MEMOIRS

OF THE

LITERARY

AND

PHILOSOPHICAL SOCIETY

OF

MANCHESTER.

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CONTENTS.

MEMOIRS

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OF

Manchester.

INVESTIGATION

OF THE

CURVE OF QUICKEST DESCENT,

IN REFERENCE TO MR. EWART'S PROBLEM,

Memoirs, vol. 2nd., New Series, page 248 :

IN A LETTER TO MR. DALTON, F. R. S.

President of the Society.

BY THE REV. EDMUND SIBSON.

Ashton, Warrington, Oct. 26th, 1824.

DEAR SIR,

You told me, last summer, that you had never received my solution to the following problem, proposed by Mr. Ewart, in the Manchester Memoirs, vol. 2, page 248: I, therefore, beg leave to send you another solution, which I will thank you to propose for insertion, in the Memoirs of the Manchester Society.

This problem was proposed, as the prize question, in No. 15, of Leybourn's Mathematical Repository; but no satisfactory answer was given to it. Indeed Euler's Theorem, by which the problem was proposed to be solved, gives a very complicated result.

The principle used in the solution of this problem is that of John Bernoulli, by which he solved his own problem of quickest descent. The way in which this principle is here used, does not give an exact solution; but it affords a good approximation.

If you have any thing to say about this problem, I shall be glad to hear from you.

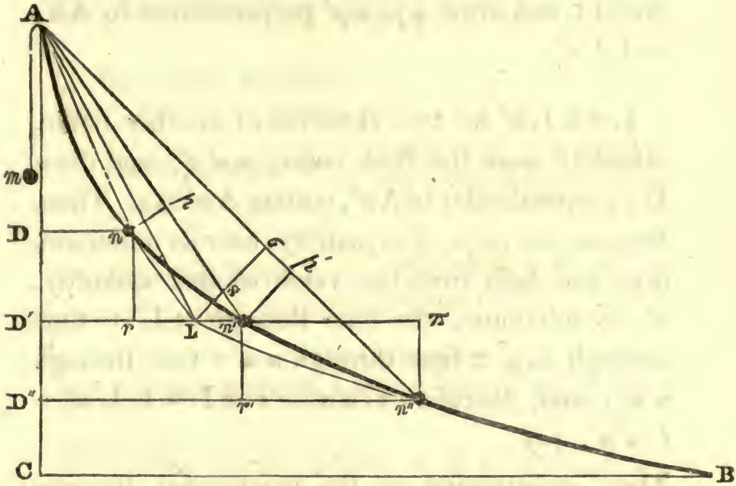
I am, dear Sir,

Yours, sincerely,

EDMUND SIBSON.

PROBLEM.

If two weights, m and n, be connected by a string, passing over a pulley, at A, in what curve must the weight n move, that it may descend from A to B, in the least time possible.



Let A $n\,n'n''$ B be the curve required. And, let the time through A n together with the time through n''B be supposed constant: then, the time through the two elements of the curve, nn' and $n'n''$ must be a minimum.

Let AD, AD′, AD″, be the consecutive values of the abscissa.

Let D n, D$'$ n', D$''$ n'', be the consecutive values of the ordinate.

Let A n, A n', A n'', be the consecutive values of the string and weight n.

Draw $n\,r$, $n'\,r'$, perpendicular to D$'$ n' and D$''$ n'': and draw $n''\,\pi$, perpendicular to D$'$ n' produced; and draw $n\,p$, $n'p'$ perpendicular to A n', and A n''.

Let n L n'' be two elements of another curve, infinitely near the first curve, $n\,n'\,n''$; and draw L σ perpendicular to An'', cutting A n' in s. Then, because the value of a quantity, near its minimum, does not differ from the value of that quantity, at its minimum, the time through n L + time through L n'' = time through $n\,n'$ + time through $n'n''$: and, therefore $t\,.\,n\,n' - t\,.\,n\,\mathrm{L} = t\,.\,\mathrm{L}\,n'' - t\,.\,n'n''$. (a)

Then, substituting in the mechanical theorem

$$4\,g\,m\,\mathrm{F}\,x + 4\,g\,m'\,\mathrm{F}'\,x' = m\,v^2 + m'\,v'^2$$
$$m = n$$
$$m' = m$$
$$\mathrm{F} = \mathrm{F}' = 1 = \text{force of gravity.}$$
$$x = \mathrm{A\,D},\ x' = -\mathrm{A}\,n.$$
$$u = \text{velocity of } m$$
$$v = \text{velocity of } n$$
$$4\,g\,n\ \mathrm{A\,D} - 4\,g\,m\ \mathrm{A}\,n = n\,v^2 + m\,u^2.$$

But $v^2 : u^2 :: n\,n'^2 : p\,n'^2 \therefore u^2 = \dfrac{p\,n'^2}{n\,n'^2} \cdot v^2$

Therefore $4gn\,\text{AD} - 4gm\,\text{A}n = \dfrac{n \,.\, nn'^2 + m \,.\, pn'^2}{nn'^2} \,.\, v^2$

$$\therefore v = 2\sqrt{g} \,.\, nn' \sqrt{\frac{n\,\text{AD} - m\,\text{A}n}{n \,.\, nn'^2 + m \,.\, pn'^2}},$$

which needs no correction.

And, time through $nn' = \dfrac{s}{v} = \dfrac{\sqrt{(n \times nn'^2 + m \times pn'^2)}}{2\sqrt{g} \,.\, \sqrt{(n \,.\, \text{AD} - m \,.\, \text{A}n,)}}$.

In the same manner,

$$\text{time through } n'n'' = \frac{\sqrt{(n \,.\, n'n''^2 + m \,.\, p'n''^2)}}{2\sqrt{g} \,.\, \sqrt{(n \,.\, \text{AD}' - m \,.\, \text{A}n')}},$$

$$\text{time through } n\text{L} = \frac{\sqrt{(n \,.\, n\text{L}^2 + m \,.\, ps^2)}}{2\sqrt{g} \,.\, \sqrt{(n \,.\, \text{AD} - m \,.\, \text{A}n)}},$$

and the time through $\text{L}n'' = 2\dfrac{\sqrt{(n \,.\, \text{L}n''^2 + m \,.\, \sigma n''^2)}}{\sqrt{g} \,.\, \sqrt{(n \,.\, \text{AD}' - m \,.\, \text{A}n')}}$,

A n' being always nearly $=$ A L, when n is at finite distance from A.

Therefore, by substituting in equation *(a)*

$$\frac{\sqrt{(n \,.\, nn'^2 + m \,.\, pn'^2)} - \sqrt{(n \,.\, n\text{L}^2 + m \,.\, ps^2)}}{\sqrt{(n \,.\, \text{AD} - m \,.\, \text{A}n)}} =$$

$$\frac{\sqrt{(n \,.\, \text{L}n''^2 + m \,.\, \sigma n''^2)} - \sqrt{(n \,.\, n'n''^2 + m \,.\, p'n''^2)}}{\sqrt{(n \,.\, \text{AD}' - m \,.\, \text{A}n')}}$$

But $n\,\mathrm{L}^2 = n\,n'^2 + \mathrm{L}\,n'^2 - 2\,\mathrm{L}\,n' \,.\, n\,r = n\,n'^2 - 2\,\mathrm{L}n'.r'\,n$; because $\mathrm{L}\,n'$ is very small when compared with $n\,n'$: also $p\,s^2 = \overline{p\,n' - s\,n'}|^2 = p\,n'^2 - 2\,p\,n' \,.\, s\,n' =$ $p\,n'^2 - 2\,p\,n' . \dfrac{\mathrm{D}\,n'}{\mathrm{A}\,n'} . \mathrm{L}\,n'$: because the triangles $\mathrm{L}\,n'\,s$ and $\mathrm{A}\,n'\,\mathrm{D}'$, are similar.

Therefore $\sqrt{(n \,.\, n\,\mathrm{L}^2 + m \,.\, p\,s^2)} =$

$$\sqrt{\left(n.n\,n'^2 + m.p\,n'^2 - n.n\,r'.2\,\mathrm{L}\,n' - m.p\,n'.\frac{\mathrm{D}\,n'}{\mathrm{A}\,n'}.2\,\mathrm{L}\,n'\right)} =$$

$$\sqrt{(n \,.\, n\,n'^2 + m \,.\, p\,n'^2)} - \frac{n \,.\, n\,r' + m \,.\, p\,n' \,.\, \dfrac{\mathrm{D}\,n}{\mathrm{A}\,n}}{\sqrt{(n.\,n\,n'^2 + m \,.\, p\,n'^2)}} \,.\, \mathrm{L}\,n';$$

for $\dfrac{\mathrm{D}\,n}{\mathrm{A}\,n} = \dfrac{\mathrm{D}\,n'}{\mathrm{A}\,n'}$, nearly, when n is at a finite distance from A: and only two terms of the series are necessary, because $\mathrm{L}\,n'$ is infinitely smaller than $n\,n'$ or $p\,n'$. Hence

$$\frac{\sqrt{(n \,.\, n\,n'^2 + m \,.\, p\,n'^2)} - \sqrt{(n \,.\, n\,\mathrm{L}^2 + m \,.\, p\,s^2)}}{\sqrt{(n.\,\mathrm{A}\,\mathrm{D} - m.\,\mathrm{A}\,n)}} =$$

$$\frac{n \,.\, n\,r' + m \,.\, \dfrac{\mathrm{D}\,n}{\mathrm{A}\,n} \,.\, p\,n'}{\sqrt{(n \,.\, n\,n'^2 + m \,.\, p\,n'^2)} \,.\, \sqrt{(n \,.\, \mathrm{A}\,\mathrm{D} - m \,.\, \mathrm{A}\,n)}} \,.\, \mathrm{L}\,n'.$$

In the same manner

$$\frac{\sqrt{(n \,.\, \text{L}\, n''^2 + m \,.\, \sigma\, n''^2)} - (\sqrt{n \,.\, n'\, n''^2 + m \,.\, p'\, n''^2})}{\sqrt{(n \,.\, \text{A D}' - m \cdot \text{A}\, n')}} =$$

$$\frac{n \,.\, n'\, \pi + m \,.\, \frac{\text{D}\, n'}{\text{A}\, n'} \,.\, p'\, n''}{\sqrt{(n \,.\, n'\, n''^2 + m \,.\, p'\, n''^2)} \,.\, \sqrt{(n \,.\, \text{A D}' - m \,.\, \text{A}\, n')}} \,.\, \text{L}\, n'.$$

Therefore, by substitution,

$$\frac{n \,.\, n\, r' + m \,.\, \frac{\text{D}\, n}{\text{A}\, n} \,.\, p\, n'}{\sqrt{(n \,.\, n'\, n'^2 + m \,.\, p'\, n'^2)} \,.\, \sqrt{(n \,.\, \text{A D} - m \,.\, \text{A}\, n)}} =$$

$$\frac{n \,.\, n''\, r' + m \,.\, \frac{\text{D}\, n'}{\text{A}\, n'} \,.\, p'\, n''}{\sqrt{(n \,.\, n'\, n''^2 + m \,.\, p'\, n''^2)} \,.\, \sqrt{(n \,.\, \text{A D}' - m \,.\, \text{A}\, n')}}.$$ Now

these quantities being consecutive and similar, it is evident, that

$$\frac{n \,.\, n' r + m \,.\, \frac{\text{D}\, n}{\text{A}\, n} \,.\, p\, n'}{\sqrt{(n \,.\, n n'^2 + m \,.\, p\, n'^2)} \,.\, \sqrt{(n \,.\, \text{A D} - m \,.\, \text{A}\, n)}} =$$ a constant quantity.

