very regular rotatory motion; whilst the metallic ring is surrounded with a luminous violet envelope, forming a sort of sheath about it, and having likewise a rotatory motion. These two rotatory movements are perfectly synchronous, although the rose-coloured ring and the violet ring, separated by an obscure space of several centimetres in extent, appear to have no connexion the one with the other.

But what is still more remarkable is what takes place in the contrary case, that in which, as happens in nature, negative electricity comes in by the soft iron which represents the terrestrial pole, and the positive by the metal ring which represents the atmosphere. In order that the discharge may be well seen, the air must not be so rarefied as in the preceding case; if it is too much so, a little air may be introduced by means of a cock placed for this purpose, or better still a few drops of water, which evaporate, until a single jet is at first observed; then at the moment the soft iron is magnetized the jet commences to rotate and to throw off a multitude of brilliant little jets issuing from the luminous ring which surrounds the end of the soft iron, and terminating in the metallic circle. These jets are equidistant, and perfectly distinct from each other, like the spokes of a wheel, and turn with more or less rapidity. We have here a perfectly faithful representation of what takes place in the auroræ boreales when the auroral arcs send out luminous jets into the high regions of the atmosphere. It is a very remarkable fact that the production of these jets can only occur when the current is directed from the circumference to the centre, as in the natural phenomenon, in which the discharge takes place from the atmosphere to the earth. None are to be seen when the current is directed from the centre to the circumference; there are then but two concentric arcs, one of a rosy red round the soft iron, the other violet which envelopes the metallic ring; the two separated by a perfectly obscure annular space.

Looking now at the galvanometer 10 or 12 metres off, where terminate the wires which separate the metallic plates fixed on the moistened bands of the sphere which represents the earth, we shall make the following observations.

The sphere is divided by the equatorial band of paper into two hemispheres, each of which has a magnetic pole, and which we will style northern hemisphere and southern hemisphere. We at first place the two metallic plates whence proceed the wires connected with the galvanometer, at a little distance from each other, on the same side of the equator, on the moistened band which extends like a meridian from one pole to the other. If the discharge takes place at the north pole, the needle of the galvanometer indicates by its deviation a derived current of 70 to 80 degrees, which shows the presence of a strong current travelling on the sphere in its northern hemisphere from N. to S. If the discharge takes place at the south pole, a much weaker derived current is remarked (from 25 to 30 degrees) directed a contrary way, and which indicates the presence on the sphere, still in its northern hemisphere, of a current much weaker than the preceding, and moving from the S. to the N., and not from the N. to the S. It is essential that care be taken, before exhibiting this second derived current, to assure oneself that the metallic plates have lost the secondary polarities which they had derived from the effect of the first. For these polarities give rise of themselves to a current moving likewise from the S. to the N., since it is the contrary of that which has produced them. This current is added to that which is derived from the alteration of the position of the discharge, and greatly augments its intensity. It shows itself alone if the discharge ceases momentarily at the north pole without taking place immediately at the south pole; and, as I have already remarked above, it may of itself alone be almost as strong as that which has given rise to it. The same phenomena present themselves, precisely in the same way, except that the direction of the current is reversed, when the two metal plates are fixed in the southern hemisphere of the sphere, instead of being in the north.

To resume, if we fix, on the moistened band which represents a meridian, two similar and equidistant pairs of metal plates, one on one side of the equator, the other on the other side, and observe the derived currents which they exhibit, we ascertain that the current exhibited by the plates of the northern hemisphere is directed from the N. to the S. when the discharge takes place at the north pole, and from the S. to the N. when the discharge is transferred to the south pole; we ascertain likewise that that which is exhibited by the plates situate in the south hemisphere is directed from the S. to the N. when the discharge takes place at the south pole, and from the N. to the S. when it is removed to the north pole. It is readily conceived that a difference in the intensity of the discharges which take place at the two poles simultaneously, is sufficient to produce the same effects, only in a slightly less degree than when there is a complete cessation of the discharge at one of the poles, accompanied by its appearance at the other. This is precisely what takes place in nature, and it explains all the variations in the movements of the galvanometers placed in the circuit of telegraphic wires, which accompany so faithfully the different phases through which pass the electric discharges constituting the auroræ boreales and australes.

