

and $(A \cos^2 \eta + B \sin^2 \eta) \sin^2 \theta = \{A + (B - A) \sin^2 \eta\} \sin^2 \theta$,

$$\therefore (A \cos^2 \eta + B \sin^2 \eta) \sin^2 \theta = A \sin^2 \theta + (B - A) \sin^2 \zeta;$$

$\therefore$ by equation 13,

$$U(\theta, \zeta) = W(H_1 \mp H_2) \text{ vers } \theta + \frac{1}{2} \mu \{A \sin^2 \theta + (B - A) \sin^2 \zeta\}, \quad (15.)$$

by which formula the dynamical stability of the ship is represented, both as it regards a pitching and a rolling motion.

If in equation 13 $\eta = \frac{\pi}{2}$, the line in which the plane PQ (parallel to the deck of the ship) intersects its plane of flotation is at right angles to the length of the ship, and we have, since in this case $\theta = \zeta$ (see equation 14.),

$$U(\zeta) = W(H_1 \mp H_2) \text{ vers } \zeta + \frac{1}{2} \mu B \sin^2 \zeta, \quad (16.)$$

which expression represents the dynamical stability, in regard to a pitching motion alone, as the equation

$$*U(\theta) = W(H_1 \mp H_2) \text{ vers } \theta + \frac{1}{2} \mu A \sin^2 \theta \quad (17.)$$

represents it in regard to a rolling motion alone.

16. If a *given* quantity of work represented by $U(\theta)$ be supposed to be done upon the vessel, the angle θ through which it is thus made to roll may be determined by solving equation 17 with respect to $\sin^2 \frac{\theta}{2}$. We thus obtain

$$\sin^2 \frac{\theta}{2} = \frac{W(H_1 \mp H_2) + \mu A - \sqrt{\{W(H_1 \mp H_2) + \mu A\}^2 - 2\mu A \cdot U(\theta)}}{2\mu A} \quad (18.)$$

17. If PR and QS be conceived to be straight lines, so that POR and QOS are triangles, then $w.z$, taken in respect to an element included between the section CAD, and another parallel to it and distant by the small space dx , is represented by

$$\frac{1}{4} \mu y_1 y_2 \sin \theta dx (mg + nh);$$

or, since $mg + nh = \frac{1}{3} y_1 \sin \theta$,

by $\frac{1}{12} \mu \sin^2 \theta y_1^2 y_2 dx$;

$$\therefore wz = \frac{1}{12} \mu \sin^2 \theta \int y_1^2 y_2 dx,$$

and, equation 11

$$U(\theta, \zeta) = W(H_1 \mp H_2) \text{ vers } \theta + \frac{1}{12} \mu \sin^2 \theta \int y_1^2 y_2 dx, \quad (19.)$$

which formula may be considered an approximate measure of the stability of the vessel under all circumstances.

* This formula may be verified experimentally by a method similar to that applied to equation 6. See Art. 10.

If, as in the case of the experiments of Messrs. FINCHAM and RAWSON, the vessel be prismatic and the direction of the disturbance perpendicular to its axis,

$$y = \text{constant} = a, \text{ and } z = \frac{1}{3} a \sin \theta;$$

$$\therefore wz = \frac{1}{3} aw \sin \theta, \text{ and}$$

$$U(\theta) = W(H_1 \mp H_2) \text{ vers } \theta + \frac{1}{3} aw \sin \theta.$$

Mr. RAWSON has obligingly undertaken the verification of this formula by comparing it with his experiments on the cylindrical model. The following is the result :—

No. of experiment.	W.	w.	H ₁ .	H ₂ .	θ .	U(θ) by formula.	U(θ) by experiment.
3	lbs. 255·43	lbs. 17·294	3·903	4·800	9	1·760	1·766
4	255·43	46·84	4·02	4·82	26	13·478	13·5015
5	197·18	37·98	0·80	3·80	26	6·807	7·3761

18. *A rigid surface on which the vessel may be supposed to rest whilst in the act of pitching and rolling.*

If we imagine the position of the centre of gravity of a vessel afloat to be continually changed by altering the positions of some of its contained weights without altering the weight of the whole, so as to cause the vessel to incline into an infinite number of different positions displacing, in each, the same volume of water, then will the different planes of flotation, corresponding to these different positions, envelope a curved surface, called the surface of the planes of flotation (*surface des flotaisons*), whose properties have been discussed at length by M. DUPIN in his excellent memoir, *Sur la Stabilité des Corps Flottants*, which forms part of his *Applications de Géométrie**. So far as the properties of this surface concern the conditions of the vessel's *equilibrium*, they have been exhausted in that memoir, but the following property, which has reference rather to the conditions of its dynamical stability than its equilibrium, is not stated by M. DUPIN :—

If we conceive the surface of the planes of flotation to become a rigid surface, and also the surface of the fluid to become a rigid plane without friction, so that the former surface may rest upon the latter and roll and slide upon it, the other parts of the vessel being imagined to be so far immaterial as not to interfere with this motion, but not so as to take away their weight or to interfere with the application of the upward pressure of the fluid to them, then will the motion of the vessel, when resting by this curved surface upon this rigid but perfectly smooth horizontal plane, be the same as it was when, acted upon by the same forces, it rolled and pitched in the fluid.

In this general case of the motion of a body resting by a curved surface upon a horizontal plane, that motion may be, and generally will be, of a complicated cha-

* BACHELIER, Paris, 1822.

racter, including a sliding motion upon the plane, and simultaneous motions round two axes passing through the point of contact of the surface with the planes and corresponding with the rolling and pitching motion of a ship. It being however possible to determine these motions by the known laws of dynamics, when the form of the surface of the planes of flotation is known, the complete solution of the question is involved in the determination of the latter surface.

The following property*, proved by M. DUPIN in the memoir before referred to (p. 32), effects this determination:—

“The intersection of any two planes of flotation, infinitely near to each other, passes through the centre of gravity of the area intercepted upon either of these planes by the external surface of the vessel.”

If, therefore, any plane of flotation be taken, and the centre of gravity of the area here spoken of be determined with reference to that plane of flotation, then that point will be one in the curved surface in question, called the surface of the planes of flotation, and by this means any number of such points may be found and the surface determined.

19. *The axis about which a vessel rolls may be determined, the direction in which it is rolling being given.*

If, after the vessel has been inclined through any angle, it be left to itself, the only forces acting upon it (the inertia of the fluid being neglected) will be its weight and the upward pressure of the fluid it displaces; the motion of its centre of gravity will therefore, by a well-known principle of mechanics, be wholly in the same vertical line.

Let HK (fig. 6) represent this vertical line, PQ the surface of the fluid, and aMb the surface of the planes of flotation. As the centre of gravity G traverses the vertical HK, this surface will partly roll and partly slide by its point of contact M on the plane PQ.

If we suppose, therefore, PRQ to be a section of the vessel through the point M, and perpendicular to the axis about which it is rolling, and if we draw a vertical line MO through the point M, and through G a horizontal line GO parallel to the plane PRQ, then the position of the axis will be determined by a line perpendicular to these, whose projection on the plane PRQ is O.

For since the motion of the point G is in the vertical line HK, the axis about which the body is revolving passes through GO, which is perpendicular to HK; and since the point M of the vessel traverses the line PQ, the axis passes also through MO which is perpendicular to PQ; and GO is drawn parallel to, and MO in the plane PRQ, which, by supposition, is perpendicular to the axis, therefore the axis is perpendicular to GO and MO.

If HK be in the plane PRQ, which is the case whenever the motion is exclusively one of rolling or one of pitching, the point O is determined by the intersection of GO and MO.

20. *The time of the rolling through a small angle of a vessel whose athwart sections*

* This property appears to have been first given by EULER.

are (in respect to the parts subject to immersion and emersion) circular, and have their centres in the same longitudinal axis.

Let EDF (fig. 3 or fig. 4) represent the midship section of such a vessel, in which section let the centre of gravity G_1 be supposed to be situated, and let HK be the vertical line traversed by G_1 as the vessel rolls. Imagine it to have been inclined from its vertical position through a given angle θ_1 and the forces which so inclined it then to have ceased to act upon it, so as to have allowed it to roll freely back again towards its position of equilibrium until it had attained the inclination OCD to the vertical, which suppose to be represented by θ .

Referring to equation 1., let it be observed that in this case $\Sigma u_2 = 0$, so that the motion is determined by the condition

$$\Sigma u_1 = \frac{1}{2g} \Sigma wv^2. \quad \dots \dots \dots (20.)$$

But the forces which have displaced it from the position in which it was, for an instant, at rest are its weight and the upward pressure of the water; and the work of these, $U(\theta_1) - U(\theta)$, done between the inclinations θ and θ_1 when the vessel was in the act of receding from the vertical, was shown to be represented by $(W_1 h_1 \mp W_2 h_2)$ (vers $\theta - \text{vers } \theta_1$) by Art. 12 (adopting the same notation as in that article); therefore the work, between the same inclinations, when the motion is in the opposite direction, is represented by the same expression with the sign changed;

$$\therefore \Sigma u_1 = (W_1 h_1 \mp W_2 h_2) (\text{vers } \theta_1 - \text{vers } \theta),$$

and since the axis about which the vessel is revolving is perpendicular to the plane EDF, and passes through the point O (Art. 19.), if $W_1 k_1$ represent its moment of inertia about an axis perpendicular to the plane EDF, and passing through its centre of gravity G_1 ,

$$\Sigma wv^2 = W_1 (k^2 + \overline{OG_1^2}) \left(\frac{d\theta}{dt} \right)^2.$$

Substituting in equation 20 and writing for OG_1 its value $h_1 \sin \theta$, we have

$$\begin{aligned} (W_1 h_1 \mp W_2 h_2) (\text{vers } \theta_1 - \text{vers } \theta) &= \frac{W_1}{2g} (k^2 + h_1^2 \sin^2 \theta) \left(\frac{d\theta}{dt} \right)^2; \\ \therefore t(\theta_1) &= \frac{-1}{\sqrt{2gh_1 \left(1 \mp \frac{W_2 h_2}{W_1 h_1} \right)}} \int_{+\theta_1}^{-\theta_1} \sqrt{\frac{k^2 + h_1^2 \sin^2 \theta}{\text{vers } \theta_1 - \text{vers } \theta}} d\theta. \quad \dots \dots \dots (21.) \\ t(\theta_1) &= \frac{1}{\sqrt{2gh_1 \left(1 \mp \frac{W_2 h_2}{W_1 h_1} \right)}} \int_{-\theta_1}^{+\theta_1} \sqrt{\frac{k^2 + 4h_1^2 \sin^2 \frac{1}{2} \theta \cos^2 \frac{1}{2} \theta}{2 \left(\sin^2 \frac{1}{2} \theta_1 - \sin^2 \frac{1}{2} \theta \right)}} d\theta \\ &= \frac{1}{\sqrt{gh_1 \left(1 \mp \frac{W_2 h_2}{W_1 h_1} \right)}} \int_{-\theta_1}^{+\theta_1} \sqrt{\frac{k^2 \sec^2 \frac{1}{2} \theta + 4h_1^2 \sin^2 \frac{1}{2} \theta}{\sin^2 \frac{1}{2} \theta_1 - \sin^2 \frac{1}{2} \theta}} \cdot \cos \frac{1}{2} \theta d\frac{1}{2} \theta, \end{aligned}$$

or assuming θ to be so small that the fourth and all higher powers of $\sin \frac{1}{2} \theta$ may be neglected, and observing that, this being the case,

$$\begin{aligned} \sqrt{k^2 \sec^2 \frac{1}{2} \theta + 4h_1^2 \sin^2 \frac{1}{2} \theta} &= \sqrt{k^2 \left(1 + \sin^2 \frac{1}{2} \theta\right) + 4h_1^2 \sin^2 \frac{1}{2} \theta} \\ &= k \sqrt{1 + \frac{4h_1^2 + k^2}{k^2} \sin^2 \frac{1}{2} \theta} = k \left\{ 1 + \frac{4h_1^2 + k^2}{2k^2} \sin^2 \frac{1}{2} \theta \right\} \\ t(\theta_1) &= \frac{k}{\sqrt{gh_1 \left(1 \mp \frac{W_2 h_2}{W_1 h_1}\right)}} \int_{-\theta_1}^{+\theta_1} \frac{1 + \frac{4h_1^2 + k^2}{2k^2} \sin^2 \frac{1}{2} \theta}{\sqrt{\sin^2 \frac{1}{2} \theta_1 - \sin^2 \frac{1}{2} \theta}} d \sin \frac{1}{2} \theta. \end{aligned}$$

But

$$\int_{-\theta_1}^{+\theta_1} \frac{d \sin \frac{1}{2} \theta}{\sqrt{\sin^2 \frac{1}{2} \theta_1 - \sin^2 \frac{1}{2} \theta}} = \pi,$$

and

$$\int_{-\theta_1}^{+\theta_1} \frac{\sin^2 \frac{1}{2} \theta d \sin \frac{1}{2} \theta}{\sqrt{\sin^2 \frac{1}{2} \theta_1 - \sin^2 \frac{1}{2} \theta}} = \frac{1}{2} \pi \sin^2 \frac{1}{2} \theta_1,$$

$$\therefore t(\theta_1) = \frac{\pi k}{\sqrt{gh_1 \left(1 \mp \frac{W_2 h_2}{W_1 h_1}\right)}} \left\{ 1 + \frac{4h_1^2 + k^2}{4k^2} \sin^2 \frac{1}{2} \theta_1 \right\}. \quad (22.)$$

The sign $\mp$ being taken according as the centre of gravity of the displaced fluid ascends or descends.

21. *The time of a vessel's rolling or pitching through a small angle, its form and dimensions being any whatever.*

Let EDF (figs. 3 or 4) represent the midship section of such a vessel, supposed to be rolling about an axis whose projection is O; and let C represent the centre of the circle of curvature of the surface of its planes of flotation (Art. 18.) at the point M where that surface is touched by the plane PQ, being above the load water-line AB in fig. 3, and beneath it in fig. 4. Let the radius of curvature CM be represented by ρ ; then adopting the same notation as in the last article, and observing that the axis O about which the vessel is turning is perpendicular to EDF, we shall find its moment of inertia to be represented by

$$W_1 \{ k^2 + (H_1 \mp \rho)^2 \sin^2 \theta \} \left(\frac{d\theta}{dt} \right)^2,$$

where H_1 represents the depth of the centre of gravity in the vertical position of the vessel.

Also, by equation 17, reasoning as in Art. 20,

$$\Sigma u_1 = U(\theta_1) - U(\theta) = W_1 (H_1 \mp H_2) (\cos \theta - \cos \theta_1) + \frac{1}{2} \mu A (\cos^3 \theta - \cos^3 \theta_1),$$

∴ by equation 20,

$$W_1(H_1 \mp H_2)(\cos \theta - \cos \theta_1) + \frac{1}{2} \mu A (\cos^2 \theta - \cos^2 \theta_1) = \frac{W_1}{2g} \left\{ k^2 + (H_1 \mp \varrho)^2 \sin^2 \theta \right\} \left(\frac{d\theta}{dt} \right)^2$$

$$\therefore t(\theta_1) = \frac{1}{\sqrt{2g}} \int_{-\theta_1}^{+\theta_1} \sqrt{\frac{k^2 + (H_1 \mp \varrho)^2 \sin^2 \theta}{(H_1 \mp H_2)(\cos \theta - \cos \theta_1) + \frac{1}{2} \frac{\mu A}{W_1} (\cos^2 \theta - \cos^2 \theta_1)}} d\theta$$

$$t(\theta_1) = \frac{1}{\sqrt{2g}} \int_{-\theta_1}^{+\theta_1} \sqrt{\frac{k^2 + (H_1 \mp \varrho)^2 \sin^2 \theta}{\left\{ H_1 \mp H_2 + \frac{1}{2} \frac{\mu A}{W_1} (\cos \theta + \cos \theta_1) \right\} \left\{ \cos \theta - \cos \theta_1 \right\}}} d\theta.$$

Assuming θ and θ_1 to be so small that $\cos \theta + \cos \theta_1 = 2$, and observing that

$$\cos \theta - \cos \theta_1 = \text{vers } \theta_1 - \text{vers } \theta,$$

$$t(\theta_1) = \frac{1}{\sqrt{2g \left\{ H_1 \mp H_2 + \frac{\mu A}{W_1} \right\}}} \int_{-\theta_1}^{+\theta_1} \sqrt{\frac{k^2 + (H_1 \mp \varrho)^2 \sin^2 \theta}{\text{vers } \theta_1 - \text{vers } \theta}} \cdot d\theta.$$

Supposing, moreover, ϱ to remain constant between the limits $-\theta_1$ and $+\theta_1$, and integrating as in Art. 20, equation 21,

$$t(\theta_1) = \frac{\pi k}{\sqrt{g \left(H_1 \mp H_2 + \frac{\mu A}{W_1} \right)}} \left\{ 1 + \frac{4(H_1 \mp \varrho)^2 + k^2}{4k^2} \sin^2 \frac{1}{2} \theta_1 \right\} \dots \dots \dots (23.)$$

where ϱ is to be taken with the sign $\mp$ according as the surface of the planes of flotation is above or below the load-water line, and H_2 , according as the centre of gravity of the displaced fluid ascends or descends.

Since the value of $\sin^2 \frac{1}{2} \theta_1$ is exceedingly small, the oscillations are nearly tautochronous, and the period of each is nearly represented by the formula

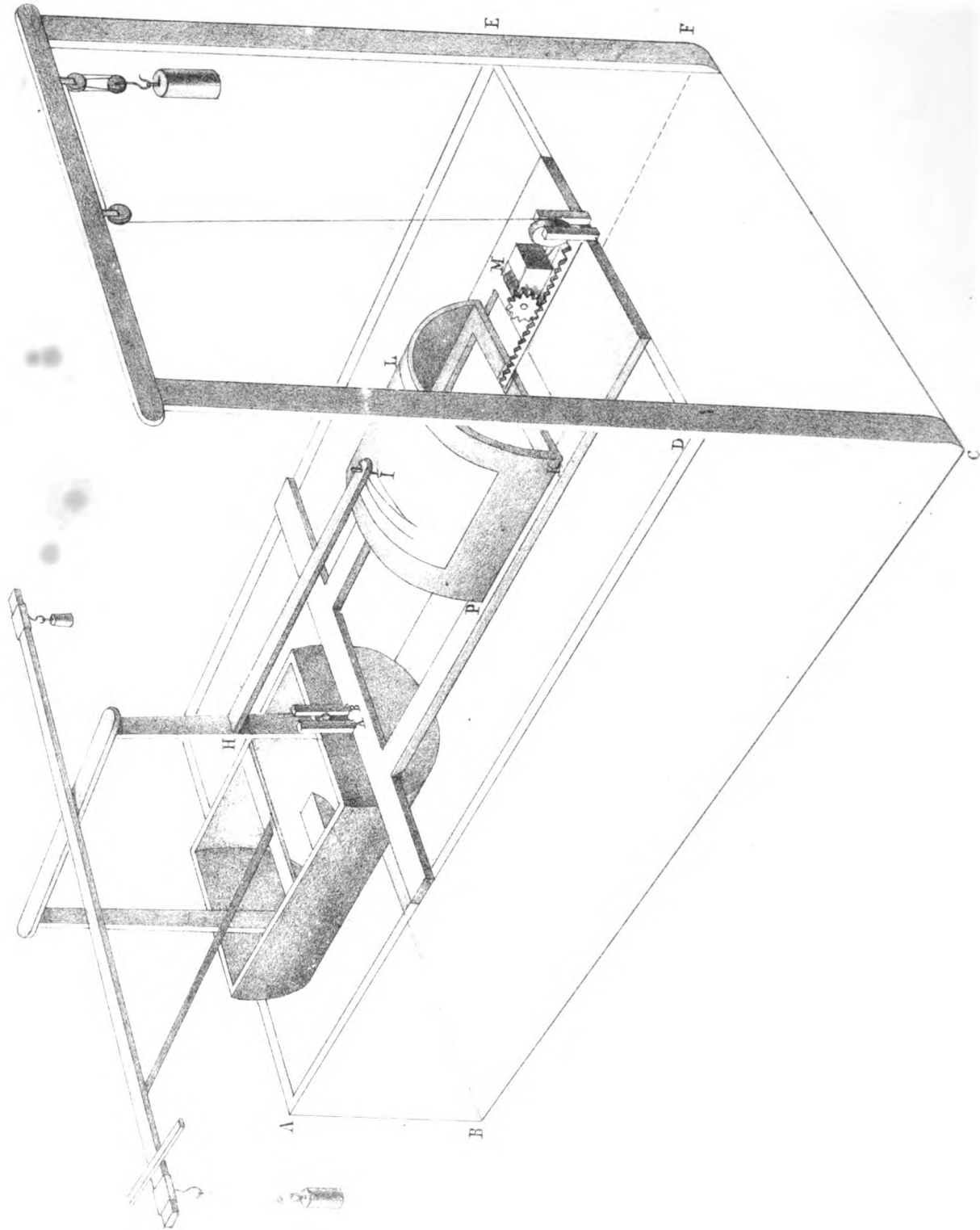
$$t(\theta_1) = \frac{\pi k}{\sqrt{g \left(H_1 \mp H_2 + \frac{\mu A}{W_1} \right)}} \dots \dots \dots (24.)$$

The following method is given by M. DUPIN for determining the value of ϱ^* :—

“If the periphery of the plane of flotation be imagined to be loaded at every point with a weight represented by the tangent of the inclination of the sides of the vessel at that point to the vertical, then will the moments of inertia of that curve, so loaded, about its two principal axes, when divided by the area of the plane of flotation, represent the radii of greatest and least curvature of the envelope of the planes of flotation.”

If ϱ be taken to represent the radius of greatest curvature, the formula 25 will represent the time of the vessel's rolling; if the radius of least curvature (B being also substituted for A), it will represent the time of pitching.

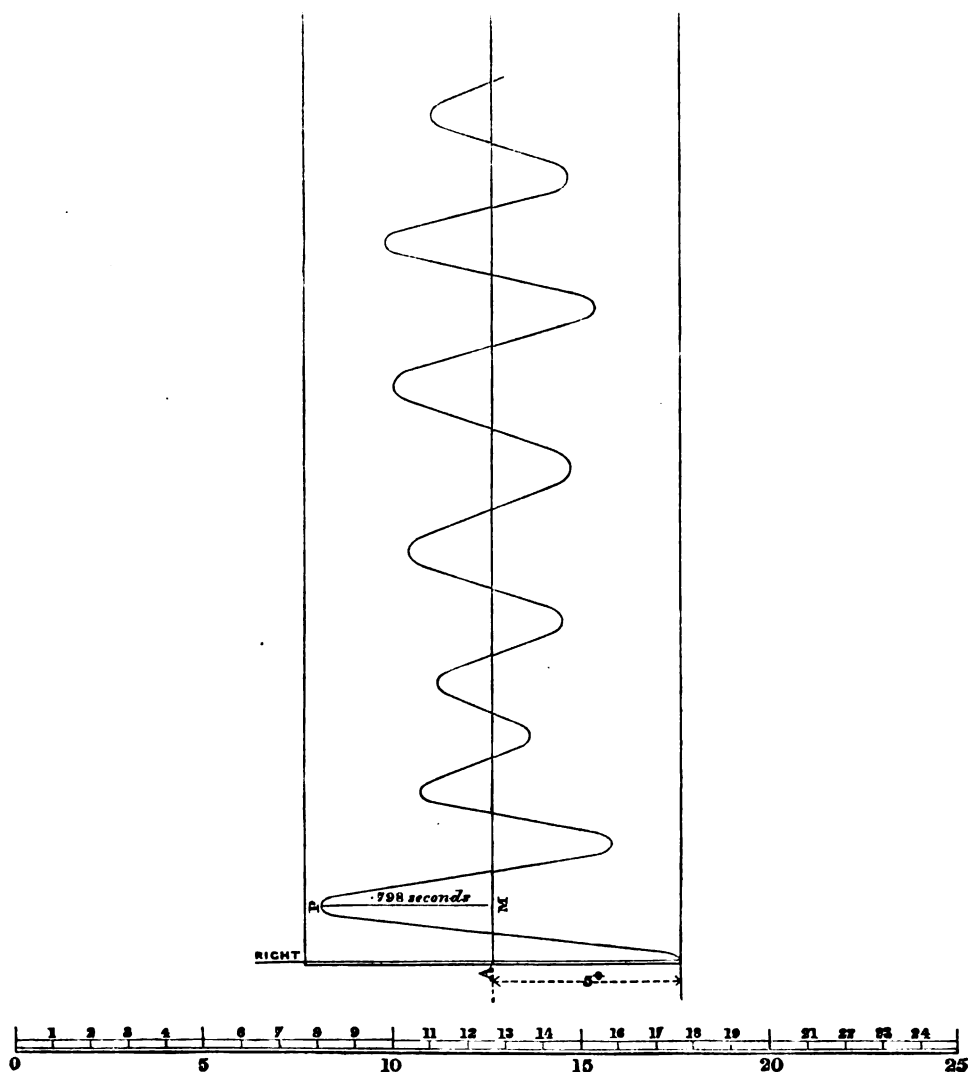
* Applications de Géométrie, p. 47.



22. *Experiments made by Messrs. FINCHAM and RAWSON with the cylindrical model (Plate XLVII. fig. 2) to verify the formula 22, which represents the time of the rolling of a vessel of that form.*

The following ingenious expedient for determining the time of the oscillations of a floating body was suggested by Mr. FINCHAM and found fully to answer the purpose.

The motion of the vessel, when floating in the tank, was limited to a rolling motion by means of upright guides (A, B, Plate XLIX.), receiving between them the extremities of a longitudinal axis fixed in the vessel so as nearly to pass through the centre of gravity. A frame, PKL, resting on two parallel rails, was made to traverse uniformly by means of clockwork, M, in the direction of this axis produced, that is, in the direction of the length of the model, and at right angles to the direction in which it rolled. This frame carried a cylindrical piece of wood, PL, having its axis in the direction of the motion of the frame, and its surface so curved that an arm, HI, fixed to the model parallel to its length and projecting beyond its extremity, should, as it



rolled, sweep parallel to the surface of the cylinder. A pencil was fixed at the extremity of this arm and pressed lightly by a spring upon the surface of the cylinder, which was covered by a piece of paper. The frame which carried this cylinder advancing in the direction of its axis, and the vessel at the same time rolling so as to sweep the arm over its surface perpendicular to that direction, a zigzag line was, by the combination of the two motions, described, as represented in the diagram on the preceding page, of which each two consecutive loops mark the beginning and end of the same oscillation, and the distance AM between them, measured in the direction in which the frame moved, shows the space traversed by it in the time occupied by that oscillation. This space being known from the experiment, and the rate at which the frame travels uniformly being also known, the time occupied in the oscillation may be determined*.

At a certain point the amplitudes of the oscillations will be observed suddenly to increase. This is due to the return of the wave created by the vessel in the act of rolling and reflected by the sides of the tank†.

Before the times of oscillation could be calculated by the formula (22.) to compare them with those determined by experiment, it was necessary that the moment of inertia $W_1 k^2$ of the floating body should be ascertained. To ascertain it by calculation would have been a difficult task, involving in some respects an uncertain result; it was sought therefore by experiment.

With this view a knife-edge was fixed at each extremity of the cylinder, so as accurately to coincide with its axis prolonged. The vessel taken out of the water was then made to rest by means of these knife-edges on two hard steel plates, accurately adjusted to the same level, and in this position it was allowed to oscillate, the times of its oscillations being determined as before. It then became possible to determine the moment of inertia from the following well-known formula—

$$t = \pi \sqrt{\frac{k^2 + h^2}{gh}} \left\{ 1 + \left(\frac{1}{2}\right)^2 \sin^2 \frac{\theta}{2} + \left(\frac{1.3}{2.4}\right)^2 \sin^4 \frac{\theta}{2} + \dots \right\} \dots \dots \dots (25.)$$

Two series of experiments were made, the model vessel being loaded in the one series so as to weigh 197.18 lbs. gross, and in the other so as to weigh 255.43 lbs. Having obtained from the above formula a mean value of k in respect to each of

* The apparatus, and the method of experimenting with it, are more fully described in the Appendix. It will be observed that in the diagram the amplitudes of the successive oscillations are shown to have diminished, up to a certain point. This diminution was found in a great number of experiments to take place with remarkable uniformity. The uniform diminution of the amplitude of oscillation as the body comes to rest, is of course due to the absorption of the *vis viva* of the rolling body by the water which it puts in motion, to the friction of the water which adheres to it on the rest of the water, to the resistance of the air, and to the friction and abrasion of the parts of the rolling body itself in the act of rolling. Mr. RAWSON has made some experiments on this subject, the results of which he communicated to the British Association for the Advancement of Science.

† The precision with which the instant of the return of this wave and its progress are indicated, seems to show this method of experimenting to be well-suited to determining the velocities of waves.

these series of experiments, Mr. RAWSON verified these mean values by calculating from them the times of oscillation by the following equivalent formula:—

$$t = \sqrt{\frac{k^2 + h^2}{gh}} \cdot Fc \frac{\pi}{2},$$

which I had previously communicated to him, in which $Fc \frac{\pi}{2}$ represents that complete elliptic function of the first order whose modulus is c . The results of these experiments and calculations are given in the following Table:—

Gross weight of model.	Amplitude of oscillation on either side of vertical.	Time of oscillation by experiment.	Value of $\sqrt{\frac{k^2 + h^2}{gh}}$ by formula $\frac{\sqrt{ag} \cdot t}{\pi(1+f\theta)}$ (see Appendix).	Mean value of k deduced from the preceding column.	Time of oscillation calculated from the formula $t = 2 \sqrt{\frac{k^2 + h^2}{gh}} \cdot F\left(c \frac{\pi}{2}\right)$ with the preceding value of k , verifying that value.
lbs.			in.	in.	
197.18	5 0 7 30 10 0	.8190 .8905 .7930	11.983 12.562 11.554	 10.465	.8348 .8352 .8359
255.43	2 30 5 0 7 30 10 0	.7280 .6500 .7800 .6760	11.070 10.451 11.784 11.706	 9.3294	.7280 .6500

Table of the times of the oscillation in water of a model vessel as determined,—1st, by the experiments of Messrs. FINCHAM and RAWSON; and 2ndly, by the formula (22.),

$$t = \frac{\pi k}{\sqrt{gh_1 \left(1 \mp \frac{W_2 h_2}{W_1 h_1}\right)}} \left\{ 1 + \frac{4h_1^2 + k^2}{4k^2} \sin^2 \frac{1}{2} \theta_1 \right\},$$

Constants of formula.	Angle of oscillation θ_1 .	Time of oscillation in seconds by formula.	Time of oscillation in seconds by experiment.
Weight of model = 197.18 lbs. $W_2 = 0$ $k = 10.465$ in. $h_1 = 4.9$ in. $t = .75668 \left\{ 1 + .46924 \sin^2 \frac{1}{2} \theta_1 \right\}$	5 10 15 20	.75637 .75840 .76172 .76641	.798 .798 .840* .784
Weight of model = 255.43 lbs. $W_2 = 0$ $k = 9.3294$ in. $h_1 = 5.97$ in. $t = .61033 \left\{ 1 + .56021 \sin^2 \frac{1}{2} \theta_1 \right\}$	5 10 15 20	.61094 .61289 .61471 .64811	.658 .665 .616 .602

23. General considerations applicable to the stability of floating bodies.

An exceedingly small pressure is sufficient to move a floating body from its position of stable equilibrium. As the inclination becomes greater the pressure must be increased until it attains, in a given position, its maximum value. Now if instead of this variable and continually increasing pressure we conceive a constant one to

* This experiment was faulty.

have been applied, less than the maximum one spoken of above, but considerably greater than that necessary to produce the initial motion, it is not difficult to see that this lesser pressure, by reason of its continued action on the body, may be sufficient to carry it *through* the position in which it would have required the *maximum* pressure to have *held* it; for in all the positions in which this pressure exceeded that necessary to move the body, the excess of its work over that of the resistance is accumulated under the form of *vis viva*, and in all those in which it fell short of that pressure, the work thus accumulated comes to its aid, and carries the body forward*.

24. To move a body from a given position into any other, it is not necessary that the work of the forces whence this change of position results should continue to be done upon it during the whole period of such change. They may be in the nature of pressures whose action ceases when they have communicated the *vis viva* necessary to continue the motion until the second position is attained, as in the case of the impact of one body on another, or of a gust of wind acting on a floating body, or of a blow of the sea.

In all these cases the excess of the work of the disturbing forces over that of the

* As an illustration of this principle, let us take the analogous case of a cylindrical body (fig. 7)—whose radius CD is represented by a —rolling on a horizontal plane, and to simplify the investigation let us suppose the force P , which causes it to roll, to be applied horizontally to the axis c . Let the weight of the cylinder be represented by W , and suppose it to be so loaded that the distance of its centre of gravity from its axis may be $CG(=c)$. Let the cylinder be made to roll through an angle (θ) from the position in which it would rest, and in which CG is vertical. The work done upon it by P , whilst it thus rolls, is represented by $Pa\theta$, and the work opposed to its rolling by the weight of the cylinder, is $Wc \text{ vers } \theta$,

$$Pa\theta - Wc \text{ vers } \theta$$

is therefore the excess of the work of the forces whose tendency is in the direction of the motion over the work of those whose tendency is in the opposite direction. This excess represents one-half the *vis viva*, and in the extreme position into which the cylinder rolls and from which it begins to roll back, this vanishes, so that in this position

$$Pa\theta - Wc \text{ vers } \theta = 0.$$

If this position be the inverted position of the body $\theta = \pi$, and

$$P = \frac{2Wc}{\pi a}.$$

Now let us suppose that P , instead of being a constant pressure, had been made so to vary that in every position into which the body rolled it was only just sufficient to make it roll *slowly* out of that position. The maximum pressure P' , applied under these circumstances, would have been that corresponding to the position in which CG is horizontal, and would have been represented by the formula

$$P' = \frac{Wc}{a},$$

whence it follows that

$$P = \frac{2}{\pi} P' = .6366 P';$$

so that the pressure, which, being continually applied the same in amount, would be sufficient to overturn the body, is less than two-thirds of that which must be exerted in its position of maximum effort, the lesser but constant pressure accumulating in its previous position, an excess of work which is sufficient to carry the body through and far beyond that position in which it would have required the greater pressure to have held it.

resistances accumulates, so as to be represented at any time by half the *vis viva* of the body, and it is the work accumulated under this form and in this amount, which, when the operation of the disturbing forces is withdrawn, carries the body forwards.

25. If, in the case of an oscillatory motion, the force which causes that motion be intermittent, and if the periods of this intermission so coincide with those of the body's oscillation that the force is withdrawn when any oscillation in its direction is completed, and renewed when the next oscillation in *that direction* begins, it is evident that the *vis viva* created by it in all such successive oscillations will be accumulated, and the amplitudes of the oscillations rendered, in succession, greater than one another, so that by the intermittent action of a small force which so synchronizes with the oscillations of the body on which it is made to act, a great inclination of its position from that of its equilibrium may eventually result*. This might for instance be produced, in respect to a floating body, by the action of gusts of wind or blows of the waves, repeated at stated intervals.

If the periods at which the intermittent work is done upon the body, instead of synchronizing with the commencement of each oscillation and thus favouring the motion, had so occurred as to oppose it at stated intervals (which may or may not synchronize with the body's own oscillations), it is evident that a complicated motion dependent on these several conditions will result, in which the oscillations will be repeated in cycles.

* As an illustration of this, let us suppose that in case of the cylinder (fig. 7), the motive force, instead of being applied horizontally to the axis, is a weight w applied to the point D on its surface when the cylinder was inclined at the angle θ to its position of stable equilibrium; and that when the point D had, by the rolling of the cylinder, been made to descend until it again touched the plane, the weight w was withdrawn, the cylinder completing its oscillation on the other side of the vertical by reason of the *vis viva* thus communicated to it.

On its return it will oscillate (the resistance being neglected) through the same angle, on the side of the vertical from which it started, as it completed on the opposite side. Let the weight w be then placed again on the point D, and taken off a second time when this point comes in contact with the plane; and so continually, a rocking motion of increasing amplitude being produced by the alternate placing and withdrawal of the weight until at last the position of CG is reversed and the cylinder is overturned.

Let $\theta_1, \theta_2, \theta_3 \dots \theta_n$ be the angles on either side of the vertical through which, in its successive oscillations, the cylinder is made to roll, then shall we obviously have the following relations:—

$$(Wc + wa) \text{ vers } \theta = Wc \text{ vers } \theta_1,$$

$$(Wc + wa) \text{ vers } \theta_1 = Wc \text{ vers } \theta_2,$$

$$\&c. \quad \quad \quad \&c.$$

$$(Wc + wa) \text{ vers } \theta_{n-1} = Wc \text{ vers } \theta_n.$$

Multiplying these equations together,

$$(Wc + wa)^n \text{ vers } \theta = (Wc)^n \text{ vers } \theta_n.$$

But

$$\theta_n = \pi, \quad \therefore \text{vers } \theta_n = 2 \text{ and } \text{vers } \theta = 2 \sin^2 \frac{1}{2} \theta.$$

$$\therefore (Wc + wa)^n \sin^2 \frac{1}{2} \theta = (Wc)^n,$$

and

$$w = \frac{Wc}{a} \left\{ \left(\operatorname{cosec} \frac{1}{2} \theta \right)^{\frac{2}{n}} - 1 \right\}.$$

26. If, when the work of any disturbing force begins to be done upon a floating body, it be already acted upon by some other force, which has caused it to incline from the position in which it would otherwise rest through some given angle α , as in the case of a squall or a heavy sea striking a ship already inclined by the action of a steady wind upon her beam; representing by θ the additional inclination given to it by the action of the disturbing force, whose work, in giving it this inclination, is represented by $u(\theta)$, and representing the work done through this same angle θ by the forces originally impressed, and still acting, upon it by $U(\theta)$, we have

$$Wc\{\text{vers}(\alpha+\theta) - \text{vers} \alpha\} = U(\theta) + u(\theta).$$

Differentiating and transposing,

$$\delta\theta = \{\delta U(\theta) + \delta u(\theta)\} \frac{1}{Wc \sin(\alpha+\theta)}.$$

The small additional angle $\delta\theta$ through which the body is made to roll by the application to it of a given small additional amount of work,

$$\delta U(\theta) + \delta u(\theta),$$

varies therefore inversely as the sine of the inclination, and is greatest in the position nearest to the vertical position; or, in other words, the body sustains in an inclined position a less change of that position by the application of a given disturbing force than it does in a vertical position. It yields most readily to the action of any disturbing force in its vertical position, and the further it is made to deviate from this position (within certain limits and under certain conditions), the more resolutely does it oppose any further deviation. This explains the liability of a ship to rolling when sailing before the wind, and her stiffness in the water when close hauled.

Conclusions applicable to Ship-building.

27. To make an alteration in the angle through which a ship rolls, it is necessary to elevate or to depress her weights. In the former case she will roll through a greater, and in the latter through a less angle. It does not alter the amplitude of rolling to move the weights horizontally, but only the time of rolling, provided the trim of the ship remain unaltered; for this does not alter the position of the centre of gravity of the ship or of the displaced fluid, and it is upon these that the stability of the ship depends (Art. 7 and equation 18.).

28. When a ship's motion is only a rolling motion, or about an axis parallel to her length, the position of that axis is, at any instant, determined by the intersection of a horizontal line through her centre of gravity, and a vertical line through the centre of gravity of her plane of flotation at that instant (Art. 19).

29. A ship should be so constructed that the centre of gravity of that plane of flotation, whose boundary is the load water-line, may be vertically above the centre of gravity of the ship. If this be not the case, the pressure of the additional water displaced by any vertical oscillation of the ship, acting obviously at the centre of gravity

of that plane and not in the same vertical with the centre of gravity of the ship, will cause, in addition to that vertical oscillation, a pitching or a rolling motion.

30. All the planes of flotation of the ship, when it is made to roll through small angles from its vertical position, should have their centres of gravity in the midship section (supposed to be that which contains the centre of gravity of the ship), and the centre of gravity of the displaced fluid should also always remain in this plane; for if that be not the case, it is obvious, from Art. 28, that the axis about which the vessel rolls cannot be parallel to its length, so that every rolling must be accompanied also by a pitching motion.

31. The angle through which a ship rolls under the action of a gust of wind, is essentially different from that at which it would be held inclined by the steady action of the same force of the wind, so that when the inclination which a given pressure would give to the ship, if applied to the centre of effort of the wind on the sails, is calculated (as is customary) by the formula known as that of ARWOOD, it is erroneous to conclude that the vessel, if subjected suddenly to that force of the wind, would incline only through that angle.

The experiments of MESSRS. FINCHAM and RAWSON, of which the results are stated in the Table, p. 615, show that it will roll through nearly double that angle, confirming, in this respect, the deductions of theory (Art. 2).

Neither can calculations, made by means of the theorem of ARWOOD, as to the respective pressures which would hold different ships inclined at the same given angle, be considered as determining with certainty their *relative* stabilities in respect to rolling; for the amplitude of each oscillation depends upon the stiffness of the vessel, not only in respect to that given inclination, but upon its stiffness at every other inclination which it must have passed through to reach that angle, and at every inclination which by reason of its acquired momentum it may pass through afterwards. The same observation is applicable to the effect of disturbances in the water-line and to blows of the waves.

32. All the causes which produce the rolling motion of a ship, whether they be gusts of wind, disturbances of the water-line, or blows of the waves, are measured in their effect upon it under the form of *work* (travail), so that according as a ship requires a greater or a less amount of work to be done upon her to cause her to roll through a given angle, there is a greater or less probability that, when at sea, she will roll through so great an angle as that. The angle through which the ship will be made to roll under the action of a given amount of *work*, is measured (Art. 7) by the product of her weight, by the difference or the sum of the vertical displacements of her centre of gravity, and the centre of gravity of her immersed part whilst she is in the act of rolling through that angle; the difference being taken or the sum according as these centres of gravity both ascend, or as the one ascends and the other descends; or in other words, it is *the work necessary to raise the vessel bodily through the difference or the sum of these vertical displacements*.

33. If therefore some existing vessel were fixed upon whose qualities in respect to rolling were well known, and if it were determined by calculation from this theorem what amount of work must be done upon that vessel to make it roll through some *given angle*; and this amount of work being so determined in respect to that existing ship, if, before all other ships of the same class were built, it were determined by a similar calculation, made from the drawings of those ships, whether a greater or a less amount of work would be necessary to make *them* roll through the same angle, then it would be known whether *these* ships would, under the like circumstances, roll more or less than *that* ship, and the forms proposed to be given *them* might be adopted or might be altered accordingly.

I conceive that by this means, if duly applied, great certainty might be given to the construction of ships in respect to rolling, and of course to pitching, for the same principles which apply to the one apply also to the other, with no other difference than in the direction in which the inclination is supposed to take place.

34. The force of the winds and waves, to the action of which a vessel is liable, may be supposed to vary as the surface she opposes to them, that is, to the area of her sails and the superficial dimensions of her hull. In vessels geometrically similar these vary as the squares of any of their similar linear dimensions, their lengths for instance. On the other hand, the weights of such vessels, supposed to be similarly loaded, varying as the cubes of their lengths; and the depths of their centres of gravity, and of the centres of gravity of their immersed parts, varying as their lengths; their dynamical stabilities, with reference to a given inclination, vary as the fourth powers of their lengths. Since, then, in reference to vessels thus geometrically similar, the disturbing forces, to the action of which they are subject, vary as the squares of their lengths and their stabilities as the fourth powers, it follows that their actual steadiness in the water will vary as the *squares* of their lengths, the greater vessel being more steady than the less in this proportion.

35. The expedients which I have pointed out for so designing a ship as to satisfy the conditions of easy rolling and pitching, suppose a knowledge of the exact position of the centre of gravity of the ship and of the centre of gravity of her displacement. The determination of these however is no new question; a knowledge of them has always been considered necessary to the skilful building of a ship, and the methods given for that purpose in books on ship-building are sufficiently accurate for the purpose, if the data are to be relied upon; and if not, nothing is required but the labour to determine these data.

36. That form of vessel in which the surfaces subject to immersion and emersion, when intersected by planes perpendicular to the vessel's length, *have circular* sections, having their centres in a common axis, is, *cæteris paribus*, eminently a *stable form*; because in a vessel of such a form (Art. 12.) the centre of gravity of the portion of the displaced fluid which is included within the solid of revolution (ATB figs. 3, 4) formed by all these circular sections, does not in the act of rolling *rise*.

If it be not practicable to give to the vessel, throughout its whole length, a form subject to these conditions, this is practicable with regard to the midship section, which is the governing section.

37. Of vessels having this general form, those are, *cæteris paribus*, the most stable in which the circular area, when completed, includes entirely the corresponding section of the ship (as shown in fig. 4), because, in respect to these ships, the centre of gravity of the displaced fluid *descends* as the ship rolls (Art. 13.); and when this is the case, the work necessary to incline the ship through any given angle (which measures its dynamical stability) is equal to that necessary to raise it bodily through a height equal to the *sum* of the vertical displacements which its centre of gravity, and the centre of gravity of the displaced fluid, suffer in that inclination, whilst in the opposite case, represented in fig. 3, it is equal to the work necessary to raise it through the *difference* of those heights*.

Of the class of vessels represented in fig. 4, the stability of those is the greatest, other things being the same, in respect to which the space STD between the circumference of the circular area and the hull of the vessel is the greatest.

38. It is not necessary to these results, taken in a general sense, that the sections of the vessels should be accurately circular as to their parts subject to immersion and emersion. If on the midship section of a ship a circle can be described, having its centre in the vertical axis of the section so as nearly to coincide with the parts of the periphery subject to immersion and emersion, then the ship may be distinguished as to whether it belongs to the class in which the centre of gravity of the immersion ascends or descends, by observing whether this circular area is wholly or only partly included within the section. The midship sections of Her Majesty's ships Vanguard, Bellerophon and Canopus present illustrations of this principle. They are represented in figs. 8, 9, 10. It will be observed, that if a circular area be struck on each section according to the conditions stated above, that area will in the Bellerophon and Canopus entirely include the corresponding sections of the hulls of the two ships, a wider space intervening between the two areas in the Canopus than the Bellerophon. Other things being the same, the former ought therefore to be the more stable ship. In the Vanguard the circular section does not wholly include that of the hull. This should therefore be the least stable ship of the three. These conclusions are, I believe, in accordance with the known qualities of the ships. If there be any ship whose midship section resembles that represented in fig. 3, the centre of gravity of its displacement will ascend in the act of rolling, and, *cæteris paribus*, it cannot but be an unstable ship.

* The hull may be so shaped as to cause the centre of gravity of the vessel to descend in the act of rolling. In this case ΔH_1 , equation 6, must be taken negatively. If the centre of gravity of the immersed part ascends, $U(\theta)$ will in this case be negative, and the position will be one of unstable equilibrium. If the centre of gravity of the immersion descends, ΔH_2 , in equation 6, must be taken positively; and if it exceeds ΔH_1 , the equilibrium will be stable.

In all these comparisons it has been supposed that the ships do not otherwise differ as it regards their stability, than in their approximation to the typical forms represented in figs. 3 and 4. It has been supposed therefore (see equation 17) that the depths of their centres of gravity and of the centres of gravity of the fluid they displace in a vertical position are the same, and that the moments of inertia of their planes of flotation are equal. This cannot be the case; and it is impossible to know to what extent this error in the hypothesis may in any particular case affect the measure of the dynamical stability, except by calculating it by formula 17. The problem is far too complicated to render the application of any general principle—except with this precaution—safe. It is not—in this respect as in others—more practicable to dispense with the resources of mathematical reasoning and of calculation, in building the ship, than, after she is built, in sailing her.

39. It is a deduction of theory (equation 24.), and is confirmed by experiment, that, within the ordinary limits of rolling, the *Time* of a vessel's rolling is independent of the angle through which it rolls, being dependent only upon the form of the vessel, its weight, the position of its centre of gravity, and its moment of inertia about an axis passing through that point (see equation 24); so that every different vessel, when loaded in a given manner, has a *time of rolling proper and peculiar to it*, and which may be said to *characterize* it. And the same is true of the time of pitching.

40. This time of the oscillations of the ship may have such a relation to the times of oscillation of the waves* as to cause the blows of the sea to be received by the ship at those instants when they will produce the greatest effect on the amplitude of her oscillations (Art. 25.), and thus a little sea may, under certain circumstances, produce very heavy rolling.

If there be not this relation between successive oscillations of the ship and of the waves, then there will be, during a certain number of oscillations, an antagonism of the two, until the times of the one class of oscillations have so gained upon those of the other as to bring about an interference. The vessel will then probably be comparatively at rest. Then will follow a series of oscillations of the waves and the ship, which will in various degrees concur to produce heavy rolling, until it reaches a maximum, when the same cycle of changes will be gone through again†.

41. The straining of the ship in the act of rolling, is dependent upon the time of its oscillations. This straining takes place in every part of it, but more particularly (by reason of their elasticity) in the masts. When the rolling begins, the higher parts of the masts, by reason of their inertia, remain behind the lower portions, and the masts bend; as the rolling proceeds they receive an independent motion

* If the ship be under sail, the rate at which she sails and the distances of the waves from one another, measured in the direction in which she sails, are also among the conditions on which her rolling depends.

† This cyclical antagonism and concurrence of the independent oscillations of the waves and the ship will interfere, to a certain extent, with the tautochronism of the ship's oscillations.

from their elasticity, influenced, besides this cause, by their weight and length, and this motion assumes the form of an independent oscillation, affecting more or less the oscillatory motion of the vessel itself.

If, at the instant when the ship would otherwise begin to roll back, the elasticity of its masts is in the act of carrying them forwards, there will be a violent strain of the ship, which would not take place in a ship so constructed or so loaded as to create that synchronism of the independent oscillations of the vessel and its masts, which is implied in the fact that she does not strain herself in rolling.

42. The properties of different ships with regard to the strain they suffer in rolling, and whether, in respect to the seas which are of most frequent occurrence, they roll easily or heavily, are probably well known, so that it would be possible to fix upon some ship of each class which might in this respect serve as a standard with which other ships of that class might be compared. The time of oscillation proper and peculiar to this ship might also be ascertained by experiment. It would then become possible to build and load all other ships of that class so as to oscillate in the same time as that ship, and thereby ensure, supposing them to be masted in the same way, very nearly the same qualities in regard to the strain they suffer in rolling.

The form of the ship so constructed need not however in any respect resemble that of the standard ship; all that is required is, that—in respect to the dimensions of its parts and the distribution of its weights when loaded—it should satisfy the conditions implied in assigning a given value to $t(\theta)$ in equation 24. In all other respects full latitude is allowed to the builder*.

43. I have shown (Art. 19.) that the axis about which the ship may at any instant be conceived to roll is perpendicular to two straight lines, one of which is drawn horizontally from the vessel's centre of gravity parallel to the direction in which it is rolling, and the other vertically through the centre of gravity of its plane of flotation at that instant.

Its *vis viva*, when rolling or pitching, is therefore greater as its weights are placed at a greater distance from this axis, and less as they are nearer to it.

Whence it follows as a general principle (equation 24.), that the ship is made to roll more slowly by moving its weights to a greater distance from this axis, as when the yards are run up; and more quickly by bringing them nearer to it, as when the guns are run back in a heavy sea to diminish the strain on the ship's timbers.

44. The form under which H_2 enters equation 24, shows that the greater the depth of the centre of gravity of the ship's displacement the more slowly (other things being the same) will she roll, provided that she be a ship of that class of which fig. 3 is the type, in which this quantity (*i. e.* the depth of the centre of gravity of the displacement) diminishes as the vessel rolls; but that if, on the contrary, she belongs to

* Taking a ship, whose form may indeed (within certain assignable limits) be any whatever, it is in his power, guided by that equation, so to load it as that it shall oscillate in the same time with any other ship whose form may be in all respects different.

the class of which fig. 4 is the type, the centre of gravity of whose displacement descends in the act of rolling, she will roll the faster as this centre of gravity is seated deeper in her hull.

45. It may be received as a general rule that (other things being the same) vessels of the class fig. 4 roll more quickly than those of the class fig. 3, but do not roll so far. So that unless some special provision be made for that end, stability in rolling may not, and will not probably be obtained except at the expense of quick rolling.

46. The form under which A enters equation 24, shows that breadth of beam is not of itself conducive to slow rolling, and that it can only tend to it indirectly, by carrying the ship's weights further from the axis about which it rolls, and thereby augmenting the quantity k , which represents in the formula the moment of inertia of the ship, and is the ruling element of the time. The form under which W_1 enters the equation shows heavy loading to be conducive to slow rolling.

47. It would be unsafe however to be guided in the construction of a ship by any one of these considerations taken separately from the rest, for there is scarcely any element of the discussion which can be changed without bringing about a change in an opposite direction in some other.

What will be the total result of any proposed change, and by what means the whole object sought by it is to be accomplished, can only be determined by that complete mathematical discussion of the question in all its elements, the principles of which it has been the object of this paper to develope, and which the formulæ contained in it afford a means of applying.

December 1, 1849.

APPENDIX.

Experiments on the Dynamical Stability and the Oscillations of Floating Bodies. By JOHN FINCHAM, Esq., Master Shipwright in Her Majesty's Dockyard, Portsmouth, and ROBERT RAWSON, Esq.

Experiments necessary to verify the formula 6,

$$U(\theta) = W(\Delta H_1 - \Delta H_2),$$

which represents the work done in deflecting a floating body through an angle such that the height through which the centre of gravity of the floating body is raised shall be ΔH_1 ; and the height through which the centre of gravity of displacement is raised, shall be ΔH_2 .

For this purpose two models were made, such that their sections were uniform throughout the whole length of the model: the section of one was a triangle, and of the other a circle (see figs. 1, 2).

If the reader will refer to Art. 10 for a general description of the apparatus, the following explanation of the drawing fig. 1 will be sufficient to show how the experi-

ments have been conducted. It represents a section of the model; TK is the yard by means of which the deflecting weight hanging at Q, deflects the model from the upright position. AC is water-line in the upright position, and BD that in the extreme position into which it rolls; EF that in the position in which it finally rests.

In the first place, the model is adjusted by means of moveable weights, until the water-line AC is parallel to the upper side, LN, of the model; and then a string, SK, is fixed at S and K, so that when the deflecting weight is placed at Q no effect is produced by the deflecting weight on the model until the string SK is cut (RS is a fixed beam independent of the model).

When the string SK is cut there is an extreme deflection where the water-line becomes BD, and a permanent deflection where the water-line becomes EF.

These lines are determined in the following manner: PX is a thin graduated scale, fixed at P at right angles to the arm of the lever TK, and having a strip of prepared paper fixed upon its surface, which shows distinctly, by the depth to which it is wetted when the vessel rolls, a point in the extreme position of the water-line. Two other points in this position of the water-line are determined by means of scales similarly applied to L and N.

When any two of these three points are observed, a section of the model being drawn of the half-size on a drawing-board, we could set off upon it the distances Pp, Ll, Nn, and thus draw in the water-line BD. There is no occasion to use prepared paper to show the water-lines, excepting in the ultimate deflection.

The paper which we used, and which answered the purpose admirably, was nothing more than common writing-paper rubbed over with a little colouring, in order to take off from the surface of the paper any oily matter which might prevent the water's making a distinct mark upon it.

This means was adopted as the best means, after several other expedients had been tried with partial success.

The centre of gravity G was determined by observing the permanent water-line EF in a number of deflections by means of various deflecting weights. The position of this water-line, for any given deflecting weight, being set out on the drawing, we were enabled, knowing the weight of the vessel, to determine the position of the centre of gravity, by well-known principles of statics, with no other aid than that of the scale and compasses. Suffice it to say, that great care was taken to get this point.

The points H and h, which are the centres of gravity of the part immersed in the vertical position of the vessel and in the position into which it rolls, were determined from the known property of the centre of gravity of a triangle in fig. 1, and from the common formula for determining the centre of gravity of the segment of circle in fig. 2.

The drawings were made to half size from the following Table, which was filled up during the time the experiments were in operation: this circumstance enabled us obtain all the data required for our computations with extreme exactness.

Cylindrical Model.

Model at rest, without the deflecting weight.			Model at rest, with the deflecting weight.			Model at ultimate deflection, with deflecting weight.			Weight of the model and deflecting weight.		Distance from centre of model to P and Q.		
L.	Lt.	Na.	Po.	Lq.	Nr.	Pp.	Ll.	Na.	Deflecting weight.	Weight of model.	P.	Q.	
	$8\frac{1}{8}$	$8\frac{1}{8}$	...	6	$10\frac{1}{8}$	$31\frac{5}{16}$	$4\frac{3}{16}$	...	lbs. Av. 1.957	lbs. Av. 197.18	ft. in. $3\ 6\frac{1}{2}$	ft. in. $5\ 4\frac{3}{8}$	} (2.)
	8	$8\frac{1}{8}$	...	6	$10\frac{1}{8}$	$31\frac{5}{16}$	$4\frac{1}{8}$	...			$3\ 6\frac{1}{2}$	$5\ 4\frac{5}{8}$	
				$6\frac{1}{2}$	$9\frac{1}{2}$		...	...			$3\ 6\frac{1}{2}$	$4\ 0\frac{5}{8}$	
	6	$6\frac{1}{2}$	...	$4\frac{3}{8}$	$7\frac{3}{8}$	$34\frac{3}{16}$	$3\frac{3}{8}$	...	1.957	255.43	$3\ 6\frac{1}{2}$	$5\ 4\frac{3}{8}$	} (3.)
				$5\frac{1}{8}$	7		...	...			$3\ 6\frac{1}{2}$	$4\ 0\frac{5}{8}$	
	6	$6\frac{1}{8}$	31	$2\frac{1}{2}$	$9\frac{7}{8}$	$20\frac{1}{2}$	$0\frac{5}{8}$	...	5.0285	255.43	$3\ 6\frac{1}{2}$	$5\ 4\frac{3}{8}$	
			$33\frac{1}{2}$	$3\frac{7}{8}$	$8\frac{9}{16}$	21	...	...			$3\ 6\frac{1}{2}$	$4\ 0\frac{5}{8}$	} (4.)
	$8\frac{1}{8}$	$8\frac{1}{8}$	34	5	$11\frac{3}{16}$	$24\frac{5}{16}$	$2\frac{3}{16}$	...	2.8225	197.18	$3\ 6\frac{1}{2}$	$5\ 4\frac{3}{8}$	
			$36\frac{1}{2}$	$5\frac{1}{2}$	$10\frac{5}{16}$	...	...	...			$3\ 6\frac{1}{2}$	$4\ 0\frac{5}{8}$	

The position of the water-line corresponding to the extreme position into which the model rolled being thus determined in every experiment, the corresponding displacement was also known. In fig. 1, this displacement having a triangular section eMf , its centre of gravity h could readily be determined by construction. In fig. 2, the displacement having a circular section, its centre of gravity h could be determined by known rules. Drawing a perpendicular hc from h upon BD , this line measures the depth of the centre of gravity of the immersion in the extreme position into which the vessel rolls, and Ha measures it in the vertical position; therefore the difference of these lines measures its elevation in the act of rolling. In like manner the perpendicular Gb measures the depth of the centre of gravity of the model when the water-line was BD , and Ga was its depth in the vertical position, therefore the difference of these lines is its elevation in the act of rolling.

Thus the elevations of the centre of gravity of the model and of the centre of gravity of the displaced fluid, in the act of rolling, are found, and its weight being known, the work which must have been done upon it to cause it thus to roll may be determined according to equation 6.

But the work actually done upon it by the deflecting weight may, in like manner, be determined; for if a perpendicular Qo be drawn from Q on BD , the length of this line will be the height of Q when the water-line was BD , and Qm was its height in the vertical position, therefore the difference of these lines is the space through which the deflecting weight has descended vertically; and the product of this distance of the deflecting weight gives the work actually done by it upon the rolling body. This amount of work ought, by the theory, to be the same with that found as above from equation 6; and the comparison of results thus obtained, by theory and experiment, constitutes a verification of the formula, and is given in the Table, p. 615.

A single example will show the way in which the calculations were made.

In the triangular model, Experiment 1, the following dimensions were measured in inches :—

$$Ga=1.75, \quad Gb=1.05, \quad Ha=2.75 \\ hc=2.24, \quad Qm=15.2, \quad Qo=3.9$$

$$\therefore \text{elevation of centre of gravity of model, in feet} = \frac{1.75-1.05}{12} = \Delta H_1$$

$$\text{elevation of centre of gravity of displaced fluid, in feet} = \frac{2.75-2.24}{12} = \Delta H_2$$

$$\text{weight of model in lbs.} \dots \dots \dots = 33.8626 = W.$$

$$\therefore \text{by equation 6, } U(\theta) = \left\{ \frac{(1.75-1.05)-(2.75-2.24)}{12} \right\} \times 33.8626 = .5361.$$

$$\text{Also vertical descent of deflecting weight, in feet} = \frac{15.2-3.9}{12},$$

$$\text{deflecting weight } Q \text{ in lbs.} = .5485;$$

$$\therefore \text{by experiment } U(\theta) = \left\{ \frac{15.2-3.9}{12} \right\} \times .5485 = .5165.$$

The computations on both these models, made in accordance with formula 6, agree with the experiments.

All the experiments show what we conceive to be important, that the same moment of force which is necessary to maintain a vessel in a position θ at rest, will deflect the vessel through nearly twice θ when made to act upon a vessel which is not deflected. Arwood's statistical stability therefore appears to us to be of little use, so far as the rolling of the vessel is concerned.

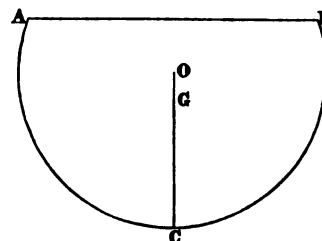
Experiments on the Time of Oscillation.

To find the moment of inertia of a cylindrical model, whose radius is 13.5 inches, weight 197.18 lbs. avoirdupois, round an axis passing through the axis of the cylinder.

ABC is a vertical section of the cylinder passing through its centre of gravity G.

It is required to find the moment of inertia of the cylindrical model, about an axis passing through O, the centre of the cylinder at right angles to the section ABC.

For this purpose the model was made to oscillate out of the water, upon knife-edges passing through O and parallel to the sides of the cylinder. Several experiments were carefully made, and the times of oscillations were observed through the amplitude of 5° , $7\frac{1}{2}^\circ$ and 10° . The apparatus by means of which the times were observed, enables us to measure the times of oscillation to an exactness of .013 of a second, or in round numbers to the one-hundredth part of a second of time.



If T = time of oscillation of a simple pendulum,

2θ = angle of amplitude of oscillation,

r = length of pendulum,

g = 32.19084 feet,

π = 3.1415927, &c. &c.,

we have

$$T = \pi \sqrt{\frac{r}{g}} \left\{ 1 + \left(\frac{1}{2}\right)^2 \left(\frac{\text{vers } \theta}{2}\right) + \left(\frac{1.3}{2.4}\right)^2 \left(\frac{\text{vers } \theta}{2}\right)^2 + \left(\frac{1.3.5}{2.4.6}\right)^2 \left(\frac{\text{vers } \theta}{2}\right)^3 + \&c.* \right\}.$$

Put R = radius of oscillation of a compound pendulum.

Then the time of oscillation of the compound pendulum will be the same as in the case of the simple pendulum, if we put R instead of r in the above formula.

$$T = \pi \sqrt{\frac{R}{g}} \left\{ 1 + \left(\frac{1}{2}\right)^2 \left(\frac{\text{versin } \theta}{2}\right) + \left(\frac{1.3}{2.4}\right)^2 \left(\frac{\text{versin } \theta}{2}\right)^2 + \left(\frac{1.3.5}{2.4.6}\right)^2 \left(\frac{\text{versin } \theta}{2}\right)^3 + \&c. \right\}. \quad (26.)$$

We may here observe that

$$R = \frac{k^2 + h^2}{h},$$

where k = the radius of gyration about the centre of gravity, h = the distance from the centre of suspension to the centre of gravity of the model.