Let $\text{A D} = x$, $\text{D}\, n = y$, $n\, r = \dot{x}$, $r\, n' = \dot{y}$, the Curve $\text{A}\, n = z$, $n n' = \dot{z}$, the Cord $\text{A}\, n = p$, and $p\, n' = \dot{p}$.

Then by substitution, $\dfrac{n\dot{y}+\dfrac{my\dot{p}}{p}}{\sqrt{(n\dot{z}^2+m\dot{p}^2)}\sqrt{(nx-mp)}}=a.$

Cor. 1. When $m=o$; this expression becomes $\dfrac{\dot{y}}{\dot{z}\sqrt{x}}=a$, the equation to a cycloid.

Cor. 2. If in the equation $\dfrac{n\dot{y}+\dfrac{my\dot{p}}{p}}{\sqrt{(n\dot{z}^2+m\dot{p}^2)}.\sqrt{(nx-mp)}}=a$, $n=2$, and $m=a=1$; then $2p\dot{y}+y\dot{p}=p\sqrt{(2x-p)}.\sqrt{(2\dot{z}^2+\dot{p}^2;)}$ and squaring both sides, $4p^2\dot{y}^2+4py\dot{p}\dot{y}+y^2\dot{p}^2=(4p^2x-2p^3)\dot{z}^2+(2p^2x-p^3)\dot{p}^2$.

But $x^2=p^2-y^2$; and taking the fluxions, $x\dot{x}=p\dot{p}-y\dot{y}$; and squaring both sides $x^2\dot{x}^2=p\dot{p}^2-2py\dot{p}\dot{y}+y^2\dot{y}^2$: and therefore $4py\dot{p}\dot{y}=2p^2\dot{p}^2+2y^2\dot{y}^2-2x^2\dot{x}^2$. And by substitution, $(6p^2-2x^2)\dot{y}^2-2x^2\dot{x}^2=(4p^2x-2p^3)\dot{z}^2+(2p^2x-p^3-3p^2+x^2)\dot{p}^2$.(1).

But $\dot{y}^2=\overline{\dfrac{p\dot{p}-x\dot{x}}{p^2-x^2}}\Big|^2=\dfrac{p^2\dot{p}^2-2px\dot{p}\dot{x}+x^2\dot{x}^2}{p^2-x^2}$: and

$\dot{z}^2=\dot{y}^2+\dot{x}^2=\dfrac{p^2\dot{p}^2-2px\dot{p}\dot{x}+p^2\dot{x}^2}{p^2-x^2}$.

Therefore, by substituting for $\dot{y}^2$, and $\dot{z}^2$, in equation (1), and by transposition,

$$-9p^4-3p^5+\frac{12p^3 x\dot{p}\dot{x}}{\dot{p}^2}+\frac{4p^4 x\dot{p}\dot{x}}{\dot{p}^2}+6p^4x+\frac{4p^4 x\dot{x}^2}{\dot{p}^2}$$

$$-\frac{2p^5\dot{x}^2}{\dot{p}^2}-\frac{4p^2x^2\dot{x}^2}{\dot{p}^2}+6p^2x^2+p^3x^2-\frac{8p^3x^2\dot{p}\dot{x}}{\dot{p}^2}$$

$$-\frac{4px^3\dot{p}\dot{x}}{\dot{p}^2}-2p^2x^3-x^4=o.$$

Let $x=p+Ap^2+Bp^3+Cp^4+Dp^5$; then,

$$-x^4 = -p^4-4Ap^5-(6A^2+4B)p^6$$
$$-(12AB+4A^3+4C)p^7 \ldots\ldots\ldots\ldots\ldots$$
$$-(A^4+12A^2B+12AC+6B^2+4D)p^8.$$

$$-2p^2x^3 = \ldots\ldots\ldots\ldots -2p^5 \ldots\ldots\ldots\ldots\ldots -6Ap^6$$
$$-(6A^2+6B)p^7-(12AB+6C+2A^3)p^8.$$

$$-\frac{4px^3\dot{p}\dot{x}}{\dot{p}_2} = -4p^4-20Ap^5-(36A^2+24B)p^6$$
$$-(28A^3+84AB+28C)p^7 - \ldots\ldots\ldots$$
$$-(8A^4+96A^2B+96AC+48B^2+32D)p^8.$$

$$-\frac{8p^3x^2\dot{p}\dot{x}}{\dot{p}^2} = \ldots\ldots\ldots -8p^5 \ldots\ldots\ldots -32Ap^6$$
$$-(40A^2+40B)p^7 \ldots\ldots\ldots\ldots\ldots\ldots$$
$$-(16A^3+96AB+48C)p^8.$$

$$+ p^3 x^2 = \ldots\ldots\ldots\ldots + p^5 \ldots\ldots\ldots\ldots + 2 A p^6$$
$$+ (2 B + A^2) p^7 + (2 A B + 2 C) p^8$$

$$+ 6 p^2 x^2 = + 6 p^4 + 12 A p^5 + (6 A^2 + 12 B) p^6$$
$$+ (12 A B + 12 C) p^7 + (12 A C + 12 D + 6 B^2) p^8 .$$

$$- \frac{4 p^2 x^2 \dot{x}^2}{\dot{p}^2} = - 4 p^4 - 24 A p^5 - (52 A^2 + 32 B) p^6$$
$$-(48 A^3 + 136 A B + 40 C) p^7 \ldots\ldots$$
$$-(16 A^4 + 184 A^2 B + 168 A C + 88 B^2 + 48 D) p^8 .$$

$$- \frac{2 p^5 \dot{x}^2}{\dot{p}^2} = \ldots\ldots\ldots - 2 p^5 \ldots\ldots\ldots\ldots - 8 A p^6$$
$$-(8 A^2 + 12 B) p^7 - (24 A B + 16 C) p^8 .$$

$$+ \frac{4 p^4 x \dot{x}^2}{\dot{p}^2} = \ldots\ldots + 4 p^5 \ldots\ldots\ldots\ldots + 20 A p^6$$
$$+ (32 A^2 + 28 B) p^7 \ldots\ldots\ldots\ldots$$
$$+ (16 A^3 + 88 A B + 36 C) p^8 .$$

$$+ 6 p^4 x = \ldots\ldots\ldots\ldots + 6 p^5 \ldots\ldots\ldots\ldots + 6 A p^6$$
$$+ 6 B p^7 \ldots\ldots\ldots + 6 C p^8 .$$

$$+ \frac{4 p^4 x \dot{p} \dot{x}}{\dot{p}^2} = \ldots\ldots + 4 p^5 \ldots\ldots\ldots\ldots + 12 A p^6$$
$$+ (16 B + 8 A^2) p^7 + (20 A B + 20 C) p^8 .$$

$$+\frac{12\,p^3\,\dot{x}\,\dot{p}\,\dot{x}}{\dot{p}^2} = +12p^4 + 36\,A\,p^5 + (48\,B + 24\,A^2)\,p^6$$

$$+(60\,A\,B + 60\,C)\,p^7 \ldots\ldots\ldots\ldots\ldots\ldots$$

$$+(72\,A\,C + 36\,B^2 + 72\,D)\,p^8.$$

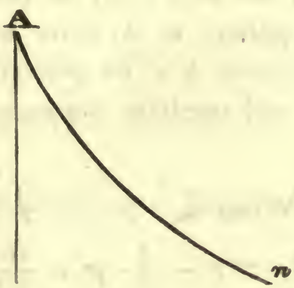
$$-3\,p^5 = \qquad -3\,p^5.$$

$$-9\,p^4 = \qquad -9\,p^4.$$

And by equating the Coefficients of the same powers of p, $A = -\frac{3}{32}$, $B = \frac{11}{18}A^2$, $C = -\frac{385}{648}A^3$.

Therefore $x = p - \frac{3}{32}p^2 + \frac{11}{18}A^2p^3 - \frac{385}{648}A^3p^4$, &c.

And when $n = 2$, and $m = a = 1$, the curve takes the following form.



Cor. 3. When A D is a maximum,

$$x = p - \frac{3}{32} p^2 + \frac{11}{18} A^2 p^3 - \frac{385}{648} A^3 p^4 =$$

$$= p - \frac{3}{2^5} p^2 + \frac{11}{2^{11}} p^3 \text{ nearly, } = \text{maximum:}$$

from which p will be found impossible.

Therefore, x and p increase together.

Cor. 4. Because $v = 2\sqrt{g . n n'} \sqrt{\frac{(n . AD - m . An)}{(n . n n'^2 + m . p n'^2)}}$,

when $v = o$; $n\,AD - m\,An = o$, or $m : n :: AD : An$.

Therefore, in any curve, when $m : n :: AD : An$, n will descend from A to n: and it will not pass beyond n; for, at n, its velocity $= o$. Neither will it rest on the curve, at n, because the curve, n A, falls below the inclined plane n A, on which n would rest: the weight, n, will, therefore, ascend to A. For the same reasons, it will descend again from A. And, therefore, if the cord A n, be perfectly flexible, and if the pulley, at A, move without friction, and if the curve A n, be perfectly smooth, the weight, n, will oscillate between A and n for ever.

Cor. 5. When $m : n :: 1 : 2 :: x : p$, $x = \frac{p}{2}$

$$\therefore \frac{p}{2} = p - \frac{3}{2^5} p^2 + \frac{11}{2^{11}} p^3 \text{ nearly,}$$

from which p will be found impossible.

Therefore, the descending body will not oscillate on this curve.

Cor. 6. If n be any point in the curve; then, when $n = 2$, it is the least weight, that will descend from A to n, in the least time.

SCHOLIUM.

This Problem may be solved, in the following manner, by Euler's Theorem.