I have already indicated how these variations explain the disturbances of the needle, which I have also succeeded in reproducing artificially, either independently of the other phenomena, or simultaneously, by causing the same discharge which is communicated to the apparatus just described, to pass through a surface of mercury above which a magnetic needle is delicately suspended. These disturbances, being the result of the direct action of the terrestrial currents upon the magnetic needle, are independent of the secondary polarities, which play an important part in the currents of telegraphic wires. Therefore, although proceeding from the same cause, these two kinds of phenomena, notwithstanding that they are subject to the same general variations, present remarkable differences in their phases.

I will add that I have failed to discover how it is that, under conditions apparently perfectly identical, the discharge takes place at one of the poles sooner than at the other, or, what is still more remarkable, why, after occurring for some time at one of the poles, it passes suddenly to the other: the changes are evidently attributable to certain modifications which occur in the medium placed in the course of the discharge, and the nature of which it is difficult to determine; but they readily explain how it happens that in nature, where it is impossible to admit that the conditions are identical and constantly the same at the two poles of the earth, the auroræ, although manifesting themselves simultaneously at one and the other, can never be completely alike in both. They also render obvious how it is that when they are observed at a single pole (at the north pole, for instance) they exhibit in their appearance, as well as in the phenomena which attend them, those oscillations which are their principal characteristic.

Supplemental Note.—I am desirous of mentioning here that the delicate apparatus by which I have succeeded in faithfully reproducing the auroræ boreales and australes with their attendant phenomena, has been constructed in the manufactory of philosophical instruments of Professor Thury, under the direction of M. Eugène Schwerd, a skilful German artist. This apparatus, executed with the greatest care, may be applied to a number of experiments; I have especially used it with success in researches on the propagation of electricity in different gases —researches which I shall soon publish.

LXXVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 492.]

June 13, 1861.—Thomas Graham, Esq., Master of the Mint, V.P., in the Chair.

THE following communications were read:—

"On the Bromide of Carbon." By Arthur C. W. Lennox, Esq.

"On the Action of Dibromide of Ethylene on Pyridine." By John Davidson, Esq.

"On a New Class of Organic Bases, in which Nitrogen is substituted for Hydrogen." By Peter Griess, Esq.

In a previous Note I have called attention to two new bodies produced by the action of nitrous acid upon the alcoholic solution respectively of aniline and nitraniline. Both substances*,

Aniline derivative	$C_{12}H_{11}N_3$,
Nitraniline derivative	$C_{12}H_9(NO_2)_2N_3$,

are generated by the substitution of one equivalent of nitrogen for three equivalents of hydrogen in the original compounds.

In continuing my experiments on the replacement of hydrogen by nitrogen in organic bodies generally, I have not only succeeded in producing similar compounds from nearly all the basic derivatives of aniline, but have also obtained corresponding products from toluidine and anisidine. The following equation represents the formation of these compounds in the aniline-series :—

$$2\underbrace{(C_n H_{(n+1)} N)}_{\text{Two equivalents of aniline.}} + HNO_2 = \underbrace{C_{2n} H_{2n-1} N_3}_{\text{New compound.}} + 2H_2O.$$

I do not at present venture to express an opinion regarding the constitution of these bodies; nevertheless their formation, their mutual relations, and their decompositions may in a measure be represented by the following formulæ :—

Type : two equivalents of aniline	$\left.\begin{matrix}C_6H_7\\C_6H_7\end{matrix}\right\}N_2.$
Azophenyldiamine	$\left.\begin{matrix}C_6H_4N'''\\C_6H_7\end{matrix}\right\}N_2.$
Azonitrophenyldiamine† . . .	$\left.\begin{matrix}C_6H_3(NO_2)N'''\\C_6H_6(NO_2)\end{matrix}\right\}N_2.$
Azobromphenyldiamine . . .	$\left.\begin{matrix}C_6H_3BrN'''\\C_6H_6Br\end{matrix}\right\}N_2.$

Nearly all these bodies are beautifully crystallized, and some possess very characteristic properties; the greater number are extremely weak bases, which it is scarcely possible to combine with acids. On the other hand, they readily produce double compounds with dichloride of platinum and trichloride of gold, the composition of which proves that these new bodies belong to the class of diatomic bases, as expressed by the following formulæ :—

Double compound of hydrochlorate of azophenyldiamine and dichloride of platinum . .	$\left.\begin{matrix}C_6H_4N'''\\C_6H_7\end{matrix}\right\}N_2, H_2Cl_2, (PtCl_2)_2.$
Double compound of hydrochlorate of azobromphenyldiamine and dichloride of platinum .	$\left.\begin{matrix}C_6H_3BrN'''\\C_6H_7\end{matrix}\right\}N_2, H_2Cl_2, (PtCl_2)_2.$

When exposed in alcoholic solution to the action of nitrous acid for

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* $H=1$; $O=16$; $C=12$, &c.