Since

$$\frac{\text{versin } \theta}{2} = \frac{1 - \cos \theta}{2} = \sin^2 \frac{\theta}{2},$$

we shall have

$$T = \pi \sqrt{\frac{k^2 + h^2}{gh}} \left\{ 1 + \left(\frac{1}{2}\right)^2 \sin^2 \frac{\theta}{2} + \left(\frac{1.3}{2.4}\right)^2 \sin^4 \frac{\theta}{2} + \left(\frac{1.3.5}{2.4.6}\right)^2 \sin^6 \frac{\theta}{2} + \&c. \right\}. \quad (27.)$$

If we put

$$f(\theta) = \left(\frac{1}{2}\right)^2 \sin^2 \frac{\theta}{2} + \left(\frac{3}{4}\right)^2 \sin^4 \frac{\theta}{2} + \left(\frac{3.5}{4.6}\right)^2 \sin^6 \frac{\theta}{2} + \&c. \&c., \quad (28.)$$

equation (4.) will become

$$T = \pi \sqrt{\frac{k^2 + h^2}{gh}} \{ 1 + f(\theta) \}.$$

From which equation we obtain

$$\sqrt{k^2 + h^2} = \frac{\sqrt{gs}}{\pi \{ 1 + f(\theta) \}} \times T. \quad (29.)$$

If T_1, T_2, T_3 be the time of the first, second and third oscillations during the time that θ and s remain constant, we shall have

$$\begin{aligned} n\sqrt{k^2 + h^2} &= \frac{\sqrt{gs}}{\pi \{ 1 + f(\theta) \}} \{ T_1 + T_2 + T_3 + \&c. \text{ to } n \text{ terms} \}, \\ \therefore \sqrt{k^2 + h^2} &= \frac{\sqrt{gs}}{n\pi \{ 1 + f(\theta) \}} \{ T_1 + T_2 + T_3 + \&c. \text{ to } n \text{ terms} \}. \quad (30.) \end{aligned}$$

* See Poisson's *Traité de Mécanique*, p. 348.

For very small angles of amplitude $f(\theta)$ is very small, and may be entirely neglected, showing that for small angles the oscillations are independent of the angles.

The value of $\sqrt{k^2+h^2}$ was in the first place calculated in respect to *one* of the quantities T_1, T_2 , &c., then in respect to *two*, and so on, to *four*. The mean of these four values being then taken in respect to each experiment, a final mean was taken in regard to all the experiments. The data and the results are stated in the following Table as it regards the value of $\sqrt{k^2+h^2}$, and the rest in the first Table, p. 629. The times of oscillation, as calculated by formula (22.) and as determined by experiment, are stated in the second Table on that page.

	Angle of oscillation θ .	$1+f\theta$.	Value of $\sqrt{k^2+h^2}$ calculated in inches from time of				Mean value of $\sqrt{k^2+h^2}$ in each experiment.	Mean value of $\sqrt{k^2+h^2}$ in all the experiments.
			One oscillation.	Two oscillations.	Three oscillations.	Four oscillations.		
W=197.18 lbs.	5	1.0004762	11.336	11.780	12.356	12.461	11.983	} 12.033
	7 30	1.0010723	12.318	12.200	12.828	12.903	12.562	
h=4.9 inches	10	1.0019072	12.003	11.620	11.620	11.870	11.550	
W=255.43 lbs.	2 30	1.0000235	11.128	10.731	11.194	11.227	11.07	} 11.076
	5	1.0004762	9.429	10.328	10.924	11.123	10.451	
	7 30	1.0010723	11.910	11.315	11.976	11.935	11.784	
h=5.97 inches	10	1.0019072	10.332	10.818	11.446	11.414	11.000	

XXXI. *Electro-Physiological Researches. On Induced Contraction.—Ninth Series.**By Signor CARLO MATTEUCCI, Professor in the University of Pisa, &c. &c.**Communicated by W. R. GROVE, Esq., F.R.S.*

Received May 13,—Read June 20, 1850.

IT is not without considerable regret that I am compelled to depart from my established rule of never publishing any facts relating to electro-physiology without having previously endeavoured to connect them with those already discovered, and without having succeeded in reproducing them in such a constant and unvarying manner as to remove the slightest doubt of their truth. The announcement, however, just made to the Académie des Sciences by M. DU BOIS RAYMOND of a work "*On the Law of Muscular Current, and on the Modification which that law undergoes by the effect of Contraction*," obliges me, though unwillingly, to transmit to the Royal Society the continuation of my researches on induced contraction, confining myself, for the present, to some fundamental experiments, made a long time since, for the publication of which I should have wished to await a more favourable moment.

In the Third and Fifth Series of my Electro-physiological Researches*, I studied, with all possible care, and in its minute details, the fact of induced contraction, in order to deduce the law of this phenomenon, and from thence to be led to the discovery of its cause. In the Fifth Series, principally, I was led to conclude, that, according to all the analogies, and without being in opposition to the experiments, induced contraction might be considered to be the effect of an electric discharge which takes place during the act of muscular contraction.

In a memoir published in the *Annales de Physique et de Chimie*, Octobre 1847, after having given an exposition of the laws of the electrical discharge of fish, and demonstrated all the analogies existing between this function and muscular contraction, I declared still more explicitly, that experiment leads to the admission that in muscular contraction there is a phenomenon analogous to that of the discharge of the Leyden phial, and which was the cause of induced contraction. Nevertheless it has been my wish that this conclusion, which I have always studiously announced with extreme reserve, should be demonstrated by direct experiment, and this object I have vainly endeavoured to attain in making use of the galvanometer.

In the memoirs above cited, are to be found a description of all the efforts made to this purpose; by forming piles with muscular elements of the frog and making these elements contract, I endeavoured, but always without satisfactory result, to obtain

* Philosophical Transactions, Part II. 1845, 1847.

from the galvanometer signs of electrical phenomena produced by the act of contraction. The variations produced in the conductivity of the circuit, by the convulsive movements of the muscles of the frogs, which alter the points of contact between the elements of the pile, the agitation of the liquid into which the platina plates of the galvanometer are plunged, are constant and inevitable causes of the production of an electric current, which involves a constant uncertainty as to the effects which may depend on contraction. These same causes of error exist in the experiment made by M. DU BOIS REYMOND on voluntary contraction of a man's arm; I have tried a great number of experiments, following his method, increasing the number of elements which contract at the same time, without ever succeeding in obtaining an evident and constant development of electricity by muscular contraction. In the researches for the discovery of this development, I have not failed to employ, with all proper care, the galvanoscopic frog. Operating on a circle of thirty and forty individuals, who all contracted the same arm at the same time, and although the galvanoscopic frog, the nerve of which formed part of the circuit (being in contact with the two outermost fingers of this pile of men), was extremely sensitive, yet never did we obtain the slightest sign of a current from voluntary contraction. Yet the circuit, though composed of a great number of individuals, was always a sufficiently good conductor for the transmission of a current of some muscular elements, capable of producing contraction in the galvanoscopic frog.

Since, therefore, the galvanometer employed in my first experiments on induced contraction, and by means of which M. DU BOIS REYMOND thinks that he has discovered the development of electricity in voluntary muscular contraction, never has with me led to any other but uncertain and very doubtful results, since by the aid of the galvanoscopic frog, used as a substitute for the galvanometer, I have *never obtained the slightest signs* of an electric current in the voluntary contraction of the muscles of the arms, I was authorized in concluding that the development of electricity by muscular contraction still remained to be demonstrated by experiment, and that the phenomenon of induced contraction was still that which led most directly to this result.

I will now give some new experiments on induced contraction, of incontestable evidence and certainty, and which undoubtedly lend an increasing probability to the conclusion that induced contraction is due to an electric discharge, and that muscular contraction is accompanied by a development of electricity.

Exp. 1. I prepare a frog in the usual manner, and I lay on the muscles of its thighs, legs, and on the articulations of its claws, the nerves of highly sensitive galvanoscopic frogs: on provoking the contraction of the muscles of the first frog by irritating its lumbar nerves, the galvanoscopic frogs contract, and when these first contractions are very violent, induced contraction may be seen to take place on contact even with the extremities of the limb of the entire* frog. The signs of in-

* I apply the word entire to the frog fastened on the glass tubes: see figs. 1 and 3.

Fig. 1.

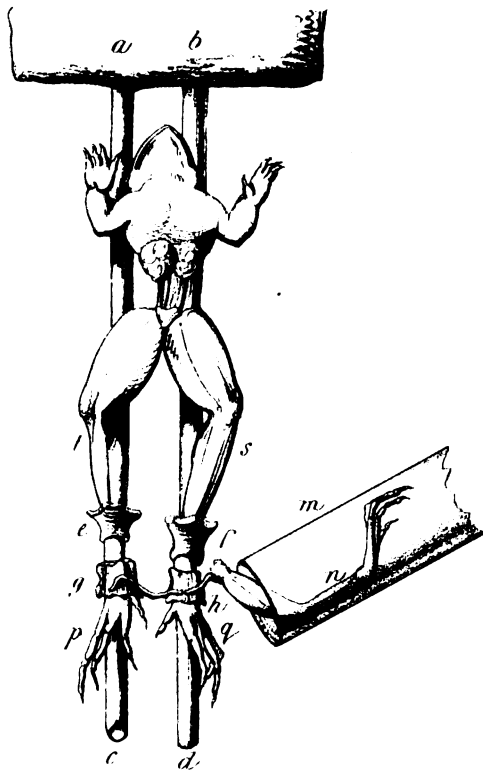


Fig. 3.

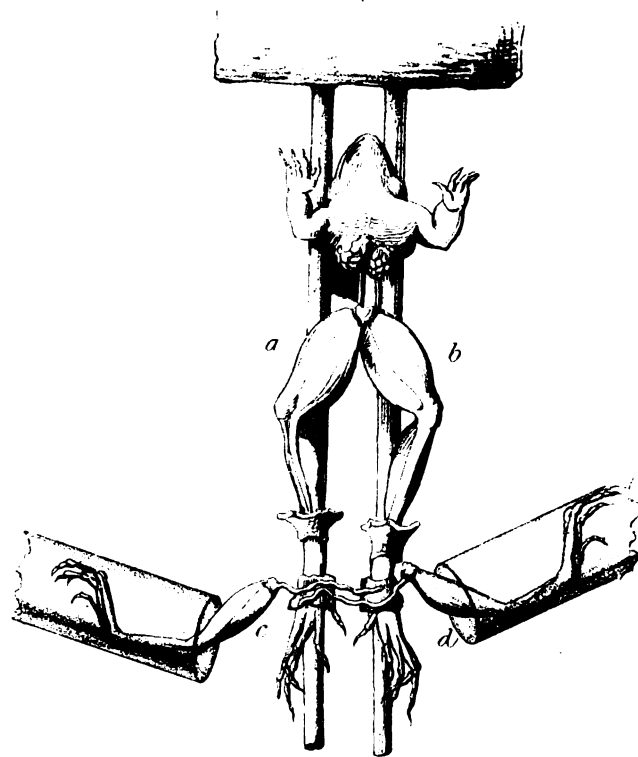


Fig. 2.

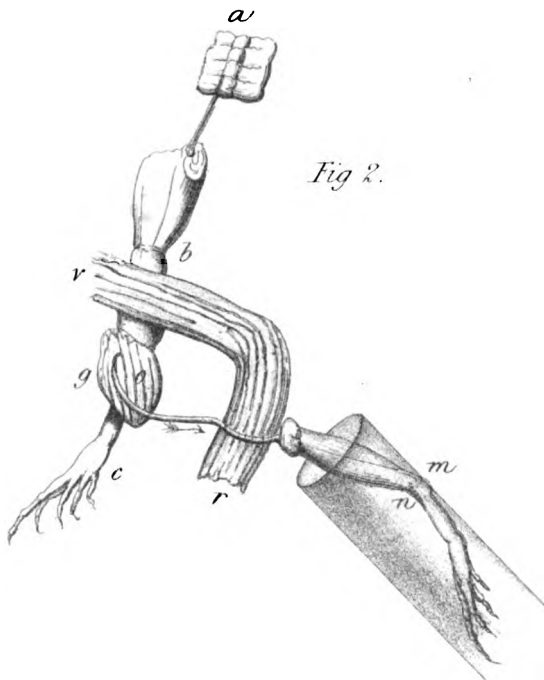
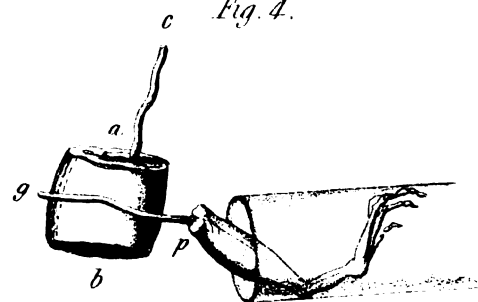


Fig. 4.



duced contraction are always obtained stronger and more durably on the muscles of the thigh and leg, than on the other parts of the frog.

Exp. 2. Induced contraction is very evident at the moment when the muscle itself, on which is laid the nerve of the galvanoscopic frog, is divided. This experiment is perfectly analogous to that which is made by cutting, in different directions, a very small piece of the organ of the torpedo, upon which a nerve of the galvanoscopic frog has been placed.

Exp. 3. Induced contraction is produced in the same manner, and with equal intensity, whatever may be the direction in which the nerve of the galvanoscopic frog is extended, in respect to the muscular fibres.

Exp. 4. If we interpose between the nerve of the galvanoscopic frog and the surface of the thigh of the entire frog several layers of wet paper, we cease to obtain induced contraction: if there is only a layer or two of very thin paper, induced contraction continues to be produced.

Exp. 5. We dispose a prepared frog, as in Plate L. fig. 1; *ac* and *bd* are two glass tubes covered with varnish, upon which the frog is fixed by means of Indian rubber rings, *e* and *f*; *g* and *h* are two bits of linen, or of thick paper moistened with salt water. If we touch separately the point *g* or the point *h* with the nerve of the galvanoscopic frog, there is no sign of induced contraction; but if the nerve of the galvanoscopic frog be disposed as in fig. 1, so that the nerve touches at the same time the points *g* and *h*, we have immediate contraction in the galvanoscopic frog, exciting contractions in the muscles of the entire frog. This takes place in whatever manner the contractions are provoked, whether by wounding or cutting the spinal marrow, or in passing an electric current from a small pair of zinc and platina along the lumbar nerves.

Exp. 6. If, while making the preceding experiment, a communication be established between the two legs of the frog, by placing a wetted cotton wick over the points *t s* or *p* and *q*, or over all the intervening points, the contractions of the galvanoscopic frog cease instantly, although the members of the entire frog are in contraction: we have only to remove the cotton wick and the phenomenon reappears. These alternatives are constant, and are always reproduced in the same manner.

Exp. 7. The results of the preceding experiments are obtained whether the legs of the entire frog are brought into contact or whether they are kept separate. We see from this the advantage of the arrangement adopted in these experiments in order to avoid placing the frog on a plane, which would soon become moistened and serve as a conductor.

Exp. 8. By means of a solution of the extract of *nux vomica*, I produce a state of nervous superexcitation in the frogs. While in this condition these animals are subject at intervals to violent contractions, which may also be excited by the slightest contact. I place a frog in this condition on the apparatus, fig. 1. The galvanoscopic frog rarely gives signs of contraction, while the entire frog contracts violently. One

of the lumbar nerves ought now to be cut; this being done, we are certain to obtain contractions in the galvanoscopic frog, at each contraction of the entire frog; these contractions cease if the two legs are brought into communication with the wetted cotton, as in the preceding experiment.

Exp. 9. I take a half frog, and instead of the other half, I employ a wick of cotton, *v, r* (fig. 2), one end of which touches the upper part of the leg or thigh, and the other the nerve of the galvanoscopic frog, *g p n*. The other extremity of this nerve rests on the lower part of the leg of the half frog, a thick stratum of wetted paper (*o*) being placed between the nerve and the leg. Every time that the half frog contracts the galvanoscopic frog contracts also; the latter ceases to contract if one removes the upper end (*v*) of the cotton wick from its contact with the upper part of the leg. It ought to be observed, that in this arrangement of the experiment we obtain the contraction of the galvanoscopic frog at the instant that we place the nerve in this sort of circuit.

This experiment has been tried with equal success operating on the limb of a living rabbit.

Exp. 10. Instead of one galvanoscopic frog I dispose two, as in fig. 3, and by irritating first one lumbar nerve and then the other, I produce contractions in each limb separately. During the first moments the two galvanoscopic frogs, *c* and *d*, contract, whichever may be the limb of the entire frog which contracts. Afterwards, when the galvanoscopic frogs begin to be less sensitive, we are sure to obtain contractions in the galvanoscopic frog, *d*, only when the limb *a* contracts, and the contraction in the galvanoscopic frog *c*, when it is the limb *b* which contracts. We obtain the same result by employing the arrangement adopted in fig. 2, representing the non-contracting limb by the wet cotton wick, disposed in the manner already described. Thus, if instead of having the nerve, *g p*, disposed as in fig. 2, we reverse the position of the nerve of the galvanoscopic frog in such a manner as to touch the cotton wick with its upper part, and the point, *o*, superposed on the limb with its lower part, either no contraction at all is obtained, or it soon ceases.

Exp. 11. I cut the thigh of a frog, above and below, so as to form a piece, *a b* (fig. 4), similar to a portion of a cylinder the two circular bases of which are the interior surfaces of muscle. The nerve of the galvanoscopic frog, *g p*, is disposed on the surface of this cylinder so as to be exactly parallel with the two bases. If we irritate the point *c* there are signs of induced contraction.

It is impossible for me to confine myself to the simple description of these experiments without deducing from them some conclusions, the most evident of which appear to me to be the following:—

1. The cause of induced contraction, according to all analogies, is the same as that which produces contraction of the galvanoscopic frog in the *sixth* and *following* experiments.

2. The cause of these contractions is evidently an electrical phenomenon developed

in the act of contraction, and which consists in a different state of electricity in the different points of the contracted limb.

3. This electrical phenomenon, like the contraction which produces it, lasts only for an instant.

4. These electric states, developed by contraction, tend to produce electrical currents which circulate in opposite directions across a conducting arch interposed between the two limbs, which contract at the same time.

5. The tenth experiment proves the existence of these currents and their directions; here we should remember that the galvanoscopic frog is more sensitive to the passage of the current when that current traverses the nerve in the direction of its ramification. Thus the contraction of the galvanoscopic frog, *d* (fig. 3), during the contraction of the limb, *a*, proves the existence of the electrical current, which goes from *a* towards *d*. The contraction of the galvanoscopic frog, *c*, during the contraction of the limb, *b*, proves, in the same manner, the existence of an electrical current going from *b* towards *c*.

Whatever the theory of these phenomena may be, it is certain that they demonstrate the production of an electrical *disequilibrium* in the act of muscular contraction.

In the experiments which have been described, this electro-physiological phenomenon may consist in a species of discharge, propagated in muscles in the direction of the ramification of the nerves. Is the cause of this discharge a phenomenon analogous to that of electrical fish, or does it consist in a change in the natural conditions of the muscular current, produced by contraction? In the present state of science it is impossible for us to answer this question. According to all the analogies, and according to our present experimental knowledge, we lean to the former of these two explanations; but we wait the light of new experiments to proceed safely further in this difficult field of science.

Pisa, April 1, 1850.

XXXII. *Contributions to the Chemistry of the Urine.*—Paper IV.*On so-called Chylous Urine.**By HENRY BENICE JONES, M.D., M.A. Cantab., F.R.S., Physician to St. George's Hospital.*

Received January 21,—Read March 14, 1850.

URINE white from the suspension of a quantity of fatty matter in it, has been called chylous urine, and the albumen and fibrin which escape from the blood with the fat have been considered to belong to the chyle and not to the blood.

An opportunity of observing a case of this disease having occurred to me, I was led to make the following experiments. The conclusions therefrom are,—

1st. That the fat on which the white colour of the urine depends does indeed appear in the urine after the chyle is absorbed, but the albumen, fibrin, blood-globules and alkaline salts, may be found in the urine previous to any food being taken, and these substances can be made to appear in, or disappear from the urine according as the circulation is hurried by motion or quieted by rest.

2nd. That the disease consists in some slight alteration in the structure of the kidney, by which, when the circulation is most active, one or more of the constituents of the blood passing through the filter escape from the vessels into the urine.

The supposition that the disease consists in an accumulation of fat in the blood which is thrown out by the kidneys carrying with it albumen, fibrin, blood-globules and salts, is altogether disproved, both by actual analysis of the blood itself, and by the frequent occurrence of a large jelly-like coagulum in the urine, when no white fatty matter can be seen to be present.

For an account of the patient and for the treatment and its ultimate effect in stopping the escape of the white water, which he had passed more or less frequently for nine months previous to my seeing him, I must refer to the *Medico-Chirurgical Transactions* for 1850.

On the 19th of October he came to me, having taken food last at 11 A.M. The water passed at 2 P.M. solidified like blanc mange in ten minutes. It contained, when passed from the bladder, some clots ready-formed. It was very feebly acid to test-paper. Thrown on a filter, a very small quantity of reddish streaked fibrin remained on it. The filtered fluid, at 60° FAHR., had a specific gravity = 1015. It coagulated by heat and acid. In appearance it was quite milky, but became clear when agitated with a considerable excess of ether. Some perfectly healthy blood-globules were seen with the microscope, and the granules of fat were so small as scarcely to be resolvable by a high power.

574·0 grs. of urine evaporated *in vacuo* over sulphuric acid.

Residue = 25·5 grs.
= 44·42 grs. per 1000 grs. of urine.

Burnt. The ash = 4·60 grs. = 8·01 grs. per 1000 grs. of urine.

1015·4 grs. of urine precipitated with more than twice their bulk of alcohol.

Albumen and fat = 22·75 grs. = 22·40 grs. per 1000 grs. of urine.

Treated with ether. Residue soluble in ether.
= 8·5 grs. fat = 8·37 grs. per 1000 grs. of urine.

Albumen = 14·03 grs. per 1000 grs. of urine.

574 grs. treated with absolute alcohol after evaporation to dryness.

Alcohol filtered when cold.

Residue . . . = 9·02 grs. = 15·70 grs. per 1000 grs. of urine.

Burnt ash . . = 1·40 gr. = 2·44 grs. per 1000 grs. of urine.

Urea and extractive . . . = 13·26 grs. per 1000 grs. of urine.

Hence

Total residue . = 44·42 grs. per 1000 grs. urine. Specific gravity = 1015.

Total ash . . = 8·01 grs.

Albumen . . = 14·03 grs.

Fat . . . = 8·37 grs.

Urea, &c. . . = 13·26 grs.

Loss . . . = 75 gr.

Water . . . = 955·58 grs.

1000·00 grs.

In order to watch the variations in the appearance of the urine at different hours of the day, it was each time passed into a bottle, marked with the hour at which it was made for five successive days.

First day. Water passed at

7 A.M. thrown away. Breakfast a pint of coffee; a quarter of a pint of milk.
Bread, with little butter or sugar.

8^h 10^m A.M., just before breakfast. Quantity one ounce, clear. Acid with pink deposit of urate of ammonia gave no precipitate with heat and nitric acid. Remained acid three days.

10 A.M. Reddish colour, milky throughout. Six ounces alkaline, in three days.

12^h 20^m. Milky, thick throughout. Six ounces, alkaline; contained some clots of fibrin.

1^h 25^m P.M. More thick. White with rather reddish clots, two ounces, alkaline, made just before dinner, which consisted of mutton chop, boiled rice, onion with bread.

2^h 20^m P.M. White, with more blood, six ounces, alkaline.

6 P.M. More white, with deposit of blood, eight ounces, alkaline; smelt strongly

of onions ; passed just before tea ; three quarters of a pint of cocoa with milk and bread.

10 P.M. Very white, milky, with some blood. Twelve ounces, alkaline.

Second day. Water passed at

7 A.M. Lost.

8^h 30^m A.M. Nearly clear ; a little blood and very slight coagulum. An ounce and a half, acid ; passed just before breakfast. Pint of boiled milk with sago, bread, and a little butter.

9^h 30^m A.M. Very slightly opalescent, with a delicate transparent coagulum filling the lower half of the bottle, containing some streaks of blood. The clear liquid coagulated strongly with heat and acid. An ounce and a half in quantity, alkaline.

11^h 30^m A.M. Milky, with a little blood, alkaline.

1^h 15^m P.M. Milky, with more blood ; six ounces more alkaline ; made just before dinner, which consisted of a mutton chop, broccoli and bread.

3 P.M. Milky ; about eight ounces, alkaline.

5 P.M. Milky ; eight ounces, feebly acid ; passed just before tea ; three quarters of a pint of milk and sago, bread, very little butter.

8 P.M. Milky ; eight ounces, feebly acid.

10^h 30^m P.M. Milky ; eight ounces, slightly more acid.

Third day. Water passed at

6 A.M. Opalescent yellow, did not clear with heat ; gave the slightest trace of albumen with heat and acid ; about eight ounces, slightly acid.

7^h 30^m A.M. Clear when passed ; cloudy about twelve from urate of ammonia ; about one ounce passed immediately after getting out of bed ; gave a more distinct trace of albumen, contained a few blood-globules ; slightly acid.

8^h 30^m A.M. The whole of the urine in the bottle consisted of coagulum of a yellow opalescent colour, not at all milky ; a few drops of liquid only could be poured from it, and this was excessively albuminous ; the quantity made was nearly two ounces, alkaline to test-paper, passed just *before* breakfast, which consisted of a pint of boiled milk and sago, bread, and a little butter.

9^h 30^m A.M. More opalescent ; slightly milky, with a tinge of blood ; the bottle was full of coagulum from which a drachm or two of liquid could be poured ; about five ounces ; alkaline highly.

12^h 30^m A.M. Milky ; eight ounces, neutral.

1^h 15^m P.M. Milky ; two ounces, neutral ; passed just before dinner ; steak, roast onion and bread.

4^h 45^m P.M. Milky ; five ounces, neutral.

5^h 30^m P.M. Milky ; five ounces, alkaline just before tea ; three quarters of a pint of coffee, bread, with little butter.

9^h 30^m P.M. Milky ; eight ounces, neutral.

10^h 30^m P.M. Milky ; four ounces, feebly acid.

Fourth day. Water passed at

6 A.M. Clear, six ounces, strongly acid; the slightest trace of albumen with heat and acid.

7^h 15^m A.M. Clear; half an ounce, very strongly acid. Passed immediately on getting out of bed for the day; the slightest trace of albumen with heat and acid. This and a little of the urine passed at six, had specific gravity 1027, 513.5 grs. precipitate with alcohol = 0.40 gr. = 0.8 gr. per 1000 grs. of urine.

9^h 50^m A.M. Slightly opalescent, solid, yellow, coagulum; three ounces, alkaline. Passed just *before* breakfast. Pint of boiled milk with sago and bread, with but little butter or sugar. When filtered a bloody coagulum remained on the filter. The clear liquid left at rest deposited a layer of blood-globules. Specific gravity = 1015.6, gave a nearly solid coagulum with heat and acid. 507.8 grs. precipitated by alcohol = 7.20 grs. = 14.1 grs. per 1000 grs. of urine.

11 A.M. Chylous with clots and pink coagulum; six ounces, alkaline, filtered; bloody coagulum was left on the filter. Milky fluid on standing deposited a layer of blood-globules. Specific gravity = 1013.4, 506.7 grs. precipitated by alcohol = 7.35 grs. = 14.5 grs. per 1000 grs. of urine.

1^h 35^m P.M. Very milky; eight ounces, with little blood; neutral. Passed just before dinner; mutton chop with broccoli and bread.

3^h 40^m P.M. Very milky; eight ounces, very little blood; neutral.

5^h 40^m P.M. Very milky; one and a half ounce; blood doubtful; feebly acid; passed just before tea; three quarters of a pint of coffee, half a pint of milk with bread, but little butter or sugar.

7^h 40^m P.M. Very milky; four and a half ounces; little blood; neutral.

10 P.M. Very milky; five ounces; little blood; very feebly acid.

Fifth day. Water passed at

6 A.M. Slightly milky; yellowish; eight ounces; strongly acid. Specific gravity = 1017. No blood; contained a very minute trace of albumen; did not clear with heat.

8^h 15^m A.M. Slightly cloudy; no blood; no coagulum; one and a half ounce, acid. Specific gravity = 1020.7. Contained a considerable quantity of albumen, though he had nothing to eat and remained in bed.

9^h 30^m A.M. Almost quite clear; three quarters of an ounce, acid. Passed just before breakfast, which consisted of a pint of boiled milk, sago, bread, with a little butter. Apparently rather less albumen. Specific gravity = 1022.

11^h 20^m A.M. Yellowish milky. Three ounces made just before he got up for the day; very feebly acid. Contained a considerable quantity of albumen; more than the two preceding specimens. No spontaneous coagulation. Did not clear by heat. Specific gravity = 1019.4.

1 P.M. Milky; coagulated spontaneously; four ounces, neutral; passed just before dinner; mutton chop, onion with bread.

4^h 50^m P.M. Milky; neutral; six ounces just before tea; three quarters of a pint of milk boiled, bread with butter.

8 P.M. Milky; five ounces; neutral.

10 P.M. Milky; three ounces; feebly acid.

Sixth day. Water passed at

6^h 35^m A.M. Yellow milky; more than opalescent; six ounces, strongly acid.

Specific gravity 1026·6. Passed on getting up. 513·3 grs. precipitated by alcohol = 1·85 gr. = 3·6 grs. per 1000 grs. of urine.

8^h 10^m A.M. More milky, with large pinkish coagula; two ounces, alkaline. Specific gravity 1016·4. 508·2 grs. precipitated by alcohol = 8·40 grs. = 16·5 grs. per 1000 grs. of urine.

9 A.M. Pink coagulum filling an ounce bottle, alkaline; passed just before breakfast; a pint of boiled milk, sago, with a little butter.

11^h 30^m A.M. Very milky, with large coagula.

From these observations, and more particularly from the 3rd, 4th and 6th days, it is evident that the fibrin and albumen appear in the urine when no fat is perceptible, and previous to breakfast being taken, thus:—

The 3rd day, 8^h 30^m A.M., the urine passed before breakfast, after he had been up an hour, was highly albuminous, and a nearly solid coagulum filled the bottle.

The 4th day, 7^h 15^m A.M., on first getting up the urine contained the slightest trace of albumen. The specific gravity = 1027. The precipitate by alcohol = 0·8 gr. per 1000 grs. of urine.

The 4th day, 9^h 50^m A.M., just before breakfast the urine formed a solid coagulum, and contained a visible deposit of blood. Specific gravity = 1015·6. The precipitate by alcohol = 14·1 grs. per 1000 grs. of urine.

The 4th day, 11 A.M., the urine was chylous.

The 6th day, 6^h 35^m A.M., on first getting up, the specific gravity = 1026·6. The precipitate by alcohol = 3·6 grs. per 1000 grs. of urine.

The 6th day, 8^h 10^m A.M., before breakfast the urine coagulated spontaneously. Specific gravity = 1016·4. The precipitate by alcohol = 16·5 per 1000 grs. of urine.

Further experiments on the influence of rest and motion in lessening or increasing the albumen in the urine, previous to breakfast, were then made.

On the Influence of Rest and Motion in Lessening or Increasing the Albumen in the Urine previous to and after Food was taken.

Seventh day. Last food was taken at 5^h 15^m P.M. yesterday. He laid in bed this morning till 9^h 30^m A.M.

Urine passed at

10 P.M. Last night, milky; eight ounces.

6^h 40^m A.M. Yellow, slightly milky; six ounces, acid; contained a little albumen.

8^h 10^m A.M. Clear, healthy looking urine, made just before breakfast (boiled milk, a pint of sago, bread, with a little butter). The quantity, one and a half ounce.

Specific gravity = 1021·3. Gave no coagulum with heat and acid. 510·65 grs. precipitated by alcohol. Precipitate = 0·30 gr. = 0·6 gr. per 1000 grs. of urine.

9^h 30^m A.M. Opalescent, no spontaneous coagulation. Two and a half ounces passed when he got up. Specific gravity = 1019·0. Gave a considerable precipitate with heat and acid. 509·5 grs. precipitated by alcohol. Precipitate = 1·40 gr. = 2·7 grs. per 1000 grs. of urine.

12^h 30^m A.M. Milky; spontaneously coagulating.

Eighth, being the next day. Last food between 5 and 6 P.M. yesterday. Up this morning at 6 A.M. Urine passed at 10 P.M., last night, was chylous.

2 A.M. Yellow milky; three ounces, acid. Slightly coagulating with heat and acid.

6^h A.M. Quite clear; healthy-looking. Two ounces; acid. Contained no trace of albumen. Specific gravity = 1026·4. 513·2 grs. precipitated by alcohol. Precipitate = 0·85 gr. = 1·65 gr. per 1000 grs. of urine.

7^h 30^m A.M. Opalescent; one and a half ounce; feebly acid; gave a large precipitate with nitric acid and heat; contained multitudes of healthy blood-globules; no casts. On long standing gave a small bloody coagulum, which fell to the bottom of the bottle. Specific gravity = 1018·8. 509·4 grs. precipitated by alcohol. Precipitate = 0·65 gr. = 1·9 grs. per 1000 grs. of urine.

9 A.M. Opalescent; by 12 o'clock became a solid jelly; two ounces, alkaline. Passed just before breakfast, which consisted of a pint of milk and sago, with bread and little butter.

11^h 30^m A.M. Coagulated spontaneously to a strong, unclear, slightly milky jelly. About three ounces, alkaline, passed in my room. Says that he thinks he can tell when the urine will be most thick and bloody by the pain, pressure and dragging, with heat in the loins.

Ninth, being the next day. Last food at 6 P.M. the day previous; staid in bed today until 9^h 30^m A.M.

Urine passed at

3 A.M. Cloudy, opalescent; five ounces.

6^h 30^m A.M. Clear healthy water; an ounce and a half, highly acid. No precipitate with heat and acid.

8^h 40^m A.M. Clear, one ounce; contained the smallest trace of albumen. Specific gravity = 1024·2. He did not sleep from 6^h 30^m, but he remained in bed. This water was passed just before breakfast, which consisted of a pint of boiled milk and sago, bread, with a little butter. 512·1 grs. precipitated by alcohol. Precipitate = 0·85 gr. = 1·61 gr. per 1000 grs. of urine.

9^h 30^m A.M. Still clear, an ounce and a half in quantity, made on first getting up. Gave the most minute trace of albumen. Specific gravity = 1022·2. Very slightly acid. 511·1 grs. precipitated by alcohol. Precipitate = 0·60 gr. = 1·1 gr. per 1000 grs. of urine.

10^h 30^m A.M. Very milky, alkaline.

Tenth day. After animal food had formed the greatest part of his diet for ten days; he remained this day in bed until after breakfast.

Urine passed at

8^h 20^m A.M. Clear; no trace of albumen; acid. Specific gravity = 1023·4 grs. passed before breakfast. 511·7 grs. precipitated by alcohol. Precipitate = 0·40 gr. = 0·78 gr. per 1000 grs. of urine.

Eleventh day. The following morning; up early.

Urine passed at

8^h 10^m A.M. Cloudy; contained some blood-globules, but no trace of fibrinous casts; acid. Specific gravity = 1022·6; passed before breakfast. Gave a considerable precipitate with heat and acid. 511·3 grs. precipitated by alcohol. Precipitate = 9·40 grs. = 18·38 grs. per 1000 grs. of urine.

The following table shows these results clearly:—

Seventh day. Remained in bed late.

	Specific gravity.	Precipitate by alcohol.
8 ^h 10 ^m A.M. In bed. Urine passed before breakfast, clear	= 1021·3	= 0·6 gr. per 1000 grs. of urine.
9 ^h 30 ^m A.M. In bed. Urine passed after breakfast, opalescent	= 1019·0	= 2·7

Eighth day. Up early.

6 A.M. On getting up. Urine passed before breakfast, clear	= 1026·4	= 1·6
7 ^h 30 ^m A.M. Up. Urine passed before breakfast, opalescent	= 1018·8	= 11·9

Ninth day. Remained in bed late.

8 ^h 40 ^m A.M. In bed. Urine passed before breakfast, clear	= 1024·2	= 1·6
9 ^h 30 ^m A.M. On getting up. Urine passed after breakfast, clear	= 1022·2	= 1·1

Tenth day. Remained in bed.

8 ^h 20 ^m A.M. Urine passed before breakfast, clear	= 1023·4	= 0·78
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Eleventh day. Up early.

8 ^h 10 ^m A.M. Urine passed before breakfast, cloudy	= 1022·6	= 18·38
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Healthy urine free from albumen, precipitated by } alcohol in excess.....	= 1020·2	= 2·15
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Second experiment.....	= 1021·2	= 3·48
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The precipitate chiefly consisted of sulphates and a little phosphate of lime, and no uric acid.

These experiments show most clearly the influence of rest and the effect of rising, (and even of waking) in causing the albumen to appear in the urine. The comparison of the 8th day with the 7th or 9th, and the great difference between the 11th and 10th days is quite conclusive. In other experiments I frequently determined before-hand whether the urine before breakfast should be albuminous or not, by directing the patient to get up and move about early or to keep very quiet in bed. And by keeping in bed all the day the urine throughout the whole day was very slightly albuminous.

It appeared to me to be very desirable to see whether the blood was milky. Mr. WILSON, of Tavistock Street, through whose kindness the patient came to me, bled him for me. The veins were filled with blood rather longer than usual before the opening was made; the blood came in a tolerable stream. The dinner, at 1^h 30^m P.M., consisted of a mutton chop with vegetables and bread. The blood was taken at 4^h 30^m P.M. into a thin wide mouth bottle. It was left to stand fourteen hours. There was no appearance of buff or cupping. The serum was opalescent, but not at all milky; and when agitated with ether it did not become clear, and the quantity of fat dissolved in the ether was very small.

Blood . . . = 1216·2 grs.

(1.) Serum poured off = 288·5 grs. Residue on evaporation = 26·9 grs.

Hence water . . = 261·6 grs.

(2.) Clot washed with distilled water until colourless.

Fibrin dried, *in vacuo* . . . = 3·20 grs.

= 2·63 grs. per 1000 grs. of blood.

(3.) Blood-globules, *plus* residue of serum, } = 924·3 grs.
 minus fibrin. }

Evaporated to dryness = 261·8 grs.

Hence water = 662·5 grs.

contain by (1.) 68·1 grs. of solids of serum.

Therefore blood-globules = 261·8 — 68·1 = 193·7 grs. = 159·3 grs. per 1000 grs. of blood.

Total solids of serum = 68·1 + 26·9 = 95·0 grs. = 78·1 grs. per 1000 grs. of blood.

(4.) Dried blood-globules, and serum, in the proportion of one-quarter serum to three-quarters globules, were treated with ether frequently: 99·7 grs. gave 0·25 gr., soluble in ether = 2·6 grs. of fat per 1000 grs. of dry residue.

Hence in 1000 parts of blood—

Fibrin	2·63
Blood-globules	159·3
Solids of serum	78·1
Total solid residue	240·03
Water	759·97
Fat	0·62

The urine made the same day on which he was bled was also examined.

That passed at

6^h 30^m A.M. Clear.

8 A.M. Slightly cloudy. Then got up and had breakfast on arrow-root.

10 A.M. A solid and pinkish-white coagulum formed in the urine spontaneously.

1^h 30^m P.M. Milky, with some blood and spontaneous coagula. Six ounces passed just before dinner, which consisted of a mutton chop, with vegetables and bread.

4 P.M. Milky; five ounces.

4^h 30^m P.M. Milky; one ounce, mixed afterwards with 4 o'clock water. It contained a very little blood. He was bled immediately afterwards. Four ounces of this urine were evaporated to dryness, treated with boiling water, filtered, tested with sulphate of copper and liquor potassæ; gave no evidence of sugar.

6 P.M. Very milky urine, with blood-globules and coagula. Specific gravity = 1018; neutral to test-paper; about two ounces, made before tea.

509.0 grs. evaporated, *in vacuo*, over sulphuric acid—

Residue = 28.95 grs.

= 56.87 grs. per 1000 grs. of urine.

Ash = 5.50 = 10.80 grs. per 1000 grs. of urine.

509.0 grs., precipitated by an excess of alcohol—

Albumen and fat = 10.90 grs. . . . = 22.41 grs. per 1000 grs. of urine.

Ether dissolved = 3.8 grs. . . . = 7.46 grs. per 1000 grs. of urine.

Therefore albumen = 13.95 grs. per 1000 grs. of urine.

509.0 grs., evaporated to dryness, treated with absolute alcohol, filtered when cold—

Residue = 12.85 grs. . . . = 25.24 grs. per 1000 grs. of urine.

Ash = 0.60 gr. = 1.18 gr. per 1000 grs. of urine.

Therefore urea and extractive = 24.06 grs. per 1000 grs. of urine.

Hence—

Total dry residue = 56.87 grs. per 1000 grs. of urine. Specific gravity = 1018.

Total ash = 10.80

Albumen = 13.95

Fat = 7.46

Urea, &c. . . . = 24.06

Loss = .60

Water = 943.13

1000.00 grs.

The conclusions from these experiments are:—

1st. That so-called chylous urine may contain fat, albumen, fibrin and healthy blood-globules.

2ndly. That although the fat passes off in the urine after food is taken, yet the albumen and fibrin and blood-globules are thrown out before any food has been taken. During perfect rest the albumen ceases to be excreted, and it does not appear in quantity in the urine even after food is taken, provided there is perfect rest. A short time after rising early the urine may coagulate spontaneously, although no fat is present in perceptible quantity, and this may happen previous to any food.

3rdly. Though the urine made just before and a short time after bleeding was as milky as it usually was at that period of the day, yet the serum of the blood was not milky. It did not contain a larger quantity of fat than healthy blood does. In other

respects, excepting in the diminution of the solids in the serum, the analysis of the blood corresponds with that of a case of cerebral congestion given by ANDRAL.

	Cerebral congestion.	So-called chylous urine.
Fibrin	2·7	2·63
Globules	152·3	159·3
Solids of serum	105·0	78·1
Water	740·0	759·97

The general results are—

1st. That the most important changes in the urine take place independently of the influence of digestion.

2ndly. That the urine in one respect only resembles chyle, and that is in containing, after digestion, a large quantity of fat in a very fine state of division; but the excess of fat in the urine is not caused by any excess of fat in the blood, for no excess of fat was found there.

3rdly. It appears that some change is produced in the kidney by which fibrin, albumen, globules and salts are allowed to pass out whenever the circulation through the kidney is increased: if, at the same time, fat is present in the blood, it escapes also into the urine.

That this change of structure is not visible to the naked eye on *post mortem* examination of the kidneys, Dr. PROUT long since demonstrated; and in a case of this disease, which was in St. George's Hospital, and was examined at Plymouth, no disease of the kidney was observed. From the total absence of fibrinous casts of the tubes from this urine, it is not improbable that by the microscope a difference may be detected in the structure of the mammary processes rather than in that of the cortical part of the kidney.

XXXIII. *Contributions to the Chemistry of the Urine.*—Paper III.PART IV. *On the Variations of the Sulphates and Phosphates in Disease.*

By HENRY BENICE JONES, M.D., M.A. Cantab., F.R.S., Physician to St. George's Hospital.

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THE object of the following experiments was to determine whether the sulphates in the urine were increased or diminished in any class of diseases. The total amount of phosphates in the urine in the same diseases was at the same time made the subject of experiment, partly to see whether the deductions made in a paper published in the Philosophical Transactions for 1846 would be confirmed, and partly to determine whether the same disease produced the same or a different effect on the phosphates as on the sulphates; whether, in diseases in which the phosphates were increased the sulphates would be also increased in the same proportion; and whether, in those diseases in which the phosphates were diminished, the sulphates also would be found to be below the average amount.

Most of the following experiments were made on the urine first passed in the morning before food. When this could not be obtained, the afternoon or night urine was taken. Almost all the cases were in St. George's Hospital, and therefore under nearly the same circumstances as regards exercise. The diet usually varied with the state of the patient.

In two papers in the Philosophical Transactions for 1845 and 1849, I have shown that in the healthy state on full diet the total amount of sulphates and phosphates in the urine varies as regards sulphates—

After food from 11·85 grs. of sulphate of baryta per 1000 grs. of urine .	Spec. grav. 1033·9
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Before food to 7·93 grs. of sulphate of baryta per 1000 grs. of urine .	1026·5
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As regards the total phosphates—

After food from 7·22 grs. of phosphate of lime per 1000 grs. of urine .	1030·0
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Before food to 7·96 grs. of phosphate of lime per 1000 grs. of urine .	1027·9
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In the following paper I shall give the amount of the sulphates and the total amount of the phosphates in the urine:—

1. In acute and chronic diseases in which the muscular structures are chiefly affected.

2. In some functional diseases of the brain, as delirium tremens and some other forms of delirium.

3. In acute inflammatory disease of the nervous structures.

4. In chronic diseases of the nervous structures.

5. In *acute* diseases in which neither the nervous nor the muscular structures are chiefly affected.

6. In *chronic* diseases in which neither the muscular nor nervous structures are chiefly affected.

TABLE I.—On the amount of Sulphates and the total amount of Earthy and Alkaline Phosphates in those diseases in which the muscular structures are chiefly affected.

	Sulphate of baryta per 1000 grs. of urine.	Specific gravity.	Total phosphates per 1000 grs. of urine.
Case 1. Acute chorea after salts.			
5th day	25.09	1032.3	0.68
5th day	21.73	1035.2	
8th day	7.28	1030.0	7.34
9th day	2.56	1013.1	4.70
Case 2. Acute chorea. Boy, æt. 8.			
6th day	11.25	1030.6	3.29
7th day	10.66	1031.8	2.52
8th day	11.15	1031.2	2.54
10th day	7.39	1028.4	3.50
11th day	3.92	1018.6	1.57
102nd day	8.01	1030.6	
Case 3. Acute chorea. Girl, æt. 22.			
3rd day	19.88	1036.0	
5th day	15.86	1033.8	
6th day	13.80	1028.4	6.51
7th day	9.36	1026.8	
8th day	6.08	1025.4	
13th day	4.72	1016.4	
Case 4. Chronic chorea	1.86	1008.2	
Case 5. Chronic chorea	3.49	1014.7	1.61
	1.91	1014.3	1.12
	2.66	1013.0	4.05
Case 6. Chronic tetanus	1.58	1009.9	2.47
	2.95	1015.5	2.75

Hence in three cases of acute chorea the most remarkable increase was observed in the amount of sulphates in the urine. That this did not arise from the small quantity of urine passed is evident from the small amount of the total phosphates in the urine. Moreover the actual quantity passed, when it could be determined by measure, was found occasionally to be above thirty ounces in twenty-four hours. It is worthy of notice that the amount of urea was also very greatly increased in these cases; so much so, that on the addition of nitric acid to the urine without any evaporation, nitrate of urea immediately crystallized out.

A very small quantity of food was taken by these patients, whilst the muscular action was most severe; and the contrast between the amount of sulphates when they were recovering and taking full diet, and the amount when they took little food, but were in a state of violent muscular action, is very remarkable.

The general conclusion is, that in acute chorea the amount of sulphates in the urine is increased, whilst the phosphates are in some cases as remarkably diminished.

TABLE II.—On the amount of Sulphates and the total amount of Earthy and Alkaline Phosphates in some functional diseases of the brain, as delirium tremens and some other forms of delirium.

	Sulphate of baryta per 1000 gra. of urine.	Specific gravity.	Total phosphates.
Case 1. Delirium tremens. 6th day	5.74	1017.9	
6th night. Death.			
Case 2. Delirium tremens. 5th day	13.34	1018.0	
Case 3. Delirium tremens. 3rd day	10.60	1023.94	5.29
Case 4. Delirium tremens. 10th day	17.31	1024.74	0.87
Case 5. Delirium tremens. Chloroform. 8th day	8.51	1026.8	
Case 6. Delirium tremens. 5th day	20.77	1037.8	2.14
5th night	37.07 pink.	1041.2	5.95
6th died.			
Case 7. Delirium tremens. Uncertain day	2.96	1013.2	1.18
Uncertain day	7.83	1021.6	7.24
Uncertain day	8.61	1022.0	7.43
Case 8. Delirium tremens. 13th day	13.10	1037.4	9.83
14th day	12.95	1034.6	8.89
Case 9. Traumatic delirium tremens. 3rd day		1014.8	1.97
5th day	8.45	1028.0	3.11
6th day	8.77	1026.2	6.23
Case 10. Poisoned by laudanum, with delirium and excitement. 2nd day	7.83	1026.8	7.53
3rd day	6.78	1023.0	1.11
Re-admitted again poisoned. 1st day.....	6.35	1029.2	8.88
2nd day	6.38	1024.0	8.24
3rd day	15.89	1028.0	4.22
4th day	13.01	1026.0	4.81
6th day	7.55	1025.0	4.42
7th day	8.22	1026.3	5.88
Case 11. Delirium, with phthisis. 4th day	10.84	1027.34	1.44
4th night.....	6.97	1024.2	0.72
7th day	4.45	1018.3	1.51

In the 2nd, 3rd, 4th, 6th and 8th cases of delirium tremens the sulphates were found to be above the average. In the 6th case the sulphates are in the greatest quantity ever observed in health or disease. Sulphate of magnesia had also been taken by this patient, so that the amount of sulphates is partly owing to the medicine; but that this is not the sole or chief cause of the increase is proved by the fact that many of the patients with other diseases took the same quantity of sulphate of magnesia, but the sulphates in the urine were never increased to the same amount.

The phosphates, in seven out of the nine cases of delirium tremens, were below the average. The same fact was stated in my previous paper, but from the diminution

of the phosphates in acute chorea, it becomes doubtful how far this result is owing to the action of alcohol in the system.

Further experiments are required; meanwhile excessive abstinence from food must be admitted as a cause of the diminution of the phosphates in the urine, provided there is no inflammatory action of the nervous structures.

In a case of restless excitement leading to self-destruction, the sulphates in the urine were found to be increased, and the same thing was also observed with a diminution of the phosphates in a case of delirium and phthisis.

In the 6th and 10th cases urea was found to be in great excess when the sulphates were increased. In the other cases the excess of urea was not looked for.

TABLE III.—On the amount of Sulphates and the total amount of Earthy and Alkaline Phosphates in acute inflammatory diseases of the nervous structures.

	Sulphate of baryta per 1000 gra. of urine.	Specific gravity.	Total phosphates.
Case 1. Inflammation of the brain. 20th day	7.67	1031.8	
Case 2. Inflammation of the brain. 12th day	3.96	1018.7	5.14
13th day	11.23	1027.26	11.13
14th day	2.91	1013.1	6.06
16th day	7.34 } 7.69 }	1027.0	{ 10.75 11.04
16th night. Died.			
Case 3. Subacute hydrocephalus. 15th day	8.83	1030.0	9.41
16th day	10.69	1029.0	8.45
22nd day	8.83	1029.8	10.19
26th day	9.46	1031.4	9.01
28th day. Died.			
Case 4. Acute after chronic disease. Uncertain	3.54	1024.9	0.91
	9.39	1026.0	9.31
Died.			
Case 5. Head symptoms, with tubercles in cerebellum and lungs. 15th day	9.88	1031.6	8.72
15th night	10.07	1032.2	8.91
16th day	6.69	1016.4	4.72
16th night	8.25	1018.2	5.69
18th day. Died.			
Case 6. Inflammation of the lungs, with tu- bercles and violent head symptoms. 4th day	8.55	1027.85	7.19
6th day	7.81	1026.1	6.43
8th day	11.63	1031.4	9.30
9th day	10.13	1026.2	7.99
9th night. Died.			
Case 7. Injury, with head symptoms not violent. 2nd day	7.34	1026.8	5.20
4th day	9.66	1028.6	7.52
5th day	13.74	1028.6	7.91
32nd day	2.11	1011.66	1.52
Case 8. Injury of the head, slight. 7th day	6.59	1022.9	8.93

TABLE III. (continued.)

	Sulphate of baryta per 1000 gra. of urine.	Specific gravity.	Total phosphates.
Case 9. Fractured spine.			
5th day	3.75	1014.2	3.15
6th day	12.23	1030.2	10.68
7th day	9.91	1028.8	10.10
7th night. Died.			
Case 10. Fractured spine.			
4th day	14.57	1029.2	9.26
5th day	15.54	1029.2	8.16
6th day	13.00	1030.2	7.96
7th day	6.65	1021.6	4.30
8th day	8.80	1022.0	6.67
9th day	2.18	1007.3	1.58
10th day	2.37	1010.8	2.57
Died.			
Case 11. Fractured spine.			
1st day	5.59	1012.0	4.15
1st night. Died.			
Case 12. Fractured spine.			
1st day	5.90	1019.4	2.47
2nd day	6.45	1025.3	4.31
2nd night	3.37	1012.4	1.40
3rd day	5.87	1023.0	1.09
3rd night	10.32	1027.0	3.11
4th day	6.65	1022.6	5.77
4th night	7.61	1024.1	4.90
5th day	13.03	1027.8	4.08
6th day. Died.			

In the first eight cases of inflammatory action in the brain, there is an increase in the amount of sulphates as well as in the total amount of phosphates in the urine. In the second and third cases this increase is most apparent. In the previous paper the phosphates in the urine were shown to be decidedly increased in inflammation of the brain, and from these experiments it is evident that the sulphates are increased also.

From the amount of albumen in the nervous structures, it is certain that the amount of sulphur present therein is but little if at all less than the amount of phosphorus which it contains.

TABLE IV.—On the amount of Sulphates and the total amount of Phosphates, Earthy and Alkaline, in some slight and chronic diseases of the nervous or neighbouring structures.

	Sulphate of baryta per 1000 gra. urine.	Specific gravity.	Total phosphates.
Case 1. Pain in the head. After salts.			
40th day	9.98	1026.2	
Case 2. Hemiplegia.			
7th day	5.13	1021.6	5.22
21st day	2.60	1017.0	1.51
Case 3. Scalp wound. After salts.			
6th day	8.34	1023.7	4.63
8th day	4.84	1018.7	3.77
Case 4. Fracture of the skull. After salts	11.97	1019.1	6.08
Case 5. Recent paraplegia	7.24	1021.6	4.50

In these diseases no increase of sulphates was observed, except after sulphate of magnesia had been taken as a medicine. The quantity taken was usually two drachms; yet the amount in the urine never reached to what it was when there was excessive action of the muscular or nervous structures.

TABLE V.—On the amount of Sulphates and the total amount of Earthy and Alkaline Phosphates in acute diseases, in which neither the nervous nor muscular structures are chiefly affected.

	Sulphate of baryta per 1000 gra. of urine.	Specific gravity.	Total phosphates.
Case 1. Petechial fever.			
5th day	5.65	1014.6	2.62
6th day	3.79	1012.9	2.01
9th day	1.30	1010.0	0.73
13th day	0.93	1011.5	2.61
18th day	5.25	1016.2	0.73
19th day	10.06	1027.4	4.80
20th day	1.12	1010.94	2.10
22nd day	2.58	1020.80	2.68
55th day. Recovered.			
Case 2. The same patient with ague and			
28th day	5.71	1021.6	5.13
33rd day	4.86	1015.6	1.41
afterwards with petechial fever.			
5th day	5.39	1027.0	4.32
8th day	6.56	1026.2	4.32
16th day	3.19	1014.2	3.09
21st day. Recovered.			
Case 3. Scarlet fever.			
3rd day	8.27	1032.2	6.33
10th day	2.01	1012.0	2.90
19th day	4.14	1022.1	5.42
Case 4. Inflammation of the lungs.			
7th day	6.21	1019.7	6.41
8th day	5.91	1021.5	6.98
9th day	6.59	1021.5	4.24
10th day	6.96	1025.4	6.96
11th day	8.13	1025.5	4.23
13th day	8.02	1027.1	2.86
16th day	4.16	1018.0	3.77
Case 5. Petechial fever.			
16th day	3.05	1013.7	4.08
Case 6. Petechial fever.			
10th day	3.35	1013.2	2.90
11th day	4.04	1013.7	3.94
14th day	3.73	1016.8	7.08
Case 7. Inflammation of the lungs and membranes of the brain.			
7th day	4.05	1012.5	2.86
Case 8. Acute rheumatism. After sulphur.			
9th day	11.75	1031.10	
Case 9. Acute rheumatism.			
11th day	6.96	1025.7	5.59
Case 10. Acute rheumatism. After salts.			
5th day	11.89	1026.0	9.84
7th day. After salts	10.78	1029.1	5.53
20th day	6.94	1021.8	5.97
Case 11. Acute rheumatism.			
8th day	7.17	1023.2	5.60
17th day	5.68	1027.5	6.26

In these acute diseases, except after sulphate of magnesia, the amount of sulphates was not found above the average. The amount both of phosphates and sulphates appeared not to be influenced by these diseases.

TABLE VI.—On the amount of Sulphates and the total amount of Earthy and Alkaline Phosphates in Chronic Diseases, in which neither the nervous nor muscular structures are chiefly affected.

	Sulphate of baryta per 1000 gra. of urine.	Specific gravity.	Total phosphates.
Case 1. Exostosis	1.75	1016.6	
Case 2. Exostosis	7.09	1023.4	6.03
Case 3. Exostosis	9.08	1026.4	11.58
Case 4. Diabetes	7.45	1025.2	3.94
Case 5. Diabetes	4.21	1030.2	0.91
Case 6. Diuresis. Albumen	0.49	1003.6	1.09
	0.89	1005.36	1.49
Case 7. Obstruction of the œsophagus. Albumen	1.38	1012.2	4.70
Case 8. Aneurism of aorta?	6.03	1017.8	2.39
Case 9. Obstruction of the bowels. Sulphate of magnesia. 8th day	22.55	1024.3	3.51
9th. Died.			
Case 10. Excessive oxalate of lime	7.89	1025.5	6.14
Case 11. Indigestion. Alkaline from fixed alkali	3.26	1024.7	3.65
Case 12. Chronic rheumatism. After sulphur	5.85	1011.82	4.86
Case 13. Chronic rheumatism	7.93	1023.44	
Case 14. Chronic gout. Vichy water.....	10.04	1025.8	5.26

In these chronic diseases nothing remarkable was observed as to the amount of sulphates or phosphates, excepting in case 3 of exostosis, in which the amount of sulphates and of phosphates was above the average.

In case 9, of total obstruction of the bowels, sulphate of magnesia was given frequently during the day in small doses: nothing passed through the intestine. The whole of the sulphate was absorbed, and hence the great increase in the sulphates in the urine.

In a patient, case 14, who was taking Vichy water, the sulphates were observed to be slightly above the average.

The conclusions I have drawn may be thus enumerated:—

Table I. In three cases of acute chorea the most remarkable increase was observed in the amount of sulphates in the urine. In the same cases the quantity of urea was very much increased. The quantity of urine made in twenty-four hours was not excessively diminished, and the total amount of earthy and alkaline phosphates was below the average amount.

The general conclusion was, that in acute chorea the amount of sulphates in the urine is increased, whilst sometimes the phosphates are as remarkably diminished.

Table II. In delirium tremens and in other delirium a remarkable increase in the amount of sulphates in the urine was frequently observed, and the total phosphates were in the same cases occasionally remarkably diminished; and the resemblance to

the state of chorea was still closer, inasmuch as occasionally a very great excess of urea was found in these cases also.

Table III. In acute inflammatory diseases of the nervous structures, during the most febrile symptoms, an increase was observed in the amount of sulphates in the urine, and the total amount of earthy and alkaline phosphates in these diseases appeared to be increased in the same proportion as the sulphates were increased.

Table IV. In some slight and chronic diseases of the nervous structures, no increase in the amount of sulphates in the urine was observed, excepting when sulphate of magnesia had been taken as a medicine.

Table V. In acute diseases, in which neither the nervous nor the muscular structures were chiefly affected, no increase in the sulphates or phosphates was observed, except after sulphate of magnesia.

Table VI. In chronic diseases, in which neither the nervous nor the muscular structures were chiefly affected, no decided increase in the sulphates or phosphates in the urine was observed, except after sulphate of magnesia. One case of exostosis may be regarded as a doubtful exception to this statement.

The general conclusions are—

1. That in acute chorea, in which the muscles are in excessive action, the sulphates and urea in the urine are greatly increased.
2. That in delirium tremens the same state of urine is frequently met with; that in these diseases the phosphates are not at all increased.
3. That in acute inflammation of the nervous structures, sulphates and phosphates are both increased in the urine.
4. That in chronic diseases of the brain, and that in other acute inflammations and diseases and in other chronic diseases, no increase in the total amount of sulphates is observed, excepting when sulphate of magnesia is taken as a medicine.

The result of this investigation is that—

Muscular action increases the sulphates in the urine without increasing the phosphates; whilst

Inflammation of the brain increases both the sulphates and phosphates in the urine.

XXXIV. *Second Appendix to a paper on the Variations of the Acidity of the Urine in the state of Health.*

By HENRY BENICE JONES, M.D., M.A., Cantab., F.R.S., Physician to St. George's Hospital.

Received June 6,—Read June 20, 1850.

On the Influence of Tartrate of Ammonia and of Carbonate of Ammonia on the Acidity of the Urine.

IN a previous paper and appendix on the acidity of the urine, published in the Philosophical Transactions for 1849, I have shown the effect of different diets, of sulphuric acid, of tartaric acid, of caustic potash and of tartrate of potash on the acidity of the urine; in this Appendix I purpose tracing the effect of tartrate of ammonia and of carbonate of ammonia.

III. (e.) The effect of tartrate of potash having been so remarkable, it was thought, that by observing the effect of tartrate of ammonia, some conclusion might most rapidly and decidedly be obtained, as to the comparative effect of fixed and volatile alkalies on the acidity of the urine.

Mr. MORSON prepared for me some beautifully crystalline tartrate of ammonia: one portion was dried by pressure on blotting paper; another portion was dried in a water bath. When dissolved in water no acid reaction was perceptible.

(36.) The first day for comparison no tartrate of ammonia was taken. Breakfast at 8^h 40^m A.M. Dinner at 6^h P.M. On mixed diet.

	h m	Spec. gr.	Acidity per 1000 gra. of urine.	Appearance.
Water passed at	7 25 A.M. was thrown away.			
Water passed at	8 40	1015·3	+ 4·93 measures.	Clear.
Water passed at	9 45	1021·7	— 2·93	Clear.
Water passed at	10 50	1018·5	— 7·85	Clear.
Water passed at	1 0 P.M.	1025·8	+10·72	Thick from urates.
Water passed at	3 25	1024·2	+19·52	Thick from urates.
Water passed at	6 0	1023·5	+26·38	Clear.
Water passed at	8 15	1029·4	+25·26	Thick from urates.
Water passed at	10 15	1031·7	+15·50	Thick from urates.
Water passed at	7 10 A.M.	1022·3	+10·76	Clear.

(37.) For the following days tartrate of ammonia was taken. The first day two drachms of imperfectly dry tartrate of ammonia were taken, in two ounces of distilled water, at a few minutes after one o'clock P.M.

Water passed at	8 35 A.M.	1025·1	+13·65 measures.	Thick appearance from urates.
Water passed at	10 5	1023·9	+11·72	Thick from urates.
Water passed at	11 30	1027·2	— 7·78	Clear.

	h m	Spec. gr.	Acidity per 1000 grs. of urine.	Appearance.
Water passed at	1 5 P.M.	1027.0	+ 5.84 measures.	Thick from urates.
Water passed at	2 10	1025.2	+ 11.70	Thick from urates.
Water passed at	2 50	1025.6	+ 18.52	Thick from urates.
Water passed at	4 30	1025.9	+ 20.47	Cloudy.
Water passed at	6 30	1024.6	+ 18.54	Clear.
Water passed at	11 45	1031.0	+ 12.62	Thick from urates.
Water passed at	6 55 A.M.	1021.1	+ 16.64	Clear.

(38.) The following day 180 grains of tartrate of ammonia, dried at 100, were taken soon after twelve o'clock; and at three o'clock 108 grains more were taken. In all then, 288 grains. Breakfast at 8^h 24^m A.M. Dinner at 6^h 30^m P.M. The salt caused severe griping pain of the bowels, which lasted until the night, but the bowels were not relaxed.

Water passed at	8 24 A.M.	1026.0	+ 17.54 measures.	Thick from urates.
Water passed at	10 10	1027.0	+ 13.63	Thick from urates.
Water passed at	12 5	1027.4	+ 11.68	Thick from urates.
Water passed at	1 0 P.M.	1024.8	+ 17.56	Thick from urates.
Water passed at	3 0	1027.4	+ 23.36	Thick from urates.
Water passed at	5 0	1025.5	+ 25.35	Clear.
Water passed at	6 20	1026.0	+ 27.29	Clear.
Water passed at	11 15	1029.2	+ 15.54	Thick from urates.
Water passed at	6 40 A.M.	1022.9	+ 17.59	Clear.