Let $p = \frac{\dot{y}}{\dot{x}}$: then, fluent $v\,\dot{x} =$ fluent $\frac{\sqrt{(n\dot{z}^2 + m\dot{p}^2)}}{2\sqrt{g}.\sqrt{(nx - mp)}}$

$$= \text{fluent}\sqrt{\frac{\left(n(\dot{x}^2 + \dot{y}^2) + m\,\dot{x}^2 . \frac{\left(x + y . \frac{\dot{y}}{\dot{x}}\right)^2}{x^2 + y^2}\right)}{2\sqrt{g}.\sqrt{\left(n\,x - m\,.\sqrt{x^2 + y^2}\right)}}}$$

$$= \text{fluent}\sqrt{\frac{\left(n\,(1 + p^2) + m\,.\frac{\overline{x + y\,p}|^2}{x^2 + y^2}\right)}{2\sqrt{g}.\sqrt{\left(n\,x - m\,.\sqrt{x^2 + y^2}\right)}}}.\,\dot{x} =$$

Minimum, Woodhouse's Isoperimetrical Problems, page 51.

But $\dot{v} = M\,\dot{x} + N\,\dot{y} + P\,\dot{p}$: therefore,

$$\dot{v} = \frac{1}{2\sqrt{g}} \cdot \frac{n p\dot{p} + m.\frac{x+yp}{x^2+y^2}(\dot{x}+\dot{y}p+p\dot{y}) - m.\frac{(x+yp)^2}{(x^2+y^2)^2}(x\dot{x}+y\dot{y})}{\sqrt{\left(n(1+p^2)+m.\frac{(x+yp)^2}{x^2+y^2}\right)}\cdot\sqrt{\left(nx - m\sqrt{x^2+y^2}\right)}}$$

$$-\frac{1}{4\sqrt{g}} \cdot \frac{\left(n\dot{x} - m\ \frac{x\dot{x}+y\dot{y}}{\sqrt{x^2+y^2}}\right)\sqrt{\left(n(1+p^2)+m.\frac{\overline{x+yp})^2}{x^2+y^2}\right)}}{(nx - m\sqrt{x^2+y^2})^{\frac{3}{2}}}$$

$$= \left\{ \frac{1}{2\sqrt{g}} \cdot \frac{m.\frac{x+yp}{x^2+y^2} - mx.\frac{\overline{x+yp})^2}{\overline{x^2+y^2})^2}}{\sqrt{\left(n(1+p^2)+m\frac{\overline{x+yp})^2}{x^2+y^2}\right)}\sqrt{\left(nx - m.\sqrt{x^2+y^2}\right)}}\right.$$

$$\left. -\frac{1}{4\sqrt{g}} \cdot \frac{\left(n - \frac{mx}{\sqrt{(x_2+y^2)}}\right).\sqrt{\left(n(1+p^2)+m.\frac{\overline{x+yp})^2}{x^2+y^2}\right)}}{\left(nx - m\sqrt{x^2+y^2}\right)^{\frac{3}{2}}}\right\}\dot{x}$$

$$+\left\{ \frac{1}{2\sqrt{g}} \cdot \frac{mp.\frac{x+yp}{x^2+y^2} - my.\frac{\overline{x+yp})^2}{x^2+y^2}}{\sqrt{\left(n(1+p^2)+m\frac{\overline{x+yp})^2}{x^2+y^2}\right)}.\sqrt{\left(nx - m\sqrt{x^2+y^2}\right)}}\right.$$

$$\left. +\frac{1}{4\sqrt{g}} \cdot \frac{\frac{my}{\sqrt{(x^2+y^2)}}.\sqrt{\left(n(1+p^2)+m.\frac{\overline{x+yp})^2}{x^2+y^2}\right)}}{\left(nx - m\sqrt{x^2+y^2}\right)^{\frac{3}{2}}}\right\}\dot{y}$$

$$+\left\{\frac{1}{2\sqrt{g}} : \frac{np + my \cdot \frac{x+yp}{x^2+y^2}}{\sqrt{\left(n(1+p^2) + m\frac{\overline{x+yp)}^2}{x^2+y^2}\right)}\sqrt{\left(nx - m\sqrt{x^2+y^2}\right)}}\right\}\dot{p}$$

Now $N - \frac{\dot{P}}{\dot{x}} = o$; $\therefore$ $P =$ fluent $N\dot{x} +$

Correction; and therefore

$$\frac{1}{2\sqrt{g}} \cdot \frac{np + my \cdot \frac{x+yp}{x^2+y^2}}{\sqrt{\left(n(1+p^2) + m \cdot \frac{\overline{x+yp)}^2}{x^2+y^2}\right)}\sqrt{\left(nx - m\sqrt{x^2+y^2}\right)}}$$

$$= \frac{1}{2\sqrt{g}} \cdot \text{fluent of} \left\{ \frac{mp \cdot \frac{x+yp}{x^2+y^2} - my \cdot \frac{\overline{x+yp)}^2}{x^2+y^2)^2}}{\sqrt{\left(n(1+p^2) + m \cdot \frac{\overline{x+yp)}^2}{x^2+y^2}\right.} \cdot \sqrt{\left(nx - m\sqrt{x^2+y^2}\right)}} \cdot \dot{x} \right\}$$

$$+ \frac{1}{4\sqrt{g}} \cdot \text{fluent} \left\{ \frac{\frac{my}{\sqrt{x^2+y^2}}\sqrt{\left(n(1+p^2) + m \cdot \frac{\overline{x+yp)}^2}{x^2+y^2}\right)}}{\left(nx - m\sqrt{x^2+y^2}\right)^{\frac{3}{2}}} \dot{x} \right\}$$

+ Cor.; or, by substitution,

$$\frac{n\dot{y} + \frac{my\dot{p}}{p}}{\sqrt{(n\dot{z}^2 + m\dot{p}^2)} \cdot \sqrt{(nx - mp)}} = \text{fluent}$$

$$\frac{\frac{m\dot{y}\dot{p}}{p} - \frac{m y \dot{p}^2}{p^2}}{\sqrt{(n\dot{z}^2 + m\dot{p}^2)} \cdot \sqrt{(n x - m p)}} + \text{fluent}$$

$$\frac{m y}{2 p} \cdot \frac{\sqrt{(n\dot{z}^2 + m\dot{p}^2)}}{(n x - m p)^{\frac{3}{2}}} \pm \text{Cor.}$$ But, in

Euler's Theorem, $N' = N$; therefore

$$\frac{n\dot{y} + \frac{m y \dot{p}}{p}}{\sqrt{(n\dot{z}^2 + m\dot{p}^2)} \cdot \sqrt{(n x - m p)}} = \text{fluent}$$

$$\frac{\frac{m\dot{y}'\dot{p}'}{p'} - \frac{m y' \dot{p}'^2}{p'^2}}{\sqrt{(n\dot{z}'^2 + m\dot{p}'^2)} \cdot \sqrt{(n x' - m p')}} + \text{fluent}$$

$$\frac{m y'}{2 p'} \cdot \frac{\sqrt{(n\dot{z}'^2 + m\dot{p}'^2)}}{(n x' - m p')^{\frac{3}{2}}} + \text{Cor.}$$ And taking

the Fluxions,

$$\frac{n\dot{y}' + \frac{m y' \dot{p}'}{p'}}{\sqrt{(n\dot{z}'^2 + m\dot{p}'^2)} \cdot \sqrt{(n x' - m p')}} - \frac{n\dot{y} + \frac{m y \dot{p}}{p}}{\sqrt{(n\dot{z}^2 + m\dot{p}^2} \cdot \sqrt{(n x - m p)}}$$

$$= \frac{\frac{m\dot{y}'\dot{p}'}{p'} - \frac{m y' \dot{p}'^2}{p'^2}}{\sqrt{(n\dot{z}'^2 + m\dot{p}'^2)}\sqrt{(n x' - m p')}} + \frac{m y'}{2 p'} \cdot \frac{\sqrt{(n\dot{z}'^2 + m\dot{p}'^2)}}{(n x' - m p')^{\frac{3}{2}}}$$

But $\dot{y}'\dot{p}'$ and $\ddot{p}'^2$ are infinitely smaller than $\dot{p}'$ and therefore the term,

$$\frac{\frac{m\dot{y}'\dot{p}'}{p'} - \frac{m y' \dot{p}'^2}{p'^2}}{\sqrt{(n z'^2 + m p'^2)} \cdot \sqrt{(n x' - m p')}}$$ may be neglected.

Therefore, $$\frac{n\dot{y}' + \frac{m y' \dot{p}'}{p'}}{\sqrt{(n\dot{z}'^2 + m\dot{p}'^2)} \cdot \sqrt{(n x' - m p')}}$$

$$= \frac{n\dot{y} + \frac{m y \dot{p}}{p}}{\sqrt{(n\dot{z}^2 + m\dot{p}^2)} \cdot \sqrt{(n x - m p)}}$$

$$= \frac{m y}{2p} \cdot \frac{\sqrt{(n\dot{z}^2 + m\dot{p}^2)}}{(n x - m p)^{\frac{3}{2}}} \text{ nearly.}$$

Also $\frac{m y}{2p} \cdot \frac{\sqrt{(n\dot{z}^2 + m\dot{p}^2)}}{(n x - m p)^{\frac{3}{2}}}$

$= \frac{m y}{2p} \cdot \frac{\sqrt{(n\dot{z}^2 + m\dot{p}^2)}}{\sqrt{(n\dot{z}^2 + m\dot{p}^2)}\ \sqrt{(n x - m p)^{\frac{3}{2}}}}$; and $n\dot{z}^2 + m\dot{p}^2$ is infinitely smaller than $n\dot{y} + \frac{m y \dot{p}}{p}$

Therefore $$\frac{n\dot{y}' + \frac{m y' \dot{p}'}{p'}}{\sqrt{(n \dot{z}'^2 + m \dot{p}'^2)} \, . \sqrt{(n x' - m p')}}$$

$$-\frac{n\dot{y} + \frac{m y \dot{p}}{p}}{\sqrt{(n \dot{z}^2 + m \dot{p}^2)} \, . \sqrt{(n x - m p)}} = o;$$ or taking

the fluent, $$\frac{n\dot{y} + \frac{m y \dot{p}}{p}}{\sqrt{(n \dot{z}^2 + m \dot{p}^2)} \sqrt{(n x - m p)}} = a.$$

ERRATA.

The reader is desired to correct the following faults which have escaped notice in due time.

Page 5, line 8, 2 should be in the denominator.
—— 6, — 1, $n r$ should be $n' r$.
——16, — 8, $n y'$ should be $n \dot{y}'$ and $n \dot{z}'^2$ should be $n \dot{z}^2$.

FURTHER OBSERVATIONS

ON THE

FLOATING ISLAND OF DERWENT LAKE;*

WITH REMARKS ON CERTAIN OTHER PHENOMENA:

IN A LETTER TO MR. DALTON, F. R. S.

President of the Society.

BY MR. JONATHAN OTLEY.

(Read November 4th, 1825.)

Keswick, October 10*th*, 1825.

SIR,

THE Floating Island made its appearance for a short time in the latter part of this summer; I had looked for it some time, and should have been sadly disappointed if it had not shewn itself at all, after such a long continuance of warm weather. Notwithstanding the great drought, the surface of our lake was never so low this summer, as in June last year; but this may perhaps be partly attributable to the state of its outlet.