† This compound was obtained from the nitraniline discovered by Hofmann and Muspratt. I have already pointed out that a second body of the same composition exists, which is produced by a similar process from Arppe's nitraniline. Observations since made lead me to the conclusion that there is an isomeric representative corresponding to every compound of the phenyle-group.

a second time, most of these bases again lose three equivalents of hydrogen, which are again replaced by one equivalent of nitrogen, a new class of compounds being thus formed possessing in a still higher degree the peculiarities of the nitrogen-substituted bodies. The following equation illustrates this reaction:—

$$\underbrace{\left.\begin{matrix} C_6H_4N''' \\ C_6H_7 \end{matrix}\right\} N_2+HNO_2}_{\text{Azophenyldiamine.}} = \underbrace{\left.\begin{matrix} C_6H_4N''' \\ C_6H_4N''' \end{matrix}\right\}}_{\text{New compound.}} N_2+2H_2O.$$

In practice, however, the bases are never obtained in the free state, but always as nitrates, from which they may be liberated by the addition of an alkali. The following formulæ represent the bodies of this series which have hitherto been studied:—

Diazophenyldiamine . . $\left.\begin{matrix} C_6H_4N''' \\ C_6H_4N''' \end{matrix}\right\} N_2.$

Diazobromphenyldiamine . $\left.\begin{matrix} C_6H_3BrN''' \\ C_6H_3BrN''' \end{matrix}\right\} N_2.$

Diazotoluyldiamine . . $\left.\begin{matrix} C_7H_6N''' \\ C_7H_6N''' \end{matrix}\right\} N_2.$

In the free state, these bases generally present themselves in the form of yellow precipitates, insoluble in water. On account of their comparative instability, they are little adapted for analysis; I have therefore generally fixed their composition by the analysis of their nitrates and platinum-salts, most of which are splendidly crystallized. The following formulæ represent several of these compounds:—

Nitrate of diazophenyldiamine $\left.\begin{matrix} C_6H_4N''' \\ C_6H_4N''' \end{matrix}\right\} N_2, (HNO_3)_2.$

Gold-salt of diazobromphenyldiamine . . . . $\left.\begin{matrix} C_6H_3BrN''' \\ C_6H_3BrN''' \end{matrix}\right\} N_2, H_2Cl_2, (AuCl_3)_2.$

Platinum-salt of diazotoluyldiamine . . . . $\left.\begin{matrix} C_7H_6N''' \\ C_7H_6N''' \end{matrix}\right\} N_2, H_2Cl_2, (PtCl_2)_2.$

My attention has been especially attracted by this last class of new bodies, not only because of their peculiar constitution, but also on account of the remarkable physical properties by which, as a class, they are distinguished. One of their most striking features is the tremendous power with which, under the influence of heat or percussion, these substances explode. The greatest precaution is necessary in manipulating with these compounds.

I have scarcely commenced the study of the products of decomposition of these bodies. The following observations, however, justify the hope that they will not be without theoretical interest. Nitrate of diazophenyldiammonium, when heated with water, is rapidly decomposed according to the following equation:—

$$\underbrace{C_{12}H_8N_4, (HNO_3)_2}_{\text{Nitrate of diazophenyl-diammonium.}}+2H_2O = 2\underbrace{C_6H_6O}_{\text{Phenol.}}+2\underbrace{HNO_3}_{\text{Nitric acid.}}+4N.$$

Qualitative experiments have proved that this substance undergoes a corresponding transformation under the influence of sulphuretted hydrogen.

$$\underbrace{C_{12}H_8N_4, (HNO_3)_2}_{\text{Nitrate of diazophenyl-diammonium.}} + 2H_2S = \underbrace{2C_6H_6S}_{\text{Phenyl-mercaptan.}} + 2HNO_3 + 4N.$$

It is obvious that the last reaction supplies a general method for the production of the mercaptans belonging to the homologues of the phenyle-series, the first representative of which was discovered a few months ago by Vogt, who obtained it by submitting the chloride of sulpho-phenyle to the action of hydrogen.