(39.) The following day a single dose of tartrate of ammonia was taken. At twelve o'clock 177 grains of imperfectly dry tartrate of ammonia were taken. Breakfast at 8^h 10^m A.M. Dinner at 6^h 30^m P.M. as before.

Water passed at	8 10 A.M.	1027.0	+ 19.47 measures.	Thick from urates.
Water passed at	9 45	1024.8	+ 17.56	Thick from urates.
Water passed at	12 0	1026.9	+ 14.60	Thick from urates.
Water passed at	1 30 P.M.	1026.0	+ 16.57	Thick from urates.
Water passed at	3 20	1027.3	+ 25.30	Thick from urates.
Water passed at	4 50	1027.9	+ 27.24	Thick from urates.
Water passed at	6 30	1027.7	+ 28.21	Thick from urates.
Water passed at	11 40	1027.2	- 8.75	Clear.
Water passed at	7 0 A.M.	1017.5	+ 17.69	Thick from urates.

The medicine caused slight uneasiness of the bowels, which lasted until the evening.

(40.) The following day no tartrate of ammonia was taken. Breakfast at 8^h 30^m A.M. Dinner at 6^h 50^m P.M., as before.

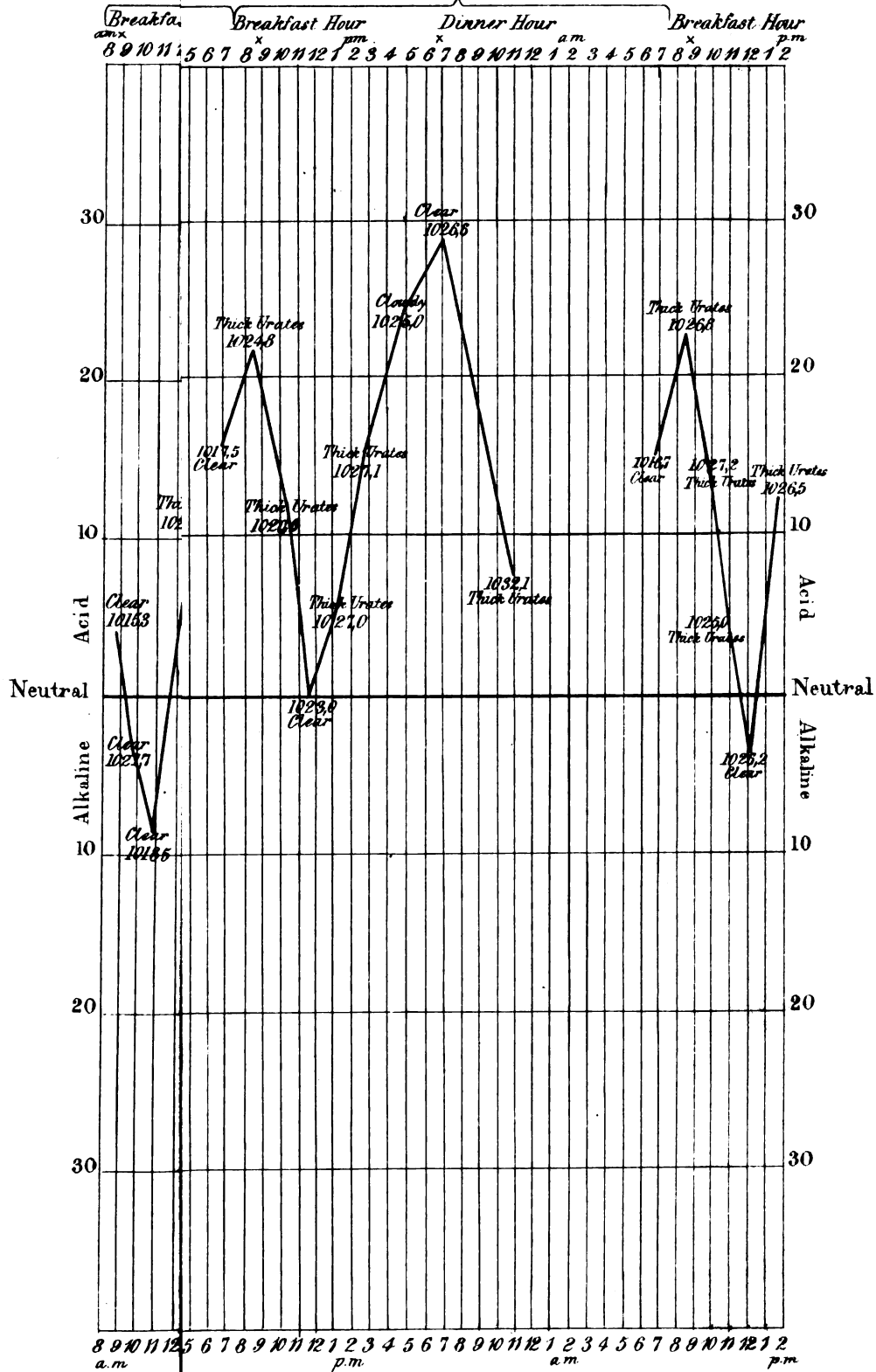
Water passed at	8 30 A.M.	1024.8	+ 21.46 measures.	Thick from urates.
Water passed at	10 10	1020.6	+ 11.75	Thick from urates.
Water passed at	11 20	1023.0	0	Clear.
Water passed at	1 0 P.M.	1027.0	+ 5.84	Thick from urates.
Water passed at	2 30	1027.1	+ 14.60	Thick from urates.
Water passed at	5 5	1025.0	+ 26.34	Cloudy from urates.
Water passed at	6 50	1026.6	+ 29.22	Clear.
Water passed at	11 0	1032.1	+ 8.72	Thick from urates.

aken.

5TH DAY

no *grain*

no Tartrate of Ammonia was taken



	h m	Spec. gr.	Acidity per 1000 gra. of urine.	Appearance.
Water passed at	6 45 A.M.	1016·7	+15·73 measures.	Clear.
Water passed at	8 30	1026·8	+23·37	Thick from urates.
Water passed at	9 45	1027·2	+15·56	Thick from urates.
Water passed at	10 40	1025·0	+ 5·85	Thick from urates.
Water passed at	12 0	1026·2	— 3·89	Clear.
Water passed at	1 30 P.M.	1026·5	+11·69	Thick from urates.

It follows from these experiments, which are best seen in Plate LI., that the influence of tartrate of ammonia in lessening the acidity of the urine is not perceptible.

By comparing this Plate with Plate XXIII. 1849, the remarkable difference between the action of volatile alkali and fixed alkali is very apparent. When two drachms of tartrate of potash were taken, the effect was perceptible in thirty-five minutes. When three drachms of tartrate of ammonia were taken, no diminution of the acidity of the urine was observed.

In three days, one ounce, one drachm, two scruples and five grains of tartrate of ammonia were taken without any apparent effect in diminishing the acidity of the urine.

III. (f.) The effect of carbonate of ammonia on the acidity of the urine was then examined; and for this purpose Mr. MORSON gave me some of the so-called sesquicarbonate of pharmacy. Each dose was reduced to powder immediately before it was taken, in distilled water.

(41.) The first day for comparison no carbonate of ammonia was taken. Breakfast at 8^h 20^m A.M. Dinner at 6^h 40^m P.M. As before.

	h m	Spec. gr.	Acidity per 1000 gra. of urine.	Appearance.
Water passed at	8 20 A.M.	1028·4	lost.	Thick from urates.
Water passed at	11 5	1025·2	+ 4·87 measures.	Cloudy.
Water passed at	12 50	1024·4	+ 6·83	Clear.
Water passed at	3 5 P.M.	1026·0	+16·57	Clear.
Water passed at	5 15	1016·7	+18·68	Clear.
Water passed at	6 40	1023·6	+25·40	Clear.
Water passed at	11 5	1031·4	0	Clear.
Water passed at	6 55 A.M.	1021·1	+12·73	Clear.

(42.) The following day. Breakfast at 8^h 15^m A.M. Dinner at 6^h 50^m P.M., as before. Eighteen grains of carbonate of ammonia were taken at 12^h 50^m.

Water passed at	8 15 A.M.	1021·7	+15·66 measures.	Clear.
Water passed at	9 50	1012·6	+ 7·90	Clear.
Water passed at	11 35	1020·3	0	Clear.
Water passed at	12 50	1022·5	+11·63	Clear.
Water passed at	3 0 P.M.	1022·1	+17·61	Clear.
Water passed at	4 30	1021·1	+20·56	Clear.
Water passed at	6 50	1023·7	+24·42	Clear.
Water passed at	11 0	1031·1	+19·39	Thick from urates.
Water passed at	6 50 A.M.	1023·4	+15·63	Clear.

(43.) The following day. Breakfast at 8^h 12^m A.M. Dinner at 6^h 35^m P.M. Twenty

grains of carbonate of ammonia were taken at 1^m P.M.; and the same quantity at 3^h 30^m P.M.

	h m	Spec. gr.	Acidity per 1000 gra. of urine.	Appearance.
Water passed at	8 12 A.M.	1024·6	+ 22·49 measures.	Clear.
Water passed at	9 45	1021·0	+ 16·65	Thick from urates.
Water passed at	11 0	1025·0	+ 11·70	Thick from urates.
Water passed at	1 0 P.M.	1027·5	+ 15·57	Thick from urates.
Water passed at	2 35	1029·6	+ 21·36	Thick from urates.
Water passed at	4 25	1027·2	+ 29·20	Thick from urates.
Water passed at	6 35	1026·2	+ 29·23	Clear.
Water passed at	9 25	1033·4	+ 27·09	Thick from urates.
Water passed at	11 25	1029·3	+ 2·91	Clear.
Water passed at	6 50 A.M.	1026·4	+ 29·23	Clear.

(44.) The following day. Breakfast at 8^h 10^m A.M. Dinner at 6^h 40^m P.M., as before. Twenty grains of carbonate of ammonia at 11^h 15^m A.M. The same quantity at 12^h 45^m noon. Repeated at 2^h 15^m P.M.; and again repeated at 4^h P.M. In all, eighty grains of carbonate of ammonia in the day.

Water passed at	8 10 A.M.	1030·6	+ 32·99 measures.	Thick from urates.
Water passed at	9 45	1025·1	+ 22·34	Thick from urates.
Water passed at	11 15	1027·4	+ 13·62	Thick from urates.
Water passed at	12 45	1029·0	+ 19·43	Thick from urates.
Water passed at	2 15 P.M.	1028·4	+ 24·31	Thick from urates.
Water passed at	4 0	1026·2	+ 27·28	Clear.
Water passed at	6 40	1027·7	+ 30·16	Clear.
Water passed at	10 45	1031·1	+ 29·09	Thick from urates.
Water passed at	6 10 A.M.	1022·6	+ 25·42	Clear.

(45.) The following day. Breakfast at 8^h 15^m A.M. Dinner at 6^h 45^m P.M., as before. For comparison no carbonate of ammonia was taken.

Water passed at	8 15 A.M.	1023·6	+ 26·37 measures.	Clear.
Water passed at	10 0	1023·5	+ 23·44	Thick from urates.
Water passed at	12 45	1027·2	+ 21·41	Thick from urates.
Water passed at	3 20 P.M.	1029·0	+ 34·02	Thick from urates.
Water passed at	5 35	1028·3	+ 35·00	Clear.
Water passed at	6 45	1030·8	+ 36·86	Thick from urates.
Water passed at	9 20	1032·9	+ 37·75	Thick from urates.
Water passed at	11 45	1031·4	+ 7·75	Cloudy.
Water passed at	7 25 A.M.	1029·8	+ 22·33	Thick from urates.

(46.) Breakfast at 8^h 30^m A.M.

Water passed at	8 30	1028·6	+ 27·22	Thick from urates.
Water passed at	9 35	1025·4	+ 17·55	Clear.
Water passed at	11 0	1027·7	+ 8·75	Clear.
Water passed at	1 0 P.M.	1030·3	+ 17·47	Thick from urates.
Water passed at	3 0	1029·3	+ 25·26	Thick from urates.
Water passed at	5 45	1029·0	+ 27·21	Thick from urates.

The result of these experiments is very evident in Plate LII.



It follows therefrom, that carbonate of ammonia, taken in large doses, does not diminish the acidity of the urine. On the contrary, the third day (experiment 44.), when most carbonate of ammonia was taken, the acidity was higher than it had been any previous day. By experiment (45.) it appears that the acidity of the urine was still high on the following day. The effect of the eighty grains of carbonate of ammonia was very evident twenty-four hours after it was taken.

The comparison between Plate XXII. Philosophical Transactions, 1849, representing the acidity when tartaric acid was taken, and this Plate, which shows the effect of carbonate of ammonia, is worthy of observation. The urine was more acid when carbonate of ammonia was taken than when tartaric acid was taken. It is possible that there was some difference in the irritability of the stomach when the two series of observations were made. But the gradual increase of the acidity, as the quantity of carbonate of ammonia taken was increased, shows that the difference of the state of the stomach was not the cause of the state of the acidity of the urine. When no carbonate of ammonia was taken, the acidity of the urine, after food was taken, was diminished to a greater degree than it was when the volatile alkali was taken.

The comparison between Plate XXI. Philosophical Transactions, 1849, which represents the acidity of the urine when liquor potassæ was taken, and this Plate, which shows the acidity when carbonate of ammonia was taken, is also very interesting, as it establishes the important difference of the effect of volatile and fixed alkali on the acidity of the urine.

That 138 grains of carbonate of ammonia, taken in three consecutive days, should not diminish the acidity of the urine is very remarkable. It is still more worthy of attention that it actually increases the acidity. It appeared very desirable to test these facts by further experiments.

(47.) The experiment with large doses of carbonate of ammonia was therefore repeated. For comparison, the day previous to that on which the carbonate of ammonia was taken, the variations in the acidity of the urine were determined. Breakfast at 8^h 30^m A.M. Dinner at 7 P.M.

	h m	Spec. gr.	Acidity per 1000 grs. of urine.	Appearance.
Water passed at	7 20 A.M. thrown away.			
Water passed at	8 30	1028·8	+ 19·44 measures.	Thick from urates.
Water passed at	9 40	1026·0	+ 14·62	Thick from urates.
Water passed at	10 35	1025·2	+ 5·85	Clear.
Water passed at	11 55	1027·6	— 14·59	Cloudy from phosphates.
Water passed at	1 0 P.M.	1027·9	+ 12·64	Cloudy from urates.
Water passed at	2 55	1026·8	+ 18·50	Thick from urates.
Water passed at	5 10	1025·4	+ 24·38	Thick from urates.
Water passed at	7 0	1027·9	+ 31·13	Thick from urates.
Water passed at	11 0	1031·8	+ 7·75	Thick from urates.
Water passed at	6 45 A.M.	1024·7	+ 9·76	Clear.

(48.) The following day. Breakfast at 8^h 15^m A.M. Dinner at 7 P.M. Twenty
MDCCCL. 4 R

grains of carbonate of ammonia were taken at 11^h 40^m A.M. The same quantity at 1^h 15^m P.M. It was repeated at 2^h 25^m, and again repeated at 3^h 55^m. In all, eighty grains dissolved in about eight ounces of distilled water.

	h m	Spec. gr.	Acidity per 1000 grs. of urine.	Appearance.
Water passed at	8 15 A.M.	1026·2	+ 17·54 measures.	Clear.
Water passed at	9 30	1025·0	+ 12·68	Thick from urates.
Water passed at	10 25	1023·8	+ 6·83	Clear.
Water passed at	11 40	1027·0	+ 2·92	Clear.
Water passed at	1 15 P.M.	1026·7	+ 11·69	Clear.
Water passed at	2 25	1027·3	+ 19·47	Thick from urates.
Water passed at	3 55	1023·9	+ 23·44	Clear.
Water passed at	5 30	1022·8	+ 25·42	Clear.
Water passed at	7 0	1025·2	+ 29·26	Clear.
Water passed at	10 55	1028·9	+ 3·94	Clear.
Water passed at	5 10 A.M.	1024·6	+ 2·94	Clear.

Plate LIII. gives the means of comparing the acidity of the urine on these days. Here also it is evident that the acidity is not so much diminished after food when carbonate of ammonia is taken, but the increase in the acidity after the volatile alkali is taken is not so evident as in the previous Plate LII.: this probably arose from the very considerable increase in the quantity of water made on the day the carbonate of ammonia was last taken. The volatile alkali acted as a diuretic. This is seen by the lower specific gravity of the urine the second day. The actual quantity of water passed was one-third more than it was on the previous day.

Still this experiment shows that eighty grains of carbonate of ammonia do not lessen the acidity of the urine.

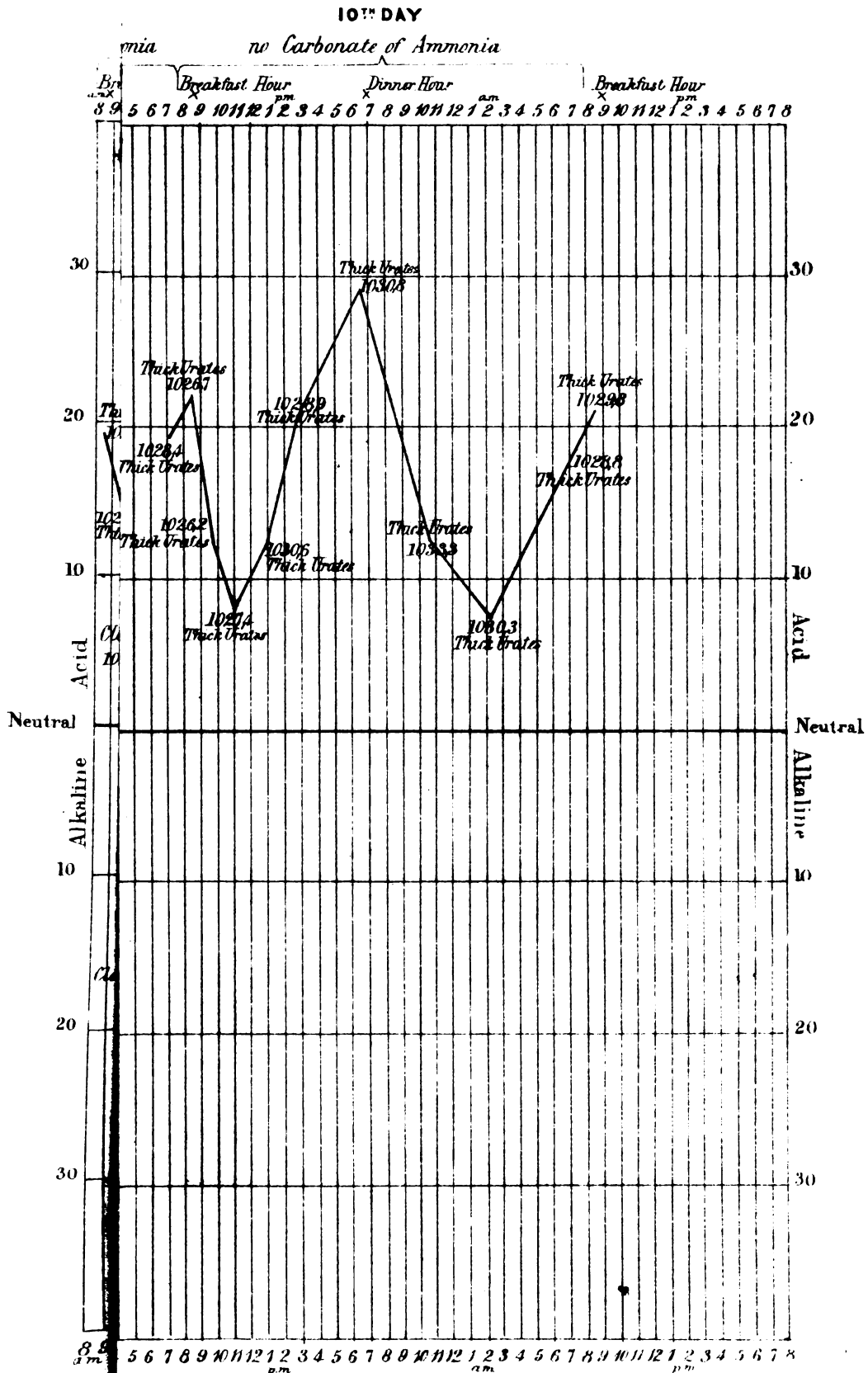
(49.) The following day no carbonate of ammonia was taken. Breakfast at 8^h 20^m A.M. Dinner at 7^h 15^m P.M. An increased quantity of urine was secreted during this day also.

	h m	Spec. gr.	Acidity per 1000 grs. of urine.	Appearance.
Water passed at	8 20 A.M.	1017·2	+ 13·76	Clear.
Water passed at	9 30	1014·5	+ 8·87	Clear.
Water passed at	10 15	1014·5	+ 7·88	Clear.
Water passed at	11 25	1021·3	+ 12·72	Clear.
Water passed at	1 10 P.M.	1023·8	+ 17·58	Clear.
Water passed at	2 30	1023·5	+ 20·51	Clear.
Water passed at	3 50	1021·5	+ 19·58	Clear.
Water passed at	5 20	1022·2	+ 21·42	Clear.
Water passed at	7 15	1025·9	+ 23·39	Clear.
Water passed at	10 30	1028·3	0	Clear.
Water passed at	6 25 A.M.	1026·4	+ 11·69	Clear.

(50.) The next day eighty grains of carbonate of ammonia were taken in divided doses: twenty grains at 11^h 45^m A.M.; the same quantity at 1^h 15^m P.M., at 2^h 30^m P.M., and at 3^h 45^m P.M. Breakfast was at 8^h 20^m A.M., and dinner at 7^h 30^m P.M.

The quantity of urine was much less than on the two previous days.

was taken



	h m	Spec. gr.	Acidity per 1000 grs. of urine.	Appearance.
Water passed at	8 20 A.M.	1026·5	+ 18·50 measures.	Thick from urates.
Water passed at	9 30	1025·6	+ 14·62	Thick from urates.
Water passed at	10 35	1028·4	+ 7·78	Thick from urates.
Water passed at	11 45	1029·3	0	Clear.
Water passed at	1 15 P.M.	1027·9	+ 14·59	Thick from urates.
Water passed at	2 30	1027·4	+ 21·41	Clear.
Water passed at	3 45	1026·7	+ 26·29	Clear.
Water passed at	5 20	1026·9	+ 28·24	Clear.
Water passed at	7 30	1028·7	+ 32·08	Clear.
Water passed at	11 15	1029·7	+ 17·48	Clear.
Water passed at	6 55 A.M.	1028·4	+ 19·44	Thick from urates.

Here again there is decided evidence of an increase in the acidity of the urine after a large dose of carbonate of ammonia. There was no diuretic action, but during the night a most profuse and unusual perspiration took place. The following morning no carbonate of ammonia was taken.

(51.) Breakfast was at 8^h 35^m A.M. Dinner at 6^h 15^m P.M. The same as on the previous days.

Water passed at	8 35 A.M.	1026·7	+ 22·40	Thick from urates.
Water passed at	9 40	1026·2	+ 13·64	Thick from urates.
Water passed at	11 0	1027·4	+ 8·76	Thick from urates.
Water passed at	1 0 P.M.	1030·6	+ 11·64	Thick from urates.
Water passed at	3 0	1028·9	+ 21·38	Thick from urates.
Water passed at	6 15	1030·8	+ 29·10	Thick from urates.
Water passed at	10 30	1033·3	+ 13·55	Thick from urates.
Water passed at	2 0 A.M.	1030·3	+ 8·73	Thick from urates.
Water passed at	7 20	1028·8	+ 18·47	Thick from urates.
Water passed at	8 20	1029·8	+ 20·39	Thick from urates.

Perhaps the acidity did not rise higher on account of the perspiration, which must have removed much acid from the system. The effect of the carbonate of ammonia may be traced in a smaller fall than usual in the acidity after breakfast and dinner.

These experiments, well seen in Plate LII., then tend to the confirmation of the results previously obtained:—first, that there is a very great difference between the effects of volatile and fixed alkalies on the acidity of the urine; secondly, that carbonate of ammonia, in large doses, does not diminish the acidity of the urine; thirdly, that carbonate of ammonia, in large doses, actually increases the acidity of the urine; and this was evident, not only in the acidity not falling so low as it did after food when no carbonate of ammonia was taken, but in an actual rise before food to a higher degree than was reached when no carbonate of ammonia was administered.

The conclusions from these experiments with tartrate and carbonate of ammonia may be shortly stated thus—

(e.) That tartrate of ammonia in large doses produces no effect on the alkalescence of the urine. It differs entirely in this respect from tartrate of potash.

(f.) That carbonate of ammonia in large doses increases the acidity of the urine.

I hope to determine the cause of this in a future paper on the variations of the nitrates in the urine.

XXXV. *An Experimental Inquiry into the Strength of Wrought-Iron Plates and their Riveted Joints as applied to Ship-building and Vessels exposed to severe strains.*

By WILLIAM FAIRBAIRN, *Esq.*

Communicated by the Rev. HENRY MOSELEY, *F.R.S.*

Received April 25,—Read June 13, 1850.

THE experiments herein recorded were instituted early in the spring of 1838, and before the close of the following winter most of them had been completed; owing however to a long series of professional engagements they have stood over (with the exception of some additions made in the following year) to the present time. The object of the inquiry was twofold—first, to ascertain by direct experiment the strength of wrought-iron plates and their riveted joints in their application as materials for ship-building; and secondly, to determine their relative value when used as a substitute for wood. On these two points it cannot be expected that our knowledge should be far advanced, as a very few years have elapsed since it was asserted that iron, from its high specific gravity, was not calculated for such a purpose, and that the greatest risk was likely to be incurred in attempting to construct vessels of what was then considered a doubtful material. Time has however proved the fallacy of these views, and I hope, in the following experiments, to show that the iron ship, when properly constructed, is not only more buoyant, but safer, and more durable than vessels built of the strongest English oak.

At the commencement of the experiments I felt desirous of conducting them upon a scale of such magnitude as would supply sound practical data, and at the same time establish a series of results calculated to ensure confidence as well as economy in the use of the material. My views were ably carried out by Mr. HODGKINSON, who conducted the experiments under my direction, and from whom I received valuable assistance.

In conducting the investigation I found it necessary to divide the subject into four distinct parts:—

1st. The strength of plates when torn asunder by a direct tensile strain in the direction of the fibre, and when torn asunder across it.

2ndly. On the strength of the joints of plates when united by rivets as compared with the plates themselves.

3rdly. On the resistance of plates to the force of compression, whether applied by a dead weight or by impact.

And lastly. On the strength and value of wrought-iron frames and ribs as applied to ships and other vessels*.

PART I.

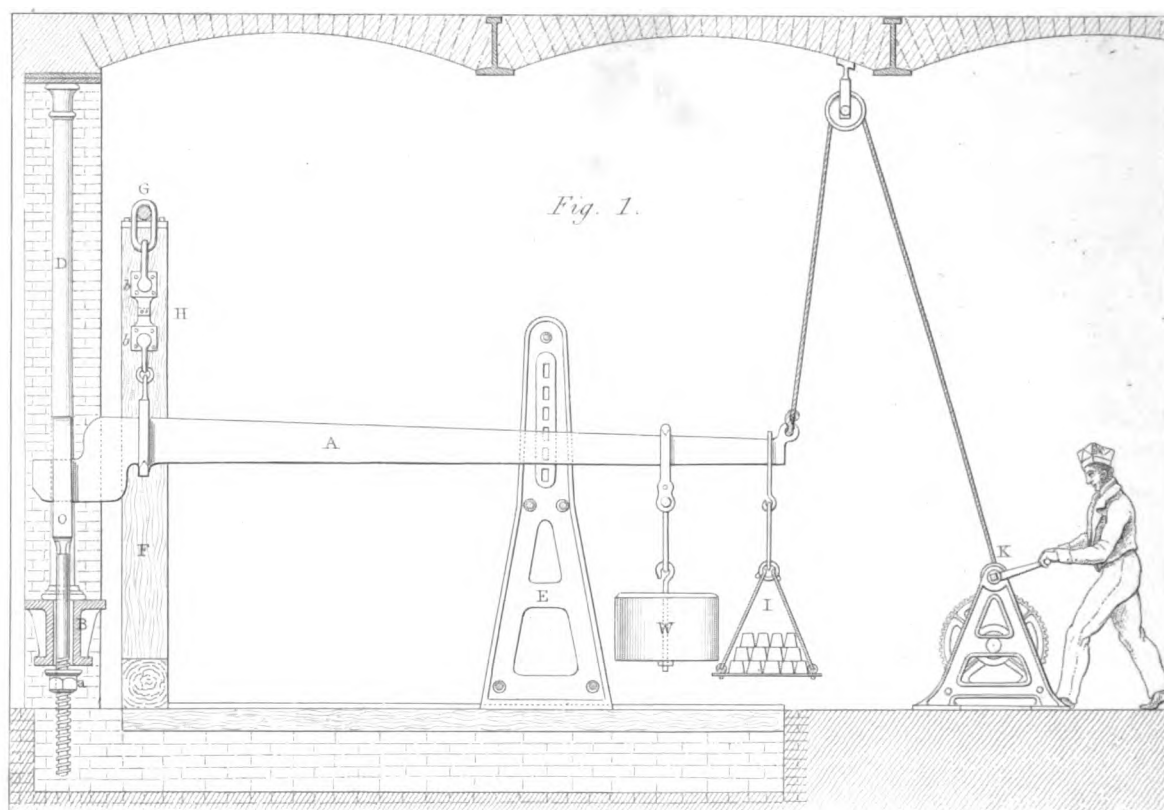
At the commencement of iron ship-building, in which I took an active part, the absence of acknowledged facts relative to the strength and varied conditions under which the material was applied, was the principal reason which induced me to enter upon this inquiry. I have extended the investigation into the best methods of riveting, and the proportional strength of rivets, joints, &c., as compared with the plates and the uses for which they are intended. The latter is a practical and highly important inquiry, as great difference of opinion exists amongst engineers and others, as to the form, strength and proportions of rivets, and the joints of which they form an essential part. I therefore considered an experimental investigation much wanted, not only on account of its important practical bearing, but what was probably of equal value, in order to remove existing discrepancies and to establish a sounder principle of construction founded upon the unerring basis of experiment. From these considerations I bestowed increased attention upon the inquiry, and endeavoured to render it practically useful. Before detailing the experiments, it may be necessary to describe the apparatus by which the results were obtained.

The annexed drawings, Plate LIV., represent a side and end view of the apparatus used in the experiments. The large lever A was made of malleable iron and was fixed to the lower cross beam B (fig. 1) by a strong bolt O, which passed through it at B. At the top end of this bolt a preparation was made to receive the end of the lever, and by means of the screw-nut at *a*, the lever A was raised or lowered to suit the length of the plates to be experimented upon. Upon the top side of the beam, and under the gable wall of a building five stories high, were placed two cast-iron columns, D, D, which retained the beam B in its place and prevented it from rising when the lever was heavily loaded during the experiment. The frame E guided the end of the lever and the weight W, and close to the fulcrum were placed two wooden standards,

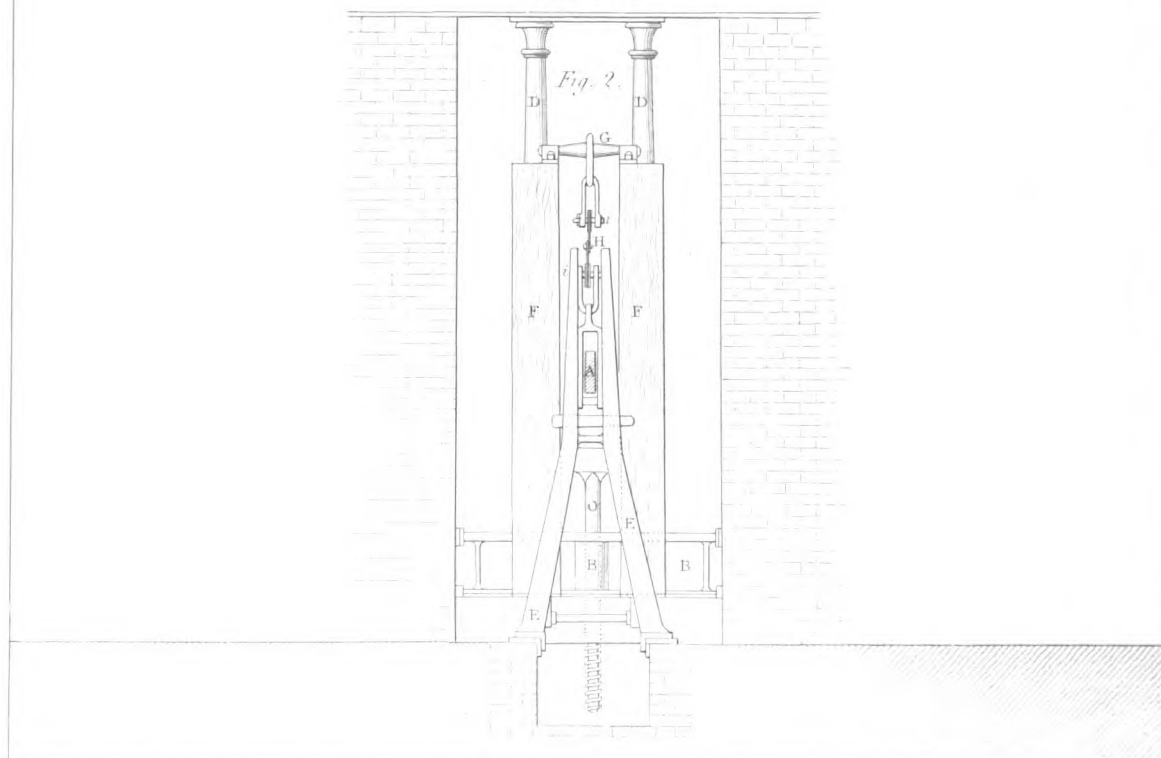
* Several important facts and improvements in the construction of iron ships have been ascertained since my experiments were made, but I apprehend none of them have tended in the least degree to diminish their value. Nor have they, to the best of my knowledge, been superseded by others of a more elaborate or more decisive character. It is true, that a series of interesting and important experiments have been made at the instance of the Admiralty on the effect of shot upon the sides of iron ships. At some of these experiments I had the honour to be present, and witnessed some curious and unexpected results.

The first series was conducted at the Arsenal, Woolwich, and subsequently others were made at Portsmouth. Both were important as respects the effect of shot upon wrought-iron plates, with enlarged and diminished charges of powder and at different velocities, but discouraging as regards the use of iron in the construction of ships of war. These experiments, however interesting in themselves, do not appear to be conclusive; and it is to be hoped that the apparent danger, indicated by the experiments, may yet be overcome, and the superiority as well as the greater security of the iron ship fully established.

Side View.



End View



F F, on which were fixed the cast-iron saddles receiving the cross bar G, from which the plates to be experimented upon were suspended. These plates were nearly all of the same form as shown at H, and were made narrower in the middle to ensure fracture in that part; the ends, as at *b, b*, had plates riveted to them on both sides, in order to strengthen them at those parts when attached to the bolts and shackle under strain.

The specimens thus prepared were suspended by the cross bolts *i, i*, and resting upon the standards were torn asunder by weights suspended from the large beam, as exhibited in the Plate.

In addition to the large weight W, a strong scale was attached to the extreme end of the lever at I, for the purpose of increasing the weights when required in the larger description of experiments, and by the application of a pair of blocks and the windlass K, the load was removed, and the changes produced upon the plates were by these means carefully determined.

The following data respecting the weight W, lever, shackle, &c., are taken from the actual weights from which the calculations are made:—

	lbs.
W. The weight with its carriage	2552
A. The weight of the beam	1070
2 A. The weight of the beam	2140
3 A. The weight of the beam	3210
K. Shackle	24
4 K. Shackle	96
6 K. Shackle	144

Experiments to ascertain the Strength of Plates, &c.

In the following experiments all the plates were of uniform thickness, and of the form exhibited in fig. 2 in the column of remarks; the ends had plates riveted to them on both sides to render them inflexible; they had holes, 0, 0, bored through them perpendicular to the plate, in order to connect it by bolts, with the apparatus for tearing it asunder in the part A B, which was made narrower than the rest. The centres of the holes 0, 0 were in a direct line through the middle between A and B*.

TABLE I. Strength of Plates.—Low Moor Yorkshire Iron.

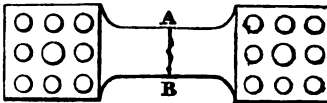
No. of exp.	Description of plate and dimensions in the middle.	Weight laid on in lbs.	Reduced dimensions in middle of plate through weights laid on.	Breaking weight in lbs.	Mean breaking weight in lbs.	Remarks.
1.	Drawn in the direction of the fibre. Area of section in middle $2.00 \times .22 = .44$ in.	24,043	$1.96 \times .21$			Fig. 2. Plan and section of the plates, the line AB being that of the fracture.  All the plates were laminated as if formed of three or more plates, the external ones being thinner than the internal ones†. In the last experiment there was a disunion between the lamina which admitted the point of a penknife.
2.		25,531	$1.89 \times .19$	25,531	25,400, or 25.77 tons per square inch.	
		23,571	Reduced.			
		24,747	$1.93 \times .18$	24,747		
3.		25,923	$1.94 \times .18$	25,923		
4.	Same iron drawn across the fibre. Area of section $2.00 \times .22 = .44$ in.	23,179 24,355 25,923 27,099	Altered. $1.99 \times .215$ $2.2 \times .19$	27,099	27,099, or 27.49 tons per square inch.	This, it will be seen, did not break at the narrowest place.

TABLE II. Strength of Plates.—Low Moor Yorkshire Iron.

No. of exp.	Description of plate and dimensions in the middle.	Weight laid on in lbs.	Reduced dimensions in middle of plate through weights laid on.	Breaking weight in lbs.	Mean breaking weight in lbs.	Remarks.
5.	Same iron as in Table I., drawn across the fibre. Area of section 2.00 × .22 = .44 in.	24,355	Stretching. 21.5 × .20	26,315	25,662, or 26.037 tons per square inch.	The form and size of specimen as before, fig. 2. In these experiments it was observed, as in No. 4. in the preceding Table, that the plate did not break at the narrowest part, a circumstance the more anomalous, as there did not appear to be anything in the apparatus to cause it.
6.		26,315 23,571	2.25 × .20	23,571		
7.	Same iron, thicker plates, drawn in direction of fibre. Area of section 2.00 × .26 = .52 in.	27,099 25,923	Thickness .25 1.96 × .24	27,099 25,923	 26,511, or 22.76 tons per square inch.	Very uniform in texture. The fracture of this specimen showed a great want of regularity; about one-third of the area had the appearance of steel. All the other plates appeared to be uniform, but laminated, as mentioned before.

* For the appearance of the fractures see Plate LV.

† Nearly the whole of the plates manufactured in this country are laminated, owing to the manner in which the shingles are formed, by piling a number of flat bars one upon another, which are made larger or smaller according as the plate may be required heavier or lighter.

YORKSHIRE PLATES.

TABLE I

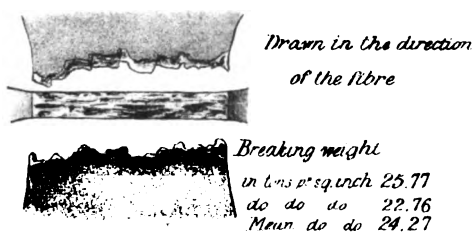
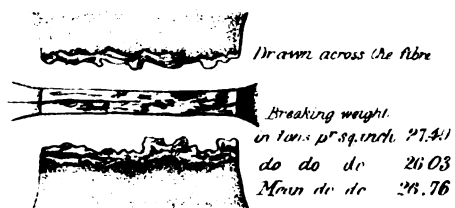
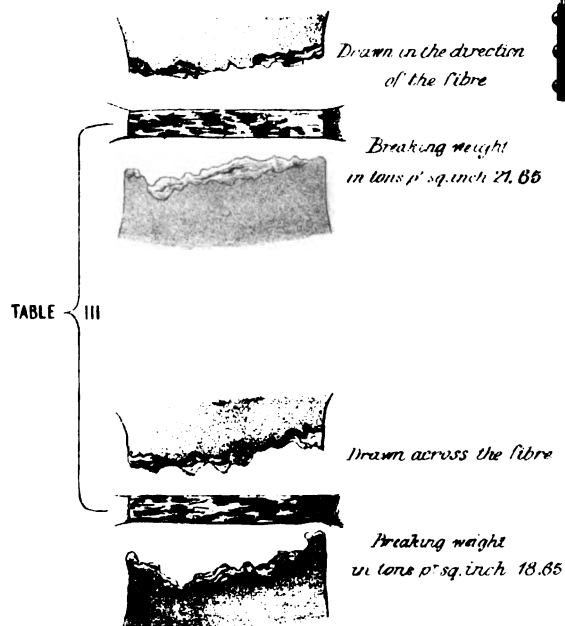


TABLE II



DERBYSHIRE PLATES



SHROPSHIRE PLATES.

TABLE IV

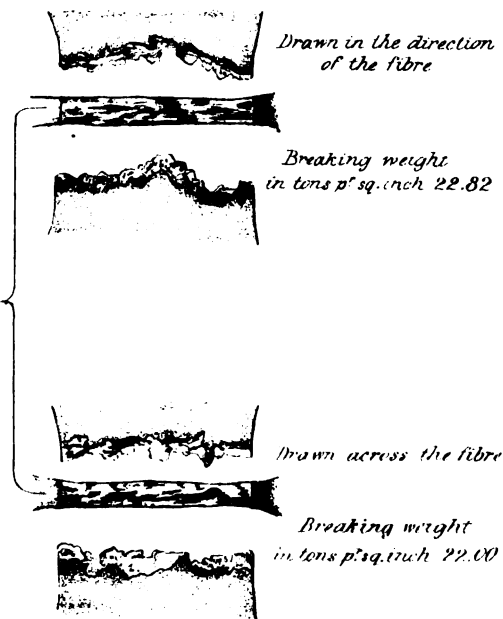
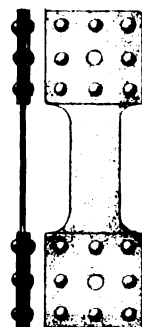
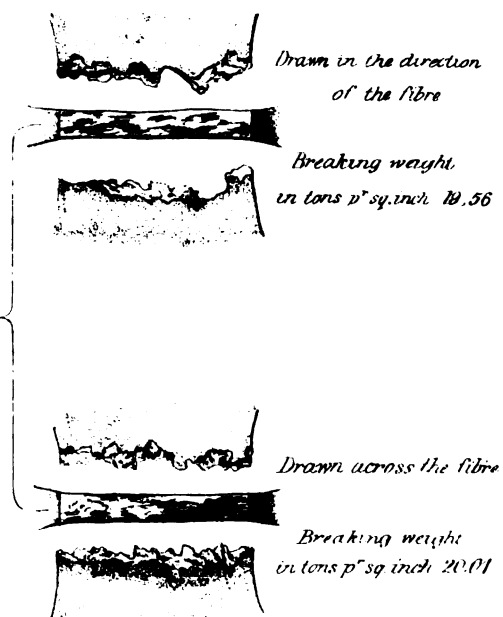


Fig. 2.



STAFFORDSHIRE PLATES

TABLE V



Liastre, Lith.

The results obtained from the Low Moor plates in the preceding Tables give fair indications of their strength. It will be observed, on comparing the mean of the breaking weights in this case with the experiments of BROWN and TELFORD, that there is a very slight difference between the strength of plates and bar iron.

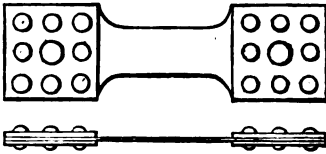
Taking the results of Captain BROWN, we have in eight experiments on Swedish, Welsh and Russian iron, 25 tons as a mean of the breaking weight when reduced to an inch square.

In Mr. TELFORD's experiments on Swedish, Welsh, Staffordshire and faggoted iron, the mean breaking weight obtained from nine different bars was $29\frac{1}{4}$ tons to the square inch. The comparison will then be—

Captain BROWN's experiments, 25 tons to the square inch	} Mean	
MINORD and DESAMES' experiments, 25 tons to the square inch		
Mr. TELFORD's experiments, $29\frac{1}{4}$ tons to the square inch		
Yorkshire plates' experiments, $24\frac{1}{2}$ tons to the square inch.		
		26·41 tons.

Making the strength of plates to that of bars as 24·5 : 26·4, being a comparatively small difference in their respective powers to resist a tensile force.

TABLE III. Strength of Plates.—Derbyshire Iron.

No. of exp.	Description of plate and dimensions in the middle.	Weight laid on in lbs.	Reduced dimensions in middle of plate through weights laid on.	Breaking weight in lbs.	Mean breaking weight in lbs.	Remarks.
9.	Drawn in the direction of the fibre. Area of section $2\cdot00 \times 28\text{in.}$	21,219 28,667	Stretched. $2\cdot00 \times 27$	28,667		Form of specimen the same as shown in Table I. fig. 2.
10.	$2\cdot00 \times 29\text{in.}$	21,219 22,789 26,707	Sinking. Sinking. $2\cdot15 \times 27$	26,707	27,687, or 21·68 tons per square inch.	 There was a stripe resembling steel across the fracture near one side.
11.	Plates drawn across the fibre. Area of section $2\cdot00 \times 28 = 56\text{in.}$	22,395 23,179	Stretching. Thickness 27	23,179		In the broken surface there seemed to be a stratum of steel, the rest was laminated but imperfectly.
12.	$2\cdot00 \times 28 = 56\text{in.}$	24,747	$2\cdot00 \times 28$	24,747	23,963, or 18·65 tons per square inch.	Short streaks of steel in fractured surface.

If we compare the results in the Derbyshire plates with those in the preceding Tables, we have in the mean of four experiments a ratio of 20·165 : 24·850, or 5 to 6 nearly.

Again, by comparing the same plates with the mean strength of bars reduced to an inch square, the difference will be as 20 to 26, being an excess of 6 tons in favour of the bars.

TABLE IV. Strength of Plates.—Shropshire Iron.

No. of exp.	Description of plate and dimensions in the middle.	Weight laid on in lbs.	Reduced dimensions in middle of plate through weights laid on.	Breaking weight in lbs.	Mean breaking weight in lbs.	Remarks.
13.	Drawn in the direction of the fibre. Area of section $2\cdot00 \times \cdot265 = \cdot53$ in.			28,275	27,099, or 22·826 tons per square inch.	Form of specimen the same as shown in Table I. fig. 2. In the first experiment the fracture showed an iron very uniform, except a few bright spots like steel.
14.				25,923		Experiment 2. Appearance of fracture as before, but a crack up the middle showed that the plate was formed of two plates of equal thickness, not well united.
15.	Plates of the same iron drawn across the fibre. Area of section $2\cdot00 \times \cdot265 = \cdot53$ in.			26,315	26,119, or 22 tons per square inch.	Fracture as before, with a laminated diversion, as in last experiment.
16.				25,923		

The Shropshire iron gives better indications of strength than those obtained from the Derbyshire plates; the mean breaking weights in the last Table being 22·41 tons. From the Yorkshire plates we have a mean breaking weight of 24·85 tons, exhibiting a difference of $2\frac{1}{2}$ tons in favour of the Yorkshire iron, and 2 tons or about $\frac{1}{10}$ th greater than the Derbyshire.

TABLE V. Strength of Plates.—Staffordshire Iron.

No. of exp.	Description of plate and dimensions in the middle.	Weight laid on in lbs.	Reduced dimensions in middle of plate through weights laid on.	Breaking weight in lbs.	Mean breaking weight in lbs.	Remarks.
17.	Drawn in the direction of the fibre. $2\cdot00 \times \cdot26 = \cdot52$ in.			23,571	22,787, or 19·563 tons per square inch.	Form of specimen the same as before, fig. 2. Fracture dark grey colour, very similar to that from the four preceding plates. It had however a few specks of bright matter in it, and was without any laminated appearance.
18.				22,003		
19.	Plates of the same iron drawn across the fibre. Area of section $2\cdot00 \times \cdot265 = \cdot53$ in.			24,335	24,943, or 21·01 tons per square inch.	Irregular in texture, air-bubbles in fractured surface, with bright crystallized matter like steel. This iron has much of the same irregularity as the Derbyshire iron.
20.		23,571	The thickness $\cdot26$	25,531		Surface of fracture showed the iron to be very irregular, one-half being bright matter like steel.

On comparing the strengths of the different irons, it appears that the Derbyshire and Staffordshire plates are nearly equal, the former indicating 20·165 tons as the mean of the breaking weight per square inch, and the latter, as in the preceding Table, 20·28 tons per square inch. The same comparison further applies to the above and those made on the Derbyshire plates in Table III.

Taking therefore the results as derived from these experiments, it will be observed, that in every instance little or no difference appears to exist in the resisting powers of plates, whether drawn in the direction of the fibre or across it. This fact is clearly established by the following comparison, which evidently shows, that in whatever direction the plates are torn asunder, their strength is nearly the same.

	Mean breaking weight in the direction of the fibre, in tons per square inch.	Mean breaking weight across the fibre, in tons per square inch.
Yorkshire plates.....	25·770	27·490
Yorkshire plates.....	22·760	26·037
Derbyshire plates	21·680	18·650
Shropshire plates	22·826	22·000
Staffordshire plates	19·563	21·010
Mean.....	22·519	23·037

Or as 22·5 : 23·0, equal to about $\frac{1}{45}$ in favour of those torn across the fibre*.

From the above it is satisfactory to know, so far as regards uniformity in the strength of plates, that the liability to rupture is as great when drawn in one direction as in the other; and it is not improbable that the same property would be exhibited, and the same resistance maintained, if the plates were drawn in any particular direction obliquely across their fibrous or laminated structure.

In order however to establish the relative powers of resistance in plates of rolled iron, I have endeavoured to tabulate the results, as derived from the preceding experiments, in such form as will indicate their respective values, and place them in comparison with each other, and also with those made on bars by TELFORD and BROWN. The comparisons are made from the Yorkshire plates, as producing the best results; and conceiving them to be a fair average of the strength of rolled iron, I have selected them as the standard of comparison.

Comparative results of rolled iron as derived from experiment, the Yorkshire plates being unity.

Names of Iron.	No. of experiments.	Mean breaking weight in tons per square inch.	Mean breaking weight in tons per square inch.	Ratio of the strength of plates drawn in the direction of the fibre, and across it. Also of rolled and faggoted bars drawn in the direction of the fibre.
Yorkshire plates.....	8	25·514		
Derbyshire plates	4		20·160	1 : 0·7882
Shropshire plates	4		22·413	1 : 0·8789
Staffordshire plates	4		20·264	1 : 0·7946
Mean.....		25·514	20·945	1 : 0·8209
From Mr. TELFORD and Captain BROWN's experiments on bars			26·41	1 : 1·0351

* In some experiments by NAVIER upon the strengths of plates of wrought iron, both in the direction of the fibre and perpendicular to it, he found them as 40·8 to 36·4. The new methods of piling the rough bars before rolling may however account for the difference, and in a great measure determines the strength of the plate. In this country the process of piling is by equal layers of flat bars at right angles to each other, which produces great uniformity of strength and texture in the manufacture. At other places there is sometimes a difference in the mode of piling, which varies the texture of the plate, and also the strength of the layers are greater in one direction than another.

Here it will be observed that the difference between the strength of the Low Moor plates in their resistance to a tensile strain, when compared with bar iron, is inconsiderable; but taking the mean of the other irons, viz. the Derbyshire, Shropshire and Staffordshire, there is a falling off in the strength of about 21 per cent., the ratio being in favour of bar iron as 1·035 : ·8209.

In treating of the strength of iron, it may be useful to compare the foregoing experiments on the tensile strength of plates with those of a similar description on timber. On this subject I feel the more desirous of establishing a comparison, as the two kinds of material are now applied to similar purposes, such as ship-building and other constructions, and the question becomes every day more important as to which of the two materials is the best. There is every reason to believe that the advocates of improvement would arrange themselves on the side of iron, and those for the "wooden walls" would be equally zealous on that of timber. This is however a question which time and experience alone can determine, and conceiving that our knowledge of the properties of iron, as a material for ship-building, is far from perfect, we may safely leave its final decision to the evidence of experimental research, and a more extended application of its practical results.

When we attempt a comparison of the value of one material, in its application to a specific purpose, with that of another material similarly applied, the comparison is only correct when the two materials are placed in juxtaposition, or when they are contrasted under the same circumstances as to the trials and tests to which they are respectively subjected. Now in this comparison I am fortunate in having before me the able experiments of MUSSCHENBROCK, BUFFON, and those of a more recent date on direct cohesion by Professor BARLOW of Woolwich. I have selected from the experiments of the latter those which appear to approach most nearly to the present inquiry; and impressed with the conviction of their having been carefully conducted and being from English timber, I attach the greatest value to them.

According to MUSSCHENBROCK's, the strengths of direct cohesion per square inch of the following kinds of timber are as follows:—

	lbs.		lbs.
Locust-tree	20,100	Pomegranate	9750
Locust-tree	18,500	Lemon.	9250
Beech and oak	17,300	Tamarind.	8750
Orange	15,500	Fir	8330
Alder	13,900	Walnut	8130
Elm	13,200	Pitch pine	7630
Mulberry	12,500	Quince	6750
Willow	12,500	Cyprus	6000
Ash	12,500	Poplar	5500
Plum	11,800	Cedar	4880
Elder	10,000		

From BARLOW the strengths are,—

	lbs.		lbs.
Box	20,000	Beech	11,500
Ash	17,000	Oak	10,000
Teak.	15,000	Pear	9800
Fir	12,000	Mahogany.	8000

Mr. BARLOW, in adverting to the experiments of MUSSCHENBROCK, observes, that some of them differ considerably from his own, a circumstance probably not difficult to account for, as the different degrees of dryness have a great effect upon the strength of timber*.

Dr. ROBISON, in speaking of the experiments of MUSSCHENBROCK, states, that we may presume they were carefully made and faithfully narrated, but they were made on such small specimens, that the unavoidable natural inequalities of growth or texture produced irregularities in the results which have too great a proportion to the whole quantities observed. It is for the same reason that I give preference to Mr. BARLOW's results, as he observes, "that the experiments from which they are drawn were made with every possible care the delicacy of the operation would admit." Assuming therefore that BARLOW is correct, and taking the mean strength of iron plates, as given in the preceding Tables, at 49,656 lbs. to the square inch, or calling it 50,000 lbs., and the resistance of the direct cohesion of different kinds of timber as given by Mr. BARLOW, the following ratio of strengths will be obtained:—

	Timber lbs.	:	Iron. lbs.	Ratio, taking timber as unity.
Ash	17,000	:	50,000,	or as 1 : 2·94
Teak.	15,000	:	50,000,	or as 1 : 3·33
Fir	12,000	:	50,000,	or as 1 : 4·16
Beech	11,500	:	50,000,	or as 1 : 4·34
Oak	10,000	:	50,000,	or as 1 : 5·00

Hence it appears that the direct cohesion of iron plates is five times greater than oak; or in other words, their powers of resistance to a force applied to tear them asunder is as 5 to 1, making an iron plate $\frac{1}{2}$ inch thick equal to an oak plank of $2\frac{1}{2}$ inches thick. In the teak wood and fir specimens, which exhibit greater resisting powers, nearly the same rule will apply, and thinner planks, as regards the tensile strength, would answer the purpose. This is a circumstance which may be applicable to teak wood, but unfavourable to fir when viewed as a building material exposed to a great variety of strains, or when used for sheathing and similar purposes in the art of ship-building. The teak wood being timber of greater density and of

* It has been shown by Mr. HODGKINSON, that timber, when wet, will be crushed by a force less than one-half of what would take to crush it when dry. It therefore follows that much depends upon the samples selected and the way in which the timber has been seasoned.

higher specific gravity, is better calculated to resist shocks than a tough fibrous substance of a soft and spongy nature, such as fir.

On this subject it should however be noticed, that whatever material is used for covering the ribs of vessels, it should be strong and elastic, in order to resist not only the force of direct tension, but that of lateral and compressed action. In a ship at sea these forces are strikingly exemplified, and that under circumstances embarrassing as well to the practical builder as the man of science.

Remarks on the foregoing experiments.

Having determined the strength of iron plates when drawn in the direction of the fibre as well as across it, and having compared the results with experiments of a similar character on timber, it may be useful to offer a few general observations on the question now under consideration.

Dr. ROBINSON, in his article on the strength of materials*, when discussing the nature of a stretching force applied to materials, observes, "that in pulling a body asunder the force of cohesion is directly opposed with very little modification of its action; that all parts are equally stretched, and the strain in every transverse section is the same in every part of that section." From this it would appear, that a body of a homogeneous texture will have the cohesion of its parts equal, and since every part is equally stretched, it follows that the particles will be drawn to equal distances, and the forces thus exerted must be equal. Now if this were true, the application of an external force to a body might be increased to such an extent as not only to separate the parts furthest asunder, but ultimately to destroy the cohesion of all the particles *at once*, a circumstance under which instantaneous rupture would follow as a result. These views are however not borne out by facts, as the experiments of Mr. HODGKINSON on iron wire show that the same iron may be torn asunder many times in succession without impairing its strength†; and some recent experiments at the Royal Dockyard, Woolwich, clearly show, that an iron bar may be stretched until its transverse section is considerably reduced and ultimately broken without injury to its tensile strength. Nay, more, the same iron (so elongated), when again submitted to experiment, exhibited increased strength, and continued to increase, under certain limitations, beyond the bearing powers of the same bar in its original form‡. That all the parts of a body "subjected to a tensile strain are equally stretched" is therefore questionable. Bodies vary considerably in their powers of resistance, and exhibit peculiar properties of cohesion under the influence of forces calculated to tear them asunder. Fibrous substances, for instance, such as ropes and some kinds of timber having their fibres twisted, are enabled to resist tension under the influence of considerable elongation without impairing their ultimate strength.

* Encyclopædia Britannica.

† Manchester Memoirs, vol. v.

‡ I am indebted to Mr. THOMAS LOYD of the Admiralty for a series of interesting results on this subject. See Appendix.

Many of the fibres are stretched, but only to the extent of bringing the others to bear upon the load, which done, their united force constitutes the maximum of resistance to a tensile strain.

Other bodies of less ductility and more of a crystalline structure, such as cast iron, stone, glass, &c., seem to be subject to the same law. In these cases it seldom happens that the whole of the particles are brought into action at once, as much depends upon the conditions of the body, the unequal state of tension of its parts, and the strain which some of the particles must sustain before the others receive their due portion of the load. Should the non-resisting particles be within the limits of elongation of the other particles, the body will then have attained its maximum power of resistance; but in the event of rupture to any of the resisting particles, the cohesive force of the body is thereby reduced, and that to the extent of the injury sustained by the fractured parts.

"There are however," as Dr. ROBINSON truly observes, "immense varieties in the structure and composition of bodies which lead to important facts, and prove that the absolute cohesion of all bodies, whatever be their texture, is proportional to the areas of their sections." Undoubtedly this is the case in bodies having an uniform texture with straight fibres, and hence it follows that the absolute strength of a body, resisting a tensile strain, will be as the area of its section.

The peculiar nature of the material combining a crystalline as well as a fibrous structure has led to these observations. In some instances the specimens experimented upon exhibited an almost distinct fibrous texture, and in others a clearly developed crystalline structure*. At other times some of the specimens were of a mixed kind, with the crystalline and fibrous forms united; the fracture having a laminated appearance, with the crystalline parts closely bound on each side by layers of the fibrous structure. These varieties are probably produced in the manufacture, and may be easily effected either by the mode of "piling" the layers of bars which form the plate, or from the unequal temperature of the parts as they pass through the rolls. But whichever way they are produced, it is evident, from the experiments, that the fractures gave, in most cases, indications of an unequal and varied texture.

In the foregoing experiments, and also in those which follow, great attention was paid to the appearance of the fracture, in order to ascertain the structure of the plate, and to determine how far it could be depended upon in its application to the varied purposes for which it was intended.

These appearances are all shown in the drawings appended to the experiments, and to which I beg to refer.

* See the fractured parts of the different specimens, Plate LV.

PART II.

On the Strength of Iron Plates united by Rivets, and the best mode of Riveting.

The extensive and almost innumerable uses to which iron is applied, constitute one of the most important features in the improvements of civilized life. It contributes to the domestic comforts and commercial greatness of the country, and from its cheapness, strength and power of being moulded, rolled and forged into almost every shape, it is not only the strongest, but in many respects the most eligible material for the construction of vessels exposed to severe strain. Large vessels composed of iron plates, such as steam-boilers, cisterns, ships, &c., cannot however be formed upon the anvil or the rolling-mill. They are constructed of many pieces, and these pieces have to be joined together in such a manner as to ensure the requisite strength and effect all the requirements of sound construction. This operation is called riveting, and although practically understood, it has not, to my knowledge, on any previous occasion, received that attention which the importance of the subject demands.

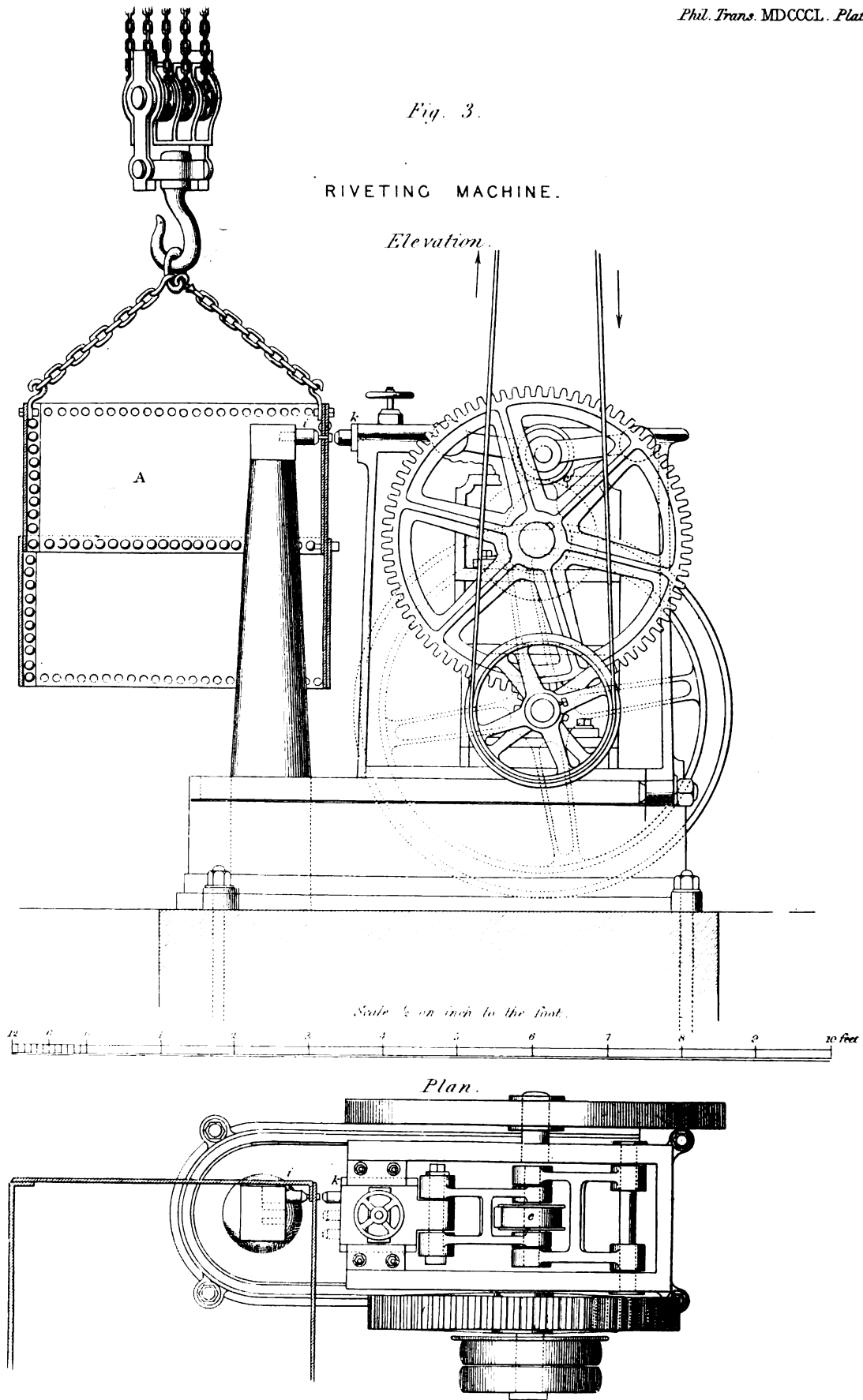
Up to the present time* nothing of consequence has been done to improve or enhance the value of this process. We possess no facts or experiments calculated to establish principles sufficient to guide our operations in effecting constructions of this kind, on which the lives of the public as well as the property of individuals depend. In fact, such has been our ignorance of the relative strength of plates and their riveted joints, that until the commencement of the present inquiry the subject was considered of scarcely sufficient importance to merit attention. Even now, it is by many assumed that a well-riveted joint is stronger than the plate itself, and a number of persons, judging from appearances alone, concur in that opinion. Now this is a great mistake, and although the double thickness of the joint indicates increased strength, it is nevertheless much weaker than the solid plate, a circumstance of some importance, as we hope to show in the following experiments.