On the 7th of September, the lake being then about four inches above its lowest water-mark of last year, and some appearance of a

* Memoirs, vol. 3rd., New Series, page 64.

change taking place in the weather, I visited the place of the floating island, and found that it had risen a little, being then in the shallowest part covered by about twelve inches of water. I then filled one of the phials with gas, which I send herewith. There was a considerable agitation in the lake, with a south wind; heavy showers came on, both that and the succeeding evening. On the 10th the island appeared above water nearly as large as I have ever seen it, but very little in height above water; on the 16th I was upon it and took a bottle of gas from the skirt of the island under water. One observation I did not sufficiently pursue, that is, on rincing the bottle an offensive odour seemed to come from the gas liberated from the water in shaking it.

On the 20th September, the rains had raised the water two feet since the island first made its appearance; yet the island also continued to rise, so that it was still above water the greatest part of its length; on the 22nd, it appeared very small, and on the 23rd, although the lake was lowered again about four inches, the island was covered. The lake afterwards rose four feet six inches above its lowest mark, and I had some expectation of the re-appearance of the island on the waters subsiding; but it has not since been seen, although the water was on the 3rd of October only about six inches higher than when the island first made its appearance.

Those who are inclined to attribute its rising to some circumstance attending the change of weather, may consider the present as supporting their argument; but on such a supposition it would be difficult to say, why it did not rise on a similar change which took place in August.

I cannot well account for the island's going down so readily; as the temperature of both the air and water have been kept up to an unusual height for the season. Could the lengthened nights and the want of light have any effect in reducing the power or quantity of the gas?

I have frequently examined the temperature of the lake, especially after its being strongly agitated by the wind, in order to obtain a mean: during the hot weather I sometimes found it above 70°, and during the time of the island's being above water it was about 62°. Before the present month I never found it below 60°, and this morning at six, after a very strong S.S.W. wind, the water was 57°, the air being 59°. I never, since I kept a thermometer, found the temperature of the air, before sun-rise, continue for a length of time so high: it has very seldom been below 55, and only three times, I believe, below 50°, viz. on the 5th, 23rd, and 28th of September. We have had but few clear nights this autumn, and few of those foggy mornings which I had predicted from the elevated temperature of the earth and water; but they may probably yet be experienced before the water is cooled down to 40°.

On the 20th of August I visited Fairy Kell, the spring in the Wood,* the temperature of the water was 47½°, which is above a degree higher than I had ever before observed it. On the 27th I made a solitary excursion over Helvellyn to Patterdale. I found the water at Brownrigg Well issuing far more copiously than in July, its temperature was between 40 and 41, the air 55°; and when I left the summit, at 5 P.M. it was 46°.

During the fine weather, I took frequent opportunities of observing the appearance of Criffell on ascending Castlehead; and the difference between the highest and lowest station

* This, in the summer season, is the coldest spring of any near Keswick. It is situate under a high cliff to the S. E.—J. D.

where its summit first appears above the rising ground near Sunderland, I have found about 21 feet perpendicular. It generally seems highest in the morning, and once in particular, after a clear night, it appeared much higher than at any other time of my observation. The distance from Castlehead to Criffell, is about thirty miles, and the intercepting ridge of land about one-third of the distance from the former place.*

An appearance which I first noticed in 1812, when we were at Wasdale Head, I have had frequent opportunities of observing since; but I think, that till lately, I formed an erroneous conclusion as to the cause. It happens on a clear dewy morning, when walking on ground sloping to the west, so that the shadow of my head falls at a considerable distance; then it appears surrounded by a kind of luminous circle, for which the only cause that occurred to me, was, that a warmer and more vapoury atmosphere might surround my head, so as to refract the sun's rays in a peculiar manner. But one morning, when upon Castlehead, the shadow

* The reader will perceive these remarks relate to the variable refractive power of the atmosphere at low altitudes. Criffel is a Scotch mountain seen from Keswick, over Solway Firth. Its summit is about 1830 feet above the level of the sea.—J. D.

extended so far that I could not distinguish my own form, but the shadow of that part of the hill on which I stood appeared highly illuminated. This caused me to reconsider the circumstance; and I felt convinced that the sole cause of the appearance was the reflection of the sun's rays from that part of each globule of dew which was directly opposite to the sun and the place where I was standing.

If you find leisure to examine the contents of the accompanying phials of gas, I shall be glad at some opportunity to learn the result.*

I remain, dear Sir,

Your obedient and much obliged

humble Servant,

JONATHAN OTLEY.

* Both phials were examined soon after their receipt, and found to contain a mixture of equal volumes of carburetted hydrogen (pond gas) and azote, with the usual 5 to 10 per cent. of carbonic acid. These are the same proportions that were found in 1815.—J. D.

OBSERVATIONS

ON THE

INFLUENCE OF MACHINERY

UPON THE

WORKING CLASSES OF THE COMMUNITY.

BY JOHN KENNEDY, ESQ.

"Read February 10th, 1826."

A FEW years ago, I had the pleasure of presenting to this society an outline of the origin and progress of the Cotton Trade, and an account of the various inventions of machinery then in use in this department of British manufactures. My object in the present paper is to state my opinion of the influence of machinery, and to lay before the society a few hints on the advantages consequent on the introduction of mechanical and scientific improvements into the various and widely-extended departments of our Manufactures.—In the first place, the object of all manufacturing machinery being the substitution of some power in the place of human labour, its immediate tendency is to diminish the necessity for manual exertion, or to render it less burdensome, and as a direct consequence of this to enable the younger and more delicate members of the community to perform those operations, which only the skilful and robust were wont to execute. Hence it is that wind, water, and steam, have been applied as moving power in the place of human or horse labour, and that women and children are enabled to execute those tasks, which formerly required the ingenuity or the strength of men.

While, by this important change in our manufacturing system, the more laborious operations are made no longer to depend entirely on human exertion, the extension of mechanical improvements causes a new division of labour, which is advantageous in some important respects to the operative members of the community.

Not only is great skill required in the construction of those beautiful machines, which are intended to diminish human labour, but a demand is thus created for the exertion of industry and skill in the superintendence of the newly-invented machine. No mechanical contrivance is so perfect as not to require continued attention, nor is there any, the efficacy of which does not materially depend on the care and dexterity of the overlooker. This is particularly the case, when the movements of the machine are of a complicated or delicate nature. Such are the various engines employed in the cotton and woollen, the silk and linen, manufactories; these, while they abridge human labour in many respects, create a demand for it in other directions, and thus the older and more experienced members of families find abundant employment.

Much labour and ingenuity and expense being incurred in the invention and construction of machinery, the owner of a costly improvement naturally wishes to employ it as far as he can to his individual advantage. He is desirous of obtaining some adequate remuneration for the money he has expended or the talent he has excited, in order to possess himself of a machine calculated to supersede in some degree the operation of mere manual labour. But still the machine itself must be worked, and this cannot be done without human labour employed at least in its superintendence. Now the price, he will pay for the labour required, will be in proportion to the necessity he feels for it, arising from its productiveness and from the demand for the manufactured article thus furnished. And as the ingenuity and skill required in the superintendence of complex machinery are not of ordinary attainment, the wages of persons thus employed will bear a proportion to the value of their labour. Hence it will appear, that in all cases the employed necessarily partake in every improvement that is made in machinery, and have their full share of interest in all new inventions.

The extended classification of labour is an advantage to the working classes themselves, and contributes in various ways to their comfort, convenience, and profit.

The farther this classification of human industry is carried, the more any individual branch of trade or manufacture becomes dependent on the subordinate branches of this subdivision of labour. All the departments being thus subservient to and dependent on each other; the very lowest (those I mean which require the least exertion of skill or industry,) have still their relative value; and whilst the highest degree of ingenuity and dexterity will be applied where it is most wanted, and will be sure of a proportionate remuneration, the inferior kinds of labour will furnish employment to a very large class of the community, whose services could not have been required, had not the invention of machinery rendered them available. Every new machine may, in fact, be considered as a source of individual advantage to the artizan, on whose skill or industry alone its productiveness must ultimately depend, and, so far from decreasing the value of human labour, the discovery and application of mechanical contrivances to the various departments of our manufactures has, in reality, created a new and perpetually-increasing demand for it.

In proportion as machinery is improved in simplicity, and becomes more uniform in its action or motion, a lower class of labour is required for its management; and as women and children are thus enabled to produce those fabrics, which it formerly required all the ingenuity, skill, and labour of the very best workmen to furnish, the latter are set at liberty from the mere drudgery of manufacturing employment, and are at leisure to engage in those more difficult and delicate operations, which the perpetual multiplication of machinery renders necessary.

Such appears the direct tendency of the introduction of machinery; it places men in a condition very different from that state of things, in which the wealthy few could and did purchase the lives and liberties and rights of the many.

Instead of being thus absorbed, capital is more justly and properly applied, and is the means of extensive benefits not to a particular class of society, but to the whole community.

Surplus wealth can now be invested, and is so, not in purchasing the fee-simple of a human being, but in the fee-simple of a machine, which relieves man from the severest slavery of labour, and enables the delicate, the feeble, the young, and the infirm, to earn a comfortable livelihood by dexterity of hand, and the ingenious application of the higher faculties of the mind. Machinery brings into exercise and competition the intelligent powers of man; mere hand and slave labour engages only his animal force.

Wealth invested in machinery improves the condition of man, and enlarges his capacities and means of happiness; but in former days it had the effect of increasing slavery and of debasing the human character. Look even at the present condition of the West India islands, and all those foreign possessions, where machinery has not been introduced to supersede the necessity of human labour, or at least to lighten it. Wealth accumulated there is invested in an increased number of slaves who are stimulated to exertion by the goad, that they may furnish luxuries for their fellow-men. Accumulating wealth in England is employed in producing comforts, by the aid of machinery, of which all are partakers; in contributing to the advancement of the human mind; and in making man, what he was intended to be, a moral agent. While the operative classes of the community are benefited by circumstances, which open to them continually new markets for their labour, and a higher rate of remuneration for it, the great mass of mankind also experience from the same cause important advantages.

The products of manufacturing industry are obtained at a lower price, and of superior quality; and those articles, which were once regarded as the peculiar accommodations of the higher classes of society, are now placed within the reach of all. There is not, perhaps, a more striking feature in the recent improvements that have taken place in this country, than the increased comforts enjoyed by the working classes, particularly as these are intimated by their better food and clothing, and the more convenient furniture of their humble dwellings. And these are so necessarily connected with an improved condition of health, that they may serve to account in a great degree for that lengthening of human life, which has been recently reported to us by the statist and political economist.

Among other advantages resulting to the labouring classes from the division of labour and the custom of piece-work, there is one which ought not to be overlooked, though it is, perhaps, not unattended with some inconvenience on the whole. From the classification of human industry, there arises to the operatives a considerable facility in associating and combining, not merely to frustrate any attempts on the part of the employers to impose unreasonable terms of remuneration, but also to make terms and conditions with them. This has been recently productive of mischief to a considerable extent, and probably will continue to be felt as an evil till the just rights of masters and men are reciprocally understood and allowed.