"Reproduction of Non-nitrogenous Acids from Amidic Acids."

By Peter Griess, Esq.

In several previous communications I have pointed out the existence of a peculiar double acid,

$$C_{14}H_{11}N_3O_4,$$

formed by the action of nitrous acid on amidobenzoic acid. This acid, which I have designated as *amidodiazobenzoic acid,* when again submitted in the presence of hot alcohol to the action of nitrous acid, is changed according to the following equation:—

$$\underbrace{C_{14}H_{11}N_3O_4}_{\text{Double acid.}} + \underbrace{2C_2H_6O}_{\text{Alcohol.}} + \underbrace{HNO_2}_{\text{Nitrous acid.}} = \underbrace{2C_7H_6O_2}_{\text{Non-nitrogenous acid.}} + \underbrace{2C_2H_4O}_{\text{Aldehyde.}} + 2H_2O + 4N.$$

The non-nitrogenous acid thus reproduced exhibits the composition of benzoic acid; it differs, however, in its properties essentially from that acid. Since the publication of Kolbe and Lautemann's experiments on salylic acid, I have found that the acid in question is identical with the latter.

Salylic acid may be much more readily obtained by submitting an alcoholic solution of amido-benzoic acid itself to the action of nitrous acid.

$$\underbrace{C_7H_7NO_2}_{\text{Amidobenzoic acid.}} + \underbrace{C_2H_6O}_{\text{Alcohol.}} + \underbrace{HNO_2}_{\text{Nitrous acid.}} = \underbrace{C_7H_6O_2}_{\text{Salylic acid.}} + \underbrace{C_2H_4O}_{\text{Aldehyde.}} + 2H_2O + 2N.$$

Nearly all the amidic acids I had an opportunity of examining exhibit a similar deportment, furnishing a non-nitrogenous acid isomeric or identical with the acid which, by conversion into a nitro-compound and subsequent reduction, had given rise to the formation of the amidic acid. In the following cases I have experimentally established this transformation.

$$\underbrace{C_7H_6O_2}_{\text{Benzoic acid.}} \qquad \underbrace{C_7H_5(NO_2)O_2}_{\text{Nitrobenzoic acid.}} \qquad \underbrace{C_7H_5(H_2N)O_2}_{\text{Amidobenzoic acid.}} \qquad \underbrace{C_7H_6O_2}_{\text{Salylic acid.}}$$

$$\underbrace{C_8H_8O_2}_{\text{Toluic acid.}} \qquad \underbrace{C_8H_7(NO_2)O_2}_{\text{Nitrotoluic acid.}} \qquad \underbrace{C_8H_7(H_2N)O_2}_{\text{Amidotoluic acid.}} \qquad \underbrace{C_8H_8O_2}_{\text{Homologue of salylic acid.}}$$

$$\underbrace{C_8H_8O_3}_{\text{Anisic acid.}} \quad \underbrace{C_8H_7(NO_2)O_3}_{\text{Nitranisic acid.}} \quad \underbrace{C_8H_7(H_2N)O_3}_{\text{Amidanisic acid.}} \quad \underbrace{C_8H_8O_3}_{\text{Anisic acid.}}$$

$$\underbrace{C_6H_4(NO_2)_2O}_{\text{Dinitrophenylic acid.}} \quad \underbrace{C_6H_3(NO_2)_3O}_{\text{Trinitrophenylic acid.}} \quad \underbrace{C_6H_3(NO_2)_2(H_2N)O}_{\text{Dinitramidophenylic acid.}} \quad \underbrace{C_6H_4(NO_2)_2O}_{\text{Dinitrophenylic acid.}}$$

It deserves to be noticed, that while amidobenzoic and amidotoluic acid furnish acids isomeric respectively with benzoic and toluic acid*, anisic and dinitrophenylic acid are re-obtained with all their original properties. The properties of salylic acid, and of its derivatives, render it probable that salylic acid—and not benzoic as was hitherto assumed—is in the group of aromatic acids the true representative of acetic acid. Salylic, chlorosalylic, and salicylic acids exhibit in fact the same intimate relations which obtain between acetic, chloracetic, and glycolic acids. It is true the same relations may be traced in the composition of benzoic, chlorobenzoic, and oxybenzoic acids, but the chemical deportment of these substances is altogether different from that of the analogous derivatives of acetic acid.