It would probably be superfluous to offer any lengthened description of the principle upon which wrought-iron plates are united together; riveting is so familiar to every person in this country, that it might appear a work of supererogation to attempt it; and, assuming that the usual method of riveting by hammers to be generally known, we shall proceed to describe another method by machinery which effects the same object in considerably less time and at less cost, and completes the union of the plates with much greater perfection than could possibly be done by the hand. In hand-riveting it will be observed, that the tightness of the joint and the soundness of the work depends upon the skill and also upon the will of the workman, or those who undertake to form the joint and close the rivets. In the machine-riveting neither the will nor the hand of man has anything to do with it, the machine closes the joint and forms the rivet with an unerring precision, and in no instance can imperfect work be accomplished so long as the rivets are heated to the extent compressible by the machine.

* 1838, when these observations were written.

Fig. 3.

RIVETING MACHINE.



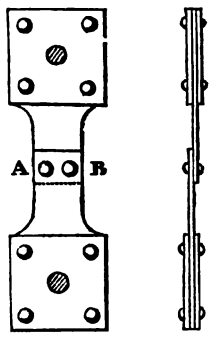
This property of unvarying soundness in the work, constitutes the superiority of the machine over the hand-riveting. The machine produces much sounder work, as the time occupied in the hand process allows the rivet to cool, and thus by destroying its ductility, the rivet is imperfectly closed, and hence follow the defects of leaky rivets and imperfect joints. It is evident that an instrument, such as the riveting-machine, having sufficient force to compress the rivet at once, or within an almost infinitely short period of time, must obviate, if not entirely remedy, these evils, as the force of compression being nearly instantaneous, the heads on both sides cannot be formed until the body of the rivet is squeezed tight into the hole; and in every case (even where the holes are not exactly straight) the compressed rivets are never loose, but fill the holes with the same degree of tightness as if placed directly opposite to each other. If, for example, we take a circular boiler, such as represented at A*, Plate LVI. fig. 3, and having all the perforations made and the plates attached to each other by temporary bolts and suspended over the machine in the position as shown at A, and having brought the holes in a line with the die marked *i*, *k*, the machine then is set to work, and by means of the cam or excentric raising the pulley of the elbow-joint C, the die *k* is advanced against the fixed die *i* in the wrought-iron stem, and the rivet is compressed into the required form with an increasing force as the die advances which gives the "nip," or greatest pressure, at the required time, namely, at the closing of the rivet.

From this description it will appear that a very limited portion of time is occupied in the process, and as twelve rivets can be inserted and finished by the machine in a minute, it follows, from the rapidity of the operation and the absence of hammering, that the ductility of the rivets is retained, and their subsequent contraction upon the plate renders the joint perfectly tight and the rivets sound in every respect. Under all the circumstances the machine-riveting is preferable to that executed by the hammer; it saves much time and labour, and that in proportion of 12 to 1, when compared to a long series of impacts applied by the hammer.

Having described the process of uniting wrought-iron plates by rivets, it may be of some importance to know the value of joints thus formed as regards their strength when compared with the plates themselves. To attain this object, and satisfactorily to determine their powers of resistance to a tensile strain, a great variety of joints were made, and having prepared the different specimens with the utmost care and attention, they were submitted to the test of experiment as follows:—

* The plan represents the machine in the act of riveting the corners of a square cistern or a locomotive fire-box.

TABLE VI. Strength of riveted Plates.—Yorkshire Iron*.

No. of exp.	Description of plates and mode of riveting.	Weight laid on in lbs.	Changes produced by weights.	Breaking weight in lbs.	Form of specimen and mode of fracture.	Remarks.
21.	Plates 22 inches thick, overlap joints, two rivets half-inch diameter, lap $1\frac{1}{2}$ inch, AB = 3 inches, riveted by the patent machine.			20,011	Fig. 4. 	Torn across at the rivet-holes. Rivet-holes torn out. Rivet-holes torn out.
22.	Same as last, area 44 in.			18,667		
23.				21,703		
				Mean 20,127		
24.	Plates as before, riveted by the hammer, the rivets half-inch diameter, being of the usual length, but rather shorter than those used for the machine.	14,839		16,115	Specimen same as before, fig. 4. 	Rivet-heads broke off and the plate torn across them in consequence. Rivet-heads cracked across and rivet-holes torn out.
25.		15,343	Plates bent.	16,099		
				Mean 16,107		
26.	Plates same as before and riveted by the hammer, with half-inch rivets, the rivets being a little longer and 2 inches lap	14,839	Plates bent nearly into a direct line with straining force. 	17,833	Specimen same as before, fig. 4. 	Here the rivets were the same length as the machine rivets, experiments 1, 2, 3, and were worked with great care on both sides. Both rivet-heads broken and the plate torn across them.
27.		14,839		20,131		
				Mean 18,982		
28.	Plates same as before, lap 2 inches, and the rivets the same as in the last experiment, but riveted by the machine	14,839	Plates bent into a direct line by the straining force. Joint apparently sound.	19,123	Specimen same as before, fig. 4. 	Both rivets cracked across, metal torn across the rivet-holes. Torn across at the rivet-holes, both rivets slightly cracked near the head.
29.		18,667		19,171		
				Mean 19,147		

The plates used in the foregoing experiments are of Yorkshire iron, the same as those employed in Tables I. and II. The specimens were prepared in the same manner and of the same thickness, but 1 inch wider at the joint. This was done in order to retain sufficient metal round the rivet-holes, making the breadth of the plate the same after the rivet-holes were punched out as that of the plates torn asunder in the preceding experiments. In all these experiments only two half-inch rivets were used in the breadth of the plate. The lap was however increased, after the three first experiments, from $1\frac{1}{2}$ to 2 inches, to give greater strength in the longitudinal line of the plate and to prevent the metal tearing in that direction. This precaution was

* The nature and appearance of the fractures of all the irons and their riveted joints are shown in Plate LVII.

TABLE VI.

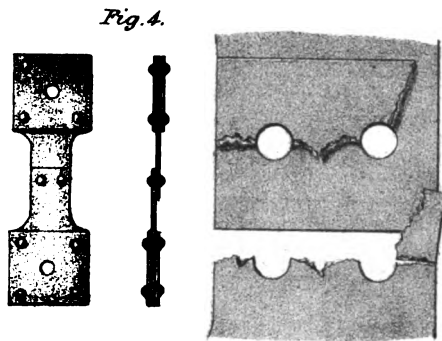


TABLE VII.

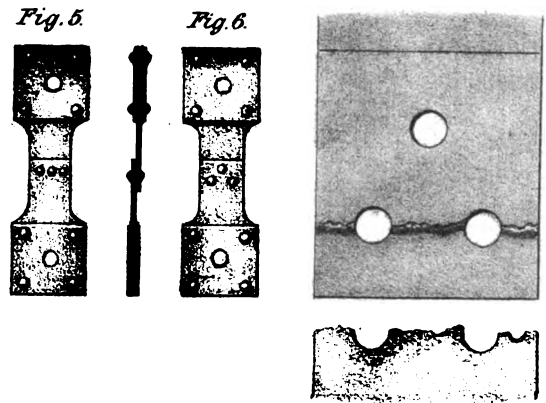


TABLE VIII.

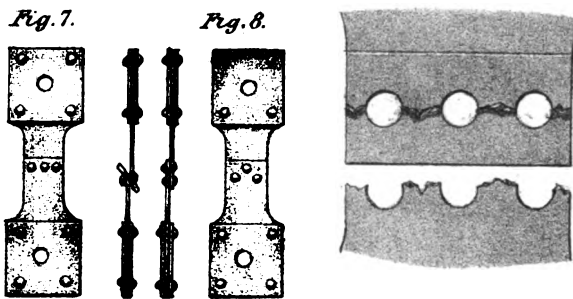


TABLE IX.

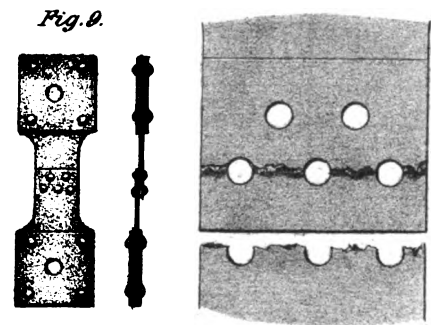


TABLE X.

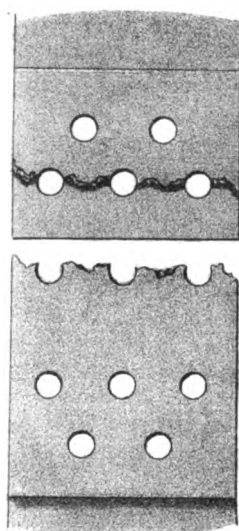


Fig. 10.

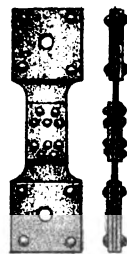


TABLE XI.

Fig. 11.

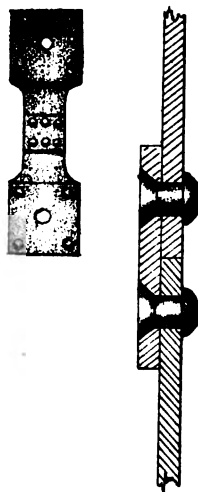


TABLE XII.

Fig. 12.

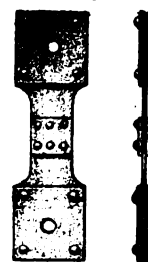
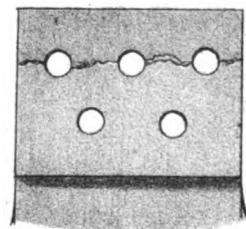
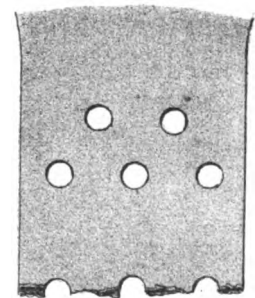
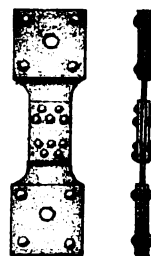


Fig. 12^a.



found necessary, as the metal gave indications of weakness in consequence of the lap being rather narrow. Another reason for enlarging the lap was a desire at the commencement to begin with the least possible quantity, and by direct experiment to ascertain the maximum distance which the plates should overlap each other in the joints, and to determine the strongest and best form of uniting them. To these points every attention was given, for the purpose of collecting the facts on which are founded the tabulated results on that part of the subject which treats of the comparative dimensions of rivets and extent of the lap in reference to the thickness of the plates. In this department of the inquiry will be found the depth of lap, diameter and length of rivets, and the distances of holes for nearly every description of joint; also the thickness of the plate, with a column of strengths as deduced from the experiments.

If we examine the nature of the fracture in the foregoing experiments, it will be found that the machine-riveting is superior to that done by the hammer; the mean of the three first experiments being to the mean of the fourth and fifth as 5 : 4. In the eighth and ninth the strengths are nearly the same.

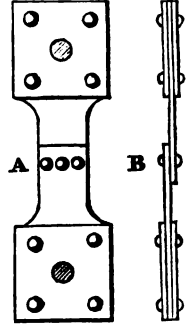
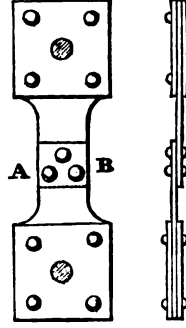
On comparing the strength of plates with their riveted joints, it will be necessary to examine the sectional areas taken in a line through the rivet-holes with the section of the plates themselves. It is perfectly obvious, that in perforating a line of holes along the edge of a plate, we must reduce its strength; and it is also clear, that the plate so perforated, will be to the plate itself nearly, as the areas of their respective sections, with a small deduction for the irregularities of the pressure of the rivets upon the plate; or in other words, the joint will be reduced in strength somewhat more than the ratio of its section, through that line, to the solid section of the plate. For example, suppose two plates, each 2 feet wide and three-eighths of an inch thick, to be riveted together with ten $\frac{3}{4}$ -inch rivets. It is evident that out of 2 feet, the length of the joint, the strength of the plates is reduced by perforation to the extent of $7\frac{1}{2}$ inches; and here the strength of the plates will be to that of the joint as 9 : 6·187, which is nearly the same as the respective areas of the solid plate, and that through the rivet-holes, namely, as 24 : 16·5. From these facts it is evident that the rivets cannot add to the strength of the plates, their object being to keep the two surfaces of the lap in contact, and being headed on both sides, the plates are brought into very close union by the contraction or cooling of the rivets after they are closed. It may be said that the pressure or adhesion of the two surfaces of the plates would add to the strength; but this is not found to be the case, to any great extent, as in almost every instance the experiments indicate the resistance to be in the ratio of their sectional areas, or nearly so.

If we take the ultimate strength of the Yorkshire plates in Tables I. and II., it will be found that the mean breaking weight of eight specimens, each with a sectional area of ·46 inch, is 26,168, and the strength of the single joint*, of the same description of plates with an area of ·44 inch, is 18,591; this reduced gives the ratio of the

* I use the term single joint to distinguish it from the double riveted joint, which will be treated of hereafter.

strength as 25,030 : 18,591, or as 1 : .742, the comparative strength of a single riveted plate of equal area through the line of the rivets. It will be observed that in this comparison the areas of the sections are nearly equal, and consequently there is a difference in strength between the solid part of the plate and that part where the perforations have been made of 32 per cent. The difference is considerable, but it probably arises from the narrowness of the specimen and the lateral strain induced by the position of the rivet, and the bending upwards of the end of the plates. From these facts I would infer that single riveting is weaker, and probably the loss of strength in this description of joint, including loss caused by the rivet-holes, is not less, under ordinary circumstances, than 40 per cent.

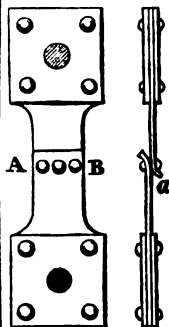
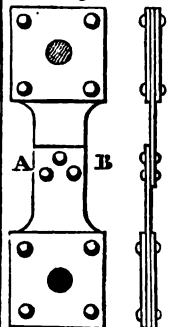
TABLE VII. Strength of riveted Plates.

No. of exp.	Description of plates and mode of riveting.	Weight laid on in lbs.	Changes produced by weight.	Breaking weight in lbs.	Form of specimen and mode of fracture.	Remarks.
30.	Plates 22 inches thick, with three rivets, each $\frac{3}{4}$ inch diameter, AB 3 inches, lap $1\frac{1}{4}$ inch, area through rivet-holes .4125	14,839	Bent into a straight line.	16,603	Fig. 5. 	The plates were sound, but two of the rivets were cut directly across. Rivets too weak.
31.	Plates the same as before, overlap joints differing from the last in having three rivets $\frac{3}{4}$ inch diameter, forming an isosceles triangle, AB 3 inches ...	18,667	Joint apparently sound.	22,699	Fig. 6. 	With the first weight the plates became bent, so as to be in a direct line with the straining force.
		20,683	Single rivet slightly opened.			
		22,027	The other two rivets quite tight.			
32.	Same as before	18,667	Separation at end of plate, single rivet slightly opened.			Tore across the two rivet-holes, in the direction AB.
		22,027				With 22,027 lbs. the single rivet seemed somewhat opened, but the other two seemed quite close.
		23,035	Slightly drawn at the rivets	23,371		Plate torn across at the single rivet and one of the double ones. Rivets sound in this and the preceding experiment.
				Mean 23,035		

In the first experiment the rivets (two in number) were evidently too weak, which caused them to shear directly across as if cut by a pair of scissors. In the next experiment the rivets were increased in number and size, which gave an excess of strength to the retaining power of the rivets and caused the plate to tear. If we take the mean of the experiments as respects the area of the rivets to that of the plates,

we find two half-inch rivets about the proportion, or the area of the rivets in the last experiments should have been $\cdot 4$ inches, which is nearly equal to the area of the plate through the rivet-holes*.

TABLE VIII. Strength of riveted Plates.

No. of exp.	Description of plates and mode of riveting.	Weight laid on in lbs.	Changes produced by weight.	Breaking weight in lbs.	Form of specimen and mode of fracture.	Remarks.
33.	Plates same as before, $\cdot 22$ inch thick, but wider, AB being $3\frac{1}{4}$ inches, with three rivets $\frac{1}{2}$ inch diameter, all in a line; lap $1\frac{1}{2}$ in.	18,667	Ends of plate much separated by bending.	19,675	Fig. 7. 	Though the ends of the plates were much separated, the light of a candle could not be seen through the line of the rivets. Plate torn across the rivet-holes.
34.	Thicker plates $\cdot 26$ in. thick, in other respects the same as in experiments 31, 32, Table VII.; lap 3 inches; rivets $\frac{1}{2}$ inch diameter	18,667	Ends of plate separated, joints apparently good.	23,707	Fig. 8. 	Tore across the two rivet-holes.
35.	Plates the same as the last.....	22,699 21,019	One plate much bent; joint apparently good. One end separated so far as to exhibit the single rivet.	27,067	Mean 25,387	Tore across the two rivet-holes, where the breadth was $3\frac{1}{2}$ inches.

Here the section of the rivets is to that of the plates, through the line of the rivets, in the ratio of $\cdot 58$ to $\cdot 44$; had they been equal, it is probable that fracture would have taken place as soon by the rivets shearing as through the plates.

During the whole of the experiments on single riveted joints, it was observed that the ends of the plates under strain curled upwards on each side; and produced a diagonal strain upon the plates, which materially reduced the strength of the joint, as shown at *a* fig. 7.

This position gave an oblique direction to the forces, and caused the plate to break in some degree transversely through the rivet-holes. In order to obviate this defect, and prevent as much as possible a transverse strain upon the plates through the

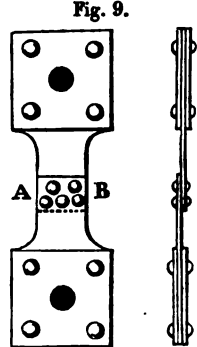
* Subsequent experiments made for ascertaining the strength of rivets (*vide* experiments on the strength of rivets for the Britannia and Conway Tubular Bridges) fully corroborate these views, namely, that riveted joints exposed to a tensile strain are directly, or nearly so, as their respective areas, or in other words, the collective areas of the rivets are equal to the sectional area of the plate taken through the line of the rivets.

points in contact, the lap was increased and a third rivet introduced to keep down the ends of the plates.

The sketches in the 31st experiment, Table VII., and those in the 34th and 35th experiment, Table VIII., represent the form of joint, and the methods adopted for securing the plates in the direct line of the strain.

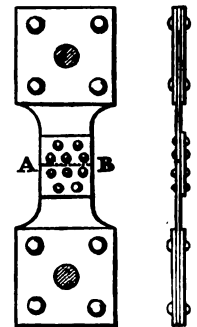
On comparing the breaking weights, it will be seen that the increased lap, with a rivet to keep down and retain the ends of the plates, gives a considerable accession of strength, and exhibits several important facts in connection with the construction of vessels exposed to severe pressure. But this becomes more apparent in the forthcoming experiments on the double-riveted joints.

TABLE IX. Strength of riveted Plates.

No. of exp.	Description of plates and mode of riveting.	Weight laid on in lbs.	Changes produced by weight.	Breaking weight in lbs.	Form of specimen and mode of fracture.	Remarks.
36.	Double-riveted plates $\frac{1}{2}$ inch thick; overlap joints riveted with five rivets of $\frac{3}{4}$ inch diameter each; lap 2 inches; AB=3 inches in breadth	16,115	Plates bent in a right line between the points of tension.	24,043 } Mean 23,531 21,019	Fig. 9. 	Torn right across at the three rivet-holes, all the rivets being sound after fracture.
37.	Plates the same as before, except that AB=2 $\frac{1}{2}$ inches in breadth	21,715	Little or no alteration.			
		18,667	Plates bent into right line, as before.			Broke as before; rivets all sound.

In these experiments, as in those in the preceding Table, the area of the rivets is in excess, and hence follows rupture through the plates.

TABLE X. Strength of riveted Plates, &c.

No. of exp.	Description of plates and mode of riveting.	Weight laid on in lbs.	Changes produced by weight.	Breaking weight in lbs.	Form of specimen and mode of fracture.	Remarks.
38.	"Jump Joints." Plates the same as before, both riveted to an extra plate of the same thickness laid on one side of them; lap of extra plate over each end 2 inches; each plate riveted by five rivets $\frac{3}{4}$ inch diameter; AB=3 $\frac{1}{2}$ inches.	14,839	Bent into straight line.	23,371 } Mean 23,707 24,043	Fig. 10. 	Tore across the rivet-holes.
		19,627	All sound.			Rivets sound after fracture.
		21,691	Rivets sound; plates much bent.			
39.	Same as above.....	18,667	Bent nearly into a straight line.	24,043		
		22,699	Rivets sound; joint apparently good.			Tore across as before; one rivet-head crooked.

The same observations will apply to these experiments as to the last; the area of the rivets is in excess of that of the plates.

The system of double riveting exhibits several remarkable properties as regards strength, and the plates appear to retain their position under strain much better than single-riveted joints. These circumstances have induced a comparison of the results of the preceding experiments with those contained in Tables VII., VIII., IX. and X. The experiments in Tables VII. and VIII. give indications of increased strength by a slight enlargement of the lap and the introduction of a single rivet to keep down the end of the plate. In those experiments it was found that the additional rivet gave an increase of 26 per cent. over those obtained from the single rivets; a circumstance which suggested a further extension of the experiments, accompanied with a minute investigation of the parts, in order to ascertain their relative strengths, and the strongest form of joint.

The mean breaking weights of equal sections of single-riveted joints, as given in Table VI. and taken from nine experiments, are respectively as follows:—

lbs.	
20,127	} Mean . . 18,590
16,107	
18,982	
19,147	

giving a mean of 18,590 lbs. for the strength of single-riveted joints. Now in the second and third experiments, Table VII., with the rivets inserted in the shape of an isosceles triangle (which in fact is double riveting), and of equal sections to the specimens in Table VI., the mean breaking weight is 23,035, which gives an excess of 4445, or a ratio of 10 : 8 in favour of the experiments recorded in Table VII.

In the experiments (Table X.), the area of the section, taken through the line of the rivet-holes, is .44 inch, or precisely equal to the section of the specimens experimented upon in Table VI., in which the mean breaking weight is 18,590 lbs. In these experiments the breaking weight is 23,707 lbs., which is rather more than that in Table IX., where the material had a smaller section, but having its dimensions exactly corresponding with the proportions given above. It therefore follows that in plates jointed with single rivets, the ratio of the strength of the single rivets is to that of the double-riveted joints as 8 to 10, the latter being one-fourth stronger.

It has been ascertained that it required a weight of 23,707 lbs. to tear asunder double-riveted plates, $3\frac{1}{8}$ inches wide and .22 inches thick, with a flush joint, having a plate on the back and held together by five $\frac{3}{8}$ -inch rivets on each side; the quantity of metal between the holes, in a direct line across the plate, being $.2 \times .22 = .44$ inch, which is the same transverse section as those operated upon in the first Table.

Now if we take the mean breaking weights of the riveted joints in Tables X. and VI. and compare them with the section of the plate itself as given in Table I., the areas being the same, we have for the tensile strength of plates—

	Section of iron torn asunder.	lbs.
In Table I., solid plate	.44	25,400
In Table X., double-riveted joints	.44	23,707
In Table VI., single-riveted joints	.44	18,590

Assuming therefore the strength of the plates to be 1000, we have—

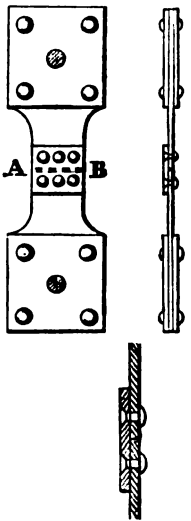
For the strength of plates of equal sections	1000
For the double-riveted joints	933
For the single-riveted joints	731

We may safely assume these ratios as the comparative values of jointed plates of equal sections when acted upon by a force calculated to tear them asunder.

The correct value of the plates, computed from a sectional area taken through the rivet-holes, will therefore be to their riveted joints as 100, 93 and 73, or in round numbers as 10, 9 and 7.

In addition to a loss of nearly one-tenth in the double-riveted joints, and three-tenths in the single ones, it will be observed that the strength of the plates is still further reduced by the quantity of iron punched out for the rivets.

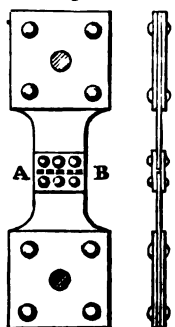
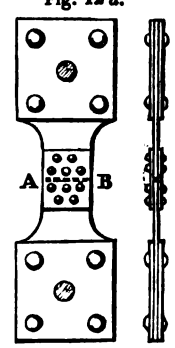
TABLE XI. Single riveted Plates.

No. of exp.	Description of plates and mode of riveting.	Weight laid on in lbs.	Changes produced by weight.	Breaking weight in lbs.	Form of specimen and mode of fracture.	Remarks.
40.	Plates same as before, $\frac{1}{2}$ inch thick, with overlap joint and double rivets; countersunk on one side; AB = $3\frac{1}{8}$ inches; five rivets, each $\frac{5}{8}$ diameter.	19,675	Plates bent in a right line; doubtful whether the joint would hold water	23,707	Fig. 11. 	By the word countersunk is understood a conical recess on one side of the plate to receive the head of the rivet, in order that it might not project beyond the surface of the plate.
41.	Plates the same strength, but different from the last in having only $\frac{3}{8}$ -inch rivets all in a line; AB = $3\frac{1}{8}$ ins.	14,839	Plates bent into a right line with the fixing.....	16,351		Tore across the three rivet-holes.
42.	Same as last, except in not having the rivet-holes countersunk; lap $1\frac{1}{2}$ inch; AB = $3\frac{1}{8}$ inches	14,839	Joint sound.....	16,351		In an unsuccessful experiment made before this upon plates precisely the same, and riveted in the same manner, they were torn across the rivet-holes in attempting to lay on 18,667 lbs. Plates tore across the rivet-holes.
						All the rivets on one side were cut in two in the middle, and the plates left sound.

The results in the two last experiments, in the above Table, are identical as to strength. In the first, with the countersunk rivets, the plates were torn asunder, and in the latter the rivets appear the weakest, owing to the increased sectional area of the plates, which in the preceding experiment was reduced by countersinking the rivets.

In both experiments it will be observed that the strengths of the rivets are proportional to the strengths of the plates, their powers of resistance being equal, or nearly so. In forty-one experiments the sectional area of the rivets was to that of the plates as '340 to '347, that is, the sections were nearly equal; and in forty-two experiments as '34 to '44, which accounts for the nature of the fracture in both cases.

TABLE XII. Strength of riveted Plates.

No. of exp.	Description of plates and mode of riveting.	Weight laid on in lbs.	Changes produced by weight.	Breaking weight in lbs.	Form of specimen and mode of fracture.	Remarks.
43.	Plates the same as before, their edges brought into contact, and each plate riveted by three rivets $\frac{3}{4}$ of an inch diameter, to a plate on each side of the joint, each external plate being half the thickness of the internal, or a little thicker; AB = $3\frac{1}{2}$ ins.	19,879	Sound; no alteration.		Fig. 12. 	Both side plates were torn across, and two of the rivets cut off. The sum of the thickness of the side plates was $\cdot 24$ inch, the middle plates being $\cdot 22$ inch thick.
44.	Plate same as last	24,715		25,723		The middle plates were left sound.
				21,355		Second experiment broken as before, the two outside plates torn off; all the rest sound.
45.	Same as the last experiment, having thicker plates outside, each being $\cdot 15$ inch thick.	23,371	Joint good	24,715	Fig. 13 a. 	Middle plate torn straight across the rivet-holes. All the rivets and both plates left sound.
46.	Differing from the last only in having five rivets to each plate in double rows instead of three rivets $\frac{3}{4}$ diameter; AB = $3\frac{1}{2}$ inches...	23,371	Joint sound			Both outside plates torn across at the three rivets.
47.	Same as the last	25,387		26,059		Outer plate sound; torn across the two rivet-holes. Rivets sound; inner plate only torn.
		23,371	Joint sound			
		24,715	Slightly altered; joint good	27,403		
				Mean 26,731		

When the comparative merits of plates and their riveted joints were under consideration, it appeared desirable to repeat several of the experiments, particularly those which seemed to throw light upon their relative powers of resistance. I considered these experiments to be of importance, as they increased our knowledge, as respects the strength of the material, and also its properties in combination.

In ship-building these objects are of some value, as any reduction in the powers or parts of a vessel by imperfect construction, or misapplied material, might lead to serious error and even great risk to the safety of the ship.

Since the first use of iron for these objects, it has been the practice to countersink the heads of the rivets in order to present a smooth surface for the passage of the vessel through the water. This practice is in general use at my works at Millwall, and I believe the same methods are pursued at the establishment of Messrs. JOHN LAIRD and Co., and others in different parts of the country. The introduction of this system of riveting caused a further extension of the experiments, in order to elucidate the various forms of joints given in the preceding Tables, and further to investigate the strength of the joint with a plate riveted on each side, which appears to be the strongest and best calculated to resist a tensile strain. This description of joint is seldom used in ship-building, but in order to render the experiments as perfect as

possible, it will be necessary to consider it in this paper with others of equal importance and probably of more general use.

The system of countersinking the rivets is only used when smooth surfaces are required; under other circumstances their introduction would not be desirable, as they do not add to the strength of the joint, but to a certain extent reduce it. This reduction is not observable in the experiments, but the simple fact of sinking the head of the rivet into the plate and cutting out a greater portion of metal, must of necessity lessen its strength, and render it weaker than the plain joint with raised heads. This must appear evident from the fact of the sectional area of the plate being diminished, and the consequent reduction of the heads of the rivets, which in this state are less able to sustain the effects of an oblique or transverse strain.

It is, however, satisfactory to observe that countersinking the heads of the rivets does not seriously injure the joint in its powers of resistance to a direct tensile force; but the rivets are liable to start when exposed to collisions or a strong impinging force, such as the sides of ships are frequently doomed to encounter.

On referring to experiments (Table XI.), the same results as to strength are obtained with the countersunk rivets as those with rounded heads; they are rather under the mean of the former experiments, but not more than is easily accounted for by the reduced section of the countersunk plates.

The joint with plates, riveted on each side, is seldom used, a circumstance which probably arises from its greater complexity of form and the danger which a treble thickness of plate would be subject to if used in boilers or vessels exposed to the action of intense heat. It is also inadmissible in ship-building, as the smooth surface requires to be maintained, and the greatest care observed in the formation of the outer sheathing to lessen the resistance of every part of the hull immersed in the water. In other respects the double-riveted plate is a strong joint, and in every case, where great strength is required, it may be used with perfect safety.

It will be unnecessary to go through a further comparison of the experiments, as sufficient data have already been furnished to enable us to calculate the force per square inch, and to resolve the whole into a general summary exhibiting the relative strengths—1st, of the plates; 2ndly, of the single- and double-riveted joints; and lastly, the ratio of the strengths as deduced from the whole series of experiments.

General summary of Results as obtained from the foregoing Experiments.

	Cohesive strength of plates: Breaking weight in lbs. per square inch.	Strength of single-riveted joints of equal section to the plates, taken through the line of rivets. Breaking weight in lbs. per square inch.	Strength of double-riveted joints of equal section to the plates, taken through the line of rivets. Breaking weight in lbs. per square inch.
	57,724	43,743	52,352
	61,579	36,606	48,821
	58,322	43,141	58,286
	50,983	43,515	54,594
	51,130	40,249	53,879
	49,281	44,715	53,879
	43,805	37,161	
	47,062		
Mean ...	52,486	41,590	53,635

The relative strengths will therefore be,—

For the plate 1000
 Double-riveted joint 1021
 Single-riveted joint 791

From the above, it will be seen that the single-riveted joints have lost one-fifth of the actual strength of the plates, whilst the double-riveted have retained their resisting powers unimpaired. These are important and convincing proofs of the superior value of the double joint, and in all cases where strength is required this description of joint should never be omitted.

On referring to the experiments contained in the separate tables, there will be found a striking coincidence in the facts tending to establish the principle of double riveting as superior in every respect to the general practice now in use of the single rivets. It appears, when plates are riveted in this manner, that the strength of the joints is to the strength of the plates of equal sections of metal as the numbers,—

1000 : 1021 and 791*.

In a former analysis it was 1000 : 933 and 731,

which gives us a mean of 1000 : 977 and 761

which in practice we may safely assume as the correct value of each. Exclusive of this difference, we must however deduct 30 per cent. for the loss of metal actually punched out for the reception of the rivets, and the absolute strength of the plates will then be, to that of the riveted joints, as the numbers 100, 68 and 46. In some cases, where the rivets are wider apart, the loss sustained is however not so great;

* The cause of the increase of strength in the double-riveted plates may be attributed to the riveted specimens being made from the best iron, whereas the mean strength of the plates is taken from all the irons experimented upon, some of inferior quality, which will account for the high value of the double-riveted joint. In ordinary cases and in practice it will therefore be safer to take the mean of the whole, viz.—

Strength of plates..... 100
 Strength of double-riveting..... 97
 And of single-riveting 76

but in boilers and similar vessels, where the rivets require to be close to each other, the edges of the plates are weakened to that extent. In this estimate we must however take into consideration the circumstances under which the results were obtained, as only two or three rivets came within the reach of experiment: and again, looking at the increase of strength which might be gained by having a greater number of rivets in combination, and the adhesion of the two surfaces of the plates in contact, which in the compressed rivets by machine is considerable, we may fairly assume the following relative strengths as the value of plates with their riveted joints:—

Taking the strength of the plate at 100

The strength of the double-riveted joint would then be . . . 70

And the strength of the single-riveted joint 56

These proportions may therefore in practice be safely taken as nearly the standard value of joints, such as used in vessels where they are required to be steam- or water-tight, and subjected to pressure varying from 10 to 100 lbs. upon the square inch.

Since the above was written, I have ascertained, on a recent visit to Bristol, that the large steam-ship* now building there is double-riveted, the plates being three-fourths of an inch thick over the bottom and bilge, and five-eighths thick up to the water-line. These plates are joined together with double rivets of 1 inch diameter, and inserted at distances of 3 inches apart. The proportions appear to be good; and conceiving the workmanship to be equally so, I should infer that this fine vessel would fairly establish the principle, that iron, in all the ramifications of ship-building, is an article of paramount importance to the war as well as to the mercantile navy.

In the pursuit of the foregoing inquiry, I was naturally led to the consideration of the best proportions and best forms of riveting plates together. I investigated this subject with great care, and from my own personal knowledge and that of others, I have collected a number of practical facts, such as long experience alone could furnish. From these data I have been enabled to complete the following Table, which for practical use I have found highly valuable in proportioning the distances and strength of rivets in joints requiring to be steam- or water-tight.

Table exhibiting the strongest forms and best proportions of riveted joints as deduced from the experiments and actual practice.

Thickness of plates in inches.	Diameter of rivets in inches.	Length of rivets from the head in inches.	Distance of rivets from centre to centre in inches.	Quantity of lap in single joints in inches.	Quantity of lap in double-riveted joints in inches.
$\cdot 19 = \frac{3}{16}$ $\cdot 25 = \frac{1}{4}$ $\cdot 31 = \frac{5}{16}$ $\cdot 38 = \frac{3}{8}$ $\cdot 50 = \frac{1}{2}$ $\cdot 63 = \frac{5}{8}$ $\cdot 75 = \frac{3}{4}$	$\cdot 38$ $\cdot 50$ $\cdot 63$ $\cdot 75$ $\cdot 81$ $\cdot 94$ $1\cdot 13$	$\cdot 88$ $1\cdot 13$ $1\cdot 38$ $1\cdot 63$ $2\cdot 25$ $2\cdot 75$ $3\cdot 25$	$1\cdot 25$ $1\cdot 50$ $1\cdot 63$ $1\cdot 75$ $2\cdot 00$ $2\cdot 50$ $3\cdot 00$	$1\cdot 25$ $1\cdot 50$ $1\cdot 88$ $2\cdot 00$ $2\cdot 25$ $2\cdot 75$ $3\cdot 25$	For the double-riveted joint, add two-thirds of the depth of the single lap.

* The Great Britain steam-ship.

The figures 2, 1·5, 4·5, 6, 5, &c. in the preceding Table are multipliers for the diameter, length and distance of rivets, also for the quantity of lap allowed for the single and double joints. These multipliers may be considered as proportionals of the thicknesses of the plates to the diameter, length, distance of rivets, &c. For example, suppose we take three-eighth plates and required the proportionate parts of the strongest form of joint, it will be—

$$·375 \times 2 = ·750 \text{ diameter of rivet, } \frac{3}{4} \text{ inch.}$$

$$·375 \times 4\frac{1}{2} = 1·688 \text{ length of rivet, } 1\frac{3}{4} \text{ inch.}$$

$$·375 \times 5 = 1·875 \text{ distance between rivets, } 1\frac{3}{4} \text{ inch.}$$

$$·375 \times 5\frac{1}{2} = 2·063 \text{ quantity of lap, 2 inches.}$$

$$·375 \times 5\frac{1}{2} = 3·438 \text{ quantity of lap for double joints, } 3\frac{1}{2} \text{ inches.}$$

·75, 1·68, 1·87, 2·06 and 3·43 are therefore the proportionate quantities necessary to form the strongest steam- or water-tight joints on plates three-eighths of an inch thick.

In the preceding pages I have endeavoured to investigate almost every circumstance having a practical bearing on the question of the strength of rolled plates, and the best methods of uniting them together. In conclusion, I would venture a few remarks on the value and judicious use of this material, in its adaptation to ship-building, and other purposes to which it may be successfully applied. It is not my intention to enter into the question as to whether wood or iron be the preferable material, as a number of circumstances, such as cost, durability, &c., must be considered in order to form a correct decision.

I would however observe, that in ship-building alone, it appears from the facts already recorded, that iron is very superior in its powers of resistance to strain; it is highly ductile in its character, and easily moulded into any required form without impairing its strength. It is also stronger in combination than timber, arising from the nature of the construction, and the materials composing the iron ship become a homogeneous mass when united together, forming as it were a solid, without joints, and presenting as a whole the most formidable powers of resistance*. These are some of the properties which appear to distinguish iron from other materials, and which give it an ascendancy of combined action, which cannot be obtained in the union of timber however ingeniously contrived. It moreover possesses the property of lightness along with strength; in fact, its buoyancy, strength and durability constitute the elements of its utility in the innumerable cases to which it may be applied. In ship-building it possesses other advantages over timber. Its hull is free from the

* Since the above was written we have had many examples of the enormous strength of iron ships, and amongst others we may instance an iron vessel which took the ground with nearly one-half of her length at the stern hanging over a shelf for a whole tide; another, the Vanguard iron steamer, which for several hours (under the action of a heavy surf) was beating upon sharp shelving rocks without going to pieces; and lastly, the Great Britain steam-ship, which was stranded in Dundrum Bay, and resisted the force of the winter storms for many months.

risk of fire; and in case of shipwreck, either on rocks or sand-banks, it will resist the heaviest sea, endure the severest concussion, and with proper attention to the construction, it may be the means of saving the lives of all on board. It moreover has the advantage of bulkheads, which, made perfectly water-tight, not only strengthen the vessel, but give greater security to it, and by a judicious arrangement in the divisions will float the ship under the adverse circumstance of a leak occurring in any one of the compartments. These are the qualities and powers of the iron ship; and I trust the present research into the strength and proportions of the material of which it is composed, will not only give increased confidence in its security, but will lead to an extension of its application in every branch of marine and mechanical architecture.

PART III.

Resistance of Wrought-iron Plates to Pressure by a Blunt Instrument at right angles to the surface of the Plate.

Irrespective of the experiments made to determine the strength of wrought-iron plates and the relative strength of the joints by which they are united, the investigation would be incomplete if we omitted another inquiry of equal importance, namely, the resistance offered by plates to a crushing force, such as exhibited in the injuries received by vessels when stranded on rocks or taking the ground in harbours where the surfaces are uneven.

Almost every person connected with nautical affairs is acquainted with the nature of the injuries received by timber-built vessels when placed in circumstances affecting their stability, or when resting on hard and unequal ground, such as frequently occurs in tidal harbours at low water. Such a position is attended with danger under every circumstance; and in order to determine the relative values of the two materials, wood and iron, it was considered desirable to institute a similar class of experiments on both, and thus to afford the means of comparison between them. English oak, as the strongest and best material used for the construction of first class vessels, was selected for this purpose, and the results obtained from both are given, under circumstances as nearly similar as the nature of the experiment would admit. They are as follows.

In each of the experiments the plate was fastened upon a frame of cast iron, 1 foot square inside and 1 foot 6 inches outside, its breadth being 3 inches and thickness half an inch. The sides of the plates, when hot, were twisted round the frame, to which they were firmly bolted. The contraction, by cooling, caused it to be very tight, and the force to burst it was applied in the centre. This was done in order that the force might in some degree resemble that from a stone or other body with a blunt end pressing against the side or bottom of a vessel: a bolt of iron, terminating in a hemisphere 3 inches in diameter, had thus its rounded end pressed perpendicularly to the plate in the middle. The results are given in the following Tables.

Beam before the Experiment rib downwards
Weight of Beam 64 lbs



Beam after the Experiment



Beam, showing the flange rib downwards

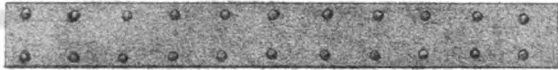


TABLE XVI

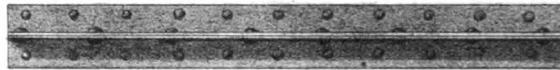
Fig. 17. Beam before the Experiment rib upwards
Weight of Beam 55 lbs



Beam after the Experiment



Plan of beam before the Experiment



Beam after the Experiment



Section.

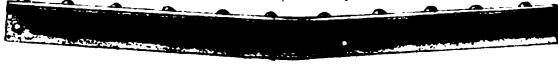


TABLE XVII

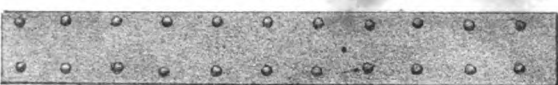
Fig. 18. Beam before the Experiment rib downwards
Weight of Beam 82½ lbs



Beam after the experiment



Beam with the flange downwards



Section.



Beam before the Experiment rib upwards
Weight of Beam 85 lbs



Beam after the Experiment



Plan of beam before the Experiment

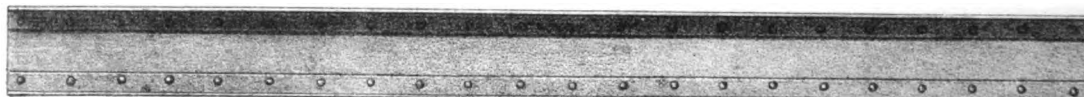


Beam after the Experiment



TABLE XX

Fig. 19. Side view of Beam before the Experiment
Weight of Beam 161½ lbs



Plan

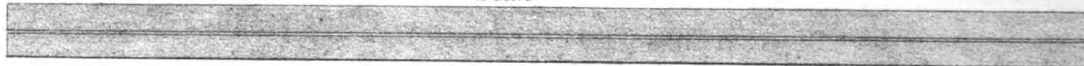
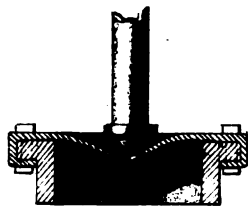


TABLE XIII



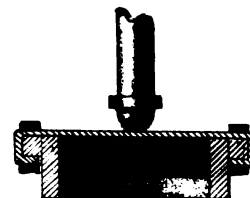
Edge view of the fracture



Plan



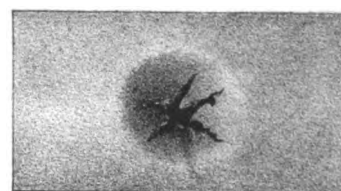
½ inch thick



Edge view of the fracture



Plan



½ inch thick

Fig. 13.

Fig. 14.

TABLE XIII. Experiments to determine the Resistance of Plates of Wrought Iron to a force tending to burst them.

No. of exp.	Description of plates.	Weight laid on in lbs.	Permanent indentation of plate.	Remarks.
1.	Plate of the best Staffordshire iron $\frac{1}{4}$ inch thick.	8,617 9,893 11,169 12,445 13,789	inch. ·3 ·35 ·5 ·6 ·7	Plate not cracked. Plate not cracked. Crack on convex side 8 inches long. Crack on convex side 9 inches long; not opened on concave side. Hole through the plate about $1\frac{1}{2}$ inch long, and $\frac{1}{2}$ inch wide.
2.	Plate of the same iron and the same thickness.	9,893 11,169 12,445 13,789 16,477 17,821 19,769	·25 ·34 ·4 ·47 ·6 ·65	Double crack on convex side 1 inch long. Double crack increased. Form of crack on convex side ($\frac{1}{2}$ inch wide). Not cracked through. Cracked through.
3.	Plate of the same iron $\frac{1}{2}$ inch thick.	18,523 21,075 22,787 25,923 29,059 32,195 35,331 36,899 37,519	 ·33 ·45 ·60 ·75 ·80 ·97 1·10	No crack. Incipient crack on convex side. Crack above-mentioned 4 inches long, forming a cross. Crack above, 6 inches long. Crack above, $\frac{1}{2}$ inch wide. No crack on concave side. Plate cracked through.
4.	Plate same as the last.	21,219 21,985 27,708 31,796 33,431 35,066 36,701 37,928	 ·35 ·47 ·7 ·75 ·83 ·97	No crack. Slight crack on convex side. Form of crack on convex side. Form of crack increased. Form of crack 4 inches deep. Not cracked through. Cracked through.

In Plate LVIII. figs. 13 and 14, will be found representations of the fractures of the plates experimented upon in this Table.

From the above we obtain the strength of plates to resist rupture from pressure from a blunt body, or a ball 3 inches diameter.

	lbs.	Mean.
In experiment 1, a plate one-fourth of an inch thick was burst by	13,789	} 16,779
In experiment 2, a plate one-fourth of an inch thick was burst by	19,769	
In experiment 3, a plate half an inch thick was burst by	37,519	} 37,723
In experiment 4, a plate half an inch thick was burst by	37,928	

Here the strengths are as the depths, a half-inch plate requiring double the weight to produce fracture that had previously burst the quarter of an inch plate. In the succeeding experiments on oak timber, the powers of resistance follow the ratio of the squares of the depth, so that a wrought-iron plate of only one-quarter of an inch thick is able to resist a force equal to that required in the rupture of a 3-inch plank.

The experiments were made upon good English oak, of different thicknesses, and of the same width as the iron plates. The specimens were laid upon solid planks, 12 inches asunder, and by the same apparatus the rounded end of the 3-inch pin was forced through them as follows:—

Resistance of planks of timber to the entrance of a ball, 3 inches diameter, the planks being laid upon props 12 inches asunder ; the object of the experiments being to burst them by pressing a pin, terminated by a hemispherical end, 3 inches diameter, through the centre of the plank, as was done with the plates of iron.

TABLE XIV.

No. of exp.	Description of plank.	Weight laid on.	Remarks.
1.	English oak, very dry and good, $11\frac{1}{2}$ inches broad, and $2\frac{1}{4}$ inches deep.	lbs. 16,115 17,235	Indentation from hemisphere $\frac{1}{4}$ inch deep; wood otherwise uninjured. Hole through the middle, 3 inches diameter nearly broke out, all the rest remaining sound.
2.	English oak, rather green, 8 inches broad, 3 inches deep.	18,941	It bore 18,941 lbs. about ten minutes, and then exploded with violence, dividing into three parts, the middle one on which the pin rested being about an inch thick at the top, and $\frac{1}{2}$ an inch at the bottom. With a ton less weight there was a crack under the plank in the centre, and an indentation by the pin $\frac{1}{4}$ inch deep on the upper side. Sap was driven out from the ends on the side nearest to the heart.
3.	English oak plank, and dimensions same as in last experiment.	12,445 16,925	Sap driven out as in last experiment; plank without crack; indentation by the pressure about $\frac{1}{4}$ inch. The plank split with bearing the pressure about ten minutes.
4.	English oak from same plank as in experiment 2 and 3; breadth 8 inches, depth $1\frac{1}{4}$ inch.	4,532	The plank broke by splitting.
5.	English oak from same plank and same size as in the last experiment.	4,280	Broke by splitting diagonally.

Taking the results of the four last experiments, which were on pieces from the same plank, we obtain—

	lbs.	Mean.
Strength from planks 3 inches thick	18,941	} 17,933
Strength from planks 3 inches thick	16,925	
Strength from planks $1\frac{1}{2}$ inch thick	4,532	} 4,406
Strength from planks $1\frac{1}{2}$ inch thick	4,280	

Here the strength to resist crushing follows the ratio of the square of the depth, as is found to be the case in the transverse fracture of rectangular bodies of constant breadth and span.

If we compare the foregoing results with the experiments performed by Mr. HODGKINSON on timber, it will be found that the strength of dry English oak to resist a crushing force is 4.24 tons to the square inch, whereas wrought iron, according to RONDELET, requires a pressure of about 31 tons per square inch, and with this weight it is reduced about one-sixteenth of its length. The resistance of wrought iron to a crushing force is therefore about seven and a quarter times greater than that of oak : and according to the experiments in the preceding Table, it appears that the resistance of wrought-iron plates to a force calculated to burst them, follows a different law to that of oak, the resistance of the former being directly as the depth and of the latter as the square of the depth. Reasoning from these facts, it may be interesting to know that in the use of timber, such as the oak sheathing of ships, the strength to ex-

ternal pressure increases in the ratio of the squares of its thickness; and, where great strength is required, it will be necessary, in the construction of vessels, to consider the nature of the service and the required thickness of the planks.

The same remarks will apply to vessels constructed of iron, computed from the formula deduced from the experiments. In a table of experimental results by Mr. HODGKINSON we have the mean force per square inch required for crushing timber of different kinds; and assuming RONDELET's experiments, which give 70,000 lbs. as the resistance per square inch of wrought iron, to be correct, we then have as the ratio of their respective powers of resistance as follows:—

TABLE XV.

Specific gravities.	Description of timber used.	Resistance per square inch.	Resistance of wrought iron per square inch.	Ratio, the wood representing unity.
		lbs.	lbs.	
7·700	Wrought iron		70,000	
0·560	Yellow pine	5375	70,000	1 : 13·02
0·540	Cedar	5674	70,000	1 : 12·33
0·580	Red deal	5748	70,000	1 : 12·16
0·640	Birch	6402	70,000	1 : 10·93
0·660	Sycamore.....	7082	70,000	1 : 9·88
0·753	Spanish mahogany ...	8198	70,000	1 : 8·53
0·780	Ash	8683	70,000	1 : 8·06
0·700	Dry English oak	9509	70,000	1 : 7·36
0·980	Box	9771	70,000	1 : 7·16

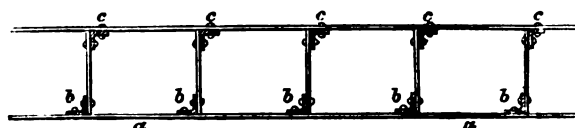
In addition to the relative resisting forces of the different kinds of timber above enumerated, will be found the specific gravities of each, which enables the reader to determine the comparative weights as well as strength of the different kinds of wood.

PART IV.

In the preceding researches I have endeavoured to determine the value of iron chiefly in reference to its application for the purposes of ship-building. It now only remains to determine the best form and condition of another part of the structure, namely, the frames and ribs of vessels, also composed of iron. Some of the forms experimented upon indicate weakness, but certain modifications which have since been introduced, have given increased support to the bilge and sides of the ship, and greater powers of resistance to the outer sheathing. The beam shown at fig. 19, Plate LVIII., is probably one of the strongest and most suitable for the support of the decks, but it is inadmissible as a frame for receiving the exterior sheathing plates. These frames are generally formed of a plate with angle-irons along the edges on both sides, of which the annexed sketch are sections. *a, a, &c.*

represents a portion of the outside plates; *b, b* the angle-iron frames or ribs, which vary from 18 to 24 inches asunder, according to the position in the direction

Fig. 15.



of the length of the ship; *c, c*, angle-iron of the same strength is riveted along the edge of each rib for the purpose of stiffening the sides of the ship and giving increased resistance to those parts, also to receive interior plates, some of which, in large vessels, are riveted diagonally to the interior angle-irons *c, c*, &c., forming stringers and braces from the keelsons round the bilge to the upper decks.

Other kinds of frames might be used with double angle-iron, as shown at *d, d*, &c. in the annexed sketch, but they are more expensive, and from the increased complexity of construction, the extra strength obtained does not compensate for the difference of cost. Altogether, the frames recorded in fig. 15 have come into general use as the most effective and easy of construction. Those experimented upon were of different kinds, as shown in Plate LVIII. fig. 17, 18, &c., and in sections given in the Tables, and from which the following results were obtained:—

Fig. 16.

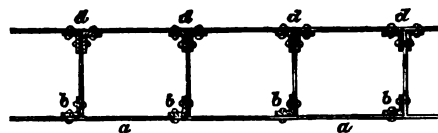
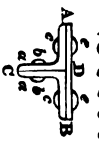


TABLE XVI. Experiments to ascertain the strengths of uniform wrought-iron beams of different forms to support the sides and other parts of vessels, the beams having their ends placed upon props and being loaded in the middle.

No. of exp.	Description and form of the beam.	Weight laid on middle.	Deflections with these weights.	Breaking weight of the beam of 7 feet between the supports.	Weight of the beam of 7 feet 6 inches long.	Oak beams.		Remarks.
						Side of square of oak beams of equal strength with the iron one.	Weight of such beams of 7 feet 6 inches long and specific gravity 900.	
1.	Beam formed from two $2\frac{1}{2}$ angle-irons, riveted together with rivets 6 inches asunder, and a plate $\frac{1}{4}$ inch thick riveted to the back, with rivets 4 inches asunder. Distance between the supports 7 feet, and whole length 7 feet 6 inches, its weight being 109 lbs. Thickness. AB = 5 inches. CD = 2.6 inches. aa = .5 inch. bb = .56 inch. cc = .64 inch. ee = .25 inch. The part C was downwards during the experiment, the weight being laid upon the part D. 	lbs.		lbs.	lbs.	inches.	lbs.	The weight 3355 lbs. was laid on at once, and the beam almost immediately sunk with it; a weight something less would have done it.
2.	The beam last used, cut in two; distance between supports 2 feet 3 inches; vertical rib downwards, that it might be stretched as before; weight of 3 feet 9 inches = 54 lbs.	4,039 5,383 6,055 6,727 7,399 7,735 sunk	.18 .30 .43 .64 .88	2486	109	3.287	31.80	With 7735 lbs. it sunk, by stretching and tearing at a rivet-hole.
3.	The other half of the beam (exp. 1.) cut in two. Distance between the supports 2 feet 3 inches; weight of 3 feet 9 inches = 55 lbs.; vertical rib upwards thus, $\perp$ that fracture might take place by the compression of that rib.	4,039 5,383 6,055 6,727 7,399 8,071 8,743 9,415 10,087 10,423 10,759 sunk	.17 .23 .26 .34 .47 .63 .85 1.10 1.95 2.90	3458	109	3.6692	39.44	With 10,759 lbs. it sunk; the vertical rib becoming twisted.

All the beams experimented upon in the foregoing Table are shown in view and in section, Plate LVIII. figs. 17 and 18. In the first experiment the beam was 7 feet between the supports, but having yielded to the first weight, 3355 lbs., laid on, it was subsequently cut in two, as shown in the drawings above referred to. In experiment 2, it will be observed that a frame of this form is weak, arising from the deficiency of material on the lower side of the rib formed by angle-iron, which, yielding to a tensile strain, becomes elongated in the act of bending, and would thus deflect through a considerable space before actual fracture took place. Reversing the other part of the beam with the broad flange downwards it carried more weight, but ultimately sunk under a load of 10,759 lbs., being in the ratio of 10 : 7 in favour of the beam with the rib upwards.

These experiments, when reduced to 7 feet between the supports, gave nearly the same proportion, viz. nearly as 34 : 24. They are however all weak, arising almost exclusively from want of material on the top edge of the ribs, and a due proportion in the construction of the beam.

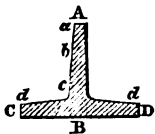
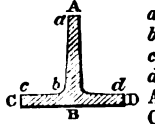
TABLE XVII. Experiments on Wrought-iron Beams (continued).

No. of exp.	Description and form of the beam.	Weight laid on middle.	Deflections with these weights.	Breaking weight of the beam of 7 feet between the supports.	Weight of the beam of 7 feet 6 inches long.	Oak beams.		Remarks.
						Side of square of oak beams of equal strength with the iron one.	Weight of such beams of 7 feet 6 inches long and specific gravity 900.	
4.	Beam differing from that in exp. 1 only in being of greater strength, this beam being formed of two 3-inch angle-irons, riveted as before to a plate a $\frac{1}{4}$ -inch thick. Distance between the supports 7 feet; weight of the beam 7 feet 6 inches long, 167 $\frac{1}{2}$ lbs.; vertical rib downwards $\perp$.	lbs. 3,355 4,711 5,383 5,719 6,055 6,391 6,727 7,063 7,399	.40 .62 .82 .98 1.12 1.30 1.87 1.92 2.29 3.25	lbs. 7399	lbs. 167.5	inches. 4.7281	lbs. 65.49	After bearing the weight 7399 lbs. a short time the beam became cracked at a rivet-hole and sunk. From the experiments of Burr on upon green oak, the side of a square beam of equal strength would be 4.558 inches, and its weight 708 lbs.
5.	Half the beam used in exp. 4, now 3 feet 9 inches long, and weighing 82 $\frac{1}{2}$ lbs. Distance between supports 3 feet 6 inches; vertical rib downwards.	4,039 7,399 10,759 11,431 12,103 12,439	0.85 0.25 0.43 0.53 0.65 broke at a rivet-hole.	6219	1675	4.462		
6.	The other half of the beam in exp. 4, weighing 85 lbs.; length 3 feet 6 inches. Distance between the supports 2 feet 3 inches; rib upwards $\perp$.	8,304 12,392 16,480 18,115	0.12 0.24 0.75 sunk, the vertical rib being twisted.	5823	1675	4.3653	55.83	

The whole of the experiments herein recorded are of the same description as the last, with the exception of the beam being composed of thicker angle-iron, and consequently rendered much stiffer and stronger than those first experimented upon. This increased stiffness reversed the resisting powers of the beam, when taken at a 7-feet

span, in the ratio of 6 : 5 in favour of the first position with the rib downwards. For plans and sections of these beams see Plate LVIII. fig. 18.

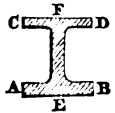
TABLE XVIII. Experiments on Wrought-iron Beams (continued).

No. of exp.	Description and form of the beam.	Weight laid on middle.	Deflections with these weights.	Breaking weight of the beam of 7 feet between the supports.	Weight of the beam of 7 feet 6 inches long.	Oak beams.		Remarks.
						Side of square of oak beams of equal strength with the iron one.	Weight of such beams of 7 feet 6 inches long and specific gravity 900.	
7.	Solid wrought-iron beam, 4 feet 2 inches long, weighing 23 lbs., placed upon props 4 feet asunder; vertical rib upwards. Form and dimension of section.  Thickness at $a = .24$ $b = .29$ $c = .41$ $d = .36$ $AB = 2.50$ $CD = 2.85$	lbs. 1394 1932 2470 2739 3008	.135 .24 .59 .90 1.35	lbs. 1790	lbs. 41.4	inches. 2.9461	lbs. 25.43	With 3008 lbs. the elasticity was entirely destroyed, and a like additional weight would have destroyed the form of the beam.
8.	Same beam rendered straight, and turned with its rib downwards  .	1394 1932 2470 2739 3008 3142	.17 .25 .66 1.22 2.20 sunk	 1870	 41.4	 2.9894	 26.18	
9.	Solid beam, same form as before; length 5 feet $\frac{3}{4}$ inch; weight 25 $\frac{1}{4}$ lbs. Distance between supports 4 feet; rib upwards thus. See fig. to experiment 7.  Thickness at $a = .23$ $b = .30$ $c = .40$ $d = .33$ $AB = 2.05$ $CD = 2.95$	1394 1932 1526 2201 2335 2335 2469	.30 .86 1.19 1.59 2.04 2.13 in a few minutes	1334	37.6	2.671	20.90	After bearing the weight, 2335 lbs., it had taken a permanent set, or flexure = 1.71 inch, and would have sunk more if it had not been unloaded.
10.	Same beam rendered straight and turned upside down thus  . Distance between supports 4 feet.	1394 1932 2201 2335 2335 2469	.27 .51 1.01 1.57 1.60 2.50 in a minute	1411	37.6	2.7215	21.70	After bearing 2469 lbs. it was unloaded, as a little additional weight would have destroyed its form.

The experiments in this Table were made on solid T iron, and indicate nearly the same proportions, as respects their strength, as the beams composed of a plate and double angle-iron riveted together. The whole of these beams appear to be defective in form, and are therefore not calculated to sustain a severe transverse strain. To attain the section of greatest strength, it is probable a different form would be required, as well as a different proportion of the parts, such as in the annexed figure with a double flange*.

* Since the experiments herein recorded were made, others have been instituted on some deck-beams by Mr. KENNEDY of Messrs. BURY, CURTIS and KENNEDY, Liverpool, the particulars of which are inserted in the Appendix.

TABLE XIX. Experiments on Wrought-iron Beams (continued).

No. of exp.	Description and form of the beam.	Weight laid on middle.	Deflections with these weights.	Breaking weight of the beam of 7 feet between the supports.	Weight of the beam of 7 feet 6 inches long.	Oak beams.		Remarks.
						Side of square of oak beams of equal strength with the iron one.	Weight of such beams of 7 feet 6 inches long and specific gravity 900.	
11.	Beam of wrought iron, formed of two bars (nearly equal), whose section is riveted together; length of the beam 4 feet 2 inches; its weight 44 lbs. 5 oz. Distance between the supports 4 feet. Dimensions of section. AB = 2.86 inches. Mean thickness of AB = .33 inch. EF = 3.70 inches. 	lbs. 4,195 7,465 10,735 10,735 14,005	0.75 0.148 0.22 0.24 broke	lbs. 8336	lbs. 79.76	inches. 4.9199	lbs. 70.91	After bearing the weight 10,735 lbs., the beam had taken a set = .06. Pieces of wood were driven tightly in between the ribs AB, CD, at each side of the beam in the middle, to prevent the load laid on it there from deranging its form. The beam broke by the bottom rib being torn asunder, preceded by one of the bars cracking at a rivet-hole.

The above is probably the strongest form of beam, if duly proportioned, by adapting the material to a balance of the two opposing forces of extension and compression.

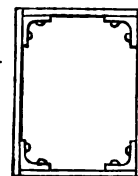
TABLE XX. Experiments on Wrought-iron Beams (continued).