The advantage to the employer from the use of machinery arises from the more extended means thus put into his possession of investing his capital and exercising his skill and ingenuity. His object will be steadily to pursue his system of business at the least possible expenditure, while the operative will, in like manner, endeavour to obtain the highest rate of remuneration for his labour and dexterity. Hence, there may times arise, when the contest between them may be carried on to some inconvenience to both parties; but if each be left to pursue his own course, without the interference of the public on the one hand, or combination laws on the other, the matters of dispute will speedily find their own level and every difference will be fairly adjusted.

That the labourers should refuse to work at their former wages is not unjust. Their labour is their capital, and they have a right to invest it in the most advantageous way possible. If, on the contrary, the employer wishes to reduce his labourers' wages, he is equally at liberty to attempt it. But it would be manifestly unjust in the master or employer to combine with other masters, and prevail on them to engage not to give employment to such workmen, because they refused to accept his reduced prices; and it would be equally unjust in the workmen to prevent others from accepting this lowered rate of wages, if inclined to work for it. It would be unjust in the master, to say to his workmen, you shall not bring up any of your family to any other trade than that in which you are yourself engaged; and it would be no less unjust in the workman to say, you shall employ none but such as have been brought up to such and such a trade, and who have served an apprenticeship, or any members of some club, or some association. It would be unjust in the master, to insist upon his labourers working longer hours than those ordinarily agreed upon, say from six A. M. to seven P. M., including the regular and sufficient time for refreshment. It would be unjust to say to the labourer, you shall work such hours as I think it proper to prescribe; and it would be unjust in the labourer to say, you shall fix only on such hours as I choose to dictate. The price for labour will and must vary from a variety of circumstances, but this most frequently arises from a redundant or scanty supply of manufactured products; and the capitalist will naturally and reasonably calculate to receive at least the ordinary interest which such capital would yield, if employed in agriculture, or which he could obtain by lending the same to those who might wish to borrow. This will lead the capitalist to procure his raw material as low as he can; and labour and food constitute a part of his raw material.

Thus the merchant, the manufacturer, and the agriculturist, proceed to lessen their expenses by every means in their power, whilst the operative tries to enhance the price of his labour. Now this seems a principle to use as the basis of our common and statute law respecting combinations, and the important inquiry is, how to frame laws to meet exigencies of this kind so as not to infringe upon the liberty of the subject on either side.

The recent repeal of the modern combination laws, joined to the great demand for labour in every department of our manufactures, has produced a singular degree of excitement among the labouring classes, and a short time will, in all probability, suffice to enable us to know, what legislative measures (if any) may be necessary on this subject.

I shall therefore defer the further consideration of it for the present, and also an inquiry into the physical and moral effects which result from the peculiar organization of our large manufacturing establishments.

ON A

REMARKABLE FACT

IN THE

NATURAL HISTORY OF THE SWALLOW TRIBE.

BY JOHN BLACKWALL, F.L.S.

(Read March 23rd, 1826.)

THE late celebrated Dr. Jenner, in a posthumous essay on the Migration of Birds, published in the first part of the Transactions of the Royal Society for 1824, has briefly adverted to an extraordinary occurrence in the domestic economy of two species of British hirundines; which, though far from uncommon, has either been altogether overlooked, or totally disregarded, by every preceding writer on ornithology whose works I have had an opportunity of consulting. The circumstance alluded to is, the occasional desertion of their last hatched broods by the swallow and house-martin. This singular fact, with which I was familiar previously to its annunciation by Dr. Jenner, my own researches confirm and illustrate; I shall, therefore, without further prelude, proceed to state the results obtained from them.

The swallow appears in the neighbourhood of Manchester on the 18th of April, and the house-martin on the 23rd of the same month, at a mean of twelve years' observations, but as these birds do not pair immediately on their arrival, and as they generally produce two, and often even three broods in a season, it frequently happens that individuals have nestlings in October, the period at which the great body of their species withdraws from this country.* Many of these young birds, from inability to accompany their congeners in their autumnal flight, are compelled to remain behind, and some of the most vigorous of them, may occasionally be seen, in favourable situations, lingering about till the close of November, endeavouring to obtain a scanty subsistence. As the temperature of the atmosphere decreases, however, the insects they prey upon gradually diminish, till, at last, their utmost exertions to procure a sufficient supply of food are unavailing: they then speedily become enfeebled, and concealing themselves, as is usual in such emergencies, numbers undoubtedly perish from exhaustion. A few accidental discoveries of birds thus situated, before the vital principle has been quite extinct, may, very possibly, have

* At Tarvin, in Cheshire, in 1819, I saw a pair of martins feeding their unfledged young on the 20th of October.

given rise to the opinion that European swallows pass the winter season in a state of torpidity.

It did not come to my knowledge, that these late broods are sometimes deserted by the parent birds, before they are capable of providing for themselves, till the spring of 1821; when a pair of martins, after taking possession of a nest that had been constructed in the preceding summer, drew out the dried bodies of three nearly full fledged nestlings which had perished in it, preparatory to appropriating it to their own purposes. About the same time, and near the same spot, a similar attempt was made by another pair of martins, but all their efforts to dislodge the young proving ineffectual, they entirely closed up the aperture with clay, and so converted the nest into a sepulchre.

At first I was disposed to attribute the untimely fate of the nestlings, thus unexpectedly discovered, to the accidental destruction of one or both of their parents; but a little reflection induced me to change my opinion. So many instances were called to mind of the sudden departure of martins, at periods when, to all appearance, they were most busily engaged in providing for their families, that what before was regarded as the unavoidable consequence of a fortuitous circumstance, I now began to suspect, might be occasioned by a voluntary act of desertion.

In order to clear up this doubtful point, an examination of a considerable number of swallows' and martins' nests was immediately resolved upon; but, as the breeding season had then commenced, it was deemed advisable, on more mature deliberation, to defer the undertaking until its termination: accordingly, the search was postponed to the 27th of October, when, on being carried into effect, several nests, of both kinds, were found to contain dead young ones. Satisfied that a fact of such frequent occurrence, could not, with any degree of probability, be ascribed to accident and convinced, that the intentional desertion of their progeny by the parent birds, afforded the only adequate explanation of it which was admissible, no further inquiry into the matter took place till November, 1825. On the 19th of that month, an intelligent person, to whom I am indebted for numerous interesting communications, relative to the natural productions of the neighbourhood in which he resides, assured me, the suspicion I had formerly intimated to him, that martins frequently leave their last hatched broods to die of hunger in the nest, was perfectly well founded. Having narrowly watched the proceedings of these birds, many of which breed annually under the eaves of a large barn situated near his house in the chapelry of Blakeley, the result of his investigation, he informed me, was, the complete confirmation of my supposition by the most unequivocal proof, namely, that obtained directly from personal observation of the fact; and he did not doubt, he remarked, that dead nestlings might then be procured in abundance, if I would take the trouble to have the nests at the barn examined. This suggestion was acted upon without delay: repairing directly to the place, a ladder was quickly provided, and fourteen nests underwent a careful inspection; of these, five were found to contain dead nestlings of various sizes, specimens of which will be laid before the society,* and from another, two eggs were taken, whose contents very evidently shewed that they had been forsaken when on the point of being hatched. The nestlings collected on this occasion did not, it is true, exceed ten, which may be thought few when compared with the number of nests they occupied; but the second

* The extremely flattened appearance of some of these young birds, especially the smaller ones, which I was quite unable to account for, greatly excited my attention. I soon learned, however, that it was occasioned by the pressure of the sparrows which every night took up their lodgings in the nests.

and third sets of eggs, produced by those martins which lay several times in a season, it should be recollected, only average three and two respectively; and even these may not all be prolific.

The sand-martin, I believe, has never been suspected of forsaking its progeny; yet, that it sometimes does abandon them, I have clearly ascertained, by repeated inspections of the nests of this species during the winter months.

Whether the swift, whose general habits are so very dissimilar to those of the other British hirundines, ever deserts its young, I have not been able to determine; as it is rather a scarce bird in the neighbourhood of Manchester, and usually builds its nest in situations to which I have no access. That this may sometimes happen, however, in cases of extreme urgency, seems probable from an anecdote related by Mr. White, in his Natural History of Selborne, letter 52. "I have just met with a circumstance respecting swifts," says that pleasing writer, "which furnishes an exception to the whole tenor of my observations, ever since I have bestowed any attention on that species of hirundines. Our swifts, in general, withdrew this year" (1781) "about the first day of August, all save one pair, which in two or three days was reduced to a single bird. The perseverance of this individual made me suspect that the strongest of motives, that of an attachment to her young, could alone occasion so late a stay. I watched, therefore, till the twenty-fourth of August, and then discovered, that, under the eaves of the church, she attended upon two young, which were fledged, and now put out their white chins from a crevice. These remained till the twenty-seventh, looking more alert every day, and seeming to long to be on the wing. After this day, they were missing at once; nor could I ever observe them with their dam coursing round the church, in the act of learning to fly, as the first broods evidently do. On the thirty-first, I caused the eaves to be searched, but we found only two callow, dead swifts, on which a second nest had been formed." Now, although the maternal affection of the female bird, in the instance before us, was sufficiently powerful to induce her to remain with her young, till they were capable of accompanying her in a distant journey, to a more genial climate, as is sometimes the case with house-martins, when deserted by their mates, yet the conduct of the male, if it does not absolutely establish the fact that swifts occasionally abandon their offspring to destruction, certainly affords strong presumptive evidence in its favour.

The frequent desertion of their last hatched broods by the swallow, house-martin, and sand-martin, which is too well authenticated to admit of a doubt, must appear surprising to every one; but particularly so to those who are aware, how highly the parental feelings of the feathered tribes are excited during the breeding season. Few people are ignorant of the care and attention bestowed upon their offspring, by our domestic fowls; and that the winged inhabitants of the fields and woods, are, in their wild state, no less attached to their progeny than the reclaimed inmates of the poultry-yard, may be inferred from the following examples.

Early in August, 1825, a neighbour took a young cuckoo out of a titlark's nest; and, carrying it home with him, put it into a cage, which he hung in a pear-tree in his garden. The foster-parents, speedily discovering where their nursling was confined, notwithstanding the distance of the place from its former abode could not be less than three-quarters of a mile, proceeded with every demonstration of delight to supply its immediate wants, and continued to provide it with food till it was unfortunately killed by a cat, though there never was the least probability that it would be restored to liberty.