Further experiments are necessary to establish this view; the existence of two varieties of lactic acid, and of alanine and sarcosine, which are both represented by the formula

$$C_3H_7NO_2,$$

appears to indicate that similar isomerisms obtain also in the group of fatty acids.

"On the Ureas of the Diamines." By J. Volhard, Ph.D.

"Notes of Researches on the Poly-Ammonias."—No. XI. Isomerism of Diatomic Compounds. By A. W. Hofmann, LL.D., F.R.S.

"Notes of Researches on the Poly-Ammonias."—No XII. Action of Cyanate of Ethyle on Urea. By A. W. Hofmann, LL.D., F.R.S.

"Notes of Researches on the Poly-Ammonias."—No. XIII. Derivatives of the Phenyle-Series. By A. W. Hofmann, LL.D., F.R.S.

"Notes of Researches on the Poly-Ammonias."—No. XIV. Diagnosis of Diamines. By A. W. Hofmann, LL.D., F.R.S.

"Notes of Researches on the Poly-Ammonias."—No. XV. Monacid Polyamines. By A. W. Hofmann, LL.D., F.R.S.

"Researches on the Phosphorus-Bases."—No. XIII. Sulphuretted Derivatives of Triethylphosphine. By A. W. Hofmann, LL.D., F.R.S.

"Researches on the Phosphorus-Bases."—No. XIV. Action of Triethylphosphine on the Substitution-compounds of Marsh-Gas. By A. W. Hofmann, LL.D., F.R.S.

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* The acid $C_8H_8O_2$, described by Strecker, probably belongs to a different group of compounds. In its transformations, and more especially in its conversion under the influence of oxidizing agents, into oil of bitter almonds, it differs both from toluic acid and the homologue of salylic acid.—P. G.

GEOLOGICAL SOCIETY.

[Continued from p. 493.]

April 2, 1862.—Prof. Ramsay, President, in the Chair.

The following communications were read;—

1. "On some Remains of *Chiton* from the Mountain-limestone of Yorkshire." By J. W. Kirkby, Esq.

These remains consist of eight separate plates of four species of *Chiton*, found by Mr. Burrow, of Settle, in the Lower Scar Limestone of that neighbourhood. These new species, determined by Mr. Kirkby, are *Chiton Burrowianus*, Kirkby, *Ch. coloratus*, Kirkby, two species undetermined, and a trace of *Chitonellus* (?). These appear to be the first Chitons observed in the Carboniferous Limestone of England; but fourteen others, and a *Chitonellus*, have been found in strata of the same age in Belgium and elsewhere, and have been described by Münster, De Koninck, and De Ryckholt.

2. "On some Fossil *Reptilia*, of the Order *Ganocephala*, from the Coal-measures of the South Joggins, Nova Scotia." By Professor Owen, F.R.S., F.G.S.

The specimens described in this communication were (together with remains of *Xylobius* and *Pupa*) obtained by Dr. Dawson, F.G.S., in 1861, from two fossil stumps of trees, and were referred to in his communication read before the Society on November 6, 1861. Professor Owen has determined among the specimens submitted to him the following small Reptilian forms—*Hylonomus Lyelli*, Dawson, *H. aciedentatus*, Dawson, *H. Wymanni*, Dawson, *Hylerpeton Dawsoni* (nov. gen. et sp.), Owen, and *Dendrerpeton Acadianum*, Owen.

3. "On the Occurrence of Mesozoic and Permian Faunæ in Eastern Australia." By the Rev. W. B. Clarke, F.G.S.

Mr. W. P. Gordon having been requested by the Rev. W. B. Clarke to search for fossils in his neighbourhood (between the Balonne and Maranoa Rivers) and in the Fitzroy Downs, Queensland, was successful in making a large collection of specimens at the Wollumbilla Creek. These he sent to Mr. Clarke, who forwarded them to Professor M'Coy, at Melbourne, for his examination. They prove to be chiefly of Lower Mesozoic genera; there are also a few (from the Fitzroy Downs, about thirteen miles to the N.W.) which belong to lower beds. Some fossils long since obtained from the Mantuan Downs (200 miles N. of Wollumbilla) prove to be of Permian character (*Aulosteges* or *Strophalosia*). *Productus* and *Cyathocrinus* (Carboniferous ?) have been met with on the Dawson River.