No. of exp.	Description and form of the beam.	Weight laid on middle.	Deflections with these weights.	Breaking weight of the beam of 7 feet between the supports.	Weight of the beam of 7 feet 6 inches long.	Oak beams.		Remarks.
						Side of square of oak beams of equal strength with the iron one.	Weight of such beams of 7 feet 6 inches long and specific gravity 900.	
12.	Beam of wrought iron composed of an uniform vertical rib (7 inches deep and 7 feet 6 inches long) with two 2-inch angle-irons riveted to both top and bottom of the rib; rivets 4 inches asunder; weight of beam 161½ lbs. Distance between the supports 7 feet. Dimensions of section. CD = 7 inches. AB = 4.5 inches. EF = 4.5 inches. Mean thickness of AB = .28 inch. EF = .30 inch. Plate GH = .25. 	lbs. 4,216 8,304 16,480 18,667 22,027 in five minutes 24,379	.10 .18 .25 .36 .52 -54 sunk	lbs. 24,379	lbs. 161½	inches. 7.0358	lbs. 145.03	With the weight 24,379 lbs. the top ribs of the beam became twisted.
13.	Same beam rendered straight and uniform; experiment 12 repeated.	16,115 18,355 19,475 20,595 21,715	.29 .36 .42 .51 sunk	21,715	161½	6.7695	134.26	The beam was heated by the smiths, and when reduced to its original form, it was allowed to cool gradually. With 21,715 lbs. it became bent, towards the wall, in a direction in which it was slightly drawn by the lever; ribs not twisted as before. It bore the weight a minute or two before giving way.

This experiment shows the superior quality of wrought-iron beams in giving timely notice before fracture; it further exhibits weakness on the top sides of the beams, a circumstance requiring great attention in their construction, which in some recent experiments, instituted for attaining the section of greatest strength, have been strikingly developed*.

In the preceding experiments, we have endeavoured to compare the strengths, as well as the weights of the beams or frames which form the ribs of ships. As regards the strengths with equal weights, it is in favour of oak; but the circumstance of the fastenings by rivets in the sheathing being so much superior to those of timber, the iron ship-builder is enabled to dispense with one-half the number of frames, and consequently a great reduction of weight is effected and more strength obtained in the vessel as a whole, than could possibly be accomplished in the timber-built ship, however ingenious the construction or the arrangement and distribution of the material. The very act of caulking the joints of a wooden vessel has a tendency to loosen the fastenings, whereas, in the iron ship, there are no actual joints, for the whole being bound together *en masse*, the same, or nearly the same, strength is obtained as if the whole ship were composed of solid plates and ribs.

The best sectional form of beams for the decks of ships is probably that exhibited in Table XX., which, along with the box beam of the annexed form for supporting the shafts and paddle-boxes of steamers, is that generally used in the construction of vessels of this description. Other forms have been adopted, particularly those suggested by Mr. KENNEDY of Liverpool, alluded to in the Appendix.



Having carefully investigated the different properties of wrought iron in its varied forms of construction, and conceiving that the results obtained from the experiments may be useful in a variety of circumstances connected with the useful arts, I have endeavoured to collect them in the abstract, in order that the practical builder and engineer may the more readily ascertain the comparative value of the different forms of beams, the properties of the material, and their adaptation to any particular construction in which he may be engaged. Should further information be required, we must then refer to the experiments, in which will be found the facts more in detail, and which are probably better calculated to satisfy the inquiring mind and to effect that conviction essential to success.

I have not attempted any inquiry into the laws of oxidation, the adhesion of barnacles and marine vegetation, and the means necessary to prevent such evils. This is a subject which does not come within the province of the present inquiry, and more properly belongs to that of the chemist. I would however briefly notice, that in the whole of my experience I have had little to complain of from the effects of oxidation, as that destructive process, as regards iron, appears to be greatly mitigated, if not almost suspended, by constant use, and under the influence of vibratory action the operation appears to be rendered nugatory, if it does not entirely cease, and that

* See my work on the Construction of the Britannia and Conway Tubular Bridges.

under circumstances exceedingly difficult to explain. This is an investigation not unworthy the attention of some of our best chemists, to whom the causes may be known, but which are at present, as far as I know, unaccounted for. For example, I may mention that an iron ship, if kept constantly in use, or nearly so, will last for a number of years exposed to all the changes of weather and temperature without any sensible appearance of decay. The same may be said of iron rails, over which are passing daily such enormous weights, and at such velocities as almost to neutralize the action of the elements. All these are striking examples of the durability of wrought iron, which may be considered as an important element of its security, and a recommendation for its extended application. There is another circumstance in connection with this subject to which it may be necessary in this place to advert, and that is the effect which a long continuance in salt water has upon the hull of an iron ship. It is well known that a long immersion of cast iron in the sea will convert it into plumbago, and that a similar process with malleable iron, from its contact with the saline particles of the ocean, produces oxidation; and in case the immersions were long continued, the effects of this destructive process might endanger the safety of the ship. As yet we have not had sufficient evidence of its effects to enable us to come to any definite conclusion, but it is not improbable that an occasional visit to harbours of fresh water may mitigate, if it does not entirely neutralize, the injurious effects which the material is likely to sustain. With these observations, which I offer with diffidence, I now beg to direct attention to the abstracts as deduced from the experiments.

Abstract of Results as obtained from the experiments.

In Part I. of this inquiry we have endeavoured to show that 50,000 lbs. per square inch is the mean breaking weight of iron plates, whether torn asunder in the direction of the fibre or across it; and we have also shown that the tensile strength of different kinds of timber drawn in the direction of the fibre varies in a given ratio to that of iron: the timber in this comparison being represented by unity, we have the following ratio of strength:—

	Timber :	Iron.
Ash as 1	:	2·94
Teak as 1	:	3·33
Fir as 1	:	4·16
Beech as 1	:	4·34
Oak as 1	:	5·00

These, for practical purposes, may be taken as a fair measure of the strength of the different woods as compared with that of iron plates.

It has been shown that wrought-iron plates, when riveted together, lose a considerable portion of their strength, as may be seen by the experiments in Part IV., where the plates, by their union with each other, lose by the ordinary process of riveting 44 per cent., and by the best mode of riveting 30 per cent. This should not however

create serious alarm, as the loss of strength is almost entirely obviated by the new process of riveting used in the bottom of the Britannia and Conway Tubular Bridges*; and it should also be observed that in timber the same injuries are sustained by splicing or any other method of forming the joints as are here exhibited in the riveting of iron plates. The two processes, that of riveting (according to the method used in the experiments) and splicing, when intended to resist a tensile strain, must therefore be considered analogous, and the comparison under such circumstances will nearly follow the same law as regards a diminution of strength.

In this section of the inquiry the results obtained from the experiments indicate a loss in the joints as compared with the solid plate, as the numbers 100, 70 and 56, viz.—

For the solid plate	100
For the double-riveted joint	70
For the single-riveted joint	56

which numbers may be considered as a fair average value of the strengths of the different parts of vessels constructed in this manner.

Part V. exhibits the strength of plates to resist vertical pressure from a blunt instrument, which was forced through them for the purpose of ascertaining their comparative powers of resistance with oak timber, placed under circumstances precisely similar and subjected to the same force. The results are interesting, as the iron plates appear to follow a different law in their resistance to pressure to that of oak, the strength being as the depth or thickness of the plates in the first case, and as the squares of the depth in the second. The resistances are therefore in the ratio of 1 : 12, the iron being 12 times stronger than oak.

In Part IV. we have some curious facts illustrative of the necessity and value of experimental research. In the earlier experiments of the inquiry it is evident, that angle and T iron beams or frames are not the best, as regards form, to resist a transverse strain. In every case they are weak, and although exceedingly useful, and in fact indispensable for many purposes of construction, they are nevertheless not calculated to resist strain in the form of beams or girders. These defects I have endeavoured to obviate by the introduction of beams with double flanges formed of a body plate and riveted angle-irons at the top and bottom. All these latter constructions may however be left with safety to the practical engineer†.

The strengths of nearly the whole of these beams have been mathematically investigated by Mr. TATE, to whose friendship and analytical research I am indebted for the annexed mathematical inquiry into the different forms of the wrought-iron beams which have been experimented upon. To the mathematician this part of the subject will be the more interesting, as the utmost care has been observed in the measurements

* See my process of chain-riveting as exhibited in the lower sides of the Britannia and Conway Tubular Bridges, where the injuries above enumerated are entirely obviated.

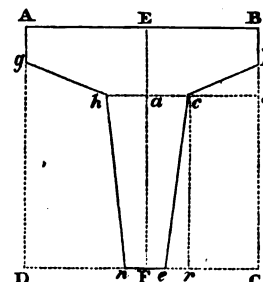
† For a more elaborate inquiry into the strengths of wrought-iron beams, see my work on the Britannia and Conway Bridges.

and exact proportions of the parts, in order to obtain the necessary formula for calculating the strength of beams and frames of this description.

FORMULÆ RELATIVE TO THE BEAMS IN THE FOREGOING EXPERIMENTS.

Beams with a Single Flanch.

Let ABF be a section of a beam having a single flanch ABkh, the material being symmetrically distributed with respect to the vertical line EF. Let hacd and DFC be parallel to AB; cr and kdC parallel to EF. Put EB=EA=e, CB=DA=e₁, ac=Fr=t, Bk=Ag=t₁, Ea=Bd=t₂, Fe=Fn=t₃, area section ABcenhg=A, and the distance of the centre of gravity of this section from the edge ne=X.



To find the Position of the Neutral Axis.

Assuming the material to be perfectly elastic, the neutral axis will be in the centre of gravity of the section. Hence we have, by calculating the moments with respect to the line DC,

$$A \times X = ee_1^2 - (e_1 - t_3)^2(e - t) - \frac{1}{3}(e_1 - t_3)^2(t - t_3) - (e - t)(t_2 - t_1)\left(e_1 - \frac{2}{3}t_2 - \frac{1}{3}t_1\right),$$

$$\therefore X = \frac{3ee_1^2 - (e_1 - t_3)^2(3e - 2t - t_3) - (e - t)(t_2 - t_1)(3e_1 - 2t_2 - t_1)}{3A}, \quad (1.)$$

which expresses the distance of the neutral axis from the edge ne, where

$$A = (t + t_3)(e_1 - t_2) + (t_1 + t_2)(e - t) + 2tt_2 \quad (2.)$$

To find the Moments of Rupture.

Let I = the moment of inertia of the section about the neutral axis.

I₁ = the moment of inertia of the section about DC.

W = the breaking weight of the beam.

l = the distance between the supports.

S = the force per square inch of the material opposed to extension or compression, as the case may be, at the thin edge of the beam.

Taking DC as the axis,—

I₁ = moment inertia ABCD - 2 moment inertia rCdc - 2 moment inertia erc - 2 moment inertia cdk.

$$\text{Now, moment inertia ABCD} = \frac{2}{3} ee_1^3,$$

$$2 \text{ moment inertia } rCdc = \frac{2}{3} (e - t)(e_1 - t_3)^3,$$

$$2 \text{ moment inertia } erc = \frac{1}{6} (t - t_3)(e_1 - t_2)^3,$$

$$2 \text{ moment inertia } cdk = \frac{1}{6} (e - t) \{ (e_1 - t_1)^3 - 3(e_1 - t_3)^3 + (e_1 - t_1)(e_1 - t_2)(2e_1 - t_1 - t_2) \}.$$

Substituting these values and reducing, we find

$$I_1 = \frac{1}{6} [4ee_1^3 - (e_1 - t_2)^3(e - t_2) - (e - t)(e_1 - t_1)\{(e_1 - t_1)^2 + (e_1 - t_2)(2e_1 - t_1 - t_2)\}] \dots (3.)$$

Also (MOSELEY'S Engineering, p. 82) we have

$$I = I_1 - AX^2 \dots (4.)$$

Moreover, by the formula of rupture,

$$\frac{Wl}{4} = \frac{SI}{X},$$

$$\therefore S = \frac{WlX}{4I} \dots (5.)$$

Taking the data of Table XVI., we have

$$e = 2.5, e_1 = 2.6, t = .32, t_1 = .35, t_2 = .25, t_3 = .42;$$

therefore, by equation (2.),

$$A = (.32 + .25)(2.6 - .42) + (.35 + .42)(2.5 - .32) + 2 \times .32 \times .42 = 3.19.$$

By equation (1.),

$$X = \{3 \times 2.5 \times 2.6^2 - (2.6 - .42)^2(7.5 - .64 - .25) - (2.5 - .32) \times (.42 - .35)(7.8 - .84 - .35)\} \div 3 \times 3.19 = 1.91,$$

which is the distance of the neutral axis from the edge *ne* of the beam.

By equation (3.),

$$I_1 = \frac{1}{6} [4 \times 2.5 \times 2.6^3 - (2.6 - .42)^3(2.5 - .25) - (2.5 - .32)(2.6 - .35) \times \{(2.6 - .35)^2 + (2.6 - .42)(5.2 - .35 - .42)\}] = 13.375.$$

By equation (4.),

$$I = 13.375 - 3.19 \times 1.91^2 = 1.738.$$

By equation (5.),

$$S = Wl \times \frac{1.91}{4 \times 1.738} \text{ lbs.} = Wl \times \frac{1.91}{15568} \text{ tons.}$$

In experiment 1, $W = 3409$, $l = 7 \times 12 = 84$,

$$\therefore S = \frac{3409 \times 84 \times 1.91}{15568} = 35 \text{ tons.}$$

Let X_1 = the distance of the neutral axis from the edges AB, and S_1 = the force per square inch opposed to extension or compression, as the case may be, at the edge AB, then

$$X_1 = 2.6 - 1.91 = .69,$$

and

$$S_1 = \frac{X_1}{X} \cdot S = \frac{.69 \times 35}{1.91} = 12.6 \text{ tons.}$$

In experiment 2, $W = 7735 + 18 = 7753$, and $l = 27$,

$$\therefore S = \frac{7753 \times 27 \times 1.91}{15568} = 25.6 \text{ tons,}$$

and

$$S_1 = \frac{.69 \times 25.6}{1.91} = 9.3 \text{ tons.}$$

In experiment 3, $W=10759+18=10777$, $l=27$,

$$\therefore S = \frac{10777 \times 27 \times 1.91}{15568} = 35 \text{ tons,}$$

and

$$S_1 = 12.6 \text{ tons.}$$

Taking the data of Table XVIII., experiments 7 and 8,

$$e=1.425, e_1=2.5, t=.2, t_1=.36, t_2=.4, t_3=.12.$$

Hence we find from equation (2.), $A=1.762$; from equation (1.), $X=1.86$, and $\therefore X_1=2.5-1.86=.64$; from equation (3.), $I_1=6.943$; and from equations (4.) and (5.), $S=W/l \times .00021$ tons.

In experiment 7, $W=3008+11=3019$, $l=48$,

$$\therefore S = 3019 \times 48 \times .00021 = 30.4 \text{ tons,}$$

and

$$S_1 = \frac{.64 \times 30.4}{1.86} = 10.4 \text{ tons.}$$

In experiment 8, $W=3142 \times 11=3153$, $l=48$,

$$\therefore S = 3153 \times 48 \times .00021 = 31.7 \text{ tons,}$$

and

$$S_1 = \frac{.64 \times 31.7}{1.86} = 10.9 \text{ tons.}$$

Observations.—The value of S determined from experiment 1, is the resistance of the material to extension, whereas the value of S determined from experiment 3, is the resistance to compression. Hence it appears, that in beams of this form and *thickness of plates* the resistance to extension is equal to that of compression. The same observation applies to the values of S determined from experiments 7 and 8; and the same law also holds true for experiments 9 and 10.

These calculations further show, that the material in these beams is not properly distributed, for while the thin side of the beam is about to undergo rupture, the broad side has not attained one-half of the tension or compression, as the case may be, which it is capable of sustaining.

It will also be observed, that the resistance of the material at the thin side, as indicated by these calculations, is greater than what it would be under ordinary circumstances, viz. about 25 tons per square inch. This apparent discrepancy may be explained as follows:—as a beam of wrought iron approaches the limit of tension it undergoes an accelerated rate of elongation, even while the cohesion of the material remains unimpaired*. Now this unusual extension of the particles in the lower laminæ (in a beam having a single flanch placed upwards) allows a succession of particles in the higher laminæ to come into full tensile strain, so that the particles at the lower edge of the beam apparently attain a tensile strain greater than they would have under ordinary circumstances. And it may be presumed, that a similar law obtains in reference to the compression of wrought-iron beams. Hence it follows, that all calculations which assume the tensile or compressive forces, in beams of this form, at the edges of the beam equal to what they are under ordinary circumstances, must lead to erroneous results.

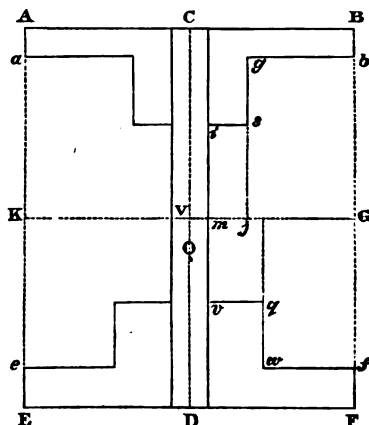
* See remarks on experiment 2, p. 720.

It may be further worthy of remark, that experiments 2 and 3, 8 and 7, show that when the flanches of the beams are placed upwards the deflections are considerably greater than what they are when the flanches are placed downwards; thus in experiments 2 and 3, we have

Weight on the beams.	Deflections when the flanch is upwards.	Deflections when the flanch is downwards.
lbs.	inch.	inch.
6055	·43	·26
6727	·68	·34
7399	·88	·47
and in experiments 8 and 7,		
2470	·66	·59
2739	1·22	·90
3008	2·20	1·35

Beams with a Double Flanch.

Let ABFE be a section of a beam having two flanches ABba and EFfe formed by angle-irons riveted to a vertical plate CD, the material being symmetrically distributed with respect to the vertical line CD. Let O be the neutral axis, and KVG a line passing through the centre V of the vertical line CD parallel to AB or EF. Put A = the area of the section of the material, A_1 = area ABFE or 2 area ABGK, A_2 = 2 area mjsi, A_3 = 2 area jgbG, a_2 = 2 area mjqu, a_3 = 2 area jwfg, D_1 = GB = GF, D_2 = sj, D_3 = bG, d_2 = jq, d_3 = fG, X = OV, I_1 = moment of inertia about KG, I = moment of inertia about O, W = the breaking upon the centre of the beam, l = the distance of the supports, S , S_1 = the resistance of the material per square inch at the edges EF and AB respectively.



To find the Neutral Axis.

Taking KG as the axis of moments,

$$A \times X = \frac{1}{2}(A_2 D_2 + A_3 D_3 - a_2 d_2 - a_3 d_3),$$

$$\therefore X = \frac{A_2 D_2 + A_3 D_3 - a_2 d_2 - a_3 d_3}{2A} \quad (6.)$$

where

$$A = A_1 - A_2 - A_3 - a_2 - a_3. \quad (7.)$$

To find the Moments of Rupture, &c.

Taking the moments of inertia with respect to the line KG,

I_1 = moment inertia ABFE - 2 moment inertia space bgsiqf

$$= \frac{1}{3}(A_1 D_1^3 - A_2 D_2^3 - A_3 D_3^3 - a_2 d_2^3 - a_3 d_3^3) \quad (8.)$$

$$I = I_1 - AX^2 \quad \dots \dots \dots (9.)$$

$$S = \frac{Wl(D_1 - X)}{4I} \quad \dots \dots \dots (10.)$$

and

$$W = \frac{4SI}{l(D_1 - X)} \quad \dots \dots \dots (11.)$$

If

$$a_2 = A_2, \quad a_3 = A_3,$$

then

$$X = 0, \quad I_1 = I,$$

and

$$I = \frac{1}{3} \{ A_1 D_1^3 - 2A_2 D_2^3 - 2A_3 D_3^3 \},$$

$$\begin{aligned} \therefore S &= \frac{WlD_1}{4I} \\ &= \frac{3WlD_1}{4 \{ A_1 D_1^3 - 2A_2 D_2^3 - 2A_3 D_3^3 \}} \quad \dots \dots \dots (12.) \end{aligned}$$

and

$$W = \frac{4S(A_1 D_1^3 - 2A_2 D_2^3 - 2A_3 D_3^3)}{3lD_1}, \quad \dots \dots \dots (13.)$$

which expresses the breaking weight when S is given.

Let $D'_1, D'_2, \dots, A', A_1, \dots$ &c. be the corresponding dimensions of a beam in all respects similar, and let r be the ratio of the linear dimensions, then

$$D'_1 = rD_1, \text{ \&c., } A' = r^2 A_1, \text{ \&c., } A'_2 D'_2 = r^3 A_2 D_2, \text{ \&c., } A'_1 D'^2_1 = r^4 A_1 D^2_1, \text{ \&c.}$$

By equation (6.),

$$\begin{aligned} X' &= \frac{A'_2 D'_2 + A'_3 D'_3 - a'_2 d'_2 - a'_3 d'_3}{2A'} \\ &= \frac{r^3 A_2 D_2 + r^3 A_3 D_3 - r^3 a_2 d_2 - r^3 a_3 d_3}{2r^2 A} \\ &= r \times \frac{A_2 D_2 + A_3 D_3 - a_2 d_2 - a_3 d_3}{2A} \\ &= r \times X. \end{aligned}$$

By equation (8.),

$$\begin{aligned} I'_1 &= \frac{1}{3} \{ A'_1 D'^3_1 - A'_2 D'^3_2 - A'_3 D'^3_3 - a'_2 d'^3_2 - a'_3 d'^3_3 \} \\ &= r^4 \times \frac{1}{3} \{ A_1 D^3_1 - A_2 D^3_2 - A_3 D^3_3 - a_2 d^3_2 - a_3 d^3_3 \} \\ &= r^4 \times I_1. \end{aligned}$$

By equation (11.),

$$\begin{aligned} W' &= \frac{4SI'}{l'(D'_1 - X')} \\ &= \frac{4S \times r^4 I}{rl(rD_1 - rX)} \\ &= r^3 \times \frac{4SI}{l(D_1 - X)} \\ &= r^3 \times W \quad \dots \dots \dots (14.) \end{aligned}$$

The values of S and S_1 , as determined by these calculations, being less for the beam in experiment 13 than they are for the beam in experiment 12, it follows that the latter has a better distribution of the material than the former. And at the same time the difference of the value of these constants is so small as to lead us to infer that the form of the beam in experiment 12 approaches to that of maximum strength with a given quantity of material. The sectional areas of the top and bottom flanches are to each other as 28 : 30 or 14 : 15, which is very nearly a ratio of equality.

APPENDIX.

Experiments by THOMAS LOYD, Esq., Inspector of Machinery, to ascertain the effect of a tensile strain upon bars of wrought iron under varied conditions. Twenty pieces of $1\frac{3}{8}$ S C  bar iron, each 10 feet long, were cut out of the middle of twenty rods of iron. These 10-foot lengths were cut into two parts of 5 feet each, and marked with the same letter. A, B, C, &c. were first broken so as to get the average breaking strain. A_1^1 , B_1^1 , C_1^1 were subjected to the constant action of three-fourths of the breaking weight for five minutes. The load was then taken off, and they were afterwards broken. It will be seen that the breaking strain was about the same as before, thus proving that the previous strain had not weakened them.

Experiment 1.

First.				Second.			
Mark on the bars.	Dimensions of the bars.	Breaking weight in tons.	Ultimate elongation of bar in inches.	Mark on the bars.	Dimensions of the bars.	Breaking weight in tons.	Ultimate elongation of bars in inches with 25 tons.
A.	1·37	33·75	9·12	A 2.		33·75	1·56
B.	1·37	30·00	9·12	B 2.		33·00	1·61
C.	1·37	33·25	9·75	C 2.		33·25	1·56
D.	1·37	32·75	9·22	D 2.		32·25	1·75
E.	1·37	32·50	9·22	E 2.		32·50	1·75
F.	1·37	33·25	10·50	F 2.		33·00	1·56
G.	1·37	32·75	8·50	G 2.		33·00	1·61
H.	1·37	33·25	10·61	H 2.		33·50	1·50
I.	1·37	33·50	8·37	I 2.		32·75	1·67
J.	1·37	33·50	9·22	J 2.		33·25	1·67
K.	1·37	32·25	8·00	K 2.		32·50	1·86
L.	1·37	32·25	7·50	L 2.		31·50	2·00
M.	1·37	30·25	9·12	M 2.		32·75	Broke mark 1·75 in s. c.
N.	1·37	34·25	9·22	N 2.		34·00	
O.	1·37	31·75	7·61	O 2.		32·50	
P.	1·37	29·75	10·00	P 2.		31·00	
Q.	1·37	33·50	9·22	Q 2.		33·75	
R.	1·37	33·75	9·75	R 2.		33·75	
S.	1·37	33·00	9·12	S 2.		33·25	
T.	1·37	32·25	8·75	T 2.		31·00	
Mean ...		32·87	9·09			32·81	1·64

In the first columns of the experiments it will be observed that the force required to break the bars was 32·37 tons, with a mean stretch of 9 inches upon twenty bars. In the second column the mean of the elongations, with a strain of 25 tons, was only 1·6 inch, whereas the ultimate breaking strain was 32·8 tons, evidently showing an increase instead of a diminution of strength from the previous strain of 25 tons, to which the bars had been respectively subjected.

Experiments made in the testing-machine of Woolwich Dockyard to ascertain the effect upon iron-bolt staves or iron bars to a tensile strain. The following results show the strains required for each of four successive breakages of the same pieces of iron as in the first experiment, $1\frac{3}{4}$ ths of an inch diameter S C .

Experiment 2.

Distinguishing mark.	First breakage.		Second breakage.		Third breakage.		Fourth breakage.		Reduced from 1·37 to
	Tons.	Stretch in 54 inches.	Tons.	Stretch in 36 inches.	Tons.	Stretch in 24 inches.	Tons.	Stretch in 15 inches.	
A.	33·75	9·125	35·5	2·00		in.		in.	
C.	33·75	9·250	35·25	·25	37·00	1·00	38·75		1·25
E.	32·5	9·250	34·75	1·25					
F.	33·25	10·500	35·50	1·12	37·25	·62	·40		1·18
G.	32·75	8·500	35·00	1·25	37·5		·41		1·25
H.	33·75	10·625	36·25	1·87					
I.	33·50	8·375	34·50	·62	36·5	1·50			
J.	33·50	9·250	36·00	·25	36·75	1·120	41·75		1·25
L.	32·25	Defective	36·50	1·5	37·75		41·00	·31	1·25
M.	30·25	Defective	36·50	·62	37·75	·06	38·50	·06	1·25
Mean	32·92		35·57		37·21		40·16		1·24
Mean per square inch.....	} 23·94		25·86		27·06		29·20		·90

The results of the above experiments are highly interesting, as they not only confirm those previously made, but they indicate a progressive increase of strength, notwithstanding the reduced sectional area of the bars. These interesting facts are of considerable value, as they show that a severe tensile strain is not injurious to the bearing powers of wrought iron even when repeated to the extent of four times. In practice it may not be prudent to test bars and chains to their utmost limit of resistance; it is however satisfactory to know that in cases of emergency those limits may be approached without incurring serious risk of injury to the ultimate strength of the material.

It is further important to observe, that the elongations are not in proportion to the forces of extension; thus in the bar F, experiment 2, the elongation of a bar 54 inches long with 33·25 tons, is 10·5 inches, giving an elongation per unit of weight and length = $\frac{10·5}{33·25 \times 54} = .0058$; whereas an additional weight of 2·25 tons produces an

elongation of 1·25 inch in 36 inches length of bar, giving an elongation per unit of weight and length = $\frac{1\cdot25}{2\cdot25 \times 36} = \cdot0154$; that is, the elongation in this latter case is about three times that in the former.

Experiments made to ascertain whether a shorter bar of iron is stronger than a longer one of the same kind and size, $1\frac{3}{8}$ ths of an inch diameter, SC .

Experiment 3.

Length between the nippers 10 feet.						
Distin- guishing mark.	Stretch in 10 feet.	Breaking strain.	Mean.	Reduced at fracture.	To else- where.	Remarks.
1.	in. 26·00	tons. 33·00	} 32·21	in. 1	in. 1·25	} Mean of elongation 26 inches.
2.	26·75	31·75			1·18	
3.	26·25	32·25			1·25	
4.	23·00	32·00				
5.	27·50	32·25		B.	B.	
6.	26·75	32·00		1·06		

Experiment 4.

Length between the nippers 42 inches.						
Distin- guishing mark.	Stretch in 42 inches.	Breaking strain.	Mean.	Reduced at fracture.	To else- where.	Remarks.
B.	in. 9·50	tons. 32·50	} 32·125	in. 1·06	in. 1·25	} Mean of elongation 9·8 inches.
B.	9·37	33·00		B.		
B.	10·25	31·75		1	B.	
B.	10·37	31·50			B.	
B.	8·62	32·00		1·06	F.	
B.	8·87	32·00		F.	F.	

Experiment 5.

Length between the nippers 36 inches.						
Distin- guishing mark.	Stretch in 36 inches.	Breaking strain.	Mean.	Reduced at fracture.	To else- where.	Remarks.
A.	in. 8·50	tons. 32·25	} 32·25	in. 1·06	in. 1·25	} Mean of elongation 8·8 inches.
A.	8·75	32·25		1	B.	
A.	9·00	31·25		F.	B.	
A.	9·12	31·50				
A.	9·37	33·50		B.	B.	
A.	8·87	33·25		B.	B.	

Experiment 6.

Length between the nippers 24 inches.						
Distin- guishing mark.	Stretch in 2 feet.	Breaking strain.	Mean.	Reduced at fracture.	To else- where.	Remarks.
C 1.	in. 6.00	tons. 31.75	} 32.00	in. 1	in. 1.25	} Mean of elongation 6.2 inches.
C 2.	6.62	31.50		1.06 B.	F.	
C 3.	6.12	32.50		1		
C 4.	6.12	31.75		1		
C.	6.00	32.25		1		
C.	6.37	32.25			B.	

Experiment 7.

Length between the nippers 10 inches.						
Distin- guishing mark.	Stretch in 10 inches.	Breaking strain.	Mean.	Reduced at fracture.	To else- where.	Remarks.
A.	in. 4.00	tons. 32.25	} 32.29	in. 1.06	in. 1.25	} Mean of elongation 4.2 inches.
A.	3.87	32.50				
A.	4.62	30.50		B.	B.	
A.	4.75	31.50		1	1.18	
A.	4.00	33.25			1.25	
A.	4.12	33.75				

Abstract of the foregoing.

Length between the nippers.	Breaking strain in tons.	Mean elongation in inches.
in. 120	32.21	26
42	32.125	9.8
36	32.35	8.8
24	32.00	6.2
10	32.29	4.2

As these experiments were made upon the same description of iron, it may be fairly inferred that the length of a bar does not in any way affect its strength.

Reduction of the preceding Table.

Length of bar.	Elongation.	Elongation per unit of length.
in. 120	26	.216
42	9.8	.233
36	8.8	.244
24	6.2	.258
10	4.2	.420

Here it appears that the rate of elongation of bars of wrought iron increases with the decrease of their length; thus while a bar of 120 inches has an elongation of .216 inch per unit of its length, a bar of 10 inches has an elongation of .42 inch per unit of its length, or nearly double what it is in the former case. The relation between the length of and its maximum elongation per unit, may be approximately expressed by the following formula, viz.—

$$l = .18 + \frac{2.5}{L},$$

where L represents the length of the bar, and l the elongation per unit of length of the bar.

These results are of some value, as they exhibit the ductility of wrought iron at a low temperature, and also the greatly increased strength which it exhibits with a reduced section under severe strain.

On some future occasion we may refer to this subject in order to show the bearing powers of wrought iron when compared with its elongated transverse section when reduced by forces sufficient to ensure fracture.

The following experiments were made to determine the transverse strength of beams, recommended by Mr. KENNEDY of Liverpool, for supporting the decks of iron ships.

Experiment 8.—October 10, 1845.

On a malleable iron beam, of the annexed sectional form, 11 feet 7 inches long, and 11 feet between the supports.

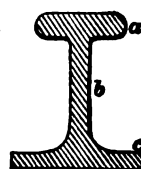
Dimensions at $a = 1.000$ in. $\times 2\frac{1}{2}$ in.

Dimensions at $b = .325$ in. $\times 7$ in.

Dimensions at $c = .380$ in. $\times 4$ in.

Weight of beam = 227 lbs.

Weight of shackle = 885 lbs.



Weight in lbs.	Deflection in inches.	Deflection load removed.	Remarks.
885	.04		
2,581	.12		
4,317	.20		
6,050	.26		
7,743	.35		
9,493	.46		
11,253	.60	.09	
12,955			With this weight the beam became distorted, and continuing the weight for some time, the deflection kept increasing until it bent laterally so as to be no longer able to sustain the load.
Ultimate deflection = .69.			

Experiment 9.—October 10, 1845.

On a malleable iron beam, of the annexed sectional form (see fig. 66), 10 feet 8 inches long, and 10 feet between the supports.

Dimensions at $a=1\cdot000$ in. $\times 2\frac{3}{4}$ in.

Dimensions at $b= \cdot350$ in. $\times 8$ in.

Dimensions at $c= \cdot440$ in. $\times 4\cdot30$ in.

Weight of beam=247 lbs.

Weight of shackle=885 lbs.

Weight in lbs.	Deflection in inches.	Deflection load removed.	Remarks.
885			With this weight the beam was distorted and the experiment discontinued.
2,631	·04		
4,358	·12		
6,098	·15		
7,827	·19		
9,585	·21		
11,278	·26		
12,980	·30	·03	
14,693	·35	·03	
16,373	·45	·09	
18,115	·68	·26	
18,962			
Ultimate deflection=·71.			

In both these experiments the beams yielded to lateral deflection, showing certain defects of form arising from want of lateral strength and breadth in the top and bottom flanges.

Experiment 10.—October 10, 1845.

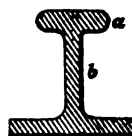
Malleable iron beam of the same form as the last, 10 feet 7 inches long and 10 feet between the supports.

Thickness, $a = 1.000 \text{ in.} \times 2.75 \text{ in.}$

Thickness, $b = .380 \text{ in.} \times 8 \text{ in.}$

Thickness, $c = .420 \text{ in.} \times 4.30 \text{ in.}$

Weight of beam = 276 lbs.



Weight in lbs.	Deflection in inches.	Deflection load removed.	Remarks.
885	.020		
2,606	.050		
4,364	.090		
6,105	.100		
7,835	.140		
9,559	.165	.03	
11,257	.195	.03	
12,999	.220	.04	
14,728	.250		
16,407	.250		
18,108	.290		
19,839	.370		
21,553	.475		With 21,553 lbs. the deflection increased in four minutes .025; in the next four minutes .10; and in four minutes more it had sunk to .34.
22,387	.590		
23,046			Bent laterally upwards of 2.65 inches, when the experiment was discontinued.
Ultimate deflection = .6.			

In these experiments it will be necessary to remark, that they were made with the narrow flange uppermost; a position rather favourable to the strength than otherwise, on account of the increased area of the top flange, which is equal to 2.75 inches; and the bottom flange is only 1.8 inch, a circumstance (deduced from subsequent experiments) favourable to the resisting powers of a wrought-iron beam.

Manchester, April 10, 1850.

XXXVI. *On the Mutual Relations of the Vital and Physical Forces* *.

By WILLIAM B. CARPENTER, M.D., F.R.S., F.G.S.,

Examiner in Physiology and Comparative Anatomy in the University of London.

Received June 20,—Read June 20, 1850.

I. *Introductory Remarks.*

THE degree to which the phenomena of Life are dependent upon Physical agencies, has been a subject of inquiry and speculation among scientific investigators of almost every school. That many of the actions taking place in the living body are conformable to the laws of mechanics, has been hastily assumed as justifying the conclusion that *all* its actions are mechanical; and hence arose the *iatro-mathematical* doctrines, which obtained considerable currency among the physicians and physiologists of the seventeenth century †. In like manner, the fact that many of the

* The author thinks it due to himself to state, that the inquiry whose results are embodied in this paper has been occupying his attention for some years; in proof of which he may cite the following passage from a review of Prof. MATTEUCCI's "Lectures on the Physical Phenomena of Living Beings," contributed by him to the "British and Foreign Medico-Chirurgical Review" for Jan. 1848 (p. 235):—"There can be no doubt that the present tendency of scientific investigation is to show a much more intimate relation than has been commonly supposed to exist between *vital* and *physical* agencies; and to prove that, whilst the former are of a nature altogether peculiar, they are yet dependent upon conditions supplied by the latter. And the more closely these phenomena are investigated, the more intimate and uniform does that dependence appear; so that we seem to have the general conclusion almost forced upon us, that the *vital* forces of various kinds bear the same relation to the several *physical* forces of the inorganic world, that they bear to each other; the great and essential modification or transformation being effected by their passage, so to speak, through the germ of the organic structure, somewhat after the same fashion that heat becomes electricity when passed through certain mixtures of metals." Of the paper communicated by Dr. FOWLER to the British Association at its last meeting (September 1849) under the title—"If Vitality be a Force having Correlations with the Forces, Chemical Affinities, Motion, Heat, Light, Electricity, Magnetism, Gravity, so ably shown by Professor GROVE to be modifications of one and the same Force?"—he has no more knowledge than that which he has obtained from the short abstract of it in the Report of that meeting, published since the greater part of his own paper had been written; and whilst it is evident from that abstract that Dr. FOWLER has been pursuing the same line of investigation with himself, and with somewhat of the same results, he has not thought this a sufficient reason for keeping back his own communication from the Royal Society. For he thinks it will appear, from the extract he has cited, that he may fairly claim priority in the enunciation of the *idea*; and he ventures to believe that the systematic working out of that idea, which he has attempted in this paper, will give it a claim to the consideration of physicists and physiologists, such as it scarcely derives from the treatment which it has received from Dr. FOWLER—so far, at least, as the author can judge from the abstract referred to. (See the *Supplementary Note*, p. 757.)

† "The body," says Dr. BOSTOCK (History of Medicine, p. 165), "was regarded simply as a machine composed of a certain system of tubes; and calculations were made of their diameter, of the friction of the fluids in

changes of composition which take place within living bodies are analogous to those occurring externally to them, was assumed by another party as the foundation of the hypothesis that *all* the phenomena of life are of the nature of Chemical actions ; and of that hypothesis the *iatro-chemical* doctrines which superseded the system of GALEN, and which held their ground under various modifications for several centuries, were the natural expressions *. The insufficiency of either of these hypotheses, or of both of them combined, to explain the phenomena of life, gave origin to a third, which was undoubtedly more correct in its fundamental conception than either of its predecessors had been ; the position assumed being, that the phenomena of each living body proceed from a *vital* agency, or *anima*, peculiar to each organized structure, and having nothing in common with chemical or mechanical principles †. The sect of the Vitalists, however, did not steer clear of the exclusiveness which had been the great fault of the chemists and physicists ; but, in looking at every action of the living body as the immediate result of vital agency, claimed for that agency much that is clearly attributable to the operation of chemical and physical forces.

Among modern Physiologists there is a distinct recognition of the fact, that many of the phenomena of living bodies may be placed in the same category with those of inanimate matter, and that such are not otherwise affected by vital agency than as this prepares or modifies the conditions under which they occur. But there is also a distinct recognition of the fact, that living bodies present a large class of phenomena which are altogether peculiar to them, and which can only be attributed to agencies of which the inorganic world is altogether independent ; and hence has arisen the notion of *vital agency* as the foundation of Physiological science, just as the notion of *affinity* is the foundation of Chemistry, and that of *mutual attraction* of General Physics. And putting aside all hypothetical considerations with regard to the abstract nature of that agency, Physiologists have been aiming to determine the laws

passing along them, of the size of the particles and the pores, the amount of retardation arising from friction and other mechanical causes, while the doctrines of derivation, revulsion, lentor, obstruction, and resolution, with others of an analogous kind, all founded upon mechanical principles, were the almost universal language of both physicians and physiologists towards the close of the seventeenth century."

* "The leading principle of the chemists," says Dr. BOSTOCK (*op. cit.* p. 138), "was, that the living body is subject to the same chemical laws with inanimate matter, and that all the phenomena of vitality may be explained by the operation of these laws." The chemical physicians of the seventeenth century held "that the operations of the living body are all guided by chemical actions, of which one of the most important and the most universal is fermentation. The states of health and disease were supposed to be ultimately referable to certain fermentations, which took place in the blood or other fluids ; while these fluids themselves were the result of specific fermentations, by which they were elaborated from the elements of which the body is composed" (*op. cit.* p. 157).

† "We are told," says Dr. BOSTOCK (*op. cit.* p. 175), "that the *anima* superintends and directs every part of the animal economy from its first formation ; that it prevents or repairs injuries, counteracts the effects of morbid causes, or tends to remove them when actually present, yet that we are unconscious of its existence ; and that, while it manifests every attribute of reason and design, it is devoid of these qualities, and is, in fact, a necessary and unintelligent agent."

of its operation ; following the same mode of inquiry for this purpose, as that which has been found successful in other departments of scientific investigation. In doing this, it has been necessary for them to *isolate*, as much as possible, those phenomena which may be regarded as Chemical or Physical, from those which must be distinguished as Vital ; in order that, by the collocation and comparison of the latter, their mutual relations may be discovered. Still, after making every possible allowance for the operation of chemical and physical agencies, in the *direct* production of the changes of composition, mechanical movements, &c. which connect living beings (so to speak) with the universe around them, it is impossible for the discriminating inquirer not to see, that the influence of these agencies is *indirectly* exerted, to a yet greater extent, in the production or modification of purely vital phenomena. Thus, to take a very simple case, it cannot be for a moment doubted that heat and light exert an influence upon the vegetable germ, which is essential to its growth and development into the perfect plant, and to the performance of all the actions of the latter, whether these have reference to the extension of its own fabric, to the formation of organic compounds from the materials supplied by the inorganic world, or to the production of the germs of new individuals which are in like manner to go through the same series of phases. Hence light and heat have been designated as “vital stimuli ;” the current idea being, that their agency upon the vegetable germ excites or awakens the forces which were dormant in it ; and that, by enabling it thus to assimilate the new materials supplied by the inorganic world, and to give to these the structure of organized bodies, they contribute to develop the latent powers of these materials, which in their turn exhibit vital properties as they are made to form part of organized structures. Such, at least, is the doctrine of those who have most clearly expressed themselves upon the relation of the “vital stimuli” to the “vital properties” of organized bodies ; and the author has not been able to find in physiological writings, any indication of a more intimate relationship between the physical forces and vital phenomena, than that just stated,—save on the part of those who have vaguely identified Heat or Electricity with the “vital principle,” with about the same amount of philosophical discrimination as that which was exercised by the iatro-chemists and iatro-mathematicians of the sixteenth and seventeenth centuries.

The views of physical philosophers have been directed of late almost exclusively to the *dynamical* aspect of the inorganic universe ; that is to say, its phenomena have been studied as the manifestations of certain *forces* ; and each department of science takes cognizance of one or more of these, its *general* laws being nothing else than expressions of the modes and conditions of their operation, so far as known to the scientific investigator. That among all these forces there are very intimate mutual relations, is a conviction which has been gradually increasing in strength in the minds of philosophical inquirers during the whole of the present century ; in consequence of the extraordinary development which the sciences of Chemistry, Electricity, Mag-

netism, Optics, and Thermotics, have undergone during that period, and of the accumulation of facts which more or less distinctly indicate the existence of such relations to those who know how to read them aright. Amongst those who have laboured most successfully in this line of inquiry, Prof. FARADAY stands pre-eminent; but the author is not aware that any other attempt has been made to *formularize* the entire series of these mutual relations, than that which has been put forth by Prof. GROVE in his short treatise 'On the Correlation of Physical Forces;' in which he seeks to establish "that the various imponderable agencies, or the affections of matter which constitute the main objects of experimental physics, viz. heat, light, electricity, magnetism, chemical affinity, and motion, are all correlative or have a reciprocal dependence:—that neither, taken abstractedly, can be said to be the essential or proximate cause of the others, but that either may, as a force, produce or be convertible into the other; thus heat may mediate or immediately produce electricity, electricity may produce heat, and so of the rest" (p. 8).

That the same view might be probably applied to the mutual relations of some of the Vital forces, did not escape Prof. GROVE's sagacity, as will appear from the following passage near the conclusion of his essay:—"I believe that the same principles and mode of reasoning might be applied to the organic, as well as the inorganic world, and that muscular force, animal and vegetable heat, &c., might, and at some time will, be shown to have similar definite correlations; but I have purposely avoided this subject, as pertaining to a department of science to which I have not devoted my attention" (p. 49). The forces here alluded to by Prof. GROVE, however,—those of muscular *motion* and *heat*,—are really *physical* in their manifestations, though generated in living bodies; the purely *vital* operations of growth, development, and reproduction are not even named by him; and not the slightest hint is given by him of the existence of any such relation between the Vital and Physical forces, as it is the chief object of this paper to establish.

Believing, as the author himself does, that all *force* which does not emanate from the will of created sentient beings, directly and immediately proceeds from the Will of the Omnipotent and Omnipresent Creator (which is evidently the idea entertained by LOCKE*),—and looking therefore at what we are accustomed to call the physical forces, as so many *modi operandi* of one and the same agency, the creative and sustaining will of the Deity,—he does not feel the validity of the objections which have been raised by some to whose opinions on philosophical questions he attaches great weight, against the idea of the absolute metamorphosis or conversion of forces. In deference to those opinions, however, he would here say *in limine* that his present object is to show, that *the same relation* (in whatever way defined) exists among the several vital forces, whose operation may be traced in living bodies, as exists among the physical; and that the vital and physical forces are themselves connected by a similar relationship. And as a mode of expressing that relationship

* Human Understanding, Book II. Chap. xxi. On Power, § 4.

without any hypothetical assumption, he would state his idea of the "correlation" of two forces, A and B, to be this;—that A, operating upon a certain form of matter, ceases to manifest itself, but that B is developed in its stead; and that, *vice versâ*, B, operating upon some other form of matter, ceases to manifest itself, but that A is reproduced in its stead. The idea of correlation also involves that of a *certain definite ratio* or equivalent between the two forces thus mutually interchangeable; so that the measure of force B, which is excited by a certain exertion of force A, shall, in its turn, give rise to the same measure of force A as that originally in operation. Thus, when an electric current is set in motion (to use the common phraseology) by galvanic action, the amount of chemical decomposition which it will effect bears a precise correspondence (*cæteris paribus*) with the amount of zinc which has undergone oxidation; chemical action thus exciting electricity, which in its turn reproduces the original equivalent of chemical action. In like manner, when water at 212° is converted into steam, the heat which it receives is no longer manifested *as* heat, but mechanical force is developed in its stead, and this in a certain definite ratio; as soon, however, as the steam, losing its elasticity by condensation, returns to the condition of water, the original equivalent of heat is again developed, its mechanical force being no longer manifested.

Whether we regard it as most consonant to our ideas of the nature of force, to consider the one force, in any such case, as itself becoming *latent*, whilst it excites an equivalent measure of a force of another kind which was previously *dormant*,—or whether we consider that the one force is actually *converted* into the other, and that there is really no such condition as dormant or latent force,—the fact of the mutual relationship, and the definite character of that relationship, remains the same; and it is upon this, rather than upon any hypothetical representation of it, that the author wishes chiefly to insist. Although, therefore, the terms "conversion" and "metamorphosis" will be occasionally employed in the present paper, as the most convenient modes of expressing the author's meaning, he is desirous that it should be understood that he does not desire to imply anything else than the existence of the relationship just defined.

One more preliminary remark is necessary, upon a point on which Prof. GROVE has not thought it requisite strongly to dwell; namely, the necessity for a certain *material substratum* as the medium of the change in question. Thus, to take a familiar case, the correlation of Electricity and Magnetism is indicated by the development of magnetic attractions and repulsions in iron, when a current of electricity is made to circulate around it. In like manner, the correlation between Heat and Electricity is shown in the disturbance of electric equilibrium, which ensues on the application of heat to bars of certain dissimilar metals (especially bismuth and antimony) in contact with each other. The iron, in the first case, is the necessary medium for the development of the magnetic force by electricity; as the bars of dissimilar metals are in the second, for the development of the electric force by heat. So, again, in the

(so-called) magnetization of light by Prof. FARADAY, it seems necessary that the magnetic force should act through some material substratum, in order to produce any effect upon the luminous ray; the intensity of the effect varying according to the medium employed.—This consideration will be found of great importance hereafter, when the mutual relations of the Physical and Vital forces are brought under discussion.

II. *Mutual Relations of the Vital Forces.*

Our clearest idea of the agencies essentially concerned in the production of vital phenomena, is derived from the study of the history of the development of any single organism; and it will be convenient to take that of the Plant in the first instance, as presenting us with these phenomena in their least complex assemblage.

The germ of a Cryptogamic plant, when set free from its parent, is a minute particle, apparently homogeneous in its character, but probably *a cell* in the earliest stage of development.—I. The first change which we witness, is its *growth* or enlargement; and this, when analysed, is found to involve several distinct operations. 1. The germ, under the influence of light, decomposes carbonic acid, and unites its carbon with the elements of water; at the same time decomposing ammonia, and uniting its azote with oxygen, hydrogen and carbon derived from the sources just named; thus forming *organic compounds*, such as no operation of ordinary chemistry has yet been able to imitate. 2. These organic compounds, at first in the condition of crude amyloseous and albuminous substances, need to be rendered *plastic* or organizable, by the process of assimilation, before they are fit to be applied to the extension of the living structure. 3. The organization of this plastic material then takes place, by which its materials are withdrawn from the fluid, and incorporated with the solid texture; and in this process they become fully possessed of the properties of the fabric of which they form part. 4. At the same time, a further process of organic transformation may generate other compounds, which occupy the cavity of the cell, and which are not destined to undergo organization, but are *secreted* or set apart for some ulterior purpose. In these, as in the organic compounds first generated, it is probable that the elements are arranged according to the laws of chemical affinity, although no agency but that of a living organized body has yet been found capable of bringing about their combination in these modes.

Thus we have in operation, in the simple *growth* of a vegetable cell, a force closely allied to chemical affinity, but so far different that it can only be exerted through a living organism; a force of assimilation or vital transformation; and a force of organization and complete vitalization. In speaking of them as distinct forces, it is only meant to affirm that their manifestations are diverse; for it cannot but be observed that they are all mutually dependent, and that they form part of a continuous series of phenomena which have but one ostensible cause,—the action of light and heat upon a living cell,—and but one destiny, the growth of that cell.

II. The *multiplication* of the cell, by spontaneous division or fission, is a process intimately connected with its growth, and takes place under precisely the same influences. It is by this kind of multiplication, that the simpler forms of vegetation are chiefly extended; and that the "germinal mass" is produced in the higher, the component cells of which, resembling each other in all their ostensible characters, seem to be nothing else than repetitions of that in which they all originated.

III. But from this homogeneous germinal mass, a complex and heterogeneous fabric is gradually evolved, of which the several parts or organs present wide diversities in structure and endowments. In this process of evolution or *development* (which obviously differs essentially both from growth and multiplication) it is usual to regard two separate agencies as in operation; namely, *Morphological Transformation*, which is concerned in the evolution of the several organs of which the entire body is composed; and *Histological Transformation*, the operation of which is limited to the component tissues of which these several organs are made up. It appears to the author, however, that, strictly speaking, there is but one such force; the form as well as the composition of each organ being determined by the *development* of particular tissues, and by their *multiplication* in one direction rather than another; he would therefore consider that the transformation of the simple primordial cells into other forms of tissue is the only indication of a distinct force which is manifested in the development of the fabric. Its assumption of its complete and perfect structure, is the result of that perfect harmony and balancing of the several forces of *growth*, *multiplication*, and *transformation*, which indicates, in the most distinct and unmistakable manner, the controlling and sustaining action of an intelligent mind, acting in accordance with a determinate plan*.

In the life of the fully-developed organism, we have still to trace the persistence of the same phenomena; for this is entirely made up of the vital manifestations of its component parts; and these, in the plant, are either cells of various forms, or are tubes formed by the coalescence of cells, which minister simply to the conveyance of liquids. The vegetable physiologist has long been familiar with the fact, that all the operations of the most truly *vital* nature are performed in *cells*, which have not departed in any considerable degree from their primitive type. These operations do not essentially differ in the most elaborate vegetable structure, from those which are performed in the simplest plants, and in the earliest stage of development of the more complex; the chief difference being, that the products of the actions of individual cells

* The term "*germ-force*" has been employed by Mr. PAGE (Lectures on Repair and Reproduction) to designate the power which each germ possesses "to develop itself into the perfection of an appropriate specific form." The author has elsewhere shown (Brit. and For. Med.-Chir. Review, Oct. 1849, p. 413) that this term cannot be logically understood as anything else than "a comprehensive expression of all the individual forces which are separately concerned in the evolution, maintenance, and reparation of a living being." And so far from regarding the whole force which produces the evolution as being possessed by, or as residing in, the germ, it will be the author's object to prove that it is of *external* origin.

are employed for other purposes in the economy, instead of being appropriated by the cells alone. Thus the cells of the spongioles absorb the water, and those of the green surfaces obtain the carbon (from the carbonic acid of the atmosphere), which are required for the nutrition of the entire fabric; and it is especially in the cells of the leaves that those assimilating processes are performed, whereby the plastic fluid is prepared, at the expense of which the organization of new tissue may take place elsewhere, or from which the cells in remote portions of the fabric may draw the materials of their peculiar secretion. So, again, we find that the process of multiplication of cells is limited to certain parts in which the actions of growth are most actively going on; and that a large proportion of the solid fabric is composed of structures which have ceased to take any active share in the performance of the vital functions, and which retain their integrity simply because they are not exposed to influences that would occasion their decomposition.

Everywhere it is to be noticed, that if the condition of the tissues is such as to cause it to be changed by the play of the ordinary chemical affinities, it can only retain its normal character so long as it is performing vital actions; and when these cease, it either undergoes decay (which is the case with the softer tissues), or it becomes transformed into a substance which resists decay, as is seen in the conversion of "sap-wood" into "heart-wood" by the filling-up of the woody tubes with sclerogen, resinous secretions, &c., which have little tendency to decomposition. And it will be observed, too, that the combined influence of warmth, air, and moisture, which favours the rapid decay of dead tissue, is that which most promotes the growth of the living plant. Further, the more rapid and energetic are the processes of growth, the sooner (generally speaking) are they succeeded by decomposing changes; this is seen especially in the Fungi, whose growth is more speedy, and whose degeneration is more immediately consequent upon the completion of their term of life, than that of any other tribe of plants; and it is seen also in cases in which the leaves have been forced into extraordinary activity by an excess of heat and light, their death and exuviation being thus induced at a comparatively early period. Conversely, if the vital operations be retarded by the withdrawal of the external agencies on which they are dependent, we find that the life of the structure is proportionally prolonged, and the decomposing changes are retarded accordingly; this is seen in the well-known fact, that a bouquet of flowers may be made to preserve its appearance of freshness for some time longer when kept in a dark room than it would do if exposed to light. These facts, and many others which might be cited, indicate that every integral part of the living fabric possesses within itself a capacity of being so acted on by external agencies, that the very forces which would tend to decompose and destroy it if it were dead, only excite it to vital activity if it be alive; but that this capacity lasts no longer than the completion of its own term of growth, every individual cell being destined to pass through a certain series of changes, the completion of which leaves it at the mercy of the physical and chemical agencies to which it may

happen to be subjected ; and the duration of its life being inversely proportional to the rapidity with which these operations are performed.

In addition to the phenomena included under the general term of growth or development, we have to study those which constitute the proper *generative* process. This appears to consist, as the author has elsewhere shown*, in the reunion of the contents of two of the cells which had been previously separated by the process of fission. In the simplest tribes of plants, it would seem as if *all* the cells thus springing from the same primordial source, were capable of performing the generative act by "conjugation;" but this capability, like other endowments, is limited in the higher plants to particular groups of cells, which are developed in organs distinct from those concerned in the acts of nutrition, and are obviously set apart from the first for the performance of the act of generation alone.

Various kinds of *motion*, again, are performed by the agency of the vegetable structure, although these are less remarkable than they are in animals. From the extended researches of Prof. SCHLEIDEN it appears probable that within *every* cell, at some stage of its formation, a circulation of fluid takes place†, which is sustained by agencies that cannot be regarded as mechanical, and that are intimately connected with the formative processes; varying in its rate with the general activity of those processes, and only ceasing with their cessation. Wherever a cytoblast exists, the currents radiate from it and return to it again; in other cases (as the *Chara*) they are observed to extend over the whole lining of the cell-wall; in both cases being connected with the part that shows the greatest vital activity. The *zoospores* of various inferior *Algæ* are covered with *cilia*, by the vibration of which these bodies are carried through the water, and deposited at a distance from their parent. These zoospores are minute cells, formed within certain cells of the parent fabric, and liberated by their rupture. So, again, the spiral filaments long since observed in the *Characeæ*, the *Hepaticæ*, and in *Mosses*, and more recently discovered in the *Fucaceæ* and in *Ferns*, have a peculiar independent movement, which seems obviously destined to diffuse them, and thus to bring them into contact with the germ-cells which they are to fertilize. Each of these spiral filaments is developed within a distinct cell. And lastly, in certain plants, both of high and low organization, sensible movements are occasionally to be witnessed, which are immediately due to changes of form in their component cells; these changes of form being sometimes spontaneous, that is, occurring as a part of the regular series of the vital operations of those cells, and not directly excited by any external influences, as we see in the rhythmical movements of the *Oscillatoria*; and being sometimes consequent upon mechanical or other irritations applied to the cells which exhibit them, as in the case of the closure of the fly-trap of the *Dionæa*, but not being at all the less dependent upon the vital endowments of those cells, which cease to exhibit them when their vital activity is diminished. The folding of the leaves of the *Mimosa pudica* appears to take place

* Brit. and For. Med.-Chir. Review, Oct. 1849, p. 346.

† Principles of Scientific Botany, p. 95.

spontaneously in some cases, and to be an excited phenomenon in others, thus combining the characters of both classes of movement, and showing their dependence on similar properties of the contractile cells.

Further, in many of the higher plants, and also in animals, we witness movements of fluid through a capillary network, which must be wholly or in part due to the vital relations of the fluid and the tissues through which it is carried; no physical agency being capable of entirely accounting for these movements, and some of them taking place under circumstances, which, as in the case of the rotation within the cells of the *Chara*, &c., seem to exclude the idea of such agency. Thus the *cyclosis* of SCHULTZ (a recent observation upon which, apparently free from all fallacy, has been recorded by Prof. BALFOUR, 'Manual of Botany,' p. 128), whether or not an universal phenomenon, seems unquestionably to present the spectacle of a rapid capillary circulation, not maintained by any *vis a tergo*, but depending upon forces connected with the vital endowments of the parts through which it takes place. The movement of nutritive fluid in the canals excavated in the tissues of many of the lower Animals, in like manner, seems to be but little dependent on mechanical propulsion, and to be chiefly maintained by some power originating in the living tissues. And even the capillary circulation of the highest animals, in which the regular flow is sustained by the propulsive power of the heart, exhibits certain residual phenomena,—such as local accelerations and stagnations,—which cannot be attributed to changes in the rate or power of the heart's contractions, and indicate the existence of influences arising out of the vital relations of the nutritious fluids to the tissues through which its movement takes place;—this movement being most active when the formative actions of the part are being most energetically performed, and exhibiting a retardation as soon as any influence depresses them*.

The forces concerned in the growth, development, and movements of Animals appear to be essentially the same with those whose existence has thus been traced in plants. The animal, however, deriving its nutriment from organic compounds previously elaborated, does not perform that preliminary operation, which is so remarkably intermediate between chemical and vital agency, viz. the production of ternary and quaternary compounds, of complex atomic constitution, by the union of their elements. But in animals we find an additional power, termed Nervous Agency, nothing analogous to which exists in plants; this power, related on the one hand to the conscious mind, to which it communicates impressions derived from the external world, is also related, in a very remarkable manner, to the vital endowments of the organism in general, as will be presently seen, and particularly to the contractile tissues; the most perfect form of which (the striated muscular fibre) is usually called into action through its instrumentality, in obedience to mental impulses.

* That such is the case, must be admitted, the author believes, by all Physiologists and Pathologists who study the phenomena of the capillary circulation, whether or not they be disposed to admit the validity of the hypothesis of "vital attractions and repulsions" which has been offered by Prof. ALISON as an explanation of this order of phenomena.

In order to meet the more varied requirements of the Animal organism, a much larger proportion of its tissues undergoes various transformations, so as to depart more or less widely from the original cellular type, than we find to be the case in the plant; still it is no less true in the animal than in the plant—as proved by the researches of SCHWANN, extended and confirmed as they have been in this particular by the researches of all subsequent histologists—that *all the tissues possessing distinctly vital endowments*, originate, directly or indirectly, in the transformation of cells*. And further, it may be stated, that *all the most active vital operations, in the Animal as in the Vegetable organism, are performed by tissues which retain their original cellular constitution with little or no change†*.

The several modifications of vital force which have now been enumerated will be found, when closely examined, to have a very intimate mutual relationship, however dissimilar may be the phenomena they produce. In the first place they are all exerted, even in the most highly organized living being, through a common instrumentality, the simple cell. Secondly, the entire assemblage of cells making up the totality of any organism, have all a common parentage; being linearly descended from the single primordial cell in which the organism originated. Thirdly, they are manifested in connection with each other, in those single-celled organisms, which are the lowest members of the two kingdoms respectively, and in which there is no separation or specialization of function.—Hence we may express them collectively under the general term of *Cell-force*; and seem entitled to affirm that each is a particular *modus operandi* of the same force as that which is concerned in cell-formation‡.

* It may now be considered as a well-established fact, that the *simple fibrous tissues* may originate in a structureless blastema, and may be produced by its fibrillation, without passing through the intermediate condition of cells. But these tissues cannot be regarded as possessing any truly *vital* endowments; their properties being simply physical, and their uses in the economy merely mechanical.

† This general proposition was first advanced by the author in regard to the operations in which organic life consists (and which are common, therefore, to plants and animals), in his "Report on the Origin and Functions of Cells" in the Brit. and For. Med. Review for January 1843. The subsequent discovery of the cellular composition of the ultimate fibrilla of striated muscular fibre, by himself and Prof. SHARPEY contemporaneously, and the accumulation of various facts relative to the existence of cells or cell-nuclei at the peripheral extremities of the afferent nerves, as well as in the central organs, seem to justify the assertion that unmetamorphosed cells are the active agents in the production of Muscular and Nervous force; in the former case effecting contraction by their change of form, and in the latter developing nerve-force, which is transmitted along the fibres as its conductors.

‡ The author is particularly desirous that he should be understood as implying by the term "cell-force," *not* that the force is produced or generated *by* the cell, but that the growth of the cell is the most general objective manifestation of that force, and that the cell affords the ordinary instrumental condition for its exertion, though there can be no doubt that the force may be exerted in many cases in which cell-development does not take place. The use which he would make of the term is just that which is commonly made of the term "Engine-power;" every one knowing that the steam-engine possesses no power itself, but that it is simply the instrument most commonly employed, because the most convenient and advantageous yet devised, for the application of the expansive force of steam, generated by the application of heat, to the production of mechanical motion.