A still more extraordinary account is given by Montagu in the introduction to the Ornithological Dictionary, p. 33, and following, of some golden-crested wrens, which were brought up in captivity by the parent birds. The narrator took the nest, he informs us, when the young were about six days old, and, putting it in a small basket, enticed the old ones by degrees to his study window. After allowing them sufficient time to become familiar with that situation, he placed the basket within the window, and then at the opposite side of the room. It is remarkable, he observes, that, although the female seemed regardless of danger, from her affection for her offspring, yet the male never once ventured into the room, though he constantly fed the young birds while they were at the outside of the window. The female, on the contrary, would feed them at the table at which he sat, and even when he held the nest in his hand, provided he remained motionless; but, on moving his head one day, while she was on the edge of the nest, she made a precipitate retreat, mistook the closed for the open part of the window, knocked herself against the glass, and fell breathless on the floor, where she lay for some time. However, recovering a little, she made her escape; and, in about an hour after, he was agreeably surprised by her return, and she would afterwards frequently feed the young while he held the nest in his hand.

The partridge has generally been represented by ornithologists as possessing a more than ordinary share of affection for its offspring, and the anecdote I am about to relate tends greatly to corroborate this idea. A near relation of my own* was told by the late Rev. W. Evans, of Mayfield, near Ashburn, that, some years since, his men, who were employed in cutting a field of mowing-grass, brought him a hen partridge which they had caught on her nest. Being desirous to save the eggs from destruction, he ordered that they should be removed to his house, and placed on some hay in an unoccupied room, intending to put them under the care of a domestic hen; but wishing to know whether the parent bird would take any notice of them in this novel situation or not, he directed that she should be set down near them, when, to his great astonishment, she immediately ran to the spot where they were deposited, and, covering them with the utmost care, continued

* John Blackwall, Esq., of Blackwall, Derbyshire.

to sit till they were hatched. At first she was unremitting in her attention to her young, many of which were ultimately reared and set at liberty, but her anxiety to regain her freedom, evidently increased with their growth; and, as soon as her assistance could be dispensed with, she was suffered to make her escape. This instance is the more remarkable, as the partridge has never been known to breed in captivity.

In a conversation which I had with Mr. Dalton, in the summer of 1822, on the force of that impulse which leads birds to sit upon their eggs with so much patience and assiduity, he informed me that he had removed hen redbreasts from their nests, during the period of incubation, and that, upon gently replacing them, they had continued to sit as if they had not been disturbed. This experiment of Mr. Dalton's, which affords a striking instance of one of the most constant and powerful dictates of nature, self-preservation, being counteracted by a temporary excitation of superior energy, I have repeated with the redbreast, whinchat, swallow, house-martin, the marsh, cole, and great titmice, &c., not only when they have been sitting, but also when they have had small young ones, and almost always with success.

These examples, to which many more might easily be added, will be sufficient, I am persuaded, to convince every unprejudiced mind, that the parental affections of the feathered tribes in general, and, what is more immediately to the purpose, of the swallow and house-martin in particular, are powerfully excited during the breeding season. Now, what, we may ask, can induce the two last-named species, and the sand-martin, deliberately to consign their offspring to a painful and lingering death, in direct opposition to such intense feelings as these? The cause assigned by Dr. Jenner, for conduct so anomalous, is the desire to migrate; and this desire, he maintains, is produced by a change in the reproductive system, which, in the case of the birds under consideration, is supposed to take place prematurely. I say is supposed to take place, for I do not see, how it is possible to ascertain what individuals will desert their progeny, before they carry their intention into effect; and after the accomplishment of the act, no opportunity of examining the internal state of their organization can present itself; this notion, therefore, it is pretty obvious, must have originated in conjecture. That the sudden departure of the swallow, house-martin, and sand-martin, under circumstances so peculiar as those we have been contemplating, is occasioned by the desire to migrate, I do not dispute; but that this desire results from certain changes which occur periodically in the condition of the reproductive system, seems quite inadmissible. Indeed, the undeniable facts, that every species of the feathered tribes, though subject to these changes, is not migratory; and that snipes, wild-ducks, &c., breed annually, and woodcocks occasionally, in countries where the majority of these birds is known to sojourn during the winter only, are so totally subversive of Dr. Jenner's hypothesis, that to attempt a more complete refutation of it, in this place, would be superfluous.

It is particularly deserving of remark, that the early death, which invariably terminates the sufferings of those devoted nestlings that are abandoned by their parents, powerfully militates against an opinion, extremely prevalent amongst ornithologists of the present day, that many of our summer birds of passage, especially the swallows, are capable of passing the winter season in a state of torpidity; for, if this belief in the liability of the European hirundines to become torpid in autumn be well founded, how does it happen, that late hatched broods of swallows, house-martins, and sand-martins, when deserted, uniformly perish, even under circumstances which are represented as rendering individuals of their species, too young or feeble to undergo the fatigues of migration, merely dormant? The advocates of torpidity will do well to consider this difficulty with attention, since, if not removed, it leaves them no alternative but to renounce, as untenable, the doctrine they maintain.

APPENDIX.

Since the foregoing observations on the occasional desertion of their last hatched broods, by several species of British hirundines, were submitted to the consideration of the society, a favourable opportunity of pursuing the investigation has again presented itself.

On the departure of the house-martins, in October, 1826, it was perceived, that they left some broods to perish in the nests built under the eaves of a barn, situated at the Hill-top, in the chapelry of Blakeley; the edifice being the same to which I have alluded in the former part of my paper, as a favourite haunt of these birds. This occurrence determined me to have the nests carefully examined; accordingly, after procuring the requisite assistance, a minute inspection of the whole, twenty-two in number, took place on the 11th of November; when, to my great surprise, thirteen were discovered to contain eggs and dead nestlings. With regard to the particulars, which are given below, it is only necessary to remark, that the nests are denoted by the progressive numbers, and that the state of the contents, as there described, is the same in which they were left by the parent birds.

NESTS IN WHICH EGGS WERE FOUND.

NESTS.	CONTENTS.
No. 1	Three Eggs which had not been sat upon.
2	One Egg which had not been sat upon.
3	Five Eggs which had been sat upon a short time.
4	Four Eggs which had been sat upon a considerable time.
5	Three Eggs on the point of being hatched.
	Sixteen, Total.

NESTS IN WHICH YOUNG BIRDS WERE FOUND.

NESTS.	CONTENTS.
No. 6	Two Nestlings newly disengaged from the egg.
7	Three Nestlings a few days old.
8	Two Nestlings about a week old.
9	Two Nestlings nearly half grown.
10	Two Nestlings about three parts grown.
11	Two Nestlings nearly fledged.
12	Five Nestlings nearly fledged.
13	One Nestling quite fledged.
	Nineteen, Total.

From the unusual quantity of eggs and young deserted by the house-martins on this occasion, it may be inferred, that the desire to perpetuate their species was protracted, in a more than ordinary number of individuals, to the termination of their stay in this country, by the high temperature of the season,* and the great abundance of food consequent upon it.

The circumstance of fresh laid eggs being in several instances forsaken, furnishes an addi-

* On referring to my meteorological journal, I find, that the mean temperature of the months of June, July, and August, respectively, was higher in 1826, than in many preceding years.

tional argument to those previously urged against the hypothesis advanced by Dr. Jenner, that a premature change uniformly takes place in the physical condition of the reproductive system of those birds which abandon their progeny to destruction; for it is in the highest degree improbable, that an organic change, sufficient to induce a total alienation of parental affection, a change, let it be remembered, which, in every observed case, has been found to proceed gradually, should so suddenly succeed to the extremely active state of the system indicated by the recent production of prolific eggs. The simultaneous departure of both sexes also, when they desert their offspring, which, as far as my own researches extend, appears to occur with great regularity, is too remarkable a fact to be accounted for on a principle so uncertain in its operation as that maintained by Dr. Jenner.

A belief, represented by Dr. Fleming, in his Philosophy of [Zoology, vol. II. pages 72—3, as prevalent throughout Scotland, that swallows are sometimes found torpid in their nests, has most likely originated in the discovery of the forsaken young of the swallow and house-martin, (for both species are termed swallows indiscriminately by the multitude,) in a perishing condition, or dead.

It appears from the following passage, extracted from Pennant's British Zoology, vol. II. page 155, that the puffin, when placed under circumstances similar to those which induce birds of the swallow tribe to desert their offspring, sometimes abandons its progeny. "The first young" (of this species) "are hatched the beginning of July: the old ones shew vast affection towards them, and seem totally insensible of danger, in the breeding season. If a parent is taken at that time, and suspended by the wings, it will, in a sort of despair, treat itself most cruelly, by biting any part it can reach, and when it is loosed, instead of escaping, will often resort to its unfledged young; this affection ceases at the stated time of migration, which is most punctually about the eleventh of August, when they leave such young as cannot fly, to the mercy of the peregrine falcon, who watches the mouths of the holes for the appearance of the little deserted puffins, which, forced by hunger, are compelled to leave their burrows."

METEOROLOGICAL OBSERVATIONS,

MADE IN THE TOWNSHIP OF CRUMPSALL,
FROM 1821 TO 1828, INCLUSIVE,

WITH

REMARKS.

BY JOHN BLACKWALL, F. L. S.

(Read October 20th, 1826.*)

CONSIDERING the greatly increased attention which of late years has been bestowed upon atmospherical phenomena, it is mortifying to reflect how little has been done, the important discoveries of one or two distinguished indivi-

* It may be proper to state, that when this paper was read before the society, it comprised a series of meteorological observations made during a period of five years, commencing with 1821, and terminating with 1825. In its present form, the series, with the exception of the observations on the temperature of spring-water, and on the dew-point, which were discontinued, is, with the permission of the society, extended to the close of 1828. This circumstance will serve to explain the apparent incongruity, into which it might be supposed I had been led, of having announced facts previously to the date of their occurrence.

duals excepted, to promote the progress of meteorological science. The facilities afforded by the publication of numerous literary and philosophical journals have induced a multitude of observers, at regular periods, to lay before the public the results of their researches; but, although much ingenuity has been displayed in tabular arrangements and in the construction of diagrams, the tendency of their labours towards the establishment of sound theory has been very slight indeed. Whether this arises from any inefficiency in the ordinary modes of investigation; from discordancies occasioned by unavoidable differences in the instruments employed; or from a want of uniformity in the time and manner of taking the observations, which are thus rendered of small value comparatively; or whether it is to be attributed to some other cause, I will not take upon me to determine. That so much minute and elaborate investigation should have been productive of no greater advantage to this branch of science, is certainly a discouraging reflection: still, however, a large number of facts has been accumulated, which, when collected and carefully examined, may yield to some acute and comprehensive intellect, valuable results which may have escaped the notice of those who have only considered them partially or in detail. Under this impression, and with the desire of contributing something to the general stock, I have drawn up the following tables and remarks; leaving the task of collecting, arranging, and comparing what has been done by others, and of extracting useful information from it, to those who have more leisure and are better qualified for the undertaking.

Before we proceed to the results obtained from the observations, it should be stated, that the place of observation is situated in the township of Crumpsall, about two miles and a quarter north from Manchester, and is in most respects favourable for meteorological pursuits.