Mr. Clarke considers his unfossiliferous Wianamatta Beds, above the coal-measures, near Sydney, as being probably the equivalent of the fossiliferous strata at Wollumbilla Creek. Professor M'Coy regards the latter as belonging to the same formation as the coal-beds with *Glossopteris*. The fossils are included in the Catalogue of the Products of New South Wales for exhibition in London in 1862.

4. "On the Foot-print of an *Iguanodon,* lately found at Hastings." By A. Tylor, Esq., F.G.S.

After alluding to former accounts of fossil foot-prints (and natural casts of foot-prints) found in the cliffs near Hastings, and having stated that Dr. Harwood in 1846 suspected these prints to have been due to the *Iguanodon,* the author described a large three-toed foot-print, 21 inches long, by 9½ in width, lately exposed by a fall at East Cliff. A cast of this print was exhibited by Mr. C. J. Mann. Mr. Tylor alluded to Professor Owen's figure of the bones of the three-toed foot of an *Iguanodon* as illustrative of a foot capable of producing such imprints as those referred to. The author then showed, by a newly constructed section of the Hastings coast, that the foot-prints occur in at least two zones of the Wealden beds,—one of them being just above the chief sandstone (or Castle Sand-rock) of Hastings, and dipping down to the west on the top of the Bexhill cliffs; the other zone being about 100 feet below, as already pointed out by Mr. Beckles, near Lee Ness.

LXXVII. *Intelligence and Miscellaneous Articles.*

CONNEXION BETWEEN EARTHQUAKES AND MAGNETIC DISTURBANCES. BY DR. J. LAMONT.

On the 26th of December 1861, at 8 o'clock in the morning, as I was recording the position of the magnetic instruments (of which there are six in the magnetic observatory—two for declination, two for intensity, and two for inclination), I observed on all the instruments an unusual disturbance, consisting in the fact that the position rapidly and irregularly increased and then decreased by several divisions, and at the same time a vibration in a vertical direction took place. The vibration of the needles only lasted a short time; but the rapid alterations of the position continued, although diminishing in intensity, till about half-past 8. A few days afterwards, news arrived that at exactly the same time at which the above was observed, an earthquake in several parts of Greece had produced great devastation.

Here is a new proof, not only that the concussions which an earthquake produces are felt at great distances, but that the forces which produce the earthquake also modify the magnetism of the earth in a certain degree. The modifications doubtless consist in the fact that an *earth-current is produced,* which has also been so far confirmed, in the above case, by the fact that the arrangements at this observatory for observing the earth-current exhibited unusual activity at the time mentioned.

It is very remarkable that the earthquake which occurred in Greece on the 18th of April 1842 produced a similar action, while hitherto no action has been perceived from other earthquakes often at less distance.—Poggendorff's *Annalen,* January 1862.

ON THE FREEZING OF SALINE SOLUTIONS. BY DR. RÜDORFF.

The author showed in a previous communication that pure ice is formed by the freezing of saline solutions, and that some salts affect the freezing-point of the water in which they are dissolved as anhydrous salts, others as salts containing water. M. Dufour tried to prove, in a paper which appeared simultaneously, that the ice which separates from a saline solution contains salt which separates in the solid form at the same time as the ice. The well-known fact, that the proportion of salt contained in ice so formed is always smaller than that contained in the original saline solution, and that it is smaller the slower has been the formation of the ice, is explained by M. Dufour by supposing that the remaining solution redissolves the salt which separates out with the ice. The following experiments afford, however, an additional proof that the salt contained in ice produced in this way is due to a portion of the solution adhering to it or being enclosed by it.

It is well known that a solution of the magnificent dichroic double salt, the platinocyanide of magnesium, is perfectly colourless. If such a solution is allowed to freeze, the resulting ice is equally free from colour. At last, when the quantity of water separated in the form of ice is so great that the remainder no longer suffices to retain the whole quantity of salt in solution, the ice begins to get coloured red and green from the separation of crystals of salt.

The following experiment proves in a still more striking manner that salt does not separate out from a saline solution in a solid form at the same time as ice. As stated in the author's former paper, all saline solutions may be cooled below their freezing-point without the formation of ice taking place, such over-cooled solutions being thus analogous to supersaturated solutions of salts. A bit of ice causes the formation of ice in an over-cooled solution, just as a crystal of the dissolved salt causes the salt to separate from the supersaturated solution.