This inference derives a remarkable confirmation from a series of facts, which indicate that when that *specialization* of function takes place, which has been mentioned as a characteristic of the higher organisms, the cells which become the instruments of some one particular kind of operation seem to lose their other endowments,—as if the expenditure of the vital force of each cell upon any one purpose, unfitted it for any other agency. Thus the *assimilating* cells (whether floating in the nutrient fluids, or included in the absorbent glandulæ), whose function it is to convert the raw material supplied by the food into organizable *plasma*, exercise little or no purely chemical transformation; they do not undergo change of form; they do not exert any mechanical or nervous power; and they do not reproduce their kind. So, again, the cells which are specially endowed with the power of *multiplication*, seem to possess no other special vital endowment; simply receiving the nutriment which has been prepared for them by other agencies, and applying it to the production of new cells, which, if themselves possessed of more special endowments, do not reproduce themselves. Of this we see an example in the first development of the embryonic structure, the cells of which rapidly multiply by the process of fission, up to the time at which histological transformations commence, and then this multiplication almost or entirely ceases; so that (as in the case of the insect, whose larva is an embryonic mass of very rapid production, composed almost entirely of cells destined to undergo histological change during the metamorphosis) the perfect structure *may* be even smaller than that from which it is developed. In the formation of new parts which make their appearance at a subsequent time, the same rule generally holds good, viz. that their foundation is laid in a mass of cells which rapidly multiply up to a certain point without histological transformation, and then undergo histological transformation with little further multiplication. But the most striking illustrations of this principle are perhaps to be derived from those cases, in which a continual production of cells possessed of some special endowment goes on during adult life. Thus it is necessary for every act of *secretion*, that a new formation of secreting cells should take place within the ultimate follicles of glands. These ultimate follicles are really to be regarded (as shown by Prof. GOODSIR*) in the light of parent-cells, which produce the true secreting cells in proportion as the materials of their growth are supplied by the blood. Now *these parent-cells themselves possess no secreting power, their vital force being entirely expended in the production of the true secreting cells*. On the other hand, *the true secreting cells possess no reproductive power, but die and are cast off when they have reached their maturity; as if their whole vital force were expended in the secreting process*, which is itself nothing else than a portion of the act of growth. This will be found, the author believes, to be the type of a large order of facts, of which some others will be presently noticed. Again, the cells which are endowed with the special *reproductive* power, exercised in the true act of generation, seem to possess no other endowment; they do not exercise chemical transformation, nor do they undergo

* Anatomical and Pathological Observations, No. V.

histological change, nor do they multiply after the ordinary fashion. But here, again, we find that these cells are produced within others, whose whole endowment seems that of multiplication; the *sperm-cells* being generated in vast numbers within follicles or parent-cells, which have themselves no power of producing spermatogenic filaments; and each *germ-cell*, also, being a secondary product of the parent-cell of the ovule,—the other cells to which it gives origin (those which fill the embryo-sac in the vegetable ovule, and which form the vitellus of the animal unimpregnated ovum) being of very inferior character and transient duration. That a relation of reciprocity exists between the forces concerned in the growth, development, and maintenance of the individual organism, and those which are employed in the generative act,—so that an excessive expenditure of either diminishes the amount of vital force which is applicable to the other,—is an idea so familiar to physiologists, that the author need not here dwell upon it, further than to point out how completely it coincides with, and illustrates, the view for which he is contending.

When we look, moreover, at the tissues which have been developed from the original cells by histological transformation, we find that in proportion as they lose the cellular character, they for the most part cease to perform any strictly vital operation; as if the act of transformation had expended their vital power! We seem to see this in the development of tubes from cells, alike in the plant and in the animal, the tubes thenceforth serving merely for the conveyance of liquids; and in the development of the simple fibrous tissues of animals, the endowments of these fibres being purely physical, and what vital force they may retain serving merely to enable them to resist chemical change. When we look at the cells concerned in the production of *mechanical movement*, we find the same principle holding good in a most remarkable manner, these cells being apparently incapable of performing any other function. Thus the cells which constitute the fibrillæ of striated muscular fibre exercise no power of chemical transformation, they undergo no histological change, and they appear to be entirely destitute of the power of multiplication; the expenditure of their vital force in the act of muscular contraction involves their death and disintegration; and their renewal appears to be accomplished by a production of new cells by the continued agency of the parent-cell (or sarcolemma), which, itself possessing no contractile power, seems to hold the same relation to the contractile cells of the fibrillæ, that the parent-cells or follicles of glands hold to the true secreting cells occupying their interior. Again, the ciliary action, when the special endowment of a particular set of cells—as those lining the excretory ducts of the glands, respiratory organs, &c. of higher animals—appears to be in like manner incompatible with any other action, but to be the sole manifestation of the vital force of these cells. For the *ciliated epithelium is never a secreting epithelium*; so that in tracing the one form into the other, there seems to be such a marked transition in function (the mode of production and the general conditions of development being essentially the same) as clearly indicates that the ciliary action and the secreting agency, although very

dissimilar in themselves, are both to be looked upon as modes of operation of the same vital force as that which is exerted in the production of the cell. And this view derives remarkable confirmation from the fact, that in the history of the "zoospores" of the Algæ we have two distinct periods, one of ciliary action, and the other of growth and multiplication; *so long as the ciliary action continues, which is provided for their dispersion, no further vital change seems to take place in them; but so soon as this ceases and they become stationary, they begin to exercise chemico-vital transformations, and to grow and multiply as cells.*

These views in regard to the mutual relationship of the different kinds of vital force, are strikingly confirmed by the phenomena of *Nervous Agency*. There can be no reasonable doubt that the production of nerve-force in the central organs is dependent upon the development of the peculiar cells constituting the ganglionic or vesicular substance; and, as already remarked, the progress of physiological inquiry seems to justify the belief (long since entertained and expressed by the author) that either cells or cell-nuclei are the agents in the origination of nerve-force at the peripheral extremities of the nerve-fibres*. The nerve-force thus generated is not merely expended in arousing mental activity on the one hand, or in exciting muscular contraction on the other, but has an intimate relationship (there can be no doubt) with all the other manifestations of vital force which the animal organism exhibits. So intimate is this relationship, so obvious is the controlling and regulating action of the nervous system over the operations of nutrition, secretion, &c., especially in the higher animals, that many physiologists have regarded these actions as necessarily *dependent* upon the exertion of nervous force. On the other hand, it has been urged with great plausibility by Prof. ALISON and others, that since the functions of organic life in Animals are performed under the same essential conditions as those of Plants, and since the acts of formation, secretion, &c. are effected by the very same agency in animals as in plants,—namely, by cell-growth,—there is no valid reason for regarding them as dependent upon nervous agency; although it must be freely admitted that they are greatly affected by that agency, being not merely accelerated and retarded through its influence, but also altered in kind. The view here advocated will, it is believed, afford a definite scientific expression for all the phenomena which bear upon this question. For, just as electricity developed by chemical change may operate (by its correlation with chemical affinity) in producing other chemical changes

* There can be little doubt that such is the function of the cells of the retina, which are shown, by the history of the development of the eye, actually to originate in the optic ganglion. The same appears to be the fact in regard to the cells in relation with the peripheric expansion of the auditory nerve, which originate in the auditory ganglion. (See Mr. H. GRAY's paper "On the Development of the Retina and Optic Nerve, and of the Membranous Labyrinth and Auditory Nerve," at p. 189 of the present volume of the Philosophical Transactions.) And it seems probable from the researches of KÖLLIKER (Annales des Sciences Naturelles. Ser. III. Zool. tom. vi. p. 102), that the plexuses which appear to constitute the ultimate distribution of the sensory nerves in the skin, are really composed of nerve-cells, which have sent out very slender prolongations to inosculate with each other.

elsewhere,—so may nerve-force, which has its origin in cell-formation, excite or modify the process of cell-formation in other parts, and thus influence all the vital manifestations of the several tissues, whatever may be their own individual characters. And this expression will also be found available for the well-known influence of mental conditions upon the properties of the various tissues and secretions, since this influence can only be exerted through the medium of nervous agency. Further, it not only appears that a simple withdrawal or disturbance of the nervous force supplied to particular organs occasions a retardation or perversion of their vital operations; but there also seems evidence that an influence of *an opposite kind* may be transmitted through the nervous system, which is positively and directly antagonistic to the vital powers of the several tissues and organs;—such, at least, appears to be the only mode of accounting for the extraordinary effect of a *shock*, mechanical or mental, in at once and completely destroying the contractility of the heart, and in immediately bringing to a stand the vital operations of other parts; and it harmonizes well with the fact that, in hemiplegia, the “palsy-stroke” transmitted from the brain along the spinal cord almost invariably affects the leg less injuriously than the arm, and for a shorter duration, recovery first taking place in the leg, even when it has been at first paralysed as completely as the arm. If the nervous force be regarded as a *polar* force (as suggested by Messrs. TODD and BOWMAN, ‘Physiological Anatomy,’ vol. i. p. 237 *et seq.*), analogous in its mode of transmission to electricity or galvanism, it is not difficult to understand that the *reversal* of the usual direction of its action may produce the effects in question, regard being had to the opposite effects shown by Prof. MATTEUCCI to be produced upon nervous excitability by the *direct* and the *inverse* electric currents*.

It is hoped that the foregoing considerations (in support of which many others might be adduced†) will have served to establish the general proposition, that so close a mutual relation exists between all the vital forces, that they may be legitimately regarded as *modes* of one and the same force. The most general and characteristic of the manifestations of this force, which serves to unite and connect all the rest, is that which is concerned in *cell-formation*; and to this act, many of the other agencies appear to be essentially related. Thus the tissue of a muscle is constructed solely with a view to its manifestation of contractile power; whilst the development of nervous matter has reference entirely to the peculiar operations in which it is to be concerned. We find only one kind of tissue serving for the generation and transmission of nervous power; this alone affording the *material substratum* through which the vital force can manifest itself as nervous agency. And so, in like manner, it can scarcely be doubted that the contractile tissues, the assimilating cells,

* Lectures on the Physical Phenomena of Living Beings, translated by Dr. PEREIRA, p. 262.

† The dynamical relations of the nerve-force to mental agency, on the one hand, and to the several vital forces on the other, constitute a field of inquiry of vast extent and profound interest. To this inquiry the author purposes to apply himself, should the views enunciated in this paper be accepted as true, or even probable, by those most competent to judge of their merits.

the secreting cells, &c., have their own respective peculiarities of structure or composition, whereby they are severally enabled to serve as the material substrata, through which the vital force is exerted in the production of the various phenomena of life.

III. *Relations of the Vital and Physical Forces.*

Having thus endeavoured to develop the fundamental relations which subsist between all strictly Vital phenomena, by showing that a "correlation" may be traced among the several forces to whose agency they are attributable, it is the author's purpose to inquire, whether any similar relation can be shown to exist between the *vital* and the *physical* forces.

In the conduct of this inquiry, it will be advantageous to take, as our starting-point, a case in which the existence of such a correlation appears particularly obvious; that, namely, of Nervous Force, the strong *analogy* of which to *Electricity* is admitted by all who do not believe in the *identity* of these two agents. The disproof of their identity will be found, the author believes, in the numerous experiments of Prof. MATTEUCCI and others, who have failed to procure any manifestations of a change in the electric state of nerves, through whose agency muscular contractions were being most vigorously excited; and in the well-known fact, that the conduction of nervous force is prevented by pressure on the nerve-trunk, or by other disorganizing changes, which do not impair its power of conducting electricity. All the facts which have been adduced in support of the *identity* of these two forces will be found readily explicable on the idea of their "correlation" or mutual convertibility;—electricity, when acting through nerve-fibres, developing nervous force; and nerve-force, when operating upon a certain special form of apparatus, developing electricity. This view the author purposes now to unfold in more detail; adducing in support of it facts which are so well known to physiologists and electricians, that there can be no occasion to do more than cite them.

1. If an electric current be made to traverse the trunk of a *motor* nerve for a short distance only, it will produce contraction of the muscles which are supplied from its branches. It was formerly supposed that the contraction was excited by the immediate action of the electricity upon the muscles; but it has been clearly proved that the electric current need not proceed to them, its passage along the trunk for a very short distance being sufficient to develop the nervous force in its branches.

2. In like manner, if the electric current be passed for a short distance only along a *sensory* nerve, it will excite in the sensorium the peculiar sensations ordinarily produced by impressions conveyed through that nerve; that is to say, the ordinary tactile sensations, if the current be transmitted along a nerve of common sensation; or those of sight, hearing, smell, or taste, if the current be transmitted along the optic, auditory, olfactory, or gustative nerves. And thus, as remarked by MÜLLER, we may, by proper management, be made conscious at one and the same time of pricking sensations, of flashes of light, of a phosphoric odour, and of a peculiar taste;

all excited by a peculiar cause, the transmission of an electric current along the sensory nerves, through which these modes of consciousness are respectively excited.

This production of muscular contraction on the one hand, and of various forms of sensation on the other, by the transmission of an electric current through a nerve-trunk, along a short distance only, appear to indicate that it is to the nervous force called into activity by the electric, and not to the electric force itself, that the phenomena are immediately due; and so strong an analogy presents itself between this development of nerve-force in a nerve, and the development of the magnetic force in a piece of iron, as the immediate and direct result of a certain application of the electric current, that, whatever may be the view taken of the relation of the magnetic force to the electric, the relation of the nervous force to the electric can scarcely but be placed in the same category. It is no objection to this view to say, that the nervous force can only be excited in the nerve of a living animal, or in that of an animal recently killed. In all instances of conversion of force, as already noticed, some form of matter is required as the medium of the metamorphosis; and a slight change in the condition of that matter may have a very considerable effect in modifying the process of conversion. Thus, it is by causing an electric current to circulate around a bar of iron, that we most readily develop the magnetic force; but the molecular condition of that iron, whether hard or soft, crystalline or fibrous, has an important influence upon the result. Now we know that the normal condition of the nerve-fibre can only be kept up by the continual performance of the changes which constitute nutrition; so that if these changes be interrupted, its molecular condition speedily undergoes alteration. Hence, the fact that the electric force can no longer call forth the manifestations of nervous force, when a short time has elapsed after the suspension of the nutritive processes by the stoppage of the circulation, is in no way inconsistent with the idea here advocated of the intimate relation between the two.

In order to complete the idea of "correlation," however, it must be shown that the nervous force may be the means of developing electricity; and it seems the only feasible method of accounting for the results of the experiments of DAVY, FARADAY, MATTEUCCI, and others, upon the Electric Fishes, to look upon the development of electricity as the result of the action of their nervous force upon the peculiar organic apparatus to which its production is attributed. For the electric power has been ascertained to be entirely dependent upon the connection of that apparatus with the nervous centres, by nerve-trunks of large size, whose branches are distributed with extraordinary minuteness through the ultimate subdivisions of the electric organs; if these nerves be wholly divided, the electric discharge can no longer be called forth in the usual mode; if they be partially divided, the electric power is proportionably weakened; if the "electric lobe" of the encephalon be destroyed, removed, or injured, the electric power is annihilated or weakened, in precise accordance with the degree of damage inflicted; whilst, on the other hand, if the "electric lobe" be

mechanically irritated, or if the nerves proceeding from it be excited to action, even after their separation from the central organ, electric manifestations are obtained, the intensity of which is proportional to the excitement of nervous power thus effected. Various other phenomena recorded by MATTEUCCI make it evident, that the amount of electric force generated by the electrical apparatus is in precise accordance with the amount of nervous force which is transmitted to it*.

Thus it appears that whilst electricity excites nervous force through the instrumentality of the nervous structure, nervous force excites electricity through the instrumentality of the electrical apparatus; and the case seems to be one which points directly to the existence of the *same kind of relation* between nervous force and electricity, as exists between electricity and magnetism, heat, chemical affinity, &c., whatever may be the form in which we think it best to express our notion of that relation. No one, the author believes, who has once adopted the idea of "correlation" as subsisting among the physical forces, can look at the peculiar connections to which he has adverted, as existing between the nervous and electrical forces, without perceiving how completely it is applicable to them. And he cannot but think that some such idea must have been present to the mind of Prof. MATTEUCCI, although he has not met with any distinct expression of it in his writings†.

But Electricity is not the only physical force possessing this peculiar relation to the nervous force.

Our sensations of heat and cold are entirely dependent upon the power which *Heat* possesses of exciting nerve-force in the sensory nerves. Further, if heat be applied to a motor nerve in its course, it will call forth muscular contractions; and if applied to a sensory nerve, it will occasion sensations, both common and special; precisely after the manner of electricity. Conversely, there are phenomena well known to physiologists, which have not yet been explained upon the purely chemical doctrine of calorification, and for which it does not seem possible that any such explanation can account. Several of these phenomena appear to point to the nervous force as a direct agent in the production of heat; the amount of caloric thus generated being

* The question whether a disturbance of electric equilibrium occurs during the contraction of a muscle, and whether this is to be looked upon as the direct result of the operation of the nervous force, or is consequent upon the molecular changes taking place in the muscle under the influence of that force, must be regarded as at present *sub judice*. If the former prove to be the case, we have another instance of the direct production of electricity by nervous force; if the latter, the same metamorphosis would seem to take place through the intermediate condition of muscular force.

† [Since this paper was written, the author has had the satisfaction of learning, from the perusal of Prof. MATTEUCCI's Eighth Series of "Electro-Physiological Researches," that he has formally adopted the doctrine of the correlation between the nervous and electrical forces, which the author had himself put forth, nearly two years before, in a review of Prof. MATTEUCCI's "Lectures." (See Brit. and For. Med.-Chir. Review, Jan. 1848, p. 232.) In addition to the proofs adduced above, Prof. MATTEUCCI has furnished a new series, arising out of the action of an electric current transmitted through a *muscle*, on the *nerves* which ramify through it. (See p. 296 of the present volume of the Philosophical Transactions.)—Nov. 20th, 1850.]

proportional to the expenditure of that force.—Thus a “correlation” is distinctly indicated between Nervous force and Heat.

Precisely the same may be said of *Chemical Affinity*; for the application of various reagents to the nerve-trunks may be made to call into action their peculiar endowments, whether these be motor or sensory; whilst, on the other hand, there is ample evidence that the chemical properties of secretions may be greatly changed under the direct influence of nervous force.

The power of *Light* to excite the nervous force is clearly indicated by the influence of this agent upon the optic nerve, whose peculiar force is excited by the impression of light upon its peripheral extremities; conversely, there are certain phenomena of animal luminosity, especially among the *Annelida*, which do not appear to be directly referable to chemical change, but which seem to be rather dependent upon a direct exertion of nervous power; vivid scintillations (resembling the luminous effects of an electric discharge through a glass tube spotted with tin-foil) being excited by any irritation applied to the nervous system of these animals*.

The relation of *Motion* to the nervous force is too striking to be passed by. The peculiar vital endowments of a nerve may be called into active exercise, as well by pinching or pricking it, as by electrical or chemical stimuli; thus by pressure on a nerve of common sensation, pain is excited; by pressure on a motor nerve, muscular contraction; by pressure upon the eyeball, sensations of light and colours may be produced in complete darkness; pressure applied to the meatus of the ear, so as to affect the auditory nerve, will give rise to a ringing sound; and by quickly but lightly striking the surface of the tongue, near its tip, with the finger, a distinct taste, sometimes acid, sometimes saline, is produced†. Conversely, the nervous force appears convertible into motion through the medium of the Muscular apparatus, just as it excites electricity through the instrumentality of the electric organs of Fishes. That the motor force thus generated is always proportional, *cæteris paribus*, to the

* This is the conclusion at which the author arrived some years since, from observations which he made at Tenby on a small Annelide (probably a species of *Syllis*), in which the luminous discharges are seen with extraordinary brilliancy, when the animal is subjected to irritation, as by slightly pinching or pricking it, or by the movement of the water around it. The same conclusion was contemporaneously arrived at by M. DE QUATREFAGES, from observations made on the Annelida of the coast of France. “En étudiant, à l’aide du microscope, de petites Annélides transparents, M. DE QUATREFAGES est arrivé à découvrir un rapport curieux entre certains phénomènes de phosphorescence animale, et l’influence de l’agent qui détermine la contraction musculaire, et qui, à plusieurs égards, semble tant d’analogie avec l’électricité. Il est probable que la lumière plus ou moins vive, que répandent un grand nombre d’animaux inférieurs, ne dépend pas toujours de la même cause; que tantôt c’est un phénomène qui accompagne la décomposition des matières organiques, et que d’autres fois c’est le résultat de la sécrétion d’un liquide particulier; mais il est probable que, dans un grand nombre de cas, la cause de la phosphorescence est entièrement physique, et se lie, comme la contraction musculaire, à l’influence nerveuse.”—Rapport sur une Série de Mémoires de M. A. DE QUATREFAGES, relatifs à l’organisation des Animaux sans Vertèbres des Côtes de la Manche, par M. MILNE-EDWARDS (Annales des Sciences Naturelles, Troisième Série, tom. i. p. 23).

† See Dr. BALY’s Translation of MÜLLER’s Physiology, p. 1062.

degree of nervous power exerted, will be (the author believes) disputed by no physiologist; it is most remarkably illustrated in the extraordinary force developed under the influence of emotional excitement, which often calls forth a much greater measure of muscular power than the will can command.

Of the relations between *Magnetism* and the nervous force, the author thinks it preferable to say nothing more at present, than that various indications appear to him to be afforded, by recent investigations, of the existence of a direct and influential connection*.

The relation thus pointed out between Nervous agency and the various Physical forces, is the more remarkable, when it is considered that the nervous power must be regarded as the *highest* of all the forms of vital force, both in its relations to mental action, and in its dominant power over organic processes of every kind. Considering how closely, as already pointed out, it is correlated to the forces concerned in muscular and ciliary movement †, in nutrition and secretion, in development and reproduction, it cannot be thought improbable that what is true of it should be true of them also; and that a relation of mutual convertibility should exist between these and one or more of the physical forces. Such relations the author believes to exist; and he now proceeds to adduce facts which appear to him adequate to support that belief.

The *muscular* force may be called forth, as is well known, by electricity directly applied to the muscle itself; by heat, cold, and chemical agents; and by mechanical irritation. These agencies, however, do not appear so directly concerned in the production of the motor power, as in occasioning that metamorphosis of living organized tissue into chemical compounds, whereon the development of the muscular force seems to be immediately dependent. It is now universally admitted that the disintegration of a certain amount of muscular tissue, and the new arrangement of its components in combination with oxygen supplied by the blood, is necessary for the development of its contractile force; and the considerations adduced by Prof. LIEBIG render it highly probable, that the muscular contraction may be regarded as proceeding from the expenditure or metamorphosis of the cell-force, which ceases to exist as a *vital* power, in giving rise to *mechanical* agency. The amount of muscular force developed appears to bear an exact correspondence with the amount of urea formed by the metamorphosis of the muscular tissue; and this metamorphosis involves the cessation of its existence as a living structure, and consequently the annihilation of the vital

* Whatever scientific value we might have otherwise been disposed to attach to the researches of Baron von REICHENBACH on "Magnetism, Crystallization, &c. in their relations to Vital Force," they seem to derive some additional claims on our attention from the discoveries of Prof. FARADAY in regard to the universal operation of the magnetic force, and its relations to light and to the polar force of crystals,—discoveries which, be it observed, had not been made when the phenomena observed by Baron von REICHENBACH were first made public.

† In man and the higher animals, the ciliary movement does not appear to be in any degree controllable by nervous agency; but there can scarcely be a doubt that in the *Rotifera* it is thus governed.

forces which that structure possessed. We are, then, to regard the nervous, electrical, and other stimuli, under whose influence the muscular force is called forth, less as the immediate sources of that force, than as furnishing the conditions under which the vital force acting through the muscle is converted into the mechanical force developed in its contraction.

We do not yet know enough of the conditions under which *ciliary movement* takes place, to enable us to affirm that the production of mechanical motion through its means is in like manner the result of an expenditure of vital force ; but the considerations formerly adduced in regard to the relation of ciliary action to other vital manifestations, together with the remarkable similarity between the influence of strychnia, opium, electric discharges, &c. upon the ciliary movement and upon muscular contractility, leave little room for doubt that what is true of the muscular force is true also of ciliary motion, and that it, too, is to be regarded as directly depending upon a conversion of vital force into mechanical motion. The continuance of motion in the cilia appears to be intimately related to changes taking place in the cells on which they are borne ; and its persistence after the detachment of these cells from the remainder of the body, like the persistence in the contractility of muscular fibre which has been completely isolated from all its connections, proves that we must look to forces existing in them, and not to influences derived from any other source, for the maintenance of this curious operation.

Passing from these particular manifestations of vital force, which so remarkably indicate its relations to physical agencies, to those which, being concerned in the development and growth of organized structures, seem to have less in common with them, we shall fix our attention on the fundamental fact, that these Organizing forces (as we may conveniently designate them) are so completely dependent upon the continual agency of *Heat* (and in some cases of *Light* also), that they may be considered as the manifestations of the action of heat upon organized fabrics.

The necessity for this agency may be seen at every period of the life of organized beings of all kinds. In the lower tribes of animals, and in the entire vegetable kingdom, we trace this dependence in *the precise relation between the vital activity of each individual, and the amount of heat which it receives from external sources*. Every species is adapted to flourish within a certain range of temperature ; and that amount of heat which is most effective in sustaining the life of one species, may be injurious or even fatal to another. But within the range which is compatible with the manifestation of its vital powers, we find that the relation is most constant between the temperature and the organizing force exhibited by each species. It is scarcely necessary to accumulate facts in support of a position so generally admitted ; but the author particularly wishes to direct attention to the definiteness and exactness of this relation, and may instance the following facts as examples.

1. According to BOUSSINGAULT, the same annual plant, in arriving at its full development, and going through all the processes of flowering and maturation of its

seed, *everywhere receives the same amount of solar light and heat*, whether it be grown at the equator or in the temperate zone ; its *rate of growth being in a precisely inverse ratio to the amount it receives in any given time*. Hence it appears that the organizing force of Plants bears a relation of equivalence to the Heat and Light which act upon them.

2. This has been separately demonstrated with regard to the special influence of Light, in producing the decomposition of carbonic acid and the formation of chlorophyll, &c. ; the amount of carbon fixed by plants being *cæteris paribus* in accordance with the amount of illumination they receive. The influence of Light, it may be remarked, seems to be exerted only in this peculiar process of vital chemistry ; whilst that of Heat is exercised in *all* the other operations in which growth consists ; and hence it is that Animals are comparatively little dependent upon light, their food being prepared for them by the agency of the vegetable kingdom.

3. The rate of "rotation" of the fluid within the cells of *Chara*, &c., and the rate of "cyclosis" in the latex-vessels of *Ficus elastica*, &c., appear to be in precise relation (within certain definite limits) with the temperature to which these organisms are subjected ; the movement of the fluids being accelerated by warmth, and retarded or checked by cold*.

4. In cold-blooded Animals, the same relation may be seen, between the activity of the organizing processes, and the amount of Heat to which they are subjected. The production of larvæ from the eggs of Insects, like the germination of the seeds of plants, may be accelerated or retarded at pleasure, simply by the regulation of the temperature ; and the time required for the last metamorphosis is precisely in the inverse ratio to the heat supplied ; so that, as in the maturation of the plant, each individual of the same species receives the same amount of heat, whether the intensity of its action be greater or less. Further, it has been remarked by Mr. PAGET†, that the processes of *development* seem to require a higher degree of Vital force than those of simple *growth* ; and it harmonizes admirably with the doctrine here contended for, that there appears to be a necessity for a higher temperature for developmental operations, than for those of simple increase. Thus in the economy of the Social Bees, as shown by Mr. NEWPORT, there is a special provision for generating heat during the last few hours of the metamorphosis, in which the tissues and organs of the imago are being completed ; and in the Viper and some other ovo-viviparous Reptiles, there seems to be an unusual calorifying power, for the purpose of promoting the development of the embryo. So, again, it has been found by Dr. EDWARDS and Mr. HIGGINBOTTOM that the metamorphosis of Batrachia requires a larger amount of light and heat than

* It would almost seem as if some anti-vital influence, resembling that of "shock" in animals, could be exerted by mechanical injury on plants. When a portion of the leaf of *Vallisneria* is detached for the exhibition of the movement of "rotation" in its cells, the movement generally ceases for some little time ; the application of warmth will usually re-excite it ; and it may then continue for several hours or even days.

† Lectures on Repair and Reproduction.

suffices for their growth in the larva state, being retarded or even prevented by the want of a due amount of these agencies (see p. 753); and it has been also shown by Mr. HIGGINBOTTOM that the *development* of new limbs in the Triton, to replace those which have been lost, cannot take place at a lower temperature than about 60°, although the processes of *growth* go on under a much less degree of heat*. The general propositions enunciated by Prof. MILNE-EDWARDS†, in regard to the geographical distribution of the Crustacea, indicate the existence of this relation in the most decided manner. They are, briefly, as follows:—I. The varieties of form and organization (which may be regarded as so many varied manifestations of the organizing force) increase as we pass from the Polar Seas towards the equator, the number of species thus augmenting greatly as we go southwards. II. The differences of form and organization are not only more numerous and more characteristic in the warm than in the cold regions of the globe; they are also more important. III. Not only are those Crustacea which are most elevated in the scale deficient in the polar regions, but their relative number decreases rapidly as we pass from the equator towards the pole. IV. The average size of the Crustacea of tropical regions is considerably greater than that of the tribes inhabiting temperate or frigid climes. V. It is where the temperature is most elevated, that the peculiarities of structure which characterize the several groups are most strongly manifested. And VI., there is a remarkable coincidence between the temperature of different regions, and the prevalence of certain forms of Crustacea.—It is interesting to observe, that the want of a high temperature is no obstacle to the growth and multiplication of individuals of a comparatively small size and low grade of organization; the Arctic and Antarctic seas being as numerously peopled with such, as the tropical ocean is with higher forms. But the preceding statements point to a direct and definite relation between Heat and the Organizing force, as manifested in this group of animals. A comparison of the facts relating to the geographical distribution of other classes of cold-blooded animals would probably justify the same conclusions. There can be no doubt of their general applicability to the Vegetable kingdom.

5. The influence of temperature upon the general vital activity of cold-blooded animals is no less remarkable. The facts determined by the experiments of Dr. W. F. EDWARDS‡ lead to this general conclusion;—that the *rate of life* of Batrachia and Fishes, of which the activity of their respiratory process is the exponent, varies directly (within certain limits) as the temperature of the surrounding medium; so that the *duration of life*, when these animals are deprived of air, either partially or completely, or are placed in any other circumstances unfavourable to its sustenance, varies inversely with the external temperature. Thus when frogs were confined in a limited quantity of water, and were not allowed to come to the surface to breathe, it

* Proceedings of the Royal Society, March 18, 1847.

† Histoire des Crustacés, tom. iii. p. 555 *et seq.*

‡ On the Influence of Physical Agents on Life, *passim*

was found that they died in from 12 to 32 minutes, when its temperature was 90° ; in from 35 to 90 minutes, when its temperature was 72° ; in from 350 to 375 minutes, when its temperature was 50° ; and from 367 to 498 minutes, when it was cooled down to the freezing-point. The prolongation of life at the lower temperatures was not due to torpidity, for the animals performed the functions of voluntary motion and enjoyed the use of their senses; but it was occasioned by the diminished activity of *all* their functions, and their consequent less demand for air. On the other hand, the elevation of temperature increases the demand for air, and occasions speedier death when it is withheld, chiefly by producing a vast acceleration in the rate at which all the operations, both of animal and organic life, take place.

6. Although the warm-blooded animals are in great degree removed, by the independent calorifying power which they possess, from the influence of external temperature, yet it is very easily shown that their vital activity is no less under the direct and immediate influence of heat, than is that of cold-blooded animals. In fact, it would seem to be for the sake of keeping up their vital energy to a certain high and uniform rate, that they are endowed with the heat-generating power; and if this power be not exercised, and the body be cooled down, its vital activity is reduced, and at last extinguished. From the experiments of CHOSSAT* it appears that Birds and Mammals cannot (except in the case of the hibernating species) be cooled down more than 30° below their natural standard, without the entire suspension of their animal and organic functions. This depression of temperature consequent upon prolonged starvation, was found to take place as soon as all the fat and other disposable materials in the body had been burned off. But so soon as animals thus reduced to a moribund condition were subjected to external heat, which artificially raised the temperature of their bodies, their sensibility and muscular power were renewed; they flew about the room, and took food when it was presented to them; and their secretions were restored. If this artificial assistance was prolonged, until the digested aliment was prepared in sufficient amount to maintain the combusive process, they recovered; but if it was withdrawn too soon, they died.—The hibernating species of Mammalia differ from the rest essentially in this, that the lowering of the temperature of their bodies does not destroy their vitality, but merely suspends their activity, so that they are reduced for a time to a condition in all respects comparable to that of cold-blooded animals but little removed above absolute torpidity; and in this condition, all that has been said respecting the influence of external temperature upon the rate of life of cold-blooded animals, applies to them also.

The vast mass of facts, of which the foregoing are examples, appears to the author to justify the conclusion, that Heat is something more than a *stimulus* capable of arousing a dormant vital force; but, on the other hand, they by no means justify the assumption that heat and the “vital principle” are identical. That Heat, acting upon or through an Organized structure, then manifests itself as Vital force,—or that heat

* Expériences sur l’Inanition.

and vital force are “correlated,”—seems to be the expression of their mutual dependence, which is most in accordance with all our knowledge of the influence of heat upon organized beings; whilst conversely (as will be shown hereafter) it accords with the fact of the restoration to the inorganic world—under some form or other—of all the *force* thus withdrawn from it.

It may serve, however, to bring this idea into contrast with the notions usually entertained, and to illustrate its application more fully, if it be considered in its relation to the Development of any highly organized being from its primordial germ-cell. According to the doctrine current among some physiologists, the whole “organizing force,” “*nisus formativus*,” or “*bildungstrieb*,” which is to be exerted in the development of the complete structure, *lies dormant in this single cell*, the germ (it has been affirmed) being “potentially” the entire organism. And thus all the organizing force required to build up an oak or a palm, an elephant or a whale, is concentrated in a minute particle only discernible by microscopic aid.

As a refuge from this doctrine, which seems almost too absurd ever to have gained believers, other physiologists (among whom the author formerly ranked himself) have affirmed that *vital force must exist in a dormant condition in all matter capable of becoming organized*; that the germ-cell, in drawing to itself organizable materials, and in incorporating these into the living structure, does nothing else than evoke into activity their latent powers; and thus that, with every act of growth and cell-multiplication, new vital force is called into operation, whereby the process is continually maintained. This proposition, it may be safely asserted, does not involve any manifest absurdity. It attributes to oxygen, hydrogen, carbon, and nitrogen, properties which they were not previously supposed to possess; but no one could logically deny to these elements the possession of dormant vital powers, whilst they held that a dormant magnetic power might be attributed to iron. In the one case, as in the other (it may be affirmed), a certain combination of conditions is needed to call the property into exercise; and the living cell, combining the elementary substances into the pabulum of its growth, and then applying this to its own nutrition, calls their latent vital properties into activity,—just as (it has been argued) an electric current, made to circulate around a piece of iron, develops the latent magnetic force of that metal.

The views of Prof. GROVE, however, strike at the root of the notion of *latent force* of any description whatever; all force once generated being, in his estimation, perpetually *active* under one form or other; and its supposed “latency” being a hypothetical condition, the idea of which is quite unnecessary when the force which has ceased to manifest itself is recognized under some other form. Thus, in his view, when iron is rendered magnetic by an electric current, the development of the magnetic force is rather to be looked on as the result of the conversion of the electric, by the instrumentality of the iron, than as a case of the excitation of one force previously

dormant, by another which is expended in thus evoking it. Such an analogy should rather lead the physiologist to look for some extraneous source of the organizing force; and to suspect that when organizable materials are applied to the extension of a living structure, and are caused to manifest vital forces, *some agency external to the organism is the moving spring of the whole series of operations*. And thus, according to the view here advocated, the vital force which causes the primordial cell of the germ first to multiply itself, and then to develop itself into a complex and extensive organism, was not either originally locked up in that single cell, nor was it latent in the materials which are progressively assimilated by itself and its descendants; but is directly and immediately supplied by the Heat which is constantly operating upon it, and which is transformed into vital force by its passage through the organized fabric that manifests it. The facts already cited, which show how completely dependent the process of germ-development, both in plants and animals, is upon the constant agency of heat, and how precisely its rate may be regulated by the measure of that force supplied to it, appear to the author to be so much better accounted for upon this view than upon either of the others, that he ventures to think that they demonstrate it almost as fully as the nature of physiological evidence will admit.

Having thus contrasted the doctrine for which he is contending, with those which are current among physiologists, the author thinks it well to point out that he no more regards heat as the "vital principle," or as itself identical with the "vital force," than it is identical with electricity or with chemical affinity. Nor does he in the least recognize the possibility, that any action of heat upon the inorganic elements can of itself develop an organized structure of even the simplest kind. The pre-existence of a living organism, through which *alone* can heat be converted into vital force, is as necessary upon this theory, as it is upon any of those currently received amongst physiologists. And it is the *speciality* of the material substratum thus furnishing the medium or instrument of the metamorphosis, which in his opinion establishes, and must ever maintain, a well-marked boundary-line between the Physical and the Vital forces. Starting with the abstract notion of Force, as emanating at once from the Divine Will, we might say that this force, operating through inorganic matter, manifests itself in electricity, magnetism, light, heat, chemical affinity, and mechanical motion; but that, when directed through organized structures, it effects the operations of growth, development, chemico-vital transformation, and the like; and is further metamorphosed, through the instrumentality of the structures thus generated, into nervous agency and muscular power. If we *only* knew of heat as it acts upon the organized creation, the peculiarities of its operation upon inorganic matters would seem as strange to the physiologist, as the effects here attributed to it may appear to those who are only accustomed to contemplate the physical phenomena to which it gives rise.

The variety of organic forms called forth by the agency of heat, which may be regarded as the products of its operation upon living germs, does not present any

real obstacle to the reception of this doctrine; since in *any* hypothesis which assumes a common force as operative in the living kingdoms of nature, it is necessary to admit that this force is modified in its action by the properties of the germ, just as that the general force of chemical affinity manifests itself differently in the reactions of each elementary and composite substance. And just as the chemist seeks to determine the laws of chemical affinity by observation and experiment, so does the philosophic physiologist aim to discover the general plan on which the vital force is exerted, in the production of the wonderful series of organized structures which have successively presented themselves on this globe.

In speaking of Heat as the physical agent especially concerned in the development of living organisms, and in the maintenance of their activity, the author would by no means leave out of view the other physical forces, *all* of which, if correlated to each other, as well as to the vital forces, must be capable of exerting an important influence on these processes. He has merely selected Heat, as the one whose operation is most extensive and most easily demonstrated; and every fact which indicates that other physical agencies are also in operation, will (of course) only add weight to his argument. To the universally-admitted agency of *Light*, in directly exciting one (at least) of the most important processes of vegetable growth, reference has already been made. But there is evidence that light has an influence upon certain processes of *development*, which cannot be accounted for by its agency in the fixation of carbon from the atmosphere, and in the production of organic compounds. One of the most remarkable examples of this agency is furnished by the experiments of MIRBEL upon the *gemmæ* of *Marchantia polymorpha*. He found, after repeated trials, that during the development of these little discs, stomata are formed on the side exposed to the light, whilst root-fibres grow from the lower surface; and that it is a matter of indifference *which* side of the disc is at first turned upwards, since each has the power of developing stomata, or roots, according to the influence it receives*. The experiments of Dr. W. F. EDWARDS indicate that a decided influence is exerted by light upon the metamorphosis of the Batrachia; since, according to his statements, when tadpoles, *arrived at nearly their full growth*, were secluded from the influence of light, but were supplied with aerated water and food, *they continued to increase as tadpoles* (so as to attain an extraordinary size, doubling or even trebling their usual full weight) without undergoing any metamorphosis†. The influence of light upon the

* Nouvelles Annales du Muséum, tom. i.

† On the Influence of Physical Agents on Life, p. 53.—The results of the recent experiments of Mr. HIGGINBOTTOM appear to negative those obtained by Dr. EDWARDS, and to show that metamorphosis is only retarded by privation of light, when accompanied by reduction of temperature. But the remarkable fact above quoted from Dr. EDWARDS's statements, to which Mr. HIGGINBOTTOM has recorded nothing parallel, shows that there was some difference in the conditions of the two sets of experiments, which should prevent us from setting aside those statements, made (as they are) by a most trustworthy observer, until they shall have been more fully disproved.

minute Entomostracous Crustacea is well known. Their development is greatly retarded by the want of it; and the exuviation of their shells, which normally takes place at short intervals when they have attained their complete form and size (apparently for the purpose of freeing them from the minute plants with which their surface becomes clothed), is much less frequently performed*. With these facts before us, we can scarcely refrain from suspecting that the deprivation of light may be the cause of the atrophy of the visual organs in certain animals which pass their whole lives in complete seclusion from its influence. This condition, which has been long known to exist in the common Mole, and also in the *Proteus anguineus*, and which has also been discovered in the *Amblyopsis spelæus*, a fish inhabiting the waters of the Great Cave of Kentucky, has recently been detected in a considerable number of species of Insects discovered in the very caverns of the Tyrol whose waters afford a *habitat* to the *Proteus*†. It may be supposed that the non-development of eyes in all these animals is a part of their original constitution, and is to be looked upon as an example of the adaptation of their organisms to the peculiar conditions of their existence; and such a view cannot at present be positively disproved. But the actual dependence of the nutrition of the visual organs, or (at least) of the nervous apparatus which forms the essential part of them, upon the continued agency of light, appears from the well-known fact, that if, by the complete opacity of the cornea, light is entirely prevented from entering the eye, the retina and the optic nerve become atrophied, and in time altogether lose their characteristic structure; thus clearly indicating the direct influence of light in keeping up those nutritive actions, by which the integrity of that structure is normally maintained.

That *Electricity*, also, has an important influence on the operations concerned in the development and maintenance of organized structures, can scarcely be doubted by any one who duly considers the proofs of the disturbance of the electric equilibrium in those parts of vegetable as well as animal bodies which are in a state of greatest functional activity, afforded by the observations and experiments of Prof. MATTEUCCI and others. At present, however, it would be premature to make any positive statement as to its *modus operandi*; although it would certainly appear most probable that it is more directly related to the chemico-vital changes of composition which take place in the living body, than to the operations of cell-growth, multiplication, and development, properly so called.

If the views advocated in this communication be correct, it follows that not merely are the *materials*, withdrawn from the inorganic world by vital agencies, given back to it again by the disintegration of the living structures of which they have formed

* See Dr. BAIRD'S "Natural History of the Entomostracous Crustacea" (published by the Ray Society), p. 192.

† Specimen *Faunæ Subterraneæ*, Bidrag til den underjordiske Fauna, ved J. C. SCHNÖDTZ: Kjöbenhavn, 1849.

a part, but all the *forces*, which are operative in producing the phenomena of life, are in the first place derived from the inorganic universe, and are finally restored to it again. The author thinks it not difficult to show that such is actually the case; the very same antagonism existing, in respect to the relation of the Vegetable and Animal kingdoms respectively, to the *forces* of the universe, as exists in regard to their *material components*. Plants, it will be recollected, form those organic compounds at the expense of which animal life (as well as their own) is sustained, by the decomposition of carbonic acid, water, and ammonia; and the *light*, by whose agency alone these compounds can be generated, may be considered as metamorphosed into the *chemico-vital affinity* by which their components are held together. The *heat* which plants receive, acting through their organized structures as *vital force*, serves to augment these structures to an almost unlimited extent, and thus to supply new instruments for the agency of light and for the production of organic compounds. The whole *nisus* of vegetable life may be considered as manifested in this production; and in effecting it, each organism is not only drawing *material*, but *force*, from the universe around it. Supposing that no animals existed to consume these organic compounds, they would be all restored to the inorganic condition by spontaneous decay, which would reproduce carbonic acid, water, and ammonia, from which they were generated. In this decay, however slow, the same amount of Heat would be given off, as in more rapid processes of combustion; and the faint luminosity which has been perceived in some vegetable substances in a state of *eremacausis*, makes it probable that the same is true of Light. And though the process of decay may be prevented or modified, so that the whole or a part of the materials of vegetable structures are disposed of in other ways, yet whenever they return to the condition from which they were at first withdrawn, they not only give back to the inorganic world the materials out of which they were formed, but the light and heat to which their production was due. Thus in making use of the stores of Coal which have been prepared for his wants by the luxuriant flora of past ages, man is not only restoring to the atmosphere the carbonic acid, the water, and the ammonia which it must have contained in the carboniferous period, but is artificially reproducing the Light and Heat which were then expended in the operations of vegetable growth. That the relative proportion of the light and heat thus restored, should be the same as that which they originally bore to each other, is by no means necessary; since each (according to Prof. GROVE's views) is convertible into the other*. In the few cases in which *motion* is affected by the vital force of plants, this may be considered as restoring to the inorganic universe a certain measure of the force which they have derived from it, in the form of light and heat.

But the organic compounds which the agency of Light and Heat upon the Vege-

* [In the second edition of his *Essay on the "Correlation of the Physical Forces,"* just published, Prof. GROVE advances the opinion (p. 59) that when Light is "absorbed" (to use the ordinary phraseology), that is, when it ceases to manifest itself as light, it is usually converted into Heat.—Nov. 20, 1850.]

table structures has produced, are destined for a much higher purpose than that of being merely given back to the inorganic universe by eremacausis or combustion. In serving as the food of Animals, they not merely become the materials of their structures, but are rendered subservient to the production of the nervous and muscular forces. The animal, like the plant, receives *heat* from external sources ; and this is expended, in the form of vital force, not merely in the building-up of the organism from its germ, but also in its subsequent maintenance. For, as was first definitely stated by Prof. LIEBIG, the vital force, which is applied in Plants to the extension of the structure, is appropriated in Animals to the development of muscular and nervous power ; and this development, depending as it does upon a continual disintegration of the tissues which are its instruments, requires as continual a reconstruction of them. The organizing force required for this reconstruction or maintenance, appears, like that employed in the original operations of development, to be supplied by Heat ; and it is a confirmation of this view, that we should find a provision (in those classes of animals which are constructed for the greatest development of nervo-muscular power) for the maintenance of a constantly high temperature, by the combustion of a portion of the organic compounds supplied to them as food.—Of the amount of *light* which is appropriated by Animals, we have no means of forming an estimate ; but from the limited nature of its action on their economy, it probably bears an insignificant proportion to that which is applied to the purposes of vegetable nutrition. Thus, then, the forces on which the animal is essentially dependent, are the *affinities* which hold together the elements of its food, and which are embodiments (so to speak) of the light and heat by whose agency they were combined ; the *heat*, which it derives in part from the physical universe, and in part from the combustion of some of its alimentary materials ; and the small amount of *light* required by them, which is supplied from external sources alone. These forces may be considered as in a state of *continual restoration* during the whole life of animals, in the heat, light, and electricity, and still more in the motion, which they develope ; and, after their death, in the production of heat and light during the processes of decay. During animal life there is a continual restoration to the inorganic world of carbonic acid, water, and ammonia ; and the amount thus given up by the animal organism bears an exact proportion, on the one hand, to the amount of heat and motion which are generated by it, and on the other to the amount of organic compounds consumed as food. So that, on the whole there is strong reason to believe that *the entire amount of force of all kinds* (as of materials) *received by an animal during a given period, is given back by it during that period*, his condition at the end of the term being the same as at the beginning. And all that has been expended in the building up of the organism, is given back by its decay after death.

In bringing this communication to a close, the author would remark, that he has not sought in it to increase the knowledge of existing *facts*, so much as to develope new

relations between those already known. He has preferred, in fact, rather to build upon the foundation afforded by the generally admitted facts of Physiological science*, than to go in search of phenomena, his account of which might be questioned by those indisposed to admit his leading ideas. If those ideas be correct, they will be found, he believes, to afford a precision to Physiological doctrines which they have never before possessed; and to open out a vast number of new lines of inquiry, which promise an ample harvest of results, not only valuable in a scientific view, but likely to be fertile in applications to various departments of the therapeutic art. At any rate, it is very important that Physiological science should be considered under the same *dynamic* aspect, as that under which the Physical sciences are now viewed by the most enlightened philosophers; and he trusts that the present attempt may thus aid in its advancement, even if it should answer no higher purpose.

Supplementary Note.—[Since the foregoing paper was written, the author's attention has been drawn to the fact, that Mr. NEWPORT had been led, in the year 1845—"by the close relation shown by Dr. FARADAY to subsist between light and electricity, and by MATTEUCCI between electricity and nervous power, and by the known dependence of most of the functions of the body on the latter,"—"to consider light as the primary source of all vital and instinctive power, the degrees and variations of which, he suggested, may, perhaps, be referred to modifications of this influence on the special organization of each animal body." (See "Athenæum" for Dec. 6, 1845.) These views were embodied in a paper "On the Natural History of *Meloe*," presented to the Linnæan Society, and printed in the 20th volume of its Transactions. But as the passages in which they had been enunciated were omitted by the desire of the Council of that Society, no other public record of them exists than that just cited.—Nov. 20, 1850.]

* [To his mode of stating some of these facts, the author is aware that exceptions may be taken; but he trusts that it may be perceived that his argument is a *cumulative* one, and that his conclusions rest upon a large number of *independent* probabilities. Consequently, even if *some* of his data should be found questionable, it does not follow that the validity of his general doctrine is disproved.—Nov. 20, 1850.]

XXXVII. *On the Condition of certain Elements at the moment of Chemical Change.**By* BENJAMIN COLLINS BRODIE, *Esq.*, *F.R.S.*

Received June 6,—Read June 20, 1850.

THE experimental inquiry which I now lay before the Society is so intimately connected with certain theoretical considerations, in which it took its rise, and which are necessary to its right comprehension, that I am unwilling to separate them. These considerations alone can explain why it appeared to me desirable to devote much time and labour to the determination of a simple analytical problem, which it has been open for the last thirty years to any chemist to undertake, but which, although doubtless connected with some of the most curious and obscure phenomena of chemical science, no one has thought it worth his while to enter upon. The reason of this may have been, that from other points of view this inquiry seemed of little importance, or, which is also probable, the question may have been, at various times, partially investigated, and the answer to it thought to be other than what it truly is, because approached from a wrong side. I shall therefore lay before the Society theory as well as experiment; for the theory is necessary to render the experiment intelligible, although this latter, in so far as it is true, has an independent value, and may be explained by others in some totally different manner.

The difference which chemists draw between the chemical elements and all other bodies, is far greater and indeed of altogether another kind to that which exists between any two compound substances. Other bodies are composed and decomposed; but applied to the elements, these words are altogether inappropriate; when the element is formed there is no chemical synthesis, and when it passes from the free to the combined state, there is no chemical decomposition. This difference the atomic theory expresses by assigning to the two classes of bodies a different molecular constitution. The element it considers as consisting of single and isolated atoms, and all other bodies as systems more or less complex of combined particles. This fundamental difference of conception is well given in the following passage from BERZELIUS* :—“ Les atomes d'un même corps élémentaire ne possèdent aucune force de combinaison mutuelle; ils n'adhèrent ensemble qu'en vertu de la force d'agrégation. Plus deux corps élémentaires se ressemblent quant à leurs propriétés chimiques, moins ils tendent à s'unir, et le nouveau corps qui résulte de leur combinaison, a tant d'analogies avec ses éléments constituants, qu'il diffère peu d'un mélange mécanique. Plus, au contraire, les corps élémentaires diffèrent de propriétés chimiques, plus la

* BERZELIUS, *Traité de Chimie*, Paris, 1845, vol. i. p. 25.

force de combinaison qu'ils exercent les uns sur les autres est grande et plus aussi les propriétés de leurs composés diffèrent de celles de leurs éléments. C'est là un problème dont je ne pourrais entreprendre la solution qu'en traitant de l'influence de l'électricité sur la matière."

This view, although the received doctrine, has not passed quite unquestioned. AMPÈRE invented a molecular theory, which led him to conclusions inconsistent with it. For reasons not of a chemical nature only, but which had reference principally to the propagation of light and sound and to other physical phenomena, he had arrived at the idea that in every chemical substance there were what may be called three forms of matter, the indivisible atom, the molecule or system of atoms, and the particle or system of molecules. He conceived, moreover, that every gas contained in the same space an equal number of these molecules. From this it was a necessary inference, that in that contraction which takes place when oxygen and hydrogen combine to form water, a division must take place of the elemental molecule. He considered that the oxygen divided, half a molecule of oxygen combining with each molecule of hydrogen*. On this view therefore the atoms, even of the elements, formed what was in a certain sense a compound group. The discovery of isomeric forms of hydrocarbon in different states of condensation, and of other similar facts, gave rise to new ideas as to the possible differences of bodies, and explanations similar to that by which the differences between certain isomeric organic bodies have been accounted for came to be applied to the case of the elemental bodies themselves. Thus the allotropy of sulphur has been explained by assuming it, in its various forms, to be the same substance in different states of condensation, in which case the difference between these forms might be expressed by giving to them the different chemical formulæ of S , S_2 , S_3 . Those physical relations however of density and specific heat, which might give a true scientific value to such speculations, and prove that these substances were really thus connected, have not yet been made out to exist; nor indeed has any fact been discovered which places such a notion beyond a conjecture. GRAHAM again, to explain the mode in which the metals conduct electricity in the voltaic circuit, assigns to these bodies what he terms a sali-molecular structure, and regards them as consisting of two atoms of a chlorous or acid combined with one atom of a zincous or basile element, "the three atoms of the molecule being of one metal and of the same nature†." The latest work in this direction is a paper of M. LAURENT, in which he has attempted, among other things, to account for the differences in the different classes of salts of the same metal in which, hitherto, different oxides of a different degree of saturation have been supposed to exist, by assuming them to contain different molecules or atomic groups of the metal; and he has shown how, on this idea, our classification of chemical substances may be much simplified; on this

* *Annales de Chimie*, vol. lviii. p. 434. See also GERHARDT, *Comptes Rendus des Travaux de Chimie*, 1847, p. 90, note.

† See GRAHAM's *Chemistry*, Ed. 1842, pp. 226 and 541.

view it is necessary to suppose that the present elemental atoms are susceptible of a yet further division*.

These ideas, however philosophical and suggestive, are yet, it must be allowed, very hypothetical. The proof of the compound nature of a chemical substance is of a very simple kind. It lies in the fact, that it has been made by the composition of certain parts, or broken up into those parts, or at least in some phenomena which are supposed to be the evidence of this. Indeed the rational formula of a chemical substance is but a memorandum of its reactions, and a particular mode of expressing the law of the synthesis and analysis of the body, apart from which it has but little meaning. The true nature therefore and chemical formula of the elemental bodies, as of all other substances, is to be discovered by the study of the series of chemical changes in which they are formed, and by the phenomena which they present when they pass into the combined condition. There are even well-known facts of great importance in this point of view, some unexplained, and some, I conceive, misinterpreted.

The point which I shall seek to establish is this,—that at the moment of chemical change a chemical difference exists between the particles of which certain elemental bodies consist, perfectly the same in kind to that which exists between the particles of compound substances under similar circumstances, and on which the phenomena of combination and decomposition depend. That a peculiar chemical relation exists between two particles which combine, is generally admitted and expressed by the term affinity. The electro-chemical theory has defined more exactly in what this affinity consists, and states that the two particles are to one another in a positive and negative electric relation. But I do not know that it has ever been pointed out that this chemical relation—this affinity between the particles of a substance—is an essential condition of the decomposition as well as of the composition of the body. As I am about to infer a chemical difference between the particles of the element from the fact of their chemical separation, I must say a few words upon this point, and I shall simplify the whole question by stating briefly the mode in which I consider chemical change to be effected. I may do this sufficiently for my present purpose in the following propositions:—

1. That when two particles chemically combine, a certain chemical relation exists between them which is expressed by the terms positive and negative. The chemical difference of the particles I term the difference between their conditions in this respect.

2. That when chemical combination takes place between the particles of which any two or more substances consist, a chemical difference exists between the particles of each substance, so that the particles of the same substance are to one another in a positive and negative relation.

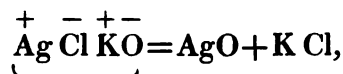
3. That the chemical relation between any two particles of these substances is

* *Comptes Rendus des Travaux de Chimie*, August 1849.

determined by the chemical relation of all the other particles with which they are for the time being associated. Substances, the particles of which are to one another in this peculiar chemical relation, I term chemically polar.

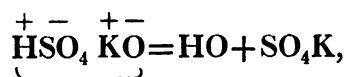
The electro-chemical theory was developed before even the true laws of the propagation of electricity had been discovered, and in the above propositions I have but transferred to chemical change some of the more exact ideas as to the nature of electric action which have since arisen; and we may transfer the ideas and mode of thought without making any hypothesis as to the mutual relation of the phenomena. This is not the place to enter fully on this question, and I shall confine myself to the application of these principles to explain certain phenomena of change in compound substances, to which I shall presently show the parallel in the case of the element. These phenomena are those which go under the name of the "phenomena of the nascent state."

Silver cannot be oxidized by the direct action of oxygen on the metal, but oxide of silver is readily formed by boiling the chloride of this metal with potash. The particles of oxygen and silver have therefore acquired by this association with the chlorine and potassium a chemical relation or affinity which at other times they have not. This is the fact. The rational conception of the fact is given in the expression



in which I have indicated the polar relation of the substances. The chemical relation therefore between the oxygen and silver is essentially dependent on the chemical relation between the oxygen and potassium, in the same way as a negative and positive electricity are related to each other. The same is true of the relation between any other two particles of the system. Hence chemical decomposition is an essential condition of chemical combination; so that when we see one of these events we may infer the other.

On the other hand, where this polar division of the substance cannot take place, there is no chemical action, or at any rate it takes place with greater difficulty; thus anhydrous sulphuric acid may (as has been shown by MILLON) be distilled off carbonate of potash without alteration, and generally the so-called anhydrous acids have none of the combining properties of the hydrates to which they correspond. The reason of this being, that when these bodies combine they do not decompose, and that it is by the very fact alone of the decomposition of the substance that the combining power is developed in the particles of which they consist, so that in the chemical change which is thus represented—

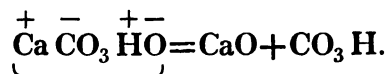


the two combinations which take place are not two combinations accidentally simul-

taneous, but correlative and mutually dependent phenomena which we cannot separate.

In the case of double decomposition, each of the four substances which enter into the change is combined, but it does not appear that this state of combination is necessary to the action. It can take place also, and in the same manner, when the combining substances are only in contact with each other, and not in combination, provided always that there is the right chemical difference between them, which however is essential. Thus, for example, when iodine and phosphorus decompose water (in the usual mode of the formation of hydriodic acid), the chemical relation between the iodine and phosphorus is an essential condition of the action. The same remark applies to the decomposition of water between nitric oxide and chlorine, which can be effected by neither body separately; so that the changes which take place in these experiments are not simply due to the fact that the chlorine or iodine stand in one relation to water, or to the elements of water, and the nitric oxide or phosphorus in another, and that thus the water breaks up, being acted upon by two opposite forces; but that there is also, and must be, a certain chemical difference between the chlorine and nitric oxide and between the iodine and phosphorus, which is as essential and important a condition to the propagation of the action as their relation to the water itself, and indeed without which they could not have this relation. I am not aware that this remark has before been made, nor do I think it likely that it should have been made, but upon the view which I have given, of which it is a consequence.

Facts corresponding to these cases of composition are to be observed, as might be expected, in the decomposition of bodies, for if decomposition be the condition of combination, so of course must combination be the condition of decomposition. FARADAY long since showed that dry carbonate of lime withstands the highest temperatures and is not to be decomposed by heat, but that when a little steam is thrown upon the heated carbonate, decomposition takes place with facility. Why is this? but that the water is the medium for the transference of the polar action which can now take place with the division of the masses: thus—

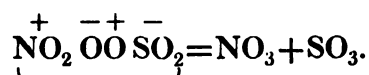


I might mention many other examples of the same class.

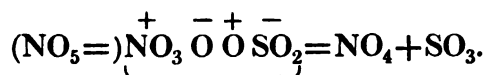
On the view which I have here given of the nature of chemical change, the very existence of the elemental bodies was a strange and unaccountable anomaly. I have regarded the molecular structure of bodies but as an expression of the law of their synthesis and analysis, and this law again as a result of the peculiar nature of chemical force. It was therefore truly difficult to conceive how an element in the sense of BERZELIUS was formed. The formation of this “uncombined particle” was a fact quite different to the formation of a compound substance, and yet it seemed as unreasonable to suppose that the laws of chemical action should vary in different

materials, as that the laws of motion should be different in different bodies. I therefore considered attentively what we really knew of the laws of chemical change in these bodies. Examples occurred to me, both of the chemical division and chemical synthesis of the elements, by which various phenomena hitherto obscure might be explained. The experiments of the first class prove that a division of the elemental bodies which is known to occur in certain cases of chemical change, is truly a chemical and not simply a mechanical division of these substances. This is shown by the fact that the particles of the element thus separated show the peculiar combining properties of "nascent bodies." It is not necessary to my argument that the precise view which I have given of the nature of this "nascent state" should be admitted; provided only it be allowed, that these properties depend upon the fact that the particle is issuing from a state of combination, which is generally allowed.

1. No theory of chemical change has given rise to more discussion among chemists, than the usual mode of the formation of sulphuric acid by the mutual action of sulphurous acid, nitric oxide, air and water. On this question there are some six recognized theories. The problem is simply this: Why, when the oxygen is made a part of this system of particles, does it possess oxidizing properties which otherwise it has not? On the view I have stated the cause is plain. When nitric oxide acts chemically upon oxygen, the gas is thrown into a polar condition; the result of which is to give to other particles of the mass a combining power, in a direction the reverse of that in which the oxygen combines with the nitric oxide; the change being in all respects analogous to the decomposition of water by the joint action of nitric oxide and chlorine*; thus—



The best of the other explanations of this fact is, in my opinion, that of PÉLIGOT†, who considers the formation of the sulphuric acid to be the result of the successive formation and decomposition of nitric acid. This, however, does but shift the difficulty to another point; for why does nitric acid oxidize sulphurous acid? In truth, in this case, a perfectly similar polarization takes place within the acid itself—

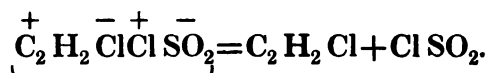


* In the following experiments I am compelled to call by the same name three very different things—the isolated element, the particles of the element at the moment of their chemical separation or synthesis, and the combined element; and I wish to observe, that when I state that there is a chemical difference between the particles of the element, I mean simply the particles of which the element consists. The chemical nature of these particles is a further question.

† *Annales de Chimie*, 3rd Series, vol. xii. p. 266. PÉLIGOT states that nitric oxide always forms hyponitric acid and no nitrous acid, in contact with atmospheric air; but it by no means follows, even if this be the substance formed with atmospheric air alone, that the same substance is formed in the presence of sulphurous acid; indeed, on the view I have given, a difference in the reaction would rather be anticipated.

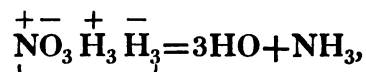
For the formation of the hyponitric acid from the nitric acid is just as much the formation of a new chemical substance as if it were made by the direct action of oxygen upon nitric oxide, and is attended with a similar division of the oxygen. But this stage of the formation of nitric acid is useless, and not necessary to the conception of the change; for there are other perfectly parallel cases which do not admit of such an explanation.

2. I will take the formation of chloro-sulphuric acid, in the remarkable experiment by which REGNAULT prepared this body*. Olefiant gas, as usually prepared from alcohol and sulphuric acid, contains a large quantity of sulphurous acid; when chlorine is brought in contact with this mixture of gases, two substances are formed, chloro-sulphuric acid, SO_2Cl , and the Dutchman's liquid, $\text{C}_2\text{H}_4\text{Cl}_2$. Upon sulphurous acid alone (in the circumstances under which the experiment is made) chlorine has no action whatever, nor, according to REGNAULT, upon olefiant gas, when both gases are dry, and only in diffused daylight. The formation, therefore, of this body is due to the polar division of the chlorine, just as the formation of sulphuric acid to the polar division of the oxygen; the olefiant gas being to the chlorine in the same relation as the nitric oxide to the oxygen in the other experiments, so that we may conceive the change to take place thus—



This example is not open to those objections which, from the formation of the oxides of nitrogen, might be raised to the other instance, for here the combinations mutually determine each other. With water, the chloro-sulphuric acid decomposes with the formation of sulphuric and hydrochloric acids; but through this experiment we can distinctly trace back the cause of the formation of the sulphuric acid, in this, as in the more usual mode of its formation, to the polarization of the element; and on considering attentively the mode of the formation of chemical substances, it may be seen that the formation of a large class of compounds, among which are the oxides of chlorine and iodine, is ever preceded by a similar fact.

3. It is well known that when a mixture of hydrogen and any oxide of nitrogen† is passed over heated spongy platinum, the oxide of nitrogen is decomposed and ammonia and water formed. Were the hydrogen a compound substance, it would be thought that the simultaneous formation of these substances was sufficiently explained, by saying that it was a case of double decomposition, and it is indeed the same phenomenon; thus—

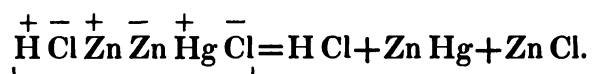


the hydrogen being “*nascent*” from itself.