The tables do not require any explanation.

Observations on the Barometer.

TABLE OF THE MEAN HEIGHT OF THE BAROMETER,

AT CRUMPSALL.

Months.	1821.	1822.	1823.	1824.	1825.	1826.	1827.	1828.	Means.
January . .	29.51	29.73	29.35	29.64	29.78	29.65	29.43	29.55	29.58
February .	29.87	29.58	29.04	29.44	29.72	29.45	29.67	29.45	29.52
March. . .	29.23	29.56	29.41	29.44	29.72	29.62	29.23	29.54	29.46
April . . .	29.28	29.57	29.50	29.51	29.65	29.59	29.57	29.38	29.50
May. . . .	29.50	29.66	29.53	29.64	29.62	29.73	29.43	29.53	29.58
June. . . .	29.75	29.75	29.57	29.58	29.62	29.92	29.59	29.64	29.67
July. . . .	29.58	29.43	29.44	29.66	29.81	29.63	29.73	29.33	29.57
August . .	29.59	29.52	29.51	29.59	29.56	29.61	29.70	29.50	29.57
September.	29.48	29.62	29.61	29.54	29.52	29.56	29.66	29.60	29.57
October . .	29.52	29.28	29.35	29.27	29.55	29.51	29.42	29.69	29.44
November.	29.36	29.28	29.71	29.18	29.28	29.46	29.63	29.49	29.42
December.	29.05	29.65	29.26	29.37	29.18	29.46	29.36	29.52	29.35
Means. . .	29.47	29.55	29.44	29.48	29.58	29.59	29.53	29.51	29.51

The barometer, which is an upright one of the usual construction, has a tube 32 inches long, with a capacious bulb at its lower extremity; its internal diameter is .22 of an inch, and it was carefully filled with dry mercury. No allowance having been made on the scale for the rise and fall of the mercury in the bulb, the variations, as registered, are somewhat too small: to be strictly accurate, they should be increased by one-fortieth part.

The height of the instrument above the level of the sea, I have reason to believe, is about 292 feet. From barometrical measurements, I find its elevation above the Duke of Bridgewater's canal, at Manchester, to be 212 feet, and the canal is represented to be nearly 80 feet above the sea,* making a total of 292 feet.

Many observations were taken each day during the foregoing period of eight years, but the highest and lowest only, with the mean obtained from them, were regularly noted down in the journal. Previously to any observation being made, a few gentle vibrations were invariably given to the mercury for the purpose of overcoming its adhesion to the tube.

* Society's Memoirs, New Series, Vol. III. p. 486.

From the time of Torricelli to the present day, the variations of the barometer have continued to attract the notice of men of science, and numerous hypotheses have been formed to account for those fluctuations in the weight of the atmosphere by which they are occasioned. As a minute inquiry, however, into the merits of the various opinions which have been broached on this subject would far exceed the limits of a paper, I shall confine my remarks to such particulars as are more immediately suggested by my own observations.

On attentively looking over my journal, I find that great and sudden depressions of the barometer, which generally happen in the winter months, are, for the most part, accompanied with high wind from the south, or some of the intermediate points between that and the west; and that with a northerly wind, whatever may be its force, the mercury usually rises; but that it attains its greatest elevation in calm, frosty weather: moreover, it appears that a rapid rise frequently follows a sudden depression of the mercury in the barometer, especially on a quick transition of the wind from south to north of the west.

Subjoined are a few examples selected from an extensive collection of observations illustrative of the accuracy of the above results.

On the 5th of December, 1822, there was a slight frost in the morning, with a gentle breeze from the W., the barometer at 10h. A.M. being at 29.31, and the thermometer at 37°. About noon the wind shifted to the S.W., and soon after passing to the S., gradually increased in strength till midnight, when it blew a complete hurricane. The effect on the barometer was remarkable: at 10h. P.M. it had gone down to 28.27, which is rather more than an inch in twelve hours; the thermometer at the same time standing at 43°. Early on the 6th, the wind, which had got up to the W. by N., still blew with unabated violence, and did not wholly subside till after the break of day; yet, at 8h. A.M. the barometer had risen to 29.02, and at 10h. P.M. it was at 29.40, the mercury having moved through a space of 2.17 inches in 36 hours; that is, from 10h. A.M. on the 5th, to 10h. P.M. on the 6th.

An extremely high wind from the S.W. occurred on the evening of the 3rd of December, 1823. The barometer, which, at 10h. A.M., was at 29.10, at 12h. 30m. A.M. on the 4th, had fallen to 28.32; the thermometer, during the same interval, having moved from 40° to 52°.5, where it remained till the storm began to abate. Between 2 and 3h. A.M. on the 4th, the wind, which had previously changed to the W., blew with its greatest violence, and continued very boisterous till noon; nevertheless, the barometer began to rise about 1h. 15m. A.M., and, at 10h. P.M., when the thermometer was at 39°.5, had got up to 29.20.

The barometer, on the 14th of February, 1824, went regularly up from 28.67 to 29.10. The wind was N.E., and in the afternoon it blew very hard.

Again there was a strong gale from the N.E. on the morning of the 2nd of April, 1824, during which the barometer rose rapidly; moving, in the course of the day, from 28.75 to 29.58.

A tempestuous S. wind on the 23rd of November, 1824, reduced the barometer from 28.64 to 27.82, which is the lowest observation recorded in the eight years.

With a high wind from the N.W., the barometer, on the 2nd of January, 1825, got up from 29.30 to 29.70.

On the 10th of January, 1825, the barometer was at 30.48, which is the highest observation for the eight years. The weather, for several days preceding, had been still and frosty.

From these facts we may fairly infer, that heat and currents of air are the principal agents in producing the fluctuations of the barometer. It is well known that bodies become dilated, or have their volume increased, by their union with heat; and that they undergo a degree of contraction, or condensation, when a portion of their caloric is abstracted: now, this being preeminently the case with aëriform fluids, it follows, that in winter, strong northerly winds will bring cold air from higher latitudes, of greater specific gravity than that which it displaces in its passage south; and that high southerly winds, in the same season, will bring warm air from lower latitudes, of less specific gravity than that which it displaces in its progress north; consequently, the barometer will rise or fall as a current from one or the other quarter prevails. When the atmosphere over any part of the globe is reduced in volume by severe and long-continued frost, the contiguous air flows in to preserve the equilibrium, and an accumulation of matter ensues which occasions a corresponding rise of the barometer at that place.

What powerfully tends to confirm the opinion, that heat and currents of air are the chief causes of vicissitudes in the weight of the atmosphere, is the fact, that the fluctuations of the barometer are much smaller in summer than in winter, and it will not be denied that heat is more equally distributed over the northern hemisphere, and that the atmosphere is more rarely disturbed by tempestuous winds in the former than in the latter season.

I am aware, it may be objected to the explanation of some of the more remarkable phenomena of the barometer here insisted upon, that the changes of the air in temperature, as shewn by the thermometer, are seldom proportionate to its contemporaneous variations in weight; but it should be recollected, that the capacity of elastic fluids for heat varies with their density, and that every increase of capacity is attended with an absorption of caloric which then ceases to affect the thermometer.*

* For a more complete developement of the causes of the variation of the barometer, see Mr. Dalton's Meteorological Observations and Essays, part second, essay third.

Observations on the Thermometer.

TABLE OF THE MEAN HEIGHT OF THE THERMOMETER,

AT CRUMPSALL.

Months.	1821.	1822	1823.	1824.	1825.	1826.	1827.	1828.	Means.
	°	°	°	°	°	°	°	°	°
January . .	38.2	40.3	31.7	39.7	38.6	32.0	34.2	40.6	36.9
February .	36.1	42.8	36.7	40.5	38.7	42.9	32.6	41.3	38.9
March. . .	42.1	45.2	40.8	40.3	41.4	42.9	41.4	43.8	42.2
April . . .	48.5	46.5	44.0	45.6	47.8	47.2	47.4	46.4	46.6
May. . . .	48.1	54.5	53.0	50.3	52.7	52.5	53.3	53.4	52.2
June. . . .	54.3	62.2	53.2	56.2	56.9	63.6	56.8	58.0	57.6
July. . . .	57.6	58.9	57.0	60.6	62.7	64.4	61.3	59.6	60.2
August . .	60.2	58.4	57.0	58.7	60.4	63.2	58.0	59.2	59.3
September.	58.5	54.4	53.8	57.0	59.6	56.7	56.2	57.8	56.7
October . .	50.5	51.0	47.2	48.4	51.5	52.4	52.8	50.2	50.5
November.	47.1	46.6	44.3	43.7	40.9	40.1	43.9	44.6	43.9
December.	42.8	34.9	40.0	40.0	39.3	42.3	44.4	45.3	41.1
Means. . .	48.6	49.6	46.5	48.4	49.2	50.0	48.5	50.0	48.8

I obtain the daily mean temperature from the extremes indicated by a pair of Rutherford's horizontal self-registering thermometers, placed 15 feet above the ground, out of the window of a room on the second floor, having a northern aspect. The situation is airy and out of the direct influence of the sun. The mean annual temperature, which, on the average of the eight years' observations, is 48°.8, in all probability is nearly correct, as it accords exactly with that deduced from the following series of observations on the temperature of spring-water.

Observations on the Temperature of Spring-water.

TABLE OF THE MEAN TEMPERATURE OF SPRING-WATER,

AT CRUMPSALL.

Months.	1821.	1822.	1823.	1824.	1825.	Means.
January . .	42.4°	43.8°	40.9°	43.8°	44.3°	43.0°
February .	41.6	44.0	39.0	43.0	43.4	42.2
March. . .	41.4	44.4	40.7	42.6	43.0	42.4
April . . .	44.2	46.0	43.9	43.6	45.0	44.5
May. . . .	48.0	49.9	47.6	47.6	48.5	48.3
June. . . .	50.4	54.8	51.0	51.8	51.9	51.9
July. . . .	53.5	56.1	53.3	53.6	55.0	54.3
August . .	55.4	56.0	54.3	55.5	57.5	55.7
September.	56.2	55.6	54.6	56.0	57.5	55.9
October . .	52.9	52.9	51.0	53.1	55.6	53.1
November.	49.8	50.8	48.1	48.6	49.6	49.3
December.	46.7	45.4	45.3	45.6	46.4	45.8
Means. . .	48.5	49.9	47.4	48.7	49.8	48.8

The observations were made once a week, and the general annual mean found from them is 48°.8, being .4 of a degree higher than that determined by means of Rutherford's thermometers, for the same period. The surface of the water in the well, below that of the ground, varies from about 3 to 5 feet.

Account of Rain.

TABLE OF THE FALL OF RAIN,

AT CRUMPSALL.