Now, with due care, it is possible to cool a supersaturated solution of sulphate of soda below its freezing-point; and in such a supersaturated and over-cooled solution, a bit of ice causes the separation of ice only and not of salt, while a crystal of sulphate of soda causes the salt alone and no ice to separate out, as may be easily seen by the ice floating at the top of the solution, whereas the sulphate of soda quickly sinks to the bottom. If both ice and salt are caused to solidify simultaneously, they separate from each other in this way. If the smallest trace of solid salt were to separate on throwing a morsel of ice into the solution, it would occasion the crystallization of the whole quantity of salt with which the solution is supersaturated.

The author has determined the influence of a great number of salts on the freezing-point of the water in which they are dissolved. For this purpose he operated in the same way as in his former experiments; that is, he cooled the solutions examined a few tenths of a degree below their freezing-points, which had been approximately ascertained by preliminary experiments, and then, by means of a small piece of ice, he occasioned a slight freezing of the solutions. In these, as in his former experiments, it was observable that the lowering of the freezing-point was in a certain manner proportional to the quantity of salt dissolved, if it is assumed that in some solutions the salt is dissolved in the anhydrous, and in others in the hydrated state. For instance, solutions of hydrochloric acid contain the compound $HCl+12\,HO$; solutions of sulphuric acid, the compound SO^3+10HO; of chloride of copper, $CuCl+12HO$; of protochloride of manganese, $Mn\,Cl+12\,HO$; of caustic soda, $NaO+4HO$; of caustic potash, $KO+5HO$; of ammonia, NH^4O+2HO.

Experiments with solutions of chloride of copper showed that solutions containing less than 20 per cent. of chloride of copper hold in solution the compound $CuCl+12\,HO$, and solutions containing more of the copper-salt the compound $CuCl+4HO$.

This change in the constitution of the solution, which is recognizable by means of the freezing-points, is accompanied also by a change in the colour of the solutions,—those which contain the salt $CuCl+12\,HO$ being blue, while those which contain $CuCl+4HO$ are green. The author had previously detected a similar case of change in the constitution of a saline solution, unaccompanied, it is true, by change of colour, in the case of chloride of sodium, which dissolves at common temperatures as an anhydrous salt, but below −9° C. is contained in solution in combination with 4 equivalents of water.

The experiments with solutions of sulphuric acid show that they contain the compound $SO^3+10\,HO$. It is proved by experiments on the electric conducting power of liquids that both water and anhydrous sulphuric acid are exceedingly bad conductors of electricity, but that a good conductor is formed by the combination of the two; the compound which offers the smallest resistance to the passage of electricity is, however, not the first hydrate, nor even the second, but, according to Wiedemann's determinations, the one which contains in 100 parts 45·8 parts of anhydrous sulphuric acid. This composition corresponds almost exactly with the formula SO^3+10HO: whether this coincidence is anything more than accidental, must for the present be left undecided.—*Monatsbericht d. K. Preuss. Akad. d. Wissenschaften zu Berlin*, 1862, p. 163 (March 6).

ON THE COMPOSITION OF MINERALS CONTAINING NIOBIUM.

BY PROFESSOR H. ROSE.

The most important class of minerals containing niobium are the columbites, which appear to occur in nature much more plentifully than the other niobiferous compounds, most of which are still among mineralogical rarities. Several of these minerals have been analysed in the author's laboratory.

Samarskite.—This remarkable mineral, first described by Professor G. Rose and named by him uranotantalite, has been examined at various times in the author's laboratory. It was analysed first by M. Wornum and afterwards by M. Peretz. Subsequently, through the liberality of M. von Samarski, the author was enabled to have the analysis repeated by Mr. Chandler, according to improved methods.

Hitherto Samarskite has been found only in the Ilmen Mountains, near Miask, in the Ural. It is indisputably in its original condition, and has not, like so many columbites, undergone decomposition under the influence of the atmosphere and of time. It has therefore always been found of the same specific gravity, namely, between 5·6 and 5·7. Ignition causes no important change in its external appearance, but, nevertheless, alters its density in a very remarkable manner. During ignition it becomes phosphorescent, and its specific gravity is afterwards found to have fallen as low as 5·37 or 5·4; whereas all other minerals which become phosphorescent on ignition are found to have afterwards a greater specific gravity than before.

Samarskite occurs compact and also crystallized, the crystals having exactly the same form as columbite, but being easily distinguished from crystals of the latter mineral by their different fracture. The two minerals are evidently isomorphous, and grow into one another, as has been already remarked by Hermann (who, however, gives to Samarskite the name Yttroïlmenite).