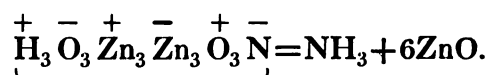
* Annales de Chimie, 2nd Series, lxi. p. 170, and lxxi. p. 445.

† See KUHLMANN, LIEBIG'S Annalen, vol. xxix. p. 286.

4. We know from the researches of H. ROSE, that in a neutral solution of the bichloride of mercury, the mercury is precipitated on zinc in the form of a black powder, but on the addition of an acid the metal becomes bright and the zinc is amalgamated*. The addition of bichloride of mercury also instantly stops the most violent evolution of hydrogen from the action of hydrochloric acid upon zinc, with the formation of the same amalgam. These curious experiments, which certainly caused much surprise in ROSE, become intelligible from the considerations which I have mentioned. The combination of zinc with mercury is a combination in a different direction to that of the zinc with chlorine, hence the latter determines and causes the first; thus—



5. A very beautiful illustration of the way in which one chemical combination determines another, is seen in another experiment with the same metal. Zinc, it is known, decomposes slowly, with evolution of hydrogen, a strong and boiling solution of caustic potash; but when into the solution a few crystals of nitre or nitrite of potash are thrown, the zinc is rapidly dissolved with the evolution of ammonia. The reason of this I consider to be, that the polar division of the zinc now takes place with the greatest facility, the zinc being oxidized in two directions; the zinc decomposes the water, by which a polarity is given to the hydrogen, which causes it to combine with the nitrogen of the nitric acid to form ammonia, by which combination the other particles of the nitric acid become in their turn polar and oxidize again the zinc; the action, as it were, proceeding from and returning back again to that element. The change, for example, may be represented thus:—



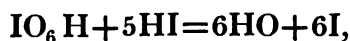
6. To the preceding experiments I will only add the phenomena of substitution, which, viewed in relation to them, assume a new form. The division of the chlorine which takes place in substitution is a very similar fact to that distribution of the element which takes place in the formation of chloro-sulphuric acid (which I have before cited), and admits of a similar explanation. The problem is, why, with even the smallest quantity of chlorine, does a portion of the chlorine ever combine with the organic body? Why is the hydrogen never simply removed? The solution is, that one of these two changes determines the other, precisely as the combination with olefiant gas determines the combination with sulphurous acid. Nor do I see what intelligible distinction is to be drawn between the action of chlorine on the organic body and that of hydrogen on nitric oxide, or between either of these two cases and that of water itself on chloro-sulphuric acid. The organic body behaves as though it were a simple compound of hydrogen on the one hand, and the remaining ele-

* LIEBIG'S Annalen, vol. lxiv. 283.

ments of the system on the other, and this, during the action and under the influence of the chlorine, it may truly be.

If it be true that in the preceding experiments this polar division of the element takes place, (a division, at any rate, in many points analogous to the decomposition of the compound body when no elemental substance is isolated, but the particles are transferred from one system to another,) it cannot but be, that in the *formation* of the element the correlative fact is to be observed, and that the element itself is made by the synthesis of polar particles. This is indicated by theory, and although the more usual mode of forming these elemental bodies is not such as to bring to light the true nature of this synthesis, there are other, although as yet rarer, instances of the formation of these bodies, which not only are not opposed to this view, but prove it.

1. When an acid is added to a perfectly pure solution of an alkaline iodide, such as may readily be procured by precipitating any iodate it contains by baryta water and filtering, the solution remains perfectly clear. Neither, when an acid is added to a pure solution of iodate of potash, is there any alteration, but on mixing these solutions an abundant precipitate of iodine is formed. It is said that the iodate and iodide of zinc, without acid, undergo a similar decomposition. It is very plain that these changes are simple cases of what is called double decomposition. The hydriodic and iodic acids decompose one another, and it is my opinion that in the change



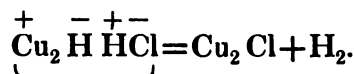
whatever be that combining relation which subsists between the oxygen and hydrogen, that same relation must also be between the particles of the iodine itself. What is commonly called the affinity of hydrogen for oxygen is not sufficient to account for this change, for hydrogen alone will not decompose the iodic acid, unless it be in the nascent condition. When hydrochloric acid is the decomposing body, chloride of iodine is formed, when hydriodic acid, the iodine itself. Are we not to admit that these two substances are formed according to the same law? To this synthesis of the iodine, the division of the element which takes place at the formation of the hydriodic and iodic acids, is the correlative fact, to explain which we must assume the same polar difference between its particles (see page 765).

2. In the course of his researches on the hypophosphites, WURZ discovered a very singular substance, the hydruret of copper*. This substance is formed by the action of hypophosphorous acid upon copper salts. It readily decomposes, so that it is not easy to determine its constitution, but his analyses very closely corresponded with the formula Cu_2H . That this is truly the formula of the substance, may be inferred with yet more certainty than from the analyses, from the following curious reaction.

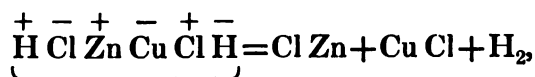
* BERZELIUS attempted to throw some doubt upon the existence of this body, but the correctness of the facts, as stated by WURZ, is guaranteed, not only by the well-known skill and ability of this chemist, but also by the fact that POGGENDORFF had succeeded in procuring a hydruret of copper by the electrolysis of the sulphate of this metal.

In contact with strong hydrochloric acid hydrogen is evolved, and the protochloride of copper, Cu_2Cl , formed. Both substances are decomposed, and WURZ satisfied himself, and gives the experiments, which show that the volume of the hydrogen evolved was the double of that due to the simple decomposition of the substance. I will add the remark which he makes on this experiment:—"On sait que l'acide chlorhydrique n'attaque le cuivre qu'avec une extrême difficulté, et la présence de l'hydrogène, loin de favoriser la réaction, devrait, d'après les lois de l'affinité, y ajouter un nouvel obstacle. La décomposition de l'hydrure de cuivre par l'acide chlorhydrique paraît donc s'effectuer en vertu d'une action de contact*."

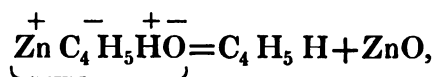
This fact, the conception of which offers these theoretical difficulties, becomes perfectly intelligible, and indeed might have been predicted from the principles I have laid down. The decomposition is perfectly similar to that of the suboxide of copper under analogous circumstances, arises from the same cause, and may be expressed thus—



This experiment enables us to see clearly the cause of a class of decompositions very analogous to it, and which have presented similar theoretical difficulties†. An alloy of platinum and silver will dissolve in nitric acid, which will not act upon the platinum alone; acids will, in like manner, dissolve the alloy of copper and zinc‡, which on the copper alone have no action. Now the hyduret of copper is itself, in its chemical relations, an alloy, and the action of the acid on the alloy of copper and zinc is a fact very analogous to the action of the hydrochloric acid upon this body, and the explanation of this fact involves similar phenomena; thus—



the polar composition of the hydrogen being essential to the comprehension of the experiment. A further confirmation of this view is found in the decomposition by water of the remarkable bodies discovered by FRANKLAND§, to which he has given the names of zinc-methyl and zinc-ethyl. The theoretical analogy of these bodies to the hydrogen compounds of the metal is perfect, and with water they give a precisely similar reaction to that of the hyduret of copper with hydrochloric acid. Zinc-ethyl, for example, breaks up thus—



in which decomposition the hydrogen and the hydrocarbon (which is the analogue of

* Annales de Chimie, 3rd Series, vol. xi. 251.

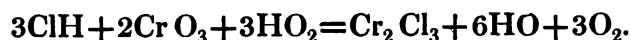
† These facts have been cited by LIEBIG with another view. LIEBIG'S Annalen, vol. xxx. p. 262.

‡ GMELIN'S Handbuch, vol. iii. p. 448.

§ Journal of the Chemical Society, January 1850.

hydrogen) are to one another in the same polar relation, and fulfill the same part in the change as the two equivalents of the hydrogen itself in the decomposition of the hydruret.

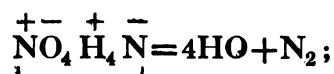
3. When a solution of bichromate of potash is poured into a strong and acid solution of the peroxide of barium in hydrochloric or nitric acid, a violent effervescence and escape of oxygen takes place; apart, these solutions are perfectly stable; bring them together, they are both decomposed; the chromic acid passes into chloride of chrome, the peroxide of hydrogen into water, and the oxygen formed is due to the simultaneous decomposition of both bodies, thus—



I certainly regard the oxygen itself, in this experiment, as the true reducing agent, and I believe that the chromic acid is decomposed by the oxygen of the peroxide of hydrogen, according to the same law of decomposition, and for the very same reason, as it would be by hydrogen itself if a piece of zinc were thrown into the acid solution, this reason being the polarity of the particle induced by chemical change. In this experiment is brought before us, in a very forcible manner, the very slight difference which truly separates the phenomena of oxidation and reduction, which are usually regarded as so distinct. In speaking of the formation of sulphuric acid, I have said that the oxidation of sulphurous acid by nitric acid is to be attributed to a polar condition of the oxygen liberated from the nitric acid, which polar condition is induced by the formation of hyponitric acid within the system of which it forms a part. Now the formation of water in the peroxide of hydrogen is a fact precisely similar to the formation of the hyponitric acid in the nitric acid, and it might reasonably be anticipated that this oxygen would have a similar oxidizing power. This is in truth the case; for when the temperature is low, the solutions dilute, and the experiment carefully managed, no gas is given off, but a deep blue solution is formed, containing perhaps, as stated by BARRESWIL*, who first examined this reaction, a higher oxide of chrome. This solution however rapidly decomposes, oxygen is evolved, the blue colour disappears, and the final result is the same as if the compound had never been formed. When manganate of potash, peroxide of manganese and various other substances are substituted for the chromate of potash in this experiment, a similar decomposition takes place, but without any signs of the intermediate stage of oxidation. Under other circumstances the oxidation and reduction may be separated. Thus, when peroxide of barium is thrown into an acid solution of the prussiate of potash, the prussiate is oxidized and the red prussiate is formed. When, on the other hand, the same experiment is made with the alkaline or neutral solution of the red prussiate, oxygen gas is given off in abundance from the peroxide of barium as well as from the red prussiate, and the latter passes into prussiate. On adding to this solution an acid solution of perchloride of iron, care being taken that

* Annales de Chimie, vol. xx. p. 364.

the whole of the peroxide of barium is decomposed, prussian blue is produced in abundance. In this experiment one atom of oxygen is first added, and this atom is then removed by the contact of another atom under suitable circumstances. I infer therefore from these experiments, not that the oxidation is a necessary antecedent to the disoxidation, but that the spontaneous decomposition of the oxidized body and the reduction of the chromic acid, where no oxidation takes place, and the oxidation of the body, are due to one and the same cause, namely, to the mutual attraction of the elemental particles. When, for example, arsenious acid is oxidized by the action of chlorine, I consider that the oxygen is oxidized as well as the arsenic. We are led to the same conclusions by various well-known experiments; in the case, for example, of the decomposition of the nitrate of ammonia on boiling, when this compound breaks up into water and nitrogen, thus—



while from no point of view is it very logical to draw a distinction between two facts so very similar as the formation of the water and the nitrogen, it is proved, by the experiments I have cited, that the mutual attraction of the particles of the nitrogen is a most important condition of the change.

The transition from these cases of spontaneous decomposition to other instances, in which no one of the substances usually called compound is formed, but the elements alone are liberated, is obvious; and I cannot but think that these phenomena, viewed as the last of a series of chemical changes, proceeding from the simplest case of double decomposition to the curious experiments I have just cited, each successive link of which is bound to the preceding by the closest analogies, reasonably admit of a very different interpretation to that which we should put upon them when regarded out of this connection. It is hard to draw a distinction between the decomposition of the chloride of nitrogen and of the nitrite of ammonia. From quite independent considerations, the conclusion is forced upon us, that the mutual attraction of the particles of chlorine and of nitrogen plays a most important part in other cases of chemical change. Why may we not admit that in this case the decomposition is determined solely by these relations? The heat which attends upon this and other similar decompositions is, on the electro-chemical theory, as confessed by its greatest supporters, an inexplicable enigma. Heat is the constant sign of combination, but here it asserts is only decomposition, so that theory and fact are plainly at issue. On the other hand, admitting that in this experiment the formation of the elements is a true chemical synthesis, the evolution of heat is accounted for; and as the elements, chlorine and nitrogen, are far more permanent forms of matter, and less readily altered by chemical action than the chloride of nitrogen, so it is reasonable to believe that the particles of which they are composed are in a far more intimate state of combination. The transition from these experiments to the most ordinary cases of the formation of

the element is obvious, it being plain that the reduction of oxide of silver* into oxygen gas and the metal, differs only from the spontaneous decomposition of the chloride of nitrogen in the temperature at which it takes place.

In the preceding statement, I have used the word polar to express that alternate difference of the condition of the particles by which I conceive chemical action to be propagated. It is no objection to the use of this word that it is undefined, and that I have not pointed out in what this difference consists. It may be a real difference, which we can use for purposes of thought and inquiry, and yet an unknown difference, to which we cannot at present assign a true value. A more serious objection lies in the way in which this word has been misused, and the false associations which, in some minds, are connected with it. It is, however, not difficult for those who must ultimately fix the meaning of this term, to draw the distinctions necessary to its right application; and as I myself have found the word very useful, and also found that through the associations which are rightly connected with it, it conveyed to some persons, whose opinion was well worth considering, an idea which could not otherwise have been so simply given, I have here used it. At the same time let me observe, that that to which I wish to direct the attention of chemists, is to a new analogy between certain chemical changes and to the correlation of phenomena which have not before been grouped together, and, provided that analogy and relation be recognized, the word by which it is expressed is of less importance†. I now proceed to an experimental inquiry which originated in the considerations I have here given.

The discoverer of the peroxide of hydrogen, THÉNARD, observed certain singular and remarkable properties of this compound, of a kind altogether new and calculated to fix the interest and attention of chemists. To prepare the peroxide of hydrogen, according to his directions, is a process so troublesome and tedious, that it may have deterred chemists from the further and full investigation of these phenomena. It is at any rate to THÉNARD alone that we owe whatever knowledge we possess of this singular body. Since his investigation, which dates from the year

* In certain cases we can trace the very mode in which this decomposition by heat takes place. Thus in the decomposition of chlorate of potash by heat, the true way in which this substance is decomposed is, as discovered by SERULLAS, the decomposition of one particle by the next. The chlorate is first oxidized to perchlorate, and this perchlorate, as shown by MILLON, again reduced to chlorate with evolution of oxygen. Thus the action proceeds by a continual oxidation and reduction of the substance, the phenomena being very similar to the oxidation of the chromic acid by the peroxide of hydrogen and the spontaneous decomposition of the compound formed. By mixing the chlorate of potash with oxide of copper, the formation of the perchlorate is entirely prevented, and the action converted into one of spontaneous decomposition.

† The idea of polarity has been applied to the explanation of chemical phenomena by other chemists as well as myself. I may refer especially to LÖWIG's Introduction to his Organic Chemistry (vol. i. p. 1. Edition 1845). GRAHAM also has given, in the last edition of his Treatise on Chemistry, a chapter on Chemical Polarity. It would be out of place to enter here on any criticism of their views or comparison of them with my own, but I give the reference for those who may be desirous to see the way in which others have treated this subject.

1818, no new fact of the slightest importance has been added to its history. The properties however of which I speak, can all be observed with the solution of the peroxide of barium, in hydrochloric or acetic acid, which can be readily prepared. The properties of this solution are different according as it is alkaline or acid. The alkaline solution is of an unstable nature; even at ordinary temperatures it continually loses oxygen, and on heating undergoes rapid decomposition. The acid solution is more permanent, may be long kept without sensible alteration, and may even be heated to the boiling-point with no evident evolution of gas. Either solution, however, is violently decomposed when certain substances (among which finely divided carbon or platinum are very effective) are thrown into it. In these cases the peroxide is decomposed, while the carbon and platinum remain, as far as we know, unaltered. There are however other bodies which cause this decomposition with perhaps greater energy, and themselves undergo a chemical change of the most surprising nature; these bodies are all those metallic oxides which can readily be reduced,—the oxides of gold, and silver and mercury, and the peroxides of lead, manganese, nickel and other similar substances. These bodies decompose the solution, as does platinum or carbon, while the substances themselves also, during the decomposition, lose oxygen, and are reduced either to a metal or to a lower degree of oxidation. The solution, whether acid or alkaline, is ever decomposed by these bodies; but the facility with which the substance itself is reduced, and in some cases whether this reduction takes place at all, depends upon the neutral or acid condition of the solution, and varies with the particular substance taken in a way on which it is not now necessary to dwell. The decomposition caused by platinum and carbon, however strange, is not quite without parallel. But the simultaneous reduction of the peroxide of hydrogen and the oxide of silver is a solitary fact, by the side of which chemists have been able to place scarcely an analogous much less a similar instance. Yet eminent chemists have offered an explanation of these phenomena, and if these are adequate, further inquiry is unnecessary. I shall not criticise the absolute or the relative value of these explanations; but they add so much to the interest of the question, so clearly show the difficulties under which chemical theory has laboured in dealing with these facts, and also, as I believe, how inadequate it has proved to meet them, that I shall briefly mention the most remarkable.

The following quotation gives the impression made by these facts upon THÉNARD, who first discovered them:—

“Quelle est la cause des phénomènes que nous venons d'exposer? Voilà maintenant ce qu'il s'agit de rechercher. Pour cela qu'il nous soit permis de rappeler ceux que présentent l'oxide d'argent et l'argent avec le nitrate oxigéné neutre du potasse. L'argent très divisé dégage rapidement l'oxigène de ce sel; il ne s'altère point, et le nitrate oxigéné devient nitrate neutre.

“L'oxide d'argent dégage plus rapidement encore que l'argent l'oxigène du nitrate oxigéné; lui-même est décomposé, il se réduit, l'argent se précipite tout entier, et l'on

ne retrouve dans la liqueur que du nitrate neutre de potasse ordinaire. Or, dans ces décompositions, *l'action chimique est évidemment nulle* ; il faut donc les attribuer à une cause physique ; mais elles ne dépendent ni de la chaleur, ni de la lumière ; d'où il suit qu'elles sont probablement dues à l'électricité. Je chercherai à m'en assurer d'une manière positive ; je chercherai aussi à savoir si la cause, quelle qu'elle soit, ne pourrait point être produite par le contact de deux liquides, et même de deux gaz. De là, découlera peut-être l'explication d'un grand nombre de phénomènes*."

An idea as to the possible cause of these phenomena was thrown out, some years back, by another distinguished French chemist, DUMAS, which, although but an incidental observation and unaccompanied with any experimental research, has a special interest as giving the scientific conception of these facts from the electro-chemical point of view, from which this chemist then regarded them.

"La répulsion que les molécules de même signe exercent l'une sur l'autre, répulsion qui nous a servi à expliquer plus haut les effets qu'on observe dans la formation des composés multiples, va nous servir maintenant à expliquer aussi ceux que l'on observe entre deux corps qui contiennent un excès de molécules semblables. Ces corps tendent à se décomposer mutuellement par suite de cette action répulsive.

"En effet c'est ainsi qu'on peut se rendre compte de l'action des acides sur beaucoup de peroxides qui perdent sur leur influence une partie de leur oxygène, c'est ainsi qu'on peut concevoir l'action si bigarre et si remarquable de l'eau oxygénée sur certains oxides. Ce composé perd par le contact de l'oxide d'argent, par exemple, la moitié de son oxygène repasse à l'état d'eau, chasse l'oxygène de l'oxide et le ramène à l'état métallique. Quant on envisage ce simple fait avec les anciennes idées de l'affinité, il est inintelligible, tandis qu'avec les idées électriques il pouvait en quelque sorte être prévu†."

Another view has been put forward by LIEBIG. These experiments with the peroxide of hydrogen he places by the side of the fermentation and decay of organic bodies, of which chemical changes he thus gives the cause‡:—"The cause," says he, "is the power which a body possesses in the act of decomposition or composition to elicit the same action in a body in contact with it, or to render it capable of undergoing the same change which itself experiences. Thus the decomposition of the ferment decomposes also the sugar, and the decomposition of the peroxide of hydrogen decomposes the metallic oxide." The simultaneous decomposition of the two substances is the fact. That it is the very chemical change itself in the one instance which determines the chemical change in the other ; this relation LIEBIG has pointed out. But why and how, consistently with what we otherwise know of the nature of chemical action, and the mode of its transference and propagation, this can be the

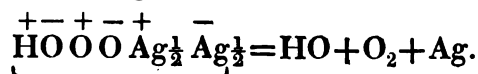
* Annales de Chimie, 2nd Series, vol. ix. p. 98 ?

† DUMAS, Chimie Appliquée aux Arts, vol. i. Introduction, p. 64.

‡ LIEBIG's Annalen. vol. xxx. pp. 280, 262 and 279.

case, is a problem which remained to us, and which, in the case of one at least of these experiments, it was my hope to solve.

Hitherto, in the breaking up of the oxide of silver and the peroxide of hydrogen, no chemical fact has been recognised but the simultaneous decomposition of two chemical substances. This is that fact for which it has been so difficult to account. Were it, for example, a hydride of silver which was thus decomposed and water formed and not simply oxygen, the experiment would have attracted no attention. On the view I have given, the formation of the oxygen itself is as truly a chemical synthesis as the formation of water, and may be substituted for it in a chemical change. This supplies an explanation of these facts at once adequate and simple; the oxide of silver in this experiment being reduced by the oxygen of the peroxide of hydrogen, just as in other cases it might be by hydrogen; the formation of the silver from the particles of which it is composed being the corresponding fact in the decomposition of the oxide of silver to the formation of water in the peroxide, so that we may represent the change thus:—



No one can be more sensible than I am of the wide interval which exists between such a mode of representing a chemical change and an ascertained fact. The very form in which I have just expressed the decomposition involves an important assumption, an assumption indeed on which, in my opinion, the whole question rests. If it be true that these bodies are thus decomposed in definite and equivalent proportions, and that the simultaneous decomposition of the bodies proceeds according to this law, it is impossible to deny the chemical relation of these changes and the mutual chemical action of the substances; and, on the other hand, if the formation of this oxygen is to be regarded as a true chemical synthesis, this synthesis must follow the universal law of the formation of chemical substances, and the masses must combine in definite and equivalent proportions. It is only the apparent absence of this relation between the decomposing bodies which removes fermentation from ordinary chemical changes, and causes its chemical nature to be denied; and in that parallel which LIEBIG draws between these decompositions and fermentation, it is tacitly assumed that in this case also, as well as in the case of fermentation, no such relation exists. The following words of LIEBIG, from the paper I have already referred to, clearly show this point:—"A certain quantity of ferment is required to cause fermentation in a portion of sugar; its action however is *no action of mass*, but is entirely limited to its presence up to that moment when the last atom of sugar is decomposed. It is therefore no peculiar body, no substance or matter which effects decomposition, but these are the carriers of an activity* which extends itself over the sphere of the decomposing body."

* "Träger einer Thätigkeit." LIEBIG's Annalen, vol. xxx. p. 279.

It was therefore the quantitative relation of the decomposing substances which seemed to me the essential point to be determined, and which experiment alone could decide; for it was quite possible (and indeed had been taken for granted) that the action varied according to some other law. It might have none of the characters of a chemical change; it might, for example, vary directly with the acting masses, or with the temperature alone, or be a function, so to say, of so many variables that the true law of action would be altogether hidden.

Preparation of the Peroxide of Barium.

The baryta used in the following experiments was prepared by the ignition of the pure nitrate in a crucible of fine white earthenware. This was protected externally by a common earthenware crucible, and the space between the two filled up with crucible dust. The nitrate was thrown gradually into the crucible, one portion being thoroughly decomposed before the addition of the next. Baryta, as commonly prepared, contains large quantities of peroxide, as may readily be ascertained by dissolving in hydrochloric acid and adding bichromate of potash to the solution; and a very long and strong ignition is necessary to drive off the last portion of oxygen. The purest baryta is not porous, but perfectly compact, very heavy, and of the crystalline grain of fine marble. The baryta thus prepared contains traces only of matter from the crucible, the adhering particles of which are to be carefully broken off, and hardly any impurities, but a little water, and a very little carbonic acid. If an ordinary Hessian crucible be used for its preparation, as was the case with the baryta used in some of the earlier of my experiments, the baryta will be very impure.

To prepare the peroxide, oxygen was passed over the baryta, broken into pieces of the size of a small pea and placed in a combustion-tube, which was heated in an ordinary charcoal trough. The oxygen was made from peroxide of manganese and carefully freed from carbonic acid, and dried before entering the tube. The gas was contained in a gas-holder so as to regulate the current, and was passed over the peroxide, after all absorption had ceased, until the tube was cool. The temperature necessary for the absorption is a low red heat, which is not to be exceeded, as a portion of oxygen is given off at a higher temperature. The operation may be distinctly followed through the tube, the baryta glowing when the stream of gas comes in contact with it. The absorption is both rapid and complete; and I have repeatedly made the experiment of attaching to the end of the tube, where the oxygen not absorbed would pass out, a small tube dipping into mercury, and have found that the experiment can readily be so regulated that a considerable stream of gas will enter at one end of the tube and not a bubble pass out at the other; and indeed the absorption was often so rapid that the gas was absorbed faster than it entered, and the mercury rose several inches in the tube; the whole oxygen therefore is absorbed.

The peroxide presents after the experiment a mass of uniform texture and appearance, and is very white, the baryta itself being of a darker colour. A little of the

powdered substance, moistened with water on the back of the hand, gives no sensible heat. The peroxide was carefully freed from any adhering glass, or pieces in which the absorption was incomplete, from which it may be distinguished by the colour, and was preserved in stoppered bottles over sulphuric acid; a precaution which was desirable, as the preparations had to be preserved for a long time without any alteration in the per-centage of oxygen.

The peroxide of barium thus obtained is a perfectly stable body. When dry, it is only a very intense heat which can drive off its oxygen. It is nearly insoluble in water, the filtered solution giving no gas with oxide of silver, which would be the case if any substance were dissolved; and when preserved from the action of the carbonic acid of the air, it may be long kept even in water without any sensible evolution of gas. When the powdered substance is even boiled with water, no gas is given off; and after long boiling, on examining the dried residue, I have found that the effect of boiling has actually been to increase the per-centage of oxygen, the baryta, of which a certain quantity is always present, being dissolved. These facts are contrary to the usual statements*, and probably this stability only belongs to a very pure substance, for a small quantity of the oxides of iron or of manganese would alter these reactions.

The peculiar experiments of the reduction of the metallic oxides, as stated by THÉNARD and other chemists, are referred to the peroxide of hydrogen; and it is on the peculiar instability of this body that the explanation of LIEBIG of these phenomena rests, the spontaneous decomposition of the peroxide being considered as antecedent to and necessary to set up the action in the other substances. I was however anxious to see whether the peroxides of metals themselves would not produce similar effects; and on placing the peroxide of potassium (the mass which is produced by the action of potassium on melted nitre) in contact with moist chloride of silver in water, I found that the chloride was reduced, just as it might be by zinc, and oxygen evolved; and the same takes place with the iodide, bromide, cyanide, nitrate and oxide of silver and with other metallic combinations. Water alone, as is well known, decomposes the peroxide of potassium; this body therefore was in a similar unstable state to that supposed in the peroxide of hydrogen; but on extending my experiments to the peroxide of barium itself, I found that all the reductions which could be made with the peroxide of hydrogen, took place with this body also with the greatest facility. Finely-divided platinum, silver or carbon, decompose it, but far less rapidly than those substances which are themselves also decomposed. I will not here discuss the general chemical reactions of this peroxide, but defer their consideration until I can treat with advantage, in a more exact manner, of the general theory of this action.

The facts I have just mentioned gave a direction to my experiments, and instead of the peroxide of hydrogen, the preparation and preservation of which involved many difficulties, and with which it was inconvenient to work, I determined to use, in the experiments I proposed, the peroxide of barium itself.

* See Barium superoxyd, Handwörterbuch der Chemie, vol. i. p. 667.

Before the study of these decompositions was possible, it was necessary to have some ready and accurate method of determining the amount of oxygen in the various preparations of the peroxide, which could never be obtained absolutely pure and corresponding to a theoretical formula, and of which I cannot indeed find that any satisfactory analysis has yet been made.

After various experiments I selected two methods, which combined the advantage of great facility of execution with perfect agreement in their results. One of these methods depends on the chemical change which takes place when the peroxide of barium is brought in contact with an acid solution of chromic acid, when, as I have before mentioned, both the peroxide and the chromic acid are decomposed with evolution of gas. Supposing this to be a definite and constant reaction, we have, it is evident, in the evolved gas, a measure of the oxygen in the peroxide, from which, if the reaction were known, the latter might be calculated. For this, however, it was necessary first to discover the nature of the decomposition. The other method depends on the decomposition of the acid solution of the peroxide by finely-divided platinum or carbon, in which case the oxygen evolved is plainly half the total oxygen in the peroxide itself. For the determination of the oxygen in the preparation of a small quantity of the peroxide, it was quite practicable to weigh the baryta before and after the absorption of the gas; and although this plan could not be applied to those larger quantities of the substance which it was desirable to prepare at once, yet, as there could be no doubt as to the general accuracy of the determination if the experiment were conducted with care, I availed myself of it for determining the reaction in question with chromic acid, and for a general control over the methods. The baryta was placed in a platinum tube about 8 inches long and half an inch wide, and the whole experiment conducted precisely in the manner already described in the preparation of the peroxide. The tube was first weighed empty, the ends being closed with dry corks, again with the baryta, and again after the experiment. The peroxide was then rapidly pounded and used for the other determinations. A glass tube cannot be used for this experiment, as the glass is always slightly acted upon, where in contact with the baryta, which, although of little consequence in an experiment on a large scale, as causing an impurity in the peroxide, yet gives rise to a discrepancy between the amount of oxygen calculated from the absorption and the percentage of oxygen deduced from the other experiments.

Two experiments, conducted in this manner, gave the following results:—

A. 12·581 grms. of baryta increased in weight 0·952 grm., corresponding to an increase in weight of 7·53 on 100 parts.

B. 15·180 grms. of baryta increased in weight 1·111 grm., corresponding to an increase in weight of 7·318 on 100 parts.

Hence the two preparations of peroxide of barium contained respectively—

Preparation A, 7·03 per cent.; preparation B, 6·81 per cent. of oxygen in addition to the oxygen of the baryta.

The experiment with the acid solution of chromic acid was made thus :—a weighed quantity of the peroxide was mixed with a very large excess of powdered bichromate of potash in a small flask, such as is often used for the determination of carbonic acid, provided, that is, with a drying tube (in this case filled with small pieces of caustic potash) and a small tube reaching to the bottom of the flask, through which a little air might be drawn after the conclusion of the experiment. Strong hydrochloric acid was contained in the usual small tube, and the whole experiment, in short, conducted precisely as a carbonic acid determination. It is essential to the accuracy of the experiment that care should be taken to have present a great excess of bichromate (at least three or four times the weight of the peroxide taken), an excess of acid, and also to have fresh potash in the drying tube, as there is a considerable rush of gas, and a little carbonic acid or water might otherwise pass through the tube. The peroxide, however, in a good preparation contains a mere trace of carbonate; and I have made the experiment of passing the gas from the potash tube through baryta water, and have found that no carbonic acid escaped the apparatus, with a far more violent evolution of gas than need take place in the experiment.

I. 2·1015 grms. of the preparation A. gave a loss of 0·243 grm., corresponding to a loss of 11·56 on 100 parts of the substance taken.

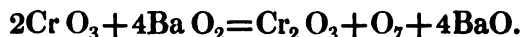
II. 2·1265 grms. of the preparation A. gave a loss of 0·252 grm., corresponding to a loss of 11·85 on 100 parts of the substance.

III. 2·2127 grms. of the preparation B. gave a loss of 0·268 grm. corresponding to a loss of 12·11 on 100 parts of the substance.

IV. 2·0975 grms. of the preparation B. gave a loss of 0·259 grm., corresponding to a loss of 12·34 on 100 parts of the substance.

V. 2·0395 grms. of the preparation B. gave a loss of 0·243 grm., corresponding to a loss of 11·91 on 100 parts of the substance.

From these experiments it results that in this reaction the chromic acid loses three and the peroxide four equivalents of oxygen, that is, that the total loss is to the loss from the peroxide as 7 : 4. For calculating on this hypothesis the amount of oxygen in the two preparations, we find for the preparation A. from experiments I. and II. respectively, 6·60 and 6·67 per cent. of oxygen, and for the preparation B. from experiments III., IV. and V., 6·91, 7·05 and 6·80 per cent. of oxygen, which are the numbers given in the other experiments. The reaction would therefore be represented thus—

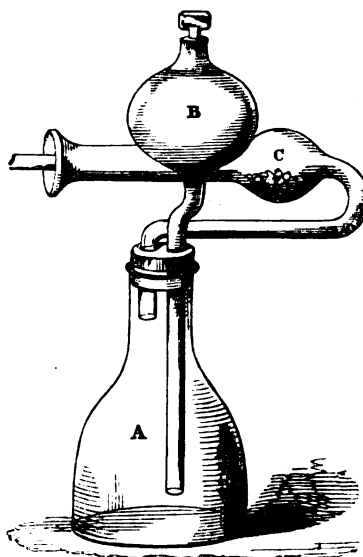


This method has the advantage of giving a considerable loss of oxygen with a small quantity only of the substance; and the perfect agreement with one another of numerous experiments made in this way, led me to place in it the greatest confidence. Facts, however, afterwards came to my knowledge, which proved that this action was not so absolutely uniform as had at first appeared; and although under the circumstances, and with the precautions (especially as to the relative quantities of the sub-

stances) which I have mentioned, the entire agreement of the experiments proves that the action is that which I have here given, yet this threw a certain doubt upon it, and it is only in some of my first experiments that I have used this plan of determining the oxygen uncontrolled by the following.

The determinations with platinum were made in a little apparatus of a very simple construction, which, as I shall elsewhere have occasion to mention it, I will call a bulb apparatus.

A is a small flask in which the weighed substance, together with a small portion of finely-divided platinum (prepared from platinum black by ignition) or of animal charcoal, which is equally effective, is placed. B is a glass bulb holding about two fluid ounces, which, before the experiment, is filled with dilute hydrochloric or acetic acid, and is closed by a well-ground stopper. C is a drying tube of potash. After weighing the apparatus, the stopper is loosened and the acid allowed to flow into the interior. The evolution of gas takes place immediately, but goes much slower than in the experiments with chromic acid. After eighteen or twenty hours the apparatus is again weighed, when the action may be considered terminated. It is, however, of course, to be weighed again until it ceases to lose weight. During the experiment a second potash tube, or a small tube dipping into a vessel of lime, is to be attached to the first, to prevent any increase of weight by absorption of moisture from the atmosphere. The following experiments were made in this manner:—



I. 2·389 grms. of the preparation A. gave a loss of 0·162 gm.

II. 2·437 grms. of the same gave a loss of 0·162 gm.

These experiments correspond to a loss per cent. of

I.
6·78

II.
6·64

III. 2·615 grms. of preparation B. gave a loss of 0·175 gm.

IV. 2·785 grms. of the same gave a loss of 0·184 gm.

These experiments correspond to a loss per cent. of

III.
6·63

IV.
6·60

These determinations entirely agree with each other; and the per-centage of oxygen in the substance is probably in fact (as is given by this experiment) a little less than that calculated from the amount of oxygen taken up, as in pulverizing it it is not possible to prevent the absorption of a little water by the baryta present, which must diminish the per-centage of oxygen. There is no violent rush of gas during the

experiment, nor is any variation of the action to be feared. I consider therefore this method to be as accurate as can be desired.

Experiments with Metallic Oxides.

The method of weighing the loss of oxygen, which succeeds very well for determining the amount of oxygen given off in the preceding experiments, cannot be employed with advantage for determining the amount given off by a metallic oxide in contact with the peroxide of barium. The evolution of gas is not rapid, continuing, at ordinary temperatures, sometimes for several days before all action ceases, during which time the apparatus is liable to alter in weight; and also more water is required to dissolve the baryta than can be introduced into the bulb of the apparatus, or conveniently and accurately be weighed. In these experiments, therefore, I determined indirectly the amount of oxygen given off by ascertaining the amount of metal reduced.

The form of the experiment was very simple. The weighed peroxide was intimately mixed in a flask with the metallic oxide, and a certain portion of water, about twelve ounces, poured upon the mixture. In the following experiments, those at the temperature of the air were placed in a cellar where the temperature was uniform, so that they might be comparable with one another, and the action was allowed to continue until all evolution of gas had ceased. In the experiments at 100° and at other temperatures, the water was first brought to the temperature required, then poured on the mixed substances and the mixture kept at the same temperature, either boiling or in a water-bath, during the action. In the experiments with oxide of silver, to determine the silver reduced, the residue, after the evolution of the gas had ceased, was treated with very dilute hydrochloric acid, with which it was allowed to remain in contact at least twelve hours. All oxide was thus converted into chloride. The fluid was now filtered from the residue and the hydrochloric acid washed out, the bulk of the solid substance being still kept in the flask in which the experiment was made. From this residue, consisting of chloride of silver and metallic silver, the silver was extracted by boiling with nitric acid, the substance on the filter being added to the rest and the filter also treated with acid. The solution was diluted and the silver dissolved, which was in fact the silver reduced, estimated as chloride. It is plain, that whether the oxide were reduced to metal or to a lower degree of oxidation, this silver must still be a correct measure of the oxygen lost. This method of determining the loss of oxygen I followed in all the experiments with silver compounds.

My first experiments were made before I had ascertained all the points necessary for the careful preparation of the peroxide, and the preparations used for these contained considerably less per-centage of oxygen than that which I afterwards readily procured, and contained also impurities from which the other preparations were free; in these experiments also the oxygen was determined only with bichromate of potash and hydrochloric acid. These circumstances in no way affect their general

result. But as the conclusions which they indicated were afterwards brought out and confirmed by experiments not open to these objections, it would be superfluous to give in their case also the numerical details of the determinations. As however they threw much light on the general nature of the inquiry, I cannot omit some account of them.

In the Tables which follow, I have arranged the chief results of these experiments.

The column A. contains the amount of oxygen $=a$ in 100 parts of the peroxide of barium employed, as given by the determination with bichromate of potash. The column B. contains the relative loss of oxygen from the metallic oxide; the loss, that is, calculated on 100 parts of the peroxide of barium. This loss $=\psi$. The column C. contains the ratio of the quantity of oxygen in the peroxide of barium to the loss from the metallic oxide; the ratio, that is, of $a : \psi$ and the calculated value of ψ . In the column D. I have given the approximate ratio of the oxygen in the amount of peroxide of barium taken, to that in the oxide of silver or peroxide of lead taken in each experiment. The first Table contains the experiments made at 100°C . The first six experiments were made with oxide of silver, the rest with peroxide of lead. In the case of the peroxide of lead, the amount of oxygen lost by the peroxide was determined by extracting the residue after the experiment with very dilute nitric acid, which, leaving the peroxide, dissolves the oxide formed. The lead thus dissolved was estimated as sulphide.

Experiments at 100°C .

	A.	B.	C.	D.
1.		1.65		
2.	4.03	1.64	5 : 2 1.61	3 : 2
3.		1.95		
4.	4.18	2.08	2 : 1 2.09	3 : 2 1 : 1
5.		3.08		
6.	5.85	2.99	2 : 1 2.92	AgO not weighed.
7.		1.85		
8.	4.32	1.71	5 : 2 1.72	5 : 4 1 : 1
9.		3.31		
10.	6.10	3.37	7 : 4 3.48	PbO ₂ not weighed.

Classifying these ten experiments with reference to the ratios given in the third column, we find that they may be arranged thus:—

5 : 2, four experiments.

4 : 2, four experiments.

$3\frac{1}{2} : 2$, two experiments.

In the next Table the experiments were made below 100° ; where the temperature is not given they were at the temperature of the air. The two last experiments were made with the peroxide of lead, the rest with the oxide of silver.

Experiments below 100°.

	A.	B.	C.	D.
1. 2.	4.18	2.35 2.53 T. 38° to 42°.	7 : 4 2.38	3 : 1
3. 4.		2.51 2.81 T. 24° to 26°.	5 : 3 2.50 3 : 2 2.78	5 : 2 3 : 1
5. 6.		2.93 3.00 T. 16°.	3 : 2	About 1 : 1 in the four experiments.
7. 8.		*2.96 2.77 T. 0° upwards.	3 : 2	
9. 10.	4.47	3.25 3.30	4 : 3 3.35	AgO not weighed.
11. 12.	6.10	4.21 3.93	3 : 2 4.06	PbO ₂ not weighed.

The experiments in this Table, arranged in the order of ratios, as those at the higher temperature, give the following series:—

$3\frac{1}{2} : 2$, one experiment.

5 : 3, two experiments.

3 : 2, seven experiments.

4 : 3, two experiments.

These experiments placed beyond doubt, that, under similar circumstances of temperature and mass, the loss of oxygen from the metallic oxide stood in a certain fixed relation to the quantity of the peroxide of barium employed. They showed also (speaking generally) that this loss varied inversely with the temperature, but yet it appeared that within certain limits (II. experiments 5, 6, 7, 8) the temperature might be very considerably altered without affecting the action. This was also the case, to a certain extent at any rate, with the mass; for a perfect agreement was found between experiments where the quantity of oxide of silver taken was considerably varied or even accidental. At the same time it was to be observed, that in determining these ratios a great deal would depend on the limits allowed for the error of analysis; and it was a question of great importance, and yet by no means easy to decide, whether they were absolutely such as I have here assumed them to be; that is to say, whether the difference between two nearly identical experiments was to be attributed to the error of analysis or to a real, though small, difference in the action. I determined therefore to make a more extended series of experiments, in which I might ob-

* The water in these two experiments was cooled down in ice before it was poured on the substances; at this low temperature no gas was given off. The action was allowed gradually to proceed as the temperature rose in the air.

serve the effect of varying the mass, at a constant temperature, and with the same preparation of the peroxide.

The great accuracy of the silver determination and the comparative facility of determining by it the loss of oxygen from the compounds of this metal, caused me to confine to them my attention. Indeed it was plain from experiments I had made with the oxide of mercury, as well as with peroxide of lead, that the general nature of the changes in question was, with any one of these substances, the same.

The following experiments were made with the chloride and the oxide of silver at 100°, and at the temperature of the air: I had already found how very greatly at the lower temperature this circumstance might be varied without alteration of the action; and in this case the experiments are arranged without any special reference to the slight variations of the temperature of the air. The peroxide taken is the same for each series; the general form of the experiments was precisely as already described.

Oxygen determinations.

The determinations of the oxygen in the different preparations of the peroxide of barium employed, gave the following results:—

I. Peroxide P.

I.—(1.) 2·8970 grms. of a peroxide (P) gave with hydrochloric acid and bichromate of potash, a total loss of 0·425 gram. of oxygen, corresponding to a loss of 14·67 per cent. on the peroxide.

(2.) 2·9215 grms. of the same substance gave, in a similar experiment, a total loss of 0·427 gram. of oxygen, corresponding to a loss of 14·61 per cent.

The oxygen in 100 parts of the peroxide, calculated from the above experiments, is,—

I.	II.
8·38	8·36.

(3.) 2·823 grms. of the same peroxide in an experiment with platinum and acetic acid, gave a loss of 0·235 gram., corresponding to a loss of 8·32 per cent. oxygen.

The mean of these three experiments gives 8·35 as the per-centage of oxygen in the substance.

II. Peroxide Q.

II.—(1.) 2·9565 grms. of a peroxide (Q) gave with hydrochloric acid and bichromate of potash, a total loss of 0·457 gram. of oxygen, corresponding to a loss of 15·45 per cent.

(2.) 2·9555 grms. of the same gave in a similar experiment, a total loss of 0·450 gram. of oxygen, corresponding to a loss of 15·22 per cent.

(3.) 2·955 grms. of the same gave with nitric acid and bichromate of potash, a total loss of 0·453 gram. of oxygen, corresponding to a loss of 15·32 per cent.

The oxygen in 100 parts of the peroxide, calculated from the above experiments, is,—

I.	II.	III.	Mean.
8·82	8·69	8·75	8·75

(1.) 2·6355 grms. of the peroxide gave with platinum and acetic acid, a total loss of 0·227 grm. of oxygen.

(2.) 2·9474 grms. of the same gave with platinum and acetic acid, a total loss of 0·257 grm. of oxygen.

(3.) 2·847 grms. of the same gave with platinum and hydrochloric acid, a total loss of 0·241 grm. of oxygen.

(4.) 2·9815 grms. of the same gave, in a similar experiment, a total loss of 0·254 grm. of oxygen.

These determinations correspond to a loss of oxygen per cent. of—

I.	II.	III.	IV.	Mean.
8·65	8·71	8·46	8·51	8·58

III. Peroxide O.

III.—(1.) 2·4875 grms. of a peroxide (O) gave with platinum and hydrochloric acid, a loss of 0·191 grm. of oxygen.

(2.) 2·515 grms. of the same, in a similar experiment, lost 0·191 grm. of oxygen.

(3.) 2·7296 grms. of the same, in a similar experiment, lost 0·204 grm. of oxygen.

These determinations correspond to a loss of oxygen per cent. of—

I.	II.	III.	Mean.
7·67	7·59	7·47	7·57

Experiments with Chloride of Silver at 100° C.

The following experiments were made with the peroxide P and chloride of silver at 100° C. The chloride of silver was in the finely-divided state in which it is produced by precipitating nitrate of silver with chloride of sodium. The precipitate with hydrochloric acid cannot so readily be mixed with the peroxide. The chloride of silver was dried either at 100° or at a gentle heat over a lamp; it was, when dried, in the state of a fine powder.

The following Table contains the details of the experiments:—

TABLE I.—A.

	Peroxide of barium P.	Chloride of silver taken.	Chloride of silver obtained.	Equivalent of oxygen.
	grms.	grms.	grms.	grms.
1.	3·0115	0·527	0·280	0·0155
2.	3·014	1·003	0·3945	0·0219
3.	3·0865	1·404	0·5633	0·0312
4.	3·0455	2·057	0·8085	0·0449
5.	3·0036	4·011	1·019	0·0566
6.	3·0206	5·963	1·0573	0·0587
7.	3·0295	7·983	1·1815	0·0656
8.	2·958	9·235	1·087	0·0605
9.	2·967	11·139	1·170	0·0652
10.	3·0465	11·064	1·3105	0·0728
11.	3·0375	15·960	1·3515	0·0759
12.	3·0695	19·920	1·3425	0·0745
13.	3·0125	8·059	1·4795	0·0822

In the second Table, B, which follows, the oxygen equivalent to the reduced silver is compared with the oxygen in the peroxide; the peroxide being taken as 100, and the oxygen therefore as 8.34. In the third Table, the oxygen in the peroxide is taken as the unit. The third and fourth columns in both cases contain the assumed and calculated ratio of the oxygen lost by the peroxide to that equivalent to the reduced silver. I have assumed as this ratio the simplest ratio which agrees with the experiments, as a convenient and sufficiently accurate expression of the loss.

TABLE I.—B.

	Oxygen in the peroxide of barium $P = a$.	Oxygen equivalent to the chloride of silver reduced $= \psi$.	Ratio, $a : \psi$.	Calculated ratio.
1.	8.34	0.51		
2.		0.72		
3.		1.01		
4.		1.47		
5.		1.88	9 : 2	1.85
6.		1.94		
7.		2.18	4 : 1	2.08
8.		2.04		
9.		2.19		
10.		2.42	7 : 2	2.38
11.		2.47		
12.		2.42		
13.		2.73	3 : 1	2.78

TABLE I.—C*.

	Oxygen in the peroxide of barium $= a$.	Oxygen equivalent to the chloride of silver reduced $= \psi$.	Ratio, $a : \psi$.	Calculated ratio.
1.	100	6.11		
2.		8.63		
3.		12.11		
4.		17.62		
5.		22.54	9 : 2	22.22
6.		23.26		
7.		26.13	4 : 1	25.0
8.		24.46		
9.		26.25		
10.		29.01	7 : 2	28.57
11.		29.61		
12.		29.01		
13.		32.73	3 : 1	33.33

* In these Tables the experiments are arranged in all cases according to the nature of the actions, and not with reference to the quantities taken of the decomposed bodies. But for the sake of a ready general compa-

100 parts of this peroxide require 149·7 parts of chloride of silver to one equivalent of the oxygen it contains. Hence 3 grms. of the peroxide require 4·491 grms. of the chloride for the same proportion.

In the first four experiments, in which comparatively small quantities of chloride of silver, at most not half an equivalent, were taken, the loss increases nearly with the mass of the chloride, and bears therefore no constant ratio to the loss from the peroxide. In all these four experiments the chloride has been reduced nearly in the same proportion, but yet between the first and second experiment there is a difference which is not found between the other three, as may be seen from the following comparison of the chloride of silver taken with the chloride found.

Exp.	Chloride of silver taken.	Chloride of silver found.
1.	100	53·1
2.	100	39·3
3.	100	40·1
4.	100	39·3

It appears from this comparison that in the three last of these experiments exactly two-fifths of the chloride taken are reduced, and we may regard this limit of reduction as the circumstance which here determines the action. The gradual increase of loss, with the mass, does not continue, or the increase becomes very small, for one and three equivalents of chloride give very nearly the same amount of reduced silver, equivalent to exactly one-fourth of the oxygen in the peroxide. There is also a constant ratio of loss, which appears likewise to be connected with the increase of mass where the ratio is as 7 : 2, and there is one exception to the order of the series where the loss does not stand in the same general relation to the mass, which may be observed in the other experiments. In this, however, the same simplicity of ratio prevails, and it is evidently a term of the series; the ratio of the loss being as 3 : 1. It is worthy of observation, that the turning-point of the action where the loss ceases to increase with the mass, lies at any rate near to that point where the decomposing bodies are taken in equal equivalent proportions.

My experiments had led me to suspect that some circumstance connected with the particular preparation of the peroxide employed materially influenced the results. This circumstance might be the proportion of free baryta in the peroxide, which was not quite the same in different preparations, or it might be some peculiarity in the molecular condition of the peroxide itself due to some other cause; I repeated therefore the experiments made with one peroxide with another preparation, and arranged these experiments apart.

The following experiments were made also with chloride of silver at 100°, but with the peroxide Q containing 8·58 per cent. of oxygen. The Tables are of the same nature as before.

rison of these quantities, nearly the same quantity of the peroxide, 3 grms., was, with some few exceptions, always employed. The experiments, as will be seen, do not call for any more exact comparison than can thus at a glance be made.

TABLE II.—A.

	Peroxide of barium Q.	Chloride of silver taken.	Chloride of silver obtained.	Equivalent of oxygen.
1.	3.0065 grms.	4.0015 grms.	0.5305 grms.	0.0295 grms.
2.	2.971	5.003	0.709	0.03952
3.	2.9995	8.0015	0.732	0.0408
4.	3.0145	8.0745	0.9845	0.0548
5.	2.962	10.129	0.968	0.05396
6.	3.001	11.9955	1.003	0.05591
7.	2.985	14.936	1.126	0.0627
8.	2.9545	12.0175	1.2245	0.06826
9.	2.9965	16.0105	1.3205	0.07361

TABLE II.—B.

	Oxygen in the peroxide of barium Q = α .	Oxygen equivalent to the chloride of silver reduced = ψ .	Ratio, $\alpha : \psi$.	Calculated ratio.
1.	8.58	0.96	9 : 1	0.95
2.		1.33	6 : 1	1.48
3.		1.36		
4.		1.81	9 : 2	1.90
5.		1.82		
6.		1.86		
7.		2.10	4 : 1	2.14
8.		2.31	7 : 2	2.45
9.		2.45		

TABLE II.—C.

	Oxygen in the peroxide of barium Q = α .	Oxygen equivalent to the chloride of silver reduced = ψ .	Ratio, $\alpha : \psi$.	Calculated ratio.
1.	100	11.18	9 : 1	11.11
2.		15.5	6 : 1	16.6
3.		15.8		
4.		21.0	9 : 2	22.22
5.		21.2		
6.		21.6		
7.		24.4	4 : 1	25.00
8.		26.9	7 : 2	28.57
9.		28.5		

3 grms. of this peroxide require 4 grms. of chloride of silver as equivalent to the oxygen it contains. Thus the experiments from 1 to 6 in this series, were made with a proportion of the chloride, on the whole greater than the experiments 5 to 9 on the first Table. Yet a great diminution is to be observed in the loss of oxygen ; in experi-

ment 1 only 13·2 parts out of 100 of the chloride are reduced. And here the ratio 9 : 2 occupies the place of the ratio 4 : 1 or 8 : 2 on the other series. But the limiting ratio of 7 : 2 is in both series the same, and on the whole a certain increase of loss, although not a gradual increase, is to be observed with the increase of the chloride taken. These experiments confirm the fact, that under similar circumstances the different preparations of the peroxide have a different reducing power.

Experiments with Chloride of Silver at the temperature of the air.

The next experiments were made also with the chloride of silver, but at a lower temperature, the temperature of the air. I had already found how very much at this low temperature the temperature might be varied without alteration of the action, and in this case the experiments are arranged without any special reference to the variation of the temperature. The peroxide in the first series was the same as that taken in the last experiments, namely, the peroxide Q containing 8·58 per cent. of oxygen.

TABLE III.—A.

	Peroxide of barium Q.	Chloride of silver taken.	Chloride of silver obtained.	Equivalent of oxygen.
1.	3·034 grms.	4·0125 grms.	1·366 grms.	0·076 grms.
2.	2·9942	4·988	1·480	0·0825
3.	3·0275	6·1315	1·545	0·08613
4.	2·994	8·0905	1·9415	0·10823
5.	2·9742	10·016	2·043	0·1138
6.	2·9532	14·916	2·019	0·1125
7.	3·014	9·9915	2·336	0·1302
8.	2·9915	12·9075	2·3125	0·1289
9.	2·0132	14·916	1·5485	0·0863

TABLE III.—B.

	Oxygen in the peroxide of barium Q = a.	Oxygen equivalent to the chloride of silver reduced = ψ .	Ratio, a : ψ .	Calculated ratio.
1.	8·58	2·50	7 : 2	2·48
2.		2·75	3 : 1	2·86
3.		2·84		
4.		3·61	7 : 3	3·67
5.		3·82	9 : 4	3·81
6.		3·80		
7.		4·31	2 : 1	4·29
8.		4·30		
9.		4·28		

TABLE III.—C.

	Oxygen in the peroxide of barium $Q = a$.	Oxygen equivalent to the chloride of silver reduced = ψ .	Ratio, $a : \psi$.	Calculated ratio.
1.	100	29.13	7 : 2	28.57
2.		32.05	3 : 1	33.33
3.		33.10		
4.		42.07	7 : 3	42.85
5.		44.52	9 : 4	44.44
6.		44.28		
7.		50.23	2 : 1	50.0
8.		50.11		
9.		49.88		

The point to be observed in these experiments is the great increase of reduction at the lower temperature. The comparative experiments in this and the last series, show that from corresponding quantities of the chloride, as nearly as possible twice the amount of silver is reduced; and these experiments, it is worthy of remark, take up the action exactly at the point where the other series terminates, so that the expression of the relative loss forms a continuation of the series of ratios commencing with 7 : 2 and terminating with 2 : 1.

The following are a few experiments made with the peroxide P, containing 8.34 per cent. of oxygen.

TABLE IV.—A.

	Peroxide of barium Q.	Chloride of silver taken.	Chloride of silver obtained.	Equivalent of oxygen.
1.	3.020 grms.	1.012 grms.	0.5715 grms.	0.03175 grms.
2.	3.032	2.009	0.765	0.0426
3.	3.0256	3.895	1.484	0.08244
4.	3.0005	8.879	1.733	0.0962
5.	3.0046	6.11	1.9495	0.1083
6.	3.05	11.983	2.065	0.1147

TABLE IV.—B.

	Oxygen in the peroxide of barium $P = a$.	Oxygen equivalent to the chloride of silver reduced = ψ .	Ratio, $a : \psi$.	Calculated ratio.
1.	8.34	1.04	8 : 1	1.04
2.		1.40	6 : 1	1.39
3.		2.72	3 : 1	2.78
4.		3.26	5 : 2	3.33
5.		3.60	9 : 4	3.70
6.		3.72		

TABLE IV.—C.

	Oxygen in the peroxide of barium P=a.	Oxygen equivalent to the chloride of silver reduced = ψ .	Ratio, a : ψ .	Calculated ratio.
1.	100	12.47	8 : 1	12.5
2.		16.06	6 : 1	16.66
3.		32.61	3 : 1	33.33
4.		39.08	5 : 2	40.0
5.		43.16	9 : 4	44.44
6.		44.60		

The experiments with this peroxide give about the same range of action as with the last. On the whole, however, the loss here, as at 100° with the same substance, is rather less, but the difference is not so apparent.

Experiments with the Oxide of Silver at 100° C.

The following are experiments with the oxide of silver at 100° C. The oxide was prepared either by precipitation with baryta water or with a pure potash. It contained no chloride but some carbonate, and was dried at 100°. The peroxide employed was the peroxide P, containing 8.34 per cent. of oxygen; 3.63 grms. of the oxide of silver are equivalent to 3 grms. of the peroxide.

TABLE V.—A.

	Peroxide of barium P.	Oxide of silver.	Chloride of silver found.	Equivalent of oxygen.
1.	3.0225 grms.	0.855 grms.	0.625 grms.	0.0347 grms.
2.	3.0295	1.525	0.8353	0.0463
3.	2.9807	1.205	0.9784	0.0543
4.	3.006	1.832	1.165	0.0647
5.	3.0107	2.235	1.369	0.0760
6.	2.997	2.920	1.5565	0.0864
7.	2.982	4.692	1.492	0.0831
8.	3.007	4.890	1.510	0.0841
9.	3.0185	6.182	1.483	0.08238
10.	3.601	6.266	1.874	0.1041
11.	2.986	6.4675	1.503	0.0837
12.	3.067	9.339	1.6345	0.0908
13.	3.019	11.051	1.7325	0.09625
14.	3.0485	7.810	1.7785	0.0988
15.	2.983	4.932	1.84	0.1025
16.	3.616	15.675	2.395	0.133
17.	2.9725	9.5345	2.001	0.1115
18.	3.0075	12.2035	2.2365	0.1248

TABLE V.—B.

	Oxygen in the peroxide of barium $P = a$.	Oxygen of the oxide of silver reduced = ψ .	Ratio, $a : \psi$.	Calculated ratio.
1.	8.34	1.14	5 : 1	1.67
2.		1.52		
3.		1.82		
4.		2.15	4 : 1	2.08
5.		2.52	7 : 2	2.38
6.		2.88	3 : 1	2.78
7.		2.78		
8.		2.79		
9.		2.72		
10.		2.88		
11.		2.80		
12.		2.96		
13.		3.18	5 : 2	3.33
14.		3.24		
15.		3.43		
16.		3.67	9 : 4	3.70
17.		3.75		
18.		4.14	2 : 1	4.17

TABLE V.—C.

	Oxygen in the peroxide of barium $P = a$.	Oxygen of the oxide of silver reduced = ψ .	Ratio, $a : \psi$.	Calculated ratio.
1.	100	13.66	5 : 1	20.0
2.		18.22		
3.		21.82		
4.		25.77	4 : 1	25.0
5.		30.21	7 : 2	28.57
6.		34.53	3 : 1	33.33
7.		33.33		
8.		33.45		
9.		32.61		
10.		34.53		
11.		33.57		
12.		35.49		
13.		38.12	5 : 2	40.00
14.		38.84		
15.		41.12		
16.		44.00	9 : 4	44.44
17.		44.84		
18.		49.63	2 : 1	50.00

These experiments give the same results as those made with the chloride of silver at the lower temperature, and comprise as these a series of ratios beginning with 7 : 2 and ending with 2 : 1. The experiments with the smaller quantities of oxide show

also the same kind of progressive increase of loss, as already observed with the chloride; the increase however is not so constant. In experiments 4 and 5 exactly half the oxide is reduced; the ratio of the oxygen in the oxide of silver taken, to the oxygen from the oxide reduced, being in the two experiments as 100 : 51·2 and 100 : 49·31; the presence however of carbonate of silver in the oxide renders this fact less certain than the similar fact observed in the case of the chloride of silver (Table I.).

I repeated these experiments with another preparation of the peroxide, the peroxide O, containing 7·57 per cent. of oxygen; 3·12 grms. of oxide of silver are equivalent to 3 grms. of this peroxide.

TABLE VI.—A.

	Peroxide of barium O.	Oxide of silver.	Chloride of silver obtained.	Equivalent of oxygen.
1.	3·0666 grms.	2·737 grms.	1·692 grms.	0·094 grms.
2.	3·006	3·659	1·7146	0·0952
3.	3·098	4·801	1·7335	0·0963
4.	3·007	5·426	2·0735	0·1151
5.	3·0806	11·592	2·5123	0·1395

TABLE VI.—B.

	Oxygen in the peroxide of barium O = α .	Oxygen of the oxide of silver reduced = ψ .	Ratio, $\alpha : \psi$.	Calculated ratio.
1.	7·57	3·09	5 : 2	3·02
2.		3·16		
3.		3·10		
4.		3·85	2 : 1	37·8
5.		4·52	5 : 3	4·74

TABLE VI.—C.

	Oxygen in the peroxide of barium O = α .	Oxygen of the oxide of silver reduced = ψ .	Ratio, $\alpha : \psi$.	Calculated ratio.
1.	100	40·81	5 : 2	40·00
2.		40·74		
3.		40·95		
4.		50·59	2 : 1	50·00
5.		59·70	7 : 3	60·00

The experiments 1, 2, 3 here correspond to the experiments 6, 7, 8, 9 in Table V.; they give however a considerably greater loss, the ratio being 5 : 2 instead of 3 : 1. This difference, it is probable, extends also to the higher terms in the series, for we

have here a ratio 5 : 3, which is a greater loss than any in the other series of experiments. It is worthy of remark, that the loss per cent. on the peroxide is nearly the same in the two, which might lead us to think that this loss stood in some fixed ratio to the baryta of the substance, with the whole of which the oxygen of the peroxide may be considered to be in combination. This however is doubtless but a coincidence, as the experiments with chloride of silver, made with different preparations of the peroxide, where similar differences exist, do not show this relation.

Experiments with the Oxide of Silver at the temperature of the air.

The following experiments were made with the peroxide P (containing 8·34 per cent. of oxygen) and oxide of silver at the temperature of the air.

TABLE VII.—A.

	Peroxide of barium P.	Oxide of silver.	Chloride of silver found.	Equivalent of oxygen.
1.	2·999 grms.	0·824 grms.	0·528 grms.	0·2293 grms.
2.	3·004	1·604	0·9135	0·05075
3.	3·017	3·123	1·3915	0·0773
4.	3·022	3·994	1·6815	0·0934
5.	3·039	6·313	1·990	0·1105
6.	3·04	7·788	2·057	0·1142
7.	3·029	9·400	2·041	0·1133
8.	2·9417	2·964	2·2055	0·1225
9.	3·059	15·720	2·3785	0·132
10.	15·925	26·522	13·677	0·759
11.	3·478	9·405	2·965	0·1647
12.	3·021	7·6195	3·222	0·1796

TABLE VII.—B.

	Oxygen in the peroxide of barium P = a.	Oxygen of the oxide of silver reduced = ψ.	Ratio, a : ψ.	Calculated ratio.
1.	8·34	0·97	7 : 2	2·38
2.		1·68		
3.		2·56		
4.		3·10	5 : 2	3·33
5.		3·63		
6.		3·75	9 : 4	3·70
7.		3·74		
8.		4·16	2 : 1	4·17
9.		4·31		
10.		4·76	7 : 4	4·76
11.		4·70		
12.		5·94	7 : 5	5·95

TABLE VII.—C.