Although Samarskite contains hyponiobic acid, this is not the only electro-negative substance which forms an essential constituent of it. In addition to very small quantities of tungstic and stannic acids, it contains a considerable quantity of uranic oxide, amounting, indeed, to as much as 20 per cent. The bases contained in it are chiefly protoxide of iron, a little protoxide of manganese, and a not quite inconsiderable quantity of yttria. The oxygen of the bases, counting that of the yttria, is one-third of that of the hyponiobic acid and uranic oxide taken together. Hence, hyponiobic acid and uranic oxide being similar in atomic composition, Samarskite has the same atomic composition as columbite.

Fergusonite.—This is a rare Greenland mineral, described a long time ago by Haidinger, and since examined by Hartwall. More recently, M. Weber has analysed it afresh; and the results of his investigation agree in essential points with those of Hartwall, except that he has recognized the metallic acid contained in it as being hyponiobic acid.

Fergusonite appears, like the generality of minerals from Greenland, to occur in its original undecomposed state, notwithstanding that Haidinger states its specific gravity to be 5·838, while Weber found 5·612.

Fergusonite contains, besides hyponiobic acid and a small quantity of oxide of uranium, zirconic acid, not, however, to any notable amount, and a smaller quantity of stannic acid. The bases are chiefly yttria, a small quantity of protoxide of cerium, and a still smaller quantity of protoxide of iron.

Seeing that the atomic constitution of zirconic acid differs from that of hyponiobic acid, we cannot assume that the bases are combined in the same ratio with both acids. With the hyponiobic acid, the bases are not combined to a neutral salt, as in the columbites and in Samarskite, but they form a basic salt wherein the oxygen of the bases amounts to two-thirds of that of the acid. With the zirconic acid, on the other hand, the bases are combined in such proportion that the oxygen contained in them is equal to that contained in the acid, thus again forming a basic salt.

It was announced some time ago by A. E. Nordenskjöld that a mineral similar to the Fergusonite of Greenland occurred, together with yttrotantalites (which contain tantalic acid and not hyponiobic acid), at Ytterby in Sweden. This Fergusonite contains, according to Nordenskjöld, no zirconic acid; and its specific gravity, namely 4·89, is also considerably lower than that of the Greenland mineral. The Fergusonite of Ytterby is the only mineral hitherto found in Sweden which contains niobium.

Tyrite.—This mineral has been supposed to be identical with Fergusonite; but a carefully conducted analysis by Potyka has proved that it consists of hyponiobic acid in combination essentially with yttria and potash. Besides hyponiobic acid, the only acids contained in it are very small quantities of zirconic, tungstic, and stannic acids; but, in addition to the bases already mentioned, it contains protoxide of uranium, protoxide of cerium, and lime in somewhat considerable quantities, together with very small quantities of the oxides of lead, copper, and iron (but not of manganese), and an equally small quantity of magnesia. The oxygen of the bases taken together is to that of the acids as 1 : 1; and this ratio remains essentially the same if the mineral is supposed to contain, in place of protoxide, sesquioxide of uranium, this compound being regarded as an acid capable of replacing hyponiobic acid—an assumption which the composition of Samarskite justifies. But if the mineral may be regarded as a basic salt, it is not a simple, but a double salt; for potash and yttria cannot mutually replace each other in combination. Tyrite consists therefore essentially of basic hyponiobate of yttria and basic hyponiobate of potash.

Tyrite contains, further, a somewhat considerable quantity of water, which, as obtained by distillation, contains a little sulphuretted hydrogen, and is milky from the presence of sulphur suspended in it. This no doubt arises from a trace of enclosed iron-pyrites. Still, notwithstanding the water contained in it, the mineral appears to be essentially undecomposed.

In the niobiferous minerals which have been examined, the hyponiobic acid is combined with strong bases in various proportions. The quantity of oxygen contained in the hyponiobic acid and in the oxides of similar composition, such as uranic oxide, is to that of the bases—

As 3 : 1 in the columbites.
As 3 : 1 in Samarskite.
As 3 : 2 in Fergusonite.
As 3 : 3 in tyrite.

—*Monatsbericht d. K. Preuss. Akad. d. Wissenschaften zu Berlin,* 1862, p. 166 (March 13).

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END OF THE TWENTY-THIRD VOLUME.

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