	Oxygen in the peroxide of barium $P = a$.	Oxygen of the oxide of silver reduced = ψ .	Ratio, $a : \psi$.	Calculated ratio.
1.	100	11.63	7 : 2	28.57
2.		20.14		
3.		30.69		
4.		37.17		
5.		43.52	9 : 4	44.44
6.		44.96		
7.		44.84		
8.		49.88	2 : 1	50.00
9.		51.67		
10.		57.07	7 : 4	57.1
11.		56.35		
12.		71.22	7 : 5	71.42

When small quantities of the oxide of silver are taken, the loss is not very different from that at the higher temperature, as is also the case in the experiments with the chloride; but after a certain point the actions diverge, and the total general result gives an entirely different range of action in the two series. In all cases, after a certain point, the mass ceases to have a determinate influence on the action; and here this influence would seem soon to be entirely lost, for after the first two or three experiments, no relation whatever can be traced between the relative masses of the substances and the proportion of reduced silver. At the same time, even these experiments, which we may regard as accidental, arrange themselves in a certain order, and give on the whole higher numbers in the series of ratios than any yet obtained. This series may be extended yet further; thus two experiments with peroxide O, at the temperature of the air, gave—

TABLE VIII.—A.

	Peroxide of barium O.	Oxide of silver.	Chloride found.	Equivalent of oxygen.
1.	2.926 grms.	5.186 grms.	2.8557 grms.	0.1586 gm.
2.	3.06	5.064	3.0402	0.1689

TABLE VIII.—B.

	Oxygen in the peroxide of barium $O = a$.	Oxygen of the oxide of silver reduced = ψ .	Ratio, $a : \psi$.	Calculated ratio.
1.	7.57	5.42	7 : 5	5.40
2.		5.51		

TABLE VIII.—C.

	Oxygen in the peroxide of barium $O = a$.	Oxygen of the oxide of silver reduced = ψ .	Ratio, $a : \psi$.	Calculated ratio.
1.	100	71.59	7 : 5	71.42
2.		72.78		

These experiments, although made with but small quantities of the oxide, give a loss as great as any made with the other preparation.

Two experiments with the peroxide Q, at the same temperature, gave a yet greater loss.

TABLE IX.—A.

	Peroxide of barium Q.	Oxide of silver.	Chloride of silver found.	Equivalent of oxygen.
1.	2.985 grms.	7.878 grms.	3.843 grms.	0.2142 grm.
2.	2.9544	7.9215	3.801	0.2119

TABLE IX.—B.

	Oxygen in the peroxide of barium $Q = a$.	Oxygen of the oxide of silver reduced = ψ .	Ratio, $a : \psi$.	Calculated ratio.
1.	8.58	7.17	6 : 5	7.15
2.		7.17		

TABLE IX.—C.

	Oxygen in the peroxide of barium $Q = a$.	Oxygen of the oxide of silver reduced = ψ .	Ratio, $a : \psi$.	Calculated ratio.
1.	100	83.56	6 : 5	83.33
2.		83.56		

This is the greatest amount of loss which I have yet obtained in these experiments with silver compounds, which has never quite equaled the oxygen in the peroxide.

The specific difference in the amount of reduction of the chloride and of the oxide of silver under otherwise similar circumstances with the same preparation of the peroxide, that is, at the same temperature, depends doubtless on the difference in the chemical nature of the substances. It was an interesting matter of inquiry whether this difference would be found with other silver compounds: I have not yet extended my experiments in this direction so far as the importance of the question demands, but the following results show both the identity and the difference which may exist in the reaction.

The following experiments were made at 100° C. with carbonate of silver and with

the peroxide Q, which was used also in the experiments with the chloride (Table II.). Two equivalents of the carbonate were taken to one of the peroxide.

TABLE X.—A.

	Peroxide of barium Q.	Carbonate of silver.	Chloride of silver found.	Equivalent of oxygen.
1.	3.025 grms.	9.2055 grms.	1.339 grm.	0.0746 grm.
2.	3.03	9.2085	1.490	0.0830

TABLE X.—B.

	Oxygen in the peroxide of barium Q = a.	Oxygen of the carbonate of silver reduced = ψ .	Ratio, a : ψ .	Calculated ratio.
1.	8.58	2.46	7 : 2	2.45
2.		2.73	3 : 1	2.86

TABLE X.—C.

	Oxygen in the peroxide of barium Q = a.	Oxygen of the carbonate of silver reduced = ψ .	Ratio, a : ψ .	Calculated ratio.
1.	100	28.67	7 : 2	28.57
2.		31.81	3 : 1	33.3

Two other experiments were made with the same substance at a lower temperature, 17° C.

TABLE XI.—A.

	Peroxide of barium Q.	Carbonate of silver.	Chloride of silver found.	Equivalent of oxygen.
1.	2.926 grms.	9.2075 grms.	1.714 grm.	0.95556 grm.
2.	3.012	9.1525	1.938	0.108

TABLE XI.—B.

	Oxygen in the peroxide of barium Q = a.	Oxygen of the carbonate of silver reduced = ψ .	Ratio, a : ψ .	Calculated ratio.
1.	8.58	3.26	5 : 2	3.43
2.		3.58		

TABLE XI.—C.

	Oxygen in the peroxide of barium Q = a.	Oxygen of the carbonate of silver reduced = ψ .	Ratio, a : ψ .	Calculated ratio.
1.	100	37.99	5 : 2	40
2.		41.72		

These two experiments were made in the small bulb-apparatus used for the oxygen determinations, and thus the total loss of oxygen was estimated as well as the reduced silver. The action proceeds more rapidly than in the experiments with the chloride and oxide, so as to render it probable that a more accurate result might in this manner be obtained with the carbonate than with those substances. In the first experiment the total loss was 12·19 per cent.; in the second, 11·91 per cent. The sum of the oxygen in the peroxide and that due to the silver reduced, as given by the determinations, is in the first experiment 11·84 per cent.; in the second, 12·16 per cent.; which so agree with the other numbers as to show that no oxygen is retained by the substances.

On turning to those experiments with the chloride of silver at 100° C. and at the lower temperature, with which these experiments are comparable (Tables I. and II.), it will be seen that the reduction is nearly the same with the two substances.

With sulphate of silver the result was very different. The two following experiments were made with pure crystallized sulphate of silver and the peroxide Q, at 100° C. In the first experiment one equivalent, in the second two equivalents of the sulphate were taken.

TABLE XII.—A.

	Peroxide of barium Q.	Sulphate of silver.	Chloride of silver found.	Equivalent of oxygen.
1.	2·969 grms.	5·449 grms.	0·118 grm.	0·00657 grm.
2.	3·005	10·805	0·542	0·03021

TABLE XII.—B.

	Oxygen in the peroxide of barium Q = α .	Oxygen of the sulphate of silver reduced = ψ .	Ratio, $\alpha : \psi$.	Calculated ratio.
1.	8·58	0·221	40 : 1	0·21
2.		1·00	9 : 1	0·95

TABLE XII.—C.

	Oxygen in the peroxide of barium Q = α .	Oxygen of the sulphate of silver reduced = ψ .	Ratio, $\alpha : \psi$.	Calculated ratio.
1.	100	2·57	40 : 1	2·5
2.		11·66	9 : 1	11·1

The amount of reduction is here so small that the action of the sulphate of silver is very nearly the same as the so-called contact action of platinum. I have already pointed out that the action of very small quantities of the chloride and of the oxide of silver is also of the same nature. In the case of these different chemical substances, therefore, it approaches the same limit, although according to a different law.

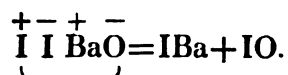
I have now brought this investigation to that point at which for the present I shall leave it. I would willingly have attempted to render these tables of experiments more complete, had it not appeared to me that there were certain defects in the method of experimenting which rendered this course inadvisable. One most important question is, whether the series of ratios by which I have expressed the relative loss of oxygen from the two substances is truly an intermittent or a continuous series, that is to say, whether there are or are not certain points where this ratio is constant although the proportions of the masses are varied. That there is with each chemical substance at least one such point, namely, a certain limit of reduction beyond which a great increase of the mass of the substance to be reduced does not alter the action, may, I think, be asserted with confidence, and without in any way implying that all the ratios which these experiments have brought to light are of this nature; yet it is difficult to account for the great coincidence of experiments made with very different proportions of the substances, without admitting a tendency of the action to fall into certain ratios rather than into others. Indeed the experiments appear to me to indicate both that for certain intervals the loss of oxygen truly does vary directly with the mass of the substance reduced, and that in certain other points the mass may be varied and the loss yet be constant. In these experiments, however, I could never secure that perfect agreement between two experiments made under exactly similar circumstances, by which alone I could hope to answer a question demanding such exactness. One principal reason of the anomalies which the experiments occasionally exhibit is doubtless, that by simply mixing the substances, even where the same quantities are taken in two experiments, the same conditions of mass are yet not realized: I therefore turned my attention to other methods of experimenting, by which it is my hope to answer this question in a more exact and satisfactory manner. My experiments are not yet complete, but I may mention, that by operating with solutions, I have obviated this difficulty. Lastly, let me thus sum up the general results of these experiments. First, the amount of oxygen lost by the substance reduced is, under circumstances otherwise similar, in a definite and constant ratio to the quantity of the peroxide of barium employed. Secondly, this loss is greater as the mass of the substance reduced is increased, and diminishes as the temperature at which the reduction takes place is raised. It varies also with the preparation of the peroxide. Thirdly, in each series of experiments there is a certain definite limit of reduction beyond which at any rate a very great increase of the mass of the substance reduced causes no increase of the loss of oxygen*. Lastly, in reference to that chemical question from which this inquiry proceeded, namely, how much oxygen is lost by the substance reduced in proportion to the loss of oxygen from the peroxide of barium, it is to be observed, that however much the circumstances are varied, there are yet two limiting

* It appears from other experiments which I have made, that with each chemical substance there is a specific limit of reduction of this kind; thus, for example, in the experiment with chromic acid (p. 779), the only reason of the constant reaction there obtained, is that by taking a very great excess of the bichromate of potash,

ratios between which the whole action is comprised, either of which may be almost indefinitely approached. These are the limit of least reduction, in which the ratio of the loss from the peroxide of barium to that from the metallic oxide is infinite, or as 1 : 0 ; and the limit of greatest reduction, which is the ratio of equality, in which the two substances would lose an equal amount of oxygen. This limit also, in no one experiment with any silver compound, have I ever exceeded or reached. The first limit is that in which the action would be a pure reduction by contact, such as takes place when platinum or carbon are the substances which cause the decomposition. The other would be a purely chemical action, such as may be seen in an experiment to which I will now proceed, and which, although perhaps not at first sight as striking as the reduction of the metallic oxides, is, philosophically considered, quite as remarkable, being indeed the same experiment, but detached from circumstances which give in the other case the apparent differences to the reaction.

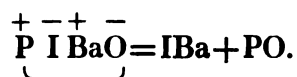
Action of Iodine on the Peroxide of Barium.

It occurred to me, that if it were for the reasons I have stated, that the iodide and chloride of silver decompose the peroxide of barium with the evolution of the oxygen it contains, iodine and chlorine should produce a similar effect. Indeed this was a very critical and important experiment ; for we know the action of chlorine and iodine on baryta ; and on the view which is usually taken of this decomposition and of other analogous changes, namely, that the iodine takes away the barium from the oxygen because of its superior affinity for the metal, and that the oxygen which is thus liberated combines with the iodine because it is in a nascent condition, and thus converts it into iodous or iodic acid ; by acting on iodine with the peroxide of barium, we ought to have just twice as much iodic acid formed as in the other case ; nor was there any apparent reason why the oxygen from the peroxide of barium should not oxidize as well as the oxygen from the baryta. The same iodine, the same barium, and the same oxygen are supposed to be present ; and if the iodine acted at all on the peroxide, the oxygen would still be nascent. Considered however from that theoretical point of view which I have given in the early part of this paper, there was no reason to expect this result. I have there suggested (p. 767), that in the action of iodine upon baryta, the combination of iodine and oxygen takes place solely on account of the polar relation in which the particles are placed under the peculiar circumstances of the action. Each particle in the change being alternately positive and negative, thus :—

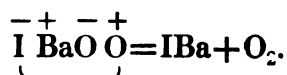


I at once determined the action to a limit which it could not exceed. When small quantities of the bichromate are employed the result is entirely different, and a very small quantity of the bichromate decomposes a very great excess of the peroxide. The reaction which I have given (p. 769) is to a certain extent hypothetical, it is the limit of greatest reduction.

When phosphorus and iodine are used instead of iodine in this experiment, no essential change has taken place in the causes which determine the combining condition of the particles, but a particle of phosphorus now stands in the same relation to the baryta as the particle of iodine in the other experiment, thus :—



Now if thus intercalating the particle of phosphorus transfers the action from the iodine to the phosphorus, so also intercalating a particle of oxygen might transfer the action from the iodine to the oxygen itself, which would now be the truly oxidized body, provided of course that the particle of oxygen thus intercalated were a body which stood to the other particles with which it was in contact, in the same chemical relation as the iodine and phosphorus in the other changes; nor do I see that this substitution of the oxygen for the iodine is more truly extraordinary than the substitution of the iodine for the phosphorus. Indeed, if we inquire into the causes of chemical change, the two would depend essentially upon one and the same fact, namely, a chemical difference between the particles of the element. In the one case we have the chemical division of the element, in the other the chemical synthesis to explain; and to give a philosophical account of either fact we must assume alike the existence of a chemical difference between its particles. Now, when we substitute peroxide of barium for baryta in the iodine experiment, this very intercalation of the oxygen has been effected, and the conditions of the change been fulfilled, the polar cycle being completed, thus :—



In fact, when water is poured upon a mixture of peroxide of barium and iodine, a violent evolution of gas occurs, and the same when chlorine is led into water containing the peroxide, or when the peroxide is thrown into a solution of hypochlorite of lime in acetic acid.

This experiment with iodine is so important, that I shall give at full length the determinations of the quantity of gas evolved in this reaction. The experiment was made in the small bulb-apparatus already described, p. 779.

The weighed peroxide and weighed iodine were mixed in the flask, the bulb being filled with distilled water; the apparatus was weighed, and the water then allowed to descend on the mixture. The loss on again weighing the apparatus is the oxygen evolved. The same precautions are to be taken in the experiment as in the oxygen determination.

The peroxide used in the following experiments was the peroxide Q, containing 8.58 per cent. of oxygen; 100 parts of this peroxide require 138 parts of iodine as one equivalent to the oxygen it contains. The first of the following Tables contains

the quantities of the substances taken and the loss; in the second the comparison is made between this loss and the oxygen of the peroxide.

TABLE XIII.—A.

	Peroxide of barium Q.	Iodine.	Loss of oxygen.
1.	1.094 grms.	30.242 grms.	0.187 grm.
2.	0.973	16.082	0.164
3.	0.843	11.633	0.140
4.	1.409	15.5485	0.238
5.	1.845	17.8225	0.315
6.	2.0675	17.130	0.358
7.	0.7995	5.5325	0.140
8.	1.9364	10.6495	0.331
9.	1.2405	5.277	0.212
10.	1.275	3.517	0.215

TABLE XIII.—B.

	Oxygen in the peroxide Q.	Loss of oxygen.	Oxygen in the peroxide.	Loss of oxygen.
1.	8.58	17.09	100	199.1
2.		16.85		196.3
3.		16.60		193.4
4.		16.89		196.8
5.		17.06		198.8
6.		17.31		201.7
7.		17.51		204.0
8.		17.09		199.1
9.		17.08		199.0
10.		16.86		196.5

The mean of these experiments gives the ratio of the oxygen in the peroxide compared with the loss, as 8.58 : 17.03, or as 100 : 198.4. They clearly prove that the reaction is as I have stated. The quantity of iodine taken varies from 2 to 20 equivalents. The number of these experiments may appear superfluous, but there were some circumstances connected with the reaction when a smaller quantity of iodine was employed, which caused me particularly to inquire whether any excess of iodine would alter its nature. It is plain that this cannot be done. I have also ascertained the fact, that with a perfectly pure peroxide an equivalent of iodine always decomposes exactly an equivalent of the peroxide, so that if an excess of the peroxide be taken it remains undecomposed. In this important point this reaction differs from the other experiments I have given, in which very small quantities of the substance decompose the *whole* of the peroxide present. With iodine this will not take place.

The decomposition of the chloride of silver by the peroxide of barium is a true link between these experiments with iodine and the reduction of the metallic oxides. In whatever rational form we may express these facts, the facts themselves are the same. We cannot *see* the mode in which the action is effected, nor for the present argument is it at all important whether we consider, as we reasonably may, that the

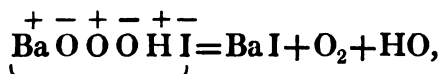
change takes place* by the decomposition of water, and that the oxygen which is formed is the result of the combination of the element of the water with the element of the peroxide, or whether we consider the iodine to be transferred directly to the barium, and the whole of the oxygen to come from the peroxide itself. Whatever theory accounts for this reaction, the same theory, I will venture to assert, will explain the decomposition of the metallic oxides. It remains only to show why it is that this decomposition with iodine comes before us as a chemical reaction of the simplest nature, and that in the other experiments the decomposition is apparently so variable. The reason I believe to be this, that in these latter experiments the peroxide is simultaneously decomposed in two ways, between which we must discriminate, by the action of contact and by the chemical action of the oxide or chloride. The decomposition by platinum is a pure contact action, in which the peroxide loses only one equivalent of oxygen. The decomposition with iodine is a pure chemical action, in which two equivalents of oxygen are given off. The action with metallic oxides is an action compounded of these two, and therefore it is that the loss, as I have already pointed out, ever lies between these two limits. There are two causes by either or by both of which this decomposition may be effected; the one, the metallic silver or other substance which is the result of the reduction; the other, the very oxide or chloride or peroxide itself which is reduced. Certain oxides which are not themselves reduced, can unquestionably act in this manner; and it is highly probable therefore that those which are reduced, and which belong to the same class of bodies, have the same property. On this view therefore the law of the formation of the oxygen is ever one and the same, and so far as relates to the chemical action of the peroxide of barium on the metallic oxide this is ever definite. The fact therefore which is expressed by the series of ratios is by no means any different forms of combination of the particles of the oxygen with each other, but simply the way in which the decomposition of the peroxide of barium is distributed between these different modes of action, and the relative velocity with which these two modes of decomposition take place. If, for example, the ratio of the loss of oxygen from the peroxide of barium to that from the metallic oxide, be as 4 : 1, three parts of the peroxide of barium will have been decomposed by the contact and one by the chemical action; if the ratio be as 2 : 1, the decomposition will have taken place in equal proportions between the two. All the phenomena are perfectly consistent with this view. As a fact, we know that the contact action proceeds far more rapidly at a high than at a low temperature; we need not therefore be surprised if the relative velocity of this to the chemical action

* I have made the experiment of heating the dry peroxide with oxide of silver until the latter was reduced, but the loss of oxygen was only that of the silver. It is however remarkable, that when it is heated in a similar manner with chloride of silver, the chloride is reduced and the two equivalents of oxygen of the peroxide evolved. This decomposition takes place also with baryta, but not with lime; and I cannot but suspect that it is connected with the peculiar property of baryta of taking up a second equivalent of oxygen which does not belong to lime. It is moreover extremely difficult to prepare a baryta quite free from peroxide and water, so as to make a truly comparative experiment.

increases with the temperature. By increasing the mass of the oxide or chloride, on the other hand, the velocity of the chemical action is relatively increased; in this case therefore we have a great reduction. Again, a substance in a fine state of division is decomposed more rapidly than one which is dense and compact; here, therefore, and in other similar differences, is the cause of the different results given by different preparations of the peroxide*.

By the words *contact* and *chemical*, I mean to express no theory, and am indeed drawing but a momentary distinction between facts, for I cannot believe that the one of these portions of oxygen is formed according to one law and the other in a different manner, that the one fact is a chemical synthesis and the other not. The only difference between them I believe to be, that in the case of the action I have called chemical, the peroxide of barium decomposes peroxide of lead, or oxide of silver, or the like; and in the contact action one particle of peroxide of barium decomposes another particle of the same substance, the platinum, silver oxide or other body causing that chemical relation between the particles which renders the decomposition possible. The important question as to the nature of that definite relation which these experiments indicate between the contact action and the chemical action, I reserve for future inquiry.

I have made various experiments to ascertain whether the same kind of reduction could be effected by means of other metallic peroxides, such, for example, as the peroxide of lead or manganese, as with the peroxide of barium and potassium, but without success. I believe this to admit of a very rational explanation, and that this reducing power is a result of the peculiar chemical nature of the alkaline metals and their compounds; in short, of what is usually termed their powerful chemical affinities and position on the electro-chemical scale, to which indeed we may distinctly trace it. This relation may be expressed in various ways, but which mean essentially one and the same thing. If, for example, in the last experiment we express the change which takes place thus—



it follows, from the view I have given of the mutual relation of these particles, and the interdependence of these chemical changes, that we cannot substitute for the barium a metal which stands in a different chemical relation to the particles between which it is placed without altering the relation of all the other particles of the system, so that if the chemical difference on which combination depends does not exist between the metal and the iodine, neither can it exist between the particles of the oxygen. In this way the fact, at first sight very anomalous, admits of a very simple explanation,

* I by no means wish to exclude from these a real chemical difference in their nature, which the behaviour of certain analogous bodies leads me to suspect.

that the peroxide of barium, the less reducible peroxide, is that which under these circumstances is thus readily decomposed.

Lastly, let me observe, that I have limited my assertions as to the nature of the elemental bodies, to the statement, that, under certain conditions, there exists a chemical difference between the particles of which they consist. On the chemical nature of these particles I have offered no opinion. The apparent and obvious inference from the experiments is doubtless that the elements form a peculiar group of chemical substances, consisting of similar particles or atoms in a state of combination, as other bodies consist of dissimilar. Of this view, in the early part of this paper, I have already spoken; it is the hypothesis of AMPÈRE. Many theoretical difficulties however meet us, when we come logically and consistently to carry out this idea to the explanation of chemical phenomena, especially to the phenomena of direct combination and to the simplest cases of chemical decomposition; and it by no means follows that, although the apparent, it is the rational and philosophical interpretation of the experiments. Besides this view and that of BERZELIUS, there is yet a third hypothesis which we may form as to the constitution of these elements, namely, that they consist of yet other and further elements. On this view, the real fact which lay hid under these phenomena, might be the synthesis of the oxygen from the ultimate and further elements of which the oxygen consisted. In the present state of our knowledge it is useless to dilate on this idea; but on the assumption that the elemental bodies are in this sense compound, we may, I believe, account for all these experiments without ever assuming a chemical difference between two similar particles; and it appears to me perfectly possible, that in such a constitution of the elemental bodies, these experiments, together with the phenomena of allotropy, may find their ultimate solution.

XXXVIII. *Supplementary Observations on the Diffusion of Liquids.**By* THOMAS GRAHAM, F.R.S., F.C.S.

Received May 2,—Read June 20, 1850.

THE experiments of my former paper furnished strong grounds for believing that isomorphous salts possess a similar diffusibility. All the salts of potash and ammonia, which were compared, appeared to be equi-diffusive; so also were the salts of certain magnesian bases. A single preliminary observation on the nitrates of lead and baryta, however, opposed the general conclusion, and demanded further inquiry. It is scarcely necessary to say that any new means of recognizing the existence of the isomorphous relation between different substances, must prove highly valuable. Let us inquire therefore how far liquid diffusion is available for that purpose.

The salts were still diffused from weak solutions, that is from solutions containing from 1 to 8 per cent. of salt; but now a measure of the solution, equal to 100 grs. of water, was made to contain 1 grain of the salt, to form what is called the 1 per cent. solution; instead of 1 grain of salt being added to 100 grs. of water, as before, without reference to the condensation which generally occurs. The quantities 1, 2, 4 and 8 per cent. thus indicate the parts of salt present in a constant volume of liquid,—as 10, 20, 40 and 80 grs. of the salt in 1000 water grain-measures of the solution. The same phials for the solution and jars for the external water-atmosphere continued to be used, and the manipulations were similar. It is believed, however, that the temperature of the liquids was maintained more uniform in the new experiments than the old, partly by the better regulation of the temperature of the apartment, and partly by placing the jars close together upon a table with upright ledges, and covering the whole over with sheets of paper during the continuance of an experiment. The mass of fluid in 80 or 100 jars, which were employed at once and placed together, made the small oscillations of temperature, which might still occur, slow and less injurious.

The investigation is also extended to several new substances, such as hydrocyanic acid, acetic acid, sulphurous acid, alcohol, ammonia and salts of organic bases, without reference to isomorphous relations. It is very necessary to have data which are minute and accurate respecting the diffusion of a considerable variety of substances. This it is my present object to endeavour to supply, leaving speculative deductions in general respecting the nature and laws of liquid diffusion for a future occasion.

The density of all the solutions was observed at a constant temperature, namely, 60° FAHR.

1. *Hydrochloric Acid.*

The period of diffusion arbitrarily chosen for this acid was five days. The diffusate, or quantity of acid diffused, was determined by precipitating the liquid of the external reservoirs with nitrate of silver, and weighing the chloride of silver formed. In the 1 and 2 per cent. solutions, the liquids of two jars were generally mixed and precipitated together.

(1.) Hydrochloric acid, 0.99 per cent.; density 1.0043. Diffused at 53°5, in six cells, 7.52, 7.52, 7.42; mean 7.49 grs. for two cells. Calculated for 1 per cent., 7.56 grs. at 53°5 for two cells, or 7.41 grs. at 51°, when corrected for that temperature.

(2.) Hydrochloric acid, 1.92 per cent.; density 1.009. Diffused at 51°, in eight cells, 14.71, 14.05, 14.54, 14.47; mean 14.44 grs. for two cells. Calculated for 2 per cent., 15.04 grs. at 51° for two cells.

(3.) Hydrochloric acid, 1.993 per cent.; density 1.0094. Diffused at 62°8, in six cells, two experiments on one-sixth part of the mixed jars gave 8.203, 8.198; mean 8.20 grs. for one cell, or 16.40 grs. for two cells. Calculated for 2 per cent., 16.46 grs. at 62°8 for two cells.

(4.) Hydrochloric acid, 3.90 per cent.; density 1.0190. Diffused at 51°, in eight cells, 29.18, 30.70, 30.70, 29.26; mean 29.96 grs. for two cells. Calculated for 4 per cent., 30.72 grs. at 51° for two cells.

(5.) Hydrochloric acid, 7.90 per cent.; density 1.0380. Diffused at 51°, in four cells, 32.71, 33.64, 33.64, 33.74; mean 33.43 grs. for one cell. Calculated for 8 per cent., 33.84 grs. at 51° for one cell.

Comparing the diffusibilities of the 2 per cent. solutions (2 and 3) at 51° and 62°8, an increase is observed from 15.04 to 16.40 grs., or from 100 to 109.1, which gives an increase of 0.77 per cent. for 1°. This method of estimating the effect of temperature is not exact, as the times only in which an equal diffusion at the different temperatures takes place are truly comparable. We may deduce from it, however, the effect of the small difference of temperature of 2°5 of the 1 per cent. solution from the others, as has been done, without sensible error. The diffusates at the same temperature would then be as follows:—

Diffusion of Hydrochloric Acid in five days at 51° FAHR.; two cells.

	Grs.	Ratio.
From 1 per cent. solution	7.41	0.97
From 2 per cent. solution	15.04	2.00
From 4 per cent. solution	30.72	4.08
From 8 per cent. solution	67.68	9.00

The increasing diffusibility with the larger proportions of acid here observed is unusual, at least in the degree exhibited by the 8 per cent. solution. Other substances, as will be immediately observed of nitric acid, appear to lose proportionally in diffusibility as their solutions are concentrated.

Hydrochloric acid belongs to the most diffusive class of substances known; it appears to exceed hydrate of potash at $53^{\circ}5$, as 7.56 to 6.12, or as 100 to 80.9*.

The rapidity with which hydrochloric acid diffuses, and the facility with which that substance may be estimated, induced me to examine the progression with which its diffusion takes place with increasing times in a minute manner. The 2 per cent. solution was diffused for times increasing by six hours, from twelve hours or 0.5 day to 4.75 days, six cells being diffused for every period. Instead of determining the acid diffused separately in each jar or pair of jars, the contents of the six jars of each experiment were mixed together, and a definite proportion of the liquid precipitated by nitrate of silver, so as to obtain at once the mean result. Another observation for 5.75 days is added, although made at a sensibly higher temperature.

Diffusion of Hydrochloric Acid, 2 per cent. solution; one cell.

Time.	Temperature.	Diffusate in grains.	Differences.
days.			
0.5	53.75	0.909	
0.75	53.75	1.312	.403
1	53.75	1.766	.454
1.25	53.75	2.353	.587
1.5	53.75	2.596	.243
1.75	53.58	3.178	.582
2	53.58	3.410	.232
2.25	53.42	3.967	.557
2.5	53.58	4.339	.372
2.75	53.50	4.618	.279
3	53.50	4.969	.351
3.25	53.50	5.304	.335
3.5	54.85	5.857	.553
3.75	54.85	6.254	.397
4	54.85	6.407	.153
4.25	54.85	6.795	.388
4.5	54.71	7.034	.239
4.75	54.71	7.473	.339
5.75	56.46	8.363	

The differences are evidently affected by accidental errors of observation. The diffusion at 3.5 days is also increased by a rise of temperature of more than 1° in that and the following experiments. The diffusion always increases with the time, but less rapidly, according to a gradually diminishing progression.

2. *Hydriodic Acid, Hydrobromic Acid and Bromine.*

Hydriodic Acid.—Time of diffusion five days, as for hydrochloric acid. The acid diffused was estimated from the iodide of silver which it gave when precipitated by nitrate of silver.

Hydriodic acid, 1.98 per cent.; density 1.0142. Diffused at $53^{\circ}5$, in eight cells, 14.90, 15.67, 15.25, 15.27; mean 15.27 grs. for two cells. Calculated for 2 per cent., 15.42 grs. at $53^{\circ}5$ for two cells, or 15.11 grs. at 51° .

* Philosophical Transactions, 1850, p. 39.

These experiments indicate a similarity of diffusion between the two isomorphous substances, hydrochloric and hydriodic acids.

Diffusion from 2 per cent. solutions at 51° FAHR.

Hydrochloric acid	15·04	100
Hydriodic acid	15·11	100·46

Hydrobromic Acid.—Time of diffusion five days. The diffusate was estimated from the bromide of silver.

(1.) Hydrobromic acid, 1·556 per cent.; density 1·0112. Diffused at 59°·7, in eight cells. The whole diffusates mixed together gave by analysis a mean of 12·90 grs. of hydrobromic acid in two cells; calculated for 2 per cent., 16·58 grs. in two cells, at 59°·7.

(2.) The experiment was repeated at 59°·8, with a solution containing 1·578 per cent. of hydrobromic acid, of density 1·0116, with five diffusion phials not employed above. The mean diffusate for a pair of cells was 13·05 grs. of hydrobromic acid; that is, 16·53 grs. for a 2 per cent. solution, which is as nearly as possible the result of the preceding series of experiments.

(3.) Another solution containing exactly 2 per cent. of hydrochloric acid was diffused for comparison in eight cells, in the same circumstances of time and temperature as (1.); its density was 1·0104.

Diffusate from 2 per cent. solutions at 59°·7 FAHR.

Hydrochloric acid	16·55	100
Hydrobromic acid	16·58	100·18

Hydrobromic acid appears therefore to coincide in diffusibility with hydrochloric acid at this temperature. It may be remarked that these three acids, hydrochloric, hydrobromic and hydriodic, do not exhibit the same correspondence in another physical property, namely, the densities of their aqueous solutions containing the same proportion of acid. The densities of 2 per cent. solutions of hydrochloric and hydriodic acids appear to be respectively 1·0104 and 1·0143, at 60° FAHR., and that of hydrobromic acid will obviously be an intermediate number. The same acids are also known to differ considerably in the boiling-points of solutions containing the same proportion of acid. A considerable diversity of physical properties appears here to be compatible with equal diffusibility in substances which are isomorphous.

Bromine.—Pure water readily dissolves more than 1 per cent. of this substance. The solution prepared, however, contained only 0·864 per cent. of bromine, as was ascertained by treating it with sulphurous acid and afterwards precipitating by nitrate of silver. Its density was 1·0070. It was evident, from the slow appearance of the brown colour in the exterior cell, that bromine diffuses less rapidly than hydrobromic acid.

101.4 per cent., 1000 grs. in volume.

58, with a solution containing 1.573 per cent., with five diffusion phials not employed, the result was 13.05 grs. of hydrobromic acid, which is as nearly as possible the result.

2 per cent. of hydrochloric acid was diffused under the same circumstances of time and temperature.

solutions at 59°·7 FAHR.

16.55	100
16.58	100.18

coincide in diffusibility with hydrochloric acid. It is deduced that these three acids, hydrochloric, hydrobromic, and hydriodic, show the same correspondence in another phial. For aqueous solutions containing the same amount of acid, the results are the same.

per cent. diffused at 62°·2, in six cells, 11.40, 11.80, 11.80; mean 11.68 grs. for two cells. Calculated for 2 per cent., 13.23 grs. at 64°·2 in two cells, or about 13.10 grs. at 62°·8, assuming this acid to be affected in the same way by temperature as hydrochloric acid.

Hydrocyanic acid here appears less diffusive than hydrochloric acid, at the same temperature 62°·8, as 13.10 to 16.40, or as 79.6 to 100, and not to belong therefore to the same class of diffusive substances.

4. Nitric Acid.

Time of diffusion five days. The quantity of this acid diffused was always determined with great exactness by neutralization by means of a normal solution of carbonate of soda.

1. Nitrate of water (HO.NO_5), 1 per cent.; density 1.0052. Diffused at 50°·8, in eight cells, 6.77, 6.77, 7.26, 6.97; mean 6.94 grs. of nitrate of water in two cells at 50°·8, and 6.99 grs. by estimate at 51°·2.

2. Nitrate of water, 1 per cent.; density 1.0052. Diffused at 53°·5, in six cells, 7.32, 7.32, 7.20; mean 7.28 grs. in two cells.

3. Nitrate of water, 1.92 per cent.; density 1.0112. Diffused at 51°·2, in eight cells, 14.34, 14.24, 14.10, 13.96; mean 14.16 grs. in two cells. Calculated for 2 per cent., 14.74 grs. at 51°·2 in two cells.

4. Nitrate of water, 2 per cent.; density 1.0106. Diffused at 63°·2, in eight cells,

The diffusion of the different proportions of this acid at one temperature is as follows :—

Diffusion of Nitrate of Water in five days at $51^{\circ}2$; two cells.

	Grs.	Ratio.
From 1 per cent. solution	6.99	0.95
From 2 per cent. solution	14.74	2
From 4 per cent. solution	28.76	3.90
From 8 per cent. solution	57.92	7.86

The 2 per cent. solution is taken as the standard of comparison for the ratios, instead of the 1 per cent. solution, from the greater accuracy with which the diffusion of the former can be observed.

The usual approach to equality of diffusion, between chlorides and nitrates, is observable in hydrochloric and nitric acids, at least in the 1 and 2 per cent. solutions.

Diffusion from 1 per cent. solution at $53^{\circ}5$.

Hydrochloric acid	7.56	100
Nitrate of water	7.28	96.3

Diffusion from 2 per cent. solution.

Hydrochloric acid at 51°	15.04	100
Nitrate of water at $51^{\circ}2$	14.74	98.0

The 2 per cent. solutions of both acids were also diffused at higher temperatures.

Diffusion from 2 per cent solution.

Hydrochloric acid at $62^{\circ}8$	16.46	100
Nitrate of water at $63^{\circ}2$	16.76	101.8

Here the diffusibility of the two acids is as nearly as possible equal.

Diffusion from 4 per cent. solution.

Hydrochloric acid at 51°	30.72	100
Nitrate of water at $51^{\circ}2$	28.76	93.7

Diffusion from 8 per cent. solution.

Hydrochloric acid at 51°	67.68	100
Nitrate of water at $51^{\circ}2$	57.92	85.3

The wide divergence between these two acids, in the 8 per cent. solution, is produced by the remarkably increased diffusion of hydrochloric acid in that high proportion.

between chlorides and nitrates, is de-
ast in the 1 and 2 per cent. solutions

solution at 53°5.

. . . 7.56	100
. . . 7.28	96.3

er cent. solution.

. . . 15.04	100
. . . 14.74	98.0

re also diffused at higher temperatures.

er cent solution.

. . . 16.46	100
. . . 16.76	101.8

nearly as possible equal.

3. Sulphate of water, 2 per cent.; density 1.0155. Diffused at 63°5, in eight cells, 19.80, 20.05, 19.67, 19.41; mean 19.73 grs. for two cells.

4. Sulphate of water, 3.87 per cent.; density 1.0261. Diffused at 49°7, in eight cells, 32.72, 32.72, 33.06, 32.58; mean 32.77 grs. for two cells. Calculated for 4 per cent., 33.89 grs. at 49°7 for two cells.

5. Sulphate of water, 7.90 per cent.; density 1.0513. Diffused at 49°7, in four cells, 34.08, 34.76, 33.74, 33.63; mean 34.05 grs. for one cell. Calculated for 8 per cent., 34.48 grs. at 49°7 for one cell.

In the 2 per cent. solution the diffusion rises, with the difference of temperature between 49°7 and 63°5, from 16.91 to 19.73 grs., or from 100 to 116.68. This is an increase of 1.209 per cent. for one degree of temperature.

The diffusion of the different proportions of sulphuric acid is as follows:—

Diffusion of Sulphate of Water in ten days at 49°7; two cells.

	Grs.	Ratio.
From 1 per cent. solution	8.69	1.03
From 2 per cent. solution	16.91	2
From 4 per cent. solution	33.89	4.01
From 8 per cent. solution	68.96	8.16

The diffusibility of different strengths of this acid appears to be pretty uniform, but with a slight tendency to increase in the higher proportions, like hydrochloric acid.

Sulphuric acid is not so diffusive as hydrochloric acid, but

3. Acetate of water, 8 per cent.; density 1·0117. Diffused at 48°·8, in four cells, 21·19, 20·13, 21·84, 20·44; mean 20·90 grs. for one cell. The diffusion of the different proportions of acetic acid is as follows:—

Diffusion of Acetate of Water in ten days at 48°·8; two cells.

	Grs.	Ratio.
From 2 per cent. solution	11·31	2
From 4 per cent. solution	22·02	3·83
From 8 per cent. solution	41·80	7·26

The diffusibility diminishes with the larger proportions of acid. This acid appears to be considerably less diffusive than sulphuric acid. I was led to over-estimate the diffusion of acetic acid in a preliminary observation of my former paper, by trusting to the acidimetical method of determination. Hydrochloric acid appears to diffuse about two and a half times more rapidly than acetate of water, at the same temperature.

8. *Sulphurous Acid.*

The time of diffusion chosen for this acid was ten days, for comparison with sulphuric acid. The usual number of eight cells of the 1 and 2 per cent. solutions

for two cells.
 1.0117. Diffused at 48° 8, in four cells
 one cell. The diffusion of the differ

ten days at 48° 8; two cells.

	Gm.	Ratio.
...	11.31	2
...	22.02	3.83
...	41.80	7.26

er proportions of acid. This acid appears
 uric acid. I was led to over-estimate the
 servation of my former paper, by trusting
 on. Hydrochloric acid appears to diffuse
 an acetate of water, at the same temp-
 ous Acid.
 acid was ten days, for comparison with
 cells of the 1 and 2 per cent. solutions

From 8 per cent. solution . . . 66.38 7.827

This substance appears to be less diffusive than sulphuric acid at the same temperature; the diffusion of sulphurous acid at 68° 1 considerably resembles that of sulphuric acid at 49° 7 (p. 811).

9. Ammonia.

The time of diffusion chosen was 4.041 days, or that of hydrate or potash with chloride of sodium at seven days. The usual number of eight cells of the 1 and 2 per cent. solutions were diffused, and four cells of the 4 and 8 per cent. solutions. The whole diffusates of each proportion were then mixed together, and the quantity of ammonia diffused for two cells determined by an alkalimetric experiment, which was always repeated twice. It was necessary for diffusion to have the ammoniacal solution made denser than water, which was effected by the addition of common salt.

1. 1.005 per cent. of ammonia, density made up to 1.00352 with chloride of sodium, diffused at 63° 4, gave 4.96 grs. for two cells; calculated for 1 per cent., 4.93 grs. of ammonia in two cells at 63° 4.

2. 2.01 per cent. of ammonia, density made up to 1.00617 with chloride of sodium, diffused at 63° 4, gave 9.64 grs. for two cells; calculated for 2 per cent., 9.59 grs. of ammonia in two cells at 63° 4.

3. 4.02 per cent. of ammonia, density made up to 1.01141 with chloride of sodium, diffused at 63° 4, gave 9.91 grs. for one cell; calculated for 4 per cent., 9.86 grs. of

at $48^{\circ}7$, in eight cells, 34.30, 30.20 ; mean 32.25 grs. for four cells, or 16.12 grs. for two cells.

3. Alcohol, 8 per cent. ; density made up to 1.0154 with chloride of sodium. Diffused at $48^{\circ}7$, in four cells, 30.80, 40.2 ; mean 35.50 grs. for two cells, or 17.75 grs. for one cell.

The results accord less closely with each other than usual, owing, I believe, chiefly to the difficulties of manipulation when the density of the liquid placed in the phials to be diffused approaches so nearly to that of water. This is more particularly true of the 8 per cent. solution.

Diffusion of Alcohol in ten days at $48^{\circ}7$; two cells.

From 2 per cent. solution	8.62
From 4 per cent. solution	16.12
From 8 per cent. solution	35.50

It would be unsafe to draw any conclusion as to the proportionality of the diffusion of alcohol to the strength of the solution from these experiments.

Alcohol does not appear to belong to the same class of diffusive substances as acetic acid, which might be expected from their similarity of composition, but possesses a considerably lower diffusibility.

7.72 grs. for two cells.

3. Nitrate of baryta, 2 per cent; density 1.01686. Diffused at 64°1, in eight cells, 15.63, 14.81, 14.41, 15.32; mean 15.04 grs. for two cells.

4. Nitrate of baryta, 4 per cent.; density 1.03319. Diffused at 64°1, in four cells, 15.36, 14.78, 14.79, 14.30; mean 14.80 grs. for one cell.

5. Nitrate of baryta, 8 per cent; density 1.06556. Diffused at 64°1, in four cells, 26.46, 26.77, 28.63, 27.13; mean 27.25 grs. for one cell.

The diffusion from the 1 per cent. solution increases by a rise of temperature from 51°5 to 64°1, from 6.73 grs. to 7.72, or from 100 to 114.7, which is an increase of 1.17 per cent for 1°.

Diffusion of Nitrate of Baryta in 11.43 days at 64°1; two cells.

	Grs.	Ratio.
From 1 per cent. solution . . .	7.72	1.026
From 2 per cent. solution . . .	15.04	2
From 4 per cent. solution . . .	29.60	3.936
From 8 per cent. solution . . .	54.50	7.247

12. Nitrate of Strontia.

Time of diffusion 11.43 days. Of anhydrous nitrate of strontia 0.82 per cent.; density 1.0063. Diffused at 51°5, in eight cells, 5.59, 5.62, 5.44, 5.69; mean 5.59 grs.

13. *Nitrate of Lime.*

Time of diffusion 11·43 days. The diffusate was evaporated to dryness with an excess of sulphuric acid, and the nitrate of lime, which is always supposed anhydrous, was estimated from the sulphate of lime produced.

1. Nitrate of lime, 1·17 per cent; density 1·0088. Diffused at 51°·5, in eight cells, 7·39, 7·76, 7·69, 7·80; mean 7·66 grs. for two cells; calculated for 1 per cent., 6·54 grs. at 51°·5 for two cells.

2. Nitrate of lime, 0·985 per cent.; density 1·00802. Diffused at 64°·1, in eight cells, 7·47, 7·38, 7·63, 7·72; mean 7·55 grs. for two cells; calculated for 1 per cent., 7·66 grs. at 64°·1 for two cells.

3. Nitrate of lime, 1·97 per cent.; density 1·01508. Diffused at 64°·1, in eight cells, 15·04, 14·74, 14·55, 14·83; mean 14·79 grs. for two cells; calculated for 2 per cent., 15·01 grs. at 64°·1 for two cells.

4. Nitrate of lime, 3·94 per cent.; density 1·0296. Diffused at 64°·1, in four cells, 14·30, 15·29, 13·79, 13·93; mean 14·33 grs. for one cell; calculated for 4 per cent., 14·52 grs. at 64°·1 for one cell.

5. Nitrate of lime, 7·88 per cent.; density 1·0582. Diffused at 64°·1, in four cells, 27·95, 27·10, 26·80, 26·73; mean 27·14 grs. for one cell; calculated for 8 per cent., 27·55 grs. at 64°·1 for one cell.

By a rise of temperature from 51°·5 to 64°·1, the diffusion of the 1 per cent. solution increases from 6·54 to 7·66 grs., or from 100 to 117·1; which is an increase of 1·357 per cent. for 1°.

Diffusion of Nitrate of Lime in 11·43 days at 64°·1; two cells.

	Grs.	Ratio.
From 1 per cent. solution . . .	7·66	1·021
From 2 per cent. solution . . .	15·01	2
From 4 per cent. solution . . .	29·04	3·872
From 8 per cent. solution . . .	55·10	7·334

The results throughout for this salt are almost identical with those of nitrate of baryta (p. 815), although these two salts differ greatly in solubility, and in one being a hydrated, and the other an anhydrous salt.

14. *Acetate of Lead.*

Diffused for 16·166 days; the time chosen before for sulphate of magnesia, with seven days for chloride of sodium. The solution contained 0·965 per cent. of anhydrous salt, with the density 1·0080. As this solution of acetate of lead was found to be precipitated by pure water, about 2 per cent. of strong acetic acid was introduced into the solution, and the same acid was added in a less proportion to the water jars. The salt of lead diffused was afterwards determined by means of sulphuric acid. Diffused in eight cells, at 53°·1, 7·45, 7·29, 7·46 and 8·07 grs.; mean 7·56; or 7·84 for 1 per cent. in two cells.

ty 1.01508. Diffused at 64°1, in eight cells for two cells; calculated for 2 per cent.

ty 1.0296. Diffused at 64°1, in four cells for one cell; calculated for 4 per cent.

ty 1.0582. Diffused at 64°1, in four cells for one cell; calculated for 8 per cent.

64°1, the diffusion of the 1 per cent. solution 100 to 117.1; which is an increase of 17.1

in 11.43 days at 64°1; two cells.

	Gr.	Ratio.
...	7.66	1.021
...	15.01	2
...	29.04	3.872
...	55.10	7.334

are almost identical with those of nitrate of baryta in solubility, and in one test

Acetate of lead at 53°1 7.84 104.53

Here, of two isomorphous salts, that of greatest atomic weight sensibly exceeds the other in diffusibility.

16. Chloride of Barium.

Time of diffusion 11.43 days. The diffused salt was weighed as sulphate of baryta.

1. Chloride of barium, 0.99 per cent. Diffused at 50°9, in eight cells, 7.91, 7.27, 7.42, 7.12; mean 7.43 grs. of chloride of barium for two cells; calculated for 1 per cent., 7.50 grs. at 50°9 for two cells.

The diffusion of this salt being manifestly more rapid than that of the chloride of calcium, a shorter time was tried, which is to seven days, the time of chloride of sodium, as the square root of 3 to the square root of 4.5. Time of diffusion 8.57 days.

2. Chloride of barium, 1.01 per cent.; density 1.0095. Diffused at 63°, in eight cells, 6.46, 6.44, 6.41, 6.27; mean 6.39 grs. for two cells; calculated for 1 per cent., 6.32 grs. at 63° for two cells.

3. Chloride of barium, 2.02 per cent.; density 1.0183. Diffused at 63°, in eight cells, 11.98, 12.03, 12.75, 12.03; mean 12.20 grs. for two cells; calculated for 2 per cent., 12.07 grs. at 63° for two cells.

4. Chloride of barium, 4.04 per cent.; density 1.0359. Diffused at 63°, in four cells, 12.43, 12.30, 11.87, 11.86; mean 12.10 grs. for one cell; calculated for 4 per cent., 11.98 grs. at 63° for one cell.

4. Chloride of strontium, 4·014 per cent.; density 1·03537. Diffused at 63°, in four cells, 12·09, 11·75, 11·64, 11·79; mean 11·82 grs. for one cell; calculated for 4 per cent., 11·78 grs. at 63° for one cell.

5. Chloride of strontium, 8·028 per cent.; density 1·06959. Diffused at 63°, in four cells, 22·29, 22·34, 22·03, 22·57; mean 22·31 grs. for one cell; calculated for 8 per cent., 22·23 grs. at 63° for two cells.

Diffusion of Chloride of Strontium in 8·57 days at 63°; two cells.

	Grs.	Ratio.
From 1 per cent. solution . . .	6·09	1·045
From 2 per cent. solution . . .	11·66	2
From 4 per cent. solution . . .	23·56	4·041
From 8 per cent. solution . . .	44·46	7·626

The series of ratios in the preceding table will be found on comparison to correspond closely with the ratios of chloride of barium. It may be useful to compare further the amounts diffused from similar solutions of these two isomorphous compounds.

Diffusion in 8·57 days at 63°; two cells.

Chloride of barium 1 per cent	6·32	100
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is sufficiently close, but in the 4 and 8 per cent. the salts diverge, as happens also with hydrochloric and nitric acids themselves. The nitrate in both cases falls off, while the chloride sustains throughout the high diffusibility of the lower proportions.

19. *Chloride of Manganese.*

Time of diffusion 11·43 days. The salt diffused was estimated by means of nitrate of silver.

The 1 per cent. solution, of density 1·0085, gave at 50°·8, in eight cells, 6·67, 6·26, 6·79 and 6·81 grs.; mean 6·63 for two cells.

20. *Nitrate of Magnesia.*

Time of diffusion 11·43 days. The salt diffused was estimated as sulphate.

The 1 per cent. solution, of density 1·0073, gave at 50°·8, in eight cells, 6·29, 6·39, 6·52 and 6·76 grs.; mean 6·49 for two cells.

21. *Nitrate of Copper.*

Time of diffusion 11·43 days. The salt diffused was estimated from the oxide of copper obtained by ignition.

The 1 per cent. solution, of density 1·0075, in eight cells, at 50°·8, gave 6·52, 6·36,

cent., 21.41 grs. of sulphate of magnesia in one cell at 65°4.

5. 8.07 per cent. of sulphate of magnesia, density 1.07830, diffused at 62°8, in four cells, 21.12, 21.20, 22.13, 21.77; mean 21.55 grs. for one cell; calculated for 8 per cent., 21.33 grs. of sulphate of magnesia in one cell at 62°8.

6. 16.14 per cent. of sulphate of magnesia, density 1.15054, diffused at 62°8, in four cells, 37.08, 38.39, 38.65, 37.50; mean 37.90 grs. for one cell; calculated for 16 per cent., 37.53 grs. of sulphate of magnesia in one cell at 62°8.

7. 24.22 per cent. of sulphate of magnesia, density 1.21882, diffused at 62°8, in four cells, 49.38, 50.40, 53.36, 53.00; mean 51.53 grs. for one cell; calculated for 24 per cent., 51.02 grs. of sulphate of magnesia in one cell at 62°8.

Diffusion of Sulphate of Magnesia in 16.16 days at 65°4; two cells.

	Grs.	Ratio.
From 1 per cent. solution	7.31	1.144
From 2 per cent. solution	12.79	2
From 4 per cent. solution	23.46	3.671
From 8 per cent. solution	42.82	6.701
From 8 per cent. solution at 62°8 .	42.66	1
From 16 per cent. solution at 62°8 .	75.06	1.759
From 24 per cent. solution at 62°8 .	109.04	2.310

8. 4.182 per cent. of sulphate of alumina, density 1.0438, diffused at 65°4, gave 3.17 grs. of alumina and 6.91 grs. of sulphuric acid, in all 10.08 grs. for one cell. Calculated for 4 per cent., 3.03 grs. of alumina and 6.61 grs. of sulphuric acid, in all 9.64 grs. of sulphate of alumina for one cell at 65°4.

3. 4.182 per cent. of sulphate of alumina, density 1.0438, diffused at 65°4, gave 3.17 grs. of alumina and 6.91 grs. of sulphuric acid, in all 10.08 grs. for one cell. Calculated for 4 per cent., 3.03 grs. of alumina and 6.61 grs. of sulphuric acid, in all 9.64 grs. of sulphate of alumina for one cell at 65°4.

4. 8.364 per cent. of sulphate of alumina, density 1.08518, diffused at 65°4, gave 5.37 grs. of alumina and 12.15 grs. of sulphuric acid, in all 17.52 grs. for one cell. Calculated for 8 per cent., 5.14 grs. of alumina and 11.62 grs. of sulphuric acid, in all 16.76 grs. of sulphate of alumina for one cell at 65°4.

Diffusion of Sulphate of Alumina in 16.166 days at 65°4; two cells.

	Grs.	Ratio.
From 1 per cent. solution . . .	5.48	1.074
From 2 per cent. solution . . .	10.21	2
From 4 per cent. solution . . .	19.28	3.780
From 8 per cent. solution . . .	33.52	6.572

The diffusion of sulphate of alumina, it will be observed, is very sensibly less than that of sulphate of zinc at the same temperature.

Diffusion of Nitrate of Soda in seven days at $63^{\circ}4$; two cells.

	Grs.	Ratio.
From solution of 2 per cent. . . .	12.35	2
From solution of 4 per cent. . . .	23.56	3.82
From solution of 8 per cent. . . .	47.74	7.73

The ratios of the last column of the preceding Table are sensibly the same as those already obtained for nitrate of silver. But the diffusibility of nitrate of soda appears to be increased less rapidly by temperature than nitrate of silver. Hence the diffusibility of these two salts appears more similar at low than high temperatures.

Diffusion from 2 per cent. solutions in seven days at 53° .

Nitrate of silver	11.24	100
Nitrate of soda	10.81	96.17

Diffusion from 2 per cent. solutions in seven days at $63^{\circ}4$.

Nitrate of silver	13.61	100
Nitrate of soda	12.35	90.74

32. *Chloride of Sodium.*

Time of diffusion seven days. The salt diffused was treated with nitrate of silver, and the chloride of silver weighed.

1. Chloride of sodium, 1 per cent. Diffused at $50^{\circ}5$, in eight cells, 5.96, 5.69, 5.54, 5.50; mean 5.70 grs. of chloride of sodium for two cells.

2. Chloride of sodium, 0.985 per cent. Diffused at $53^{\circ}4$, in eight cells, 5.86, 5.86, 5.77, 5.76; mean 5.81 grs. for two cells; calculated for 1 per cent., 5.89 grs. at $53^{\circ}4$ for two cells.

3. Chloride of sodium, 1 per cent.; density 1.00776. Diffused at $63^{\circ}4$, in eight cells, 6.30, 6.18, 6.52, 6.30; mean 6.32 grs. for two cells.

4. Chloride of sodium, 2 per cent.; density 1.01483. Diffused at $63^{\circ}4$, in eight cells, 12.37, 12.08, 12.45, 12.53; mean 12.37 grs. for two cells.

5. Chloride of sodium, 4 per cent.; density 1.02879. Diffused at $63^{\circ}4$, in four cells, 12.56, 12.65, 12.55, 12.17; mean 12.48 grs. for one cell.

6. Chloride of sodium, 8 per cent.; density 1.0562. Diffused at $63^{\circ}4$, in four cells, 25.11, 25.36, 22.82, 23.59; mean 24.22 grs. for one cell.

The rise of temperature from $50^{\circ}5$ to $63^{\circ}4$ increases the diffusion of the 1 per cent. solution of chloride of sodium from 5.70 to 6.32, or from 100 to 110.9, which is an increase of 0.843 per cent. for 1° .

Diffusion of Chloride of Sodium in seven days at $63^{\circ}4$; two cells.

	Grs.	Ratio.
From 1 per cent. solution	6.32	1.023
From 2 per cent. solution	12.37	2
From 4 per cent. solution	24.96	4.036
From 8 per cent. solution	48.44	7.832

The difference would be about 1 per cent. greater if the diffusion of both salts were reduced to the same temperature. The chloride of potassium deviates of course from the nitrate of soda in a similar manner. But chloride of potassium corresponds more closely with nitrate of silver than with chloride of sodium and nitrate of soda, at the temperature of the experiments.

Diffusion of Chloride of Potassium for 5·71 days at 62°, and of Nitrate of Silver for 7 days at 63°·4.

Chloride of potassium, 2 per cent. .	13·32	100
Nitrate of silver, 2 per cent. . . .	13·61	102·18
Chloride of potassium, 4 per cent. .	25·94	100
Nitrate of silver, 4 per cent. . . .	26·34	101·54
Chloride of potassium, 8 per cent. .	53·64	100
Nitrate of silver, 8 per cent. . . .	51·88	96·71

The coincidence in rate would appear even closer in the 2 and 4 per cent. solutions, if the diffusion of the nitrate of silver was diminished about 1 per cent., on account of its higher temperature. It might thus be supposed that the nitrate of silver followed the sodium rate more accurately than the nitrate of soda and chloride of sodium themselves do.

at 59°·8, in eight cells, it gave a mean diffusate of 12·30 grs. for two cells ; or 12·46 grs. for 2 per cent.

For comparison, a solution of *chloride of potassium*, containing exactly 2 per cent. of salt and having the density 1·0133, was diffused in the same circumstances of time and temperature as the two preceding salts. The mean diffusate of eight cells was 12·24 grs. for two cells.

Hence the following result of the diffusion of three isomorphous salts :—

Diffusion of 2 per cent. solutions in 5·716 days, at 59°·8.

	Grs.	Ratio.
Chloride of potassium	12·24	100
Bromide of potassium	12·46	101·80
Iodide of potassium	12·51	102·21
Mean	12·40	

Iodide of Sodium.—Time of diffusion 7 days, temperature 59°·8. A solution of 2·011 per cent. and density 1·01618, diffused in eight cells, gave a mean diffusate of 12·24 grs. for two cells ; that is, 12·18 grs. for 2 per cent. solution.

Bromide of Sodium.—Time of diffusion and temperature as above. A solution of 2·146 per cent., of density 1·01726, diffused in eight cells, gave a mean diffusate of 12·80 grs. ; that is, 11·93 grs. for 2 per cent.

A comparative experiment was made with a solution of *chloride of sodium*, containing 1·917 per cent. of salt and of density 1·01376, in eight cells, at 60°. The diffusates for four pairs of cells were 11·65, 11·75, 11·63 and 11·47 grs. ; mean 11·63 grs., which gives by proportion 12·14 grs. for a 2 per cent. solution. As the present salt differs only 0°·2 FAHR. in diffusion-temperature from the two preceding salts, which is inadequate to produce an assignable difference of diffusion, the three salts may be supposed to be diffused at the same temperature, without sensible error.

Diffusion of 2 per cent. solutions for 7 days.

	Grs.	Ratio.
Chloride of sodium at 60°	12·14	100
Bromide of sodium at 59°·8	11·93	98·27
Iodide of sodium at 59°·8	12·18	100·33
Mean	12·08	

In both these isomorphous groups of salts of potassium and sodium, there is certainly a near approach to equality of diffusion. The times for the salts of the two bases being in the empirical proportion of the square roots of 2 and 3, the mean diffusates also approach pretty closely ; namely, 12·40 grs. for the salts of potassium and 12·08 grs. for the salts of sodium, which are as 100 to 97·42. Here the members of each group are certainly very similar to each other in density and probably other physical pro-

the excess of chlorine was removed by agitation of the solution with air. The proportion of salt present was thus increased in weight from 1 to 1.36 per cent. The time of diffusion was 11.43 days, and the temperature 53°.

2. Chloride of copper diffused from a 1.36 per cent. solution of the salt in hydrochloric acid, 5.83, 5.66, and 5.30 grs. in two cells; mean 5.60 grs.

The corresponding diffusion from a 1 per cent. solution may be supposed to be less than 5.6 grs., in the proportion of 1.36 to 1, without any great error. The results thus become chloride of copper diffused, 3.98, 3.85 and 3.58 grs.; mean 3.80 grs. in two cells.

It hence appears that the diffusion of chloride of copper is much diminished by the presence of a great excess of hydrochloric acid in the same solution. Different causes suggest themselves for this result, such as the possibility of a combination existing of chloride of copper with chloride of hydrogen, in the acid solution; or the influence which must be admitted of the more soluble substance, in a mixture of two similar substances, in repressing the diffusion of the less soluble. The present result, however, is entirely opposed to the idea that the high diffusibility of the dichloride of copper, observed before, is due to the hydrochloric acid present.

3. The diffusion of chloride of sodium also appears to be repressed by contact with a large excess of hydrochloric acid. One per cent. of chloride of sodium raised the density of dilute hydrochloric acid from 1.035 to 1.0408. Diffused into pure water for seven days at 52° 9, in eight cells, the diffusates of chloride of sodium were 3.80, 3.87, 4.00 and 3.86 grs.; mean 3.88 for two cells. The diffusion of chloride of sodium is thus reduced in a corresponding measure with that of chloride of copper by association with seven times its weight of hydrochloric acid.

These results are interesting in a very different point of view. I have always watched for the appearance of some absorbent or imbibing power on the part of the acids, more analogous to an endosmotic attraction for water, as usually conceived. If such an attraction existed, it would complicate the phenomena of diffusion, for the volume of water absorbed by the acid would displace and project a portion of the latter into the reservoir, the phial not being extensible. The high diffusibility of hydrochloric and nitric acids would be thus explained. But by such a mechanical displacement the chloride of sodium would be thrown out in the preceding experiment, as well as the hydrochloric acid, which is not the case.

4. Even in hydrochloric acid of density 1.124 (25 per cent.), the diffusion of 1 per cent. of chloride of sodium for seven days, at 56° 6, was found to amount to 4.7 grs. only in two cells, and is less than from a solution in pure water.

5. In comparing the influence of nitric acid with that of hydrochloric acid upon the diffusion of chloride of sodium, it was found that in a 7 per cent. solution of nitric acid, the chloride of sodium (1 per cent.) was entirely decomposed in the diffusive process, at 56° 6, and gave hydrochloric acid in the full diffusive equivalent of that acid, together with nitrate of soda.

From 8 per cent. solution 50.10 7.346

The amount and progression of the diffusion of this salt correspond well, for all the proportions diffused, with the preceding isomorphous bicarbonate of potash.

39. *Bicarbonate of Soda.*

Time of diffusion 9.875 days. The usual number of eight cells of the 1 and 2 per cent. solutions were diffused, and four cells of the 4 and 8 per cent. solutions. The whole diffusates of each proportion were then mixed together, and the quantity of bicarbonate of soda, diffused for two cells, converted into chloride of sodium, evaporated to dryness and weighed.

1. 1.135 per cent. of bicarbonate of soda, $\text{HO} \cdot \text{CO}_2 + \text{NaO} \cdot \text{CO}_2$, density 1.00892, diffused at 68°F , gave 8.30 grs. for two cells. Calculated for 1 per cent., 7.31 grs. of bicarbonate of soda in two cells.

2. 2.27 per cent. of bicarbonate of soda, density 1.01703, diffused at 68°F , gave 15.68 grs. for two cells. Calculated for 2 per cent., 13.81 grs. of bicarbonate of soda in two cells.

3. 4.54 per cent. of bicarbonate of soda, density 1.03306, diffused at 68°F , gave 15.16 grs. for one cell. Calculated for 4 per cent., 13.35 grs. of bicarbonate of soda in one cell.

Hydrochlorate of strychnine

For a similar period of 11·43 days, but at a lower temperature, $53^{\circ}4$, the 1 per cent. solution of hydrochlorate of morphine gave a mean result of 5·49 grs. from two cells, and the hydrochlorate of strychnine 5·77 grs. from two cells. But the weights of chloride of silver from which these numbers are deduced were too small to admit of much precision.

The diffusion of these salts of organic bases in 11·43 days, is exceeded by the diffusion of chloride of ammonium or potassium in 5·71 days, or half the former time. The vegeto-alkalies appear thus to be divided from ammonia and potash.

The new observations of the present paper are favourable to the existence of a relation amounting to close similarity or equality in diffusibility between certain classes of substances.

The chlorides and nitrates of the same metal generally exhibit this correspondence, as in the chloride of calcium and nitrate of lime, the chloride of sodium and nitrate of soda, and also in hydrochloric and nitric acids.

Isomorphous salts exhibit the same relation, as has been observed in the chlorides, bromides and iodides of potassium, sodium and hydrogen, in various salts of baryta, strontia and lead, in numerous magnesian salts, in the salts of silver, soda, and probably those of suboxide of copper. and in several additional salts of potash and

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