Months.	1821.	1822.	1823.	1824.	1825.	1826.	1827.	1828.	Means.
January . .	1.70	2.39	1.29	1.44	1.72	.69	2.60	2.90	1.84
February .	.53	2.93	3.28	.62	1.86	2.22	.93	1.82	1.77
March . . .	3.88	4.31	2.38	2.85	1.02	.77	4.44	1.60	2.65
April	3.52	.75	1.49	1.54	1.76	1.73	1.01	3.17	1.87
May	2.88	1.81	2.62	1.55	4.12	.17	1.69	.79	1.95
June. . . .	1.20	1.45	2.87	3.29	3.06	.19	2.03	2.03	2.01
July. . . .	1.87	8.14	5.21	.98	.52	3.04	2.00	10.10	3.98
August . .	3.28	3.98	5.92	1.79	4.10	2.22	4.08	3.45	3.60
September.	4.28	1.56	4.58	4.51	1.77	2.34	3.12	2.92	3.13
October . .	3.32	3.35	3.66	6.64	4.21	3.60	4.42	2.54	3.96
November.	4.44	4.19	1.90	4.95	6.21	2.00	2.44	2.21	3.54
December.	3.86	1.44	4.30	6.25	2.58	2.87	3.77	4.57	3.70
Total. . . .	34.76	36.30	39.50	36.41	32.93	21.84	32.53	38.10	34.00

My rain-gage is placed on the garden wall, at a sufficient distance from any higher object, and is about 12 feet above the ground. The funnel, which is six inches in diameter, is of sheet copper, with a perpendicular rim three inches high. On the mean of the eight years, the annual fall of rain in this township is 34.00 inches; but this amount is probably somewhat too small, as Mr. Dalton invariably makes the annual fall for Manchester greater than I make it for Crumpsall: the difference, which sometimes exceeds six inches, may, in part, be attributed to the circumstance of my gage being considerably more elevated than Mr. Dalton's: so great a discrepance, however, can hardly be referred to this cause alone. Mr. Dalton has suggested, that in stormy, wet weather, high winds, by impinging against the wall on which the gage is fixed, may have their direction altered in such a manner as to diminish the quantity of rain that falls into the funnel; and in this opinion I entirely coincide. It appears, on inspecting the monthly means, that in the first six months of the year, much less rain falls, on an average, than in the last six; every month in the former period producing a smaller quantity than any one in the latter; and that, in the township of Crumpsall, February is the driest, and July the wettest month in the year. The number of days on which rain fell, and the number of distinct falls of rain, hail and snow observed in each year, are given in the following table.

Years.	Number of Wet Days.	Number of Distinct Showers.
1821	225	433
1822	234	537
1823	234	701
1824	242	746
1825	209	636
1826	196	402
1827	231	674
1828	237	765
Total.	1808	4894
Mean.	226	611

In the interval comprised between the commencement of 1821 and the termination of 1828, it is worthy of remark, that the atmosphere did not, in a single instance, remain completely cloudless for the space of twenty-four hours, or during the term of the natural day.

It has been the fashion to ascribe the formation of clouds, rain, and other aqueous meteors to the agency of electricity; and this fanciful hypothesis is not even yet entirely exploded: we may, however, reasonably expect, that ere long it will be wholly superseded by a more philosophical doctrine, founded on sound inferences deduced from exact experiments, and a careful examination of facts; I allude to the theory of rain, originally advanced by Dr. Hutton, of Edinburgh, and subsequently illustrated and established by Mr. Dalton, which requires no comment; it is perfectly satisfactory and quite incontrovertible.

Observations on the Winds.

TABLE OF OBSERVATIONS ON THE DIRECTION OF THE WIND,

AT CRUMPSALL.

Winds.	1821.	1822.	1823.	1824.	1825.	1826.	1827.	1828.	Total.
N.	31	29	21	40	43	25	33	20	242
N.E.	80	91	80	101	81	87	91	74	685
E.	61	69	54	50	48	40	70	59	451
S.E.	60	80	59	41	66	45	40	80	471
S.	115	102	80	75	67	73	77	80	669
S.W.	168	176	169	156	174	126	145	131	1245
W.	169	164	178	157	142	135	148	137	1230
N.W.	89	98	82	106	126	92	73	87	753
Number of Observations.	773	809	723	726	747	623	677	668	5746

I generally ascertain the direction of the wind, by observing that of the smoke which issues from a lofty chimney, favourably situated for the purpose; and my practice is to register every change observed, whose duration is not merely momentary. The prevailing currents in this neighbourhood, it will be seen, are the S.W., W., N.W., N.E., and S.; those of more rare occurrence being the N., E., and S.E. As I possess no instrument for determining the force of the wind with precision, I have contented myself with rudely estimating it from its general effects. On the present occasion, I shall only give the number of high winds recorded, with the months in which they occurred.

	Jan.	Feb.	Mar.	April.	May.	June.
High Winds.	36	54	45	38	6	1
	July.	Aug.	Sept.	Oct.	Nov.	Dec.
	3	10	31	36	44	61

From the observations thus collected and arranged, it is obvious, that the winter months are much more subject to boisterous winds, and, consequently, to those atmospherical phenomena which are dependent upon them, than the summer months.

A few remarks on the distance to which spray from the sea is sometimes carried inland by storms of wind, may not, perhaps, be deemed altogether irrelevant to the subject we are treating upon.

Sea-water is brought into the immediate neighbourhood of Manchester, which is at least thirty miles from the nearest coast, by every violent and long-continued gale from the west; and the exact proportion in given quantities of rain-water, collected on several occasions of this kind, has been determined chemically.*

That the sea is the principal source whence the salt is derived, with which the rain that falls in this town and its vicinity is occasionally impregnated, cannot, I think, be doubted; as I have clearly ascertained, by direct experiment, that its excess or deficiency depends entirely on the direction, force, and duration of the wind. Rain collected in clean glass vessels, a few miles to the north of Manchester, when the wind blows moderately from the N. or N. E., scarcely ever exhibits the slightest trace of muriatic acid, on the application of the most delicate test, (nitrate of silver,) even when reduced two-thirds or three-fourths by spontaneous evaporation;

* Society's Memoirs, New Series, vol. IV.—Essay on the Saline Impregnation of Rain, &c., and Appendix.

though samples collected in the town, precisely at the same time, on being subjected to the test, generally have their transparency more or less impaired. This fact seems to prove, that, notwithstanding muriate of soda is never raised into the atmosphere by evaporation, yet the air over large towns usually contains a very minute portion of muriatic acid, which, as Mr. Dalton observes,* is probably supplied by the sublimation of muriate of ammonia during the combustion of fuel. A considerable increase of muriatic acid takes place in the rain which falls in Manchester, when accompanied with a brisk breeze from the west, of several hours duration; as is evident from the greater degree of opacity observed in samples caught under such circumstances, when treated with a few drops of the solution of nitrate of silver; and that which falls in the adjacent country, then manifests a sensible trace also: indeed, the direction of the wind remaining the same, its force and duration seem almost entirely to regulate the quantity of muriatic acid in the atmosphere; which completely establishes the fact, that it is brought from the sea by the mechanical action of powerful currents of air.

* Society's Memoirs, New Series, vol. IV. p. 370.

The utmost distance to which sea-water is conveyed by tempestuous winds is not easily determined. Sir H. Davy, in his Elements of Agricultural Chemistry, p. 295, states, that "in great storms the spray of the sea has been carried more than 50 miles from the shore," but he does not give his authority. Being at Blackwall, in Derbyshire, the residence of my relative, John Blackwall, Esq., on the 23rd of November, 1824, when a violent hurricane occurred which did extensive damage on the southern coast, I took several opportunities of examining the rain which fell at intervals on that occasion, and uniformly found that it became extremely turbid on application of the test, evidently containing much more muriatic acid than rain collected in large towns, during calm weather, is ever found to contain. The storm commenced on the night of the 22nd of November, and continued, with little abatement, till after noon on the 23rd. The wind blew from the south all the time, and the place of observation is 140 or 150 miles from the sea in that direction. This is, perhaps, the greatest distance on record, to which sea-water has been clearly ascertained to be conveyed by the wind; and that it extended much further is highly probable.

Observations on the Dew-point.

TABLE OF THE MEAN TEMPERATURE AT WHICH THE AQUEOUS VAPOUR IN THE ATMOSPHERE BEGINS TO BE CONDENSED INTO WATER,

AT CRUMPSALL.

Months.	1821.	1822.	1823.	1824	1825.	Means.
	°	°	°	°	°	°
January. .	34.8	37.1	27.2	37.4	34.7	34.2
February .	32.4	39.3	32.2	36.7	35.4	35.2
March. . .	38.1	40.4	35.3	35.0	36.2	37.0
April . . .	42.8	41.5	37.2	39.0	41.8	40.4
May . . .	41.7	46.5	45.8	44.0	45.9	44.7
June . . .	45.8	54.7	47.2	51.0	49.8	49.7
July. . .	50.0	53.9	51.4	54.9	55.1	53.0
August . .	54.8	52.5	52.2	53.9	54.9	53.6
September	53.3	48.1	48.9	52.6	53.7	51.3
October. .	46.3	45.8	43.2	44.9	47.3	45.5
November	41.8	43.3	42.1	41.0	37.4	41.1
December	39.3	30.4	37.7	36.6	36.4	36.0
Means. . .	43.4	44.4	41.7	43.9	44.0	43.4

In finding the dew-point, or that degree of temperature at which the aqueous vapour in the atmosphere begins to be condensed into water, I employ the method introduced by Le Roi, and recommended by Mr. Dalton. An observation was made every day, in the open air, usually between nine and ten in the evening, but as this hour is rather too late, except in summer, to give the true daily mean, the monthly means obtained from the observations are a little lower than they should be, especially in the winter months. It will be perceived, that on the average of the five years, the quantity of aqueous vapour in the atmosphere is at a minimum in January, and that it goes on progressively increasing till August, when it arrives at the maximum; it then begins to diminish gradually, and continues decreasing till the month of February. The mean annual point of deposition is 43°.4, which is 5° lower than the mean annual temperature for the same period. The lowest state of vapour in the atmosphere, observed in the course of the five years, took place on the 18th of January, 1823; when the dew-point was 13°, corresponding to .1 of an inch of mercury in force, which is equal to 1.4 inches of water, the temperature of the air, at the time, being 17°; and the highest state occurred on the 18th of July, 1825; when the dew-point was 68°.5, corresponding to .69 of an inch of mercury, or 9.6 inches of water, the temperature of the air, at the time, being 73°.5. The difference between the quantities of water contained in a vertical column of the atmosphere, in these particular instances, is 8.2 inches.

It was my intention to introduce, in this place, a series of observations on the evaporation from water, but my gage was so unfavourably situated, and I experienced so much difficulty in protecting it sufficiently from rain, frost, and birds, without, at the same time, impeding, in a great measure, the free admission of air and sunshine, that the results were considered too incorrect to be admissible here; they are, therefore, withheld; and I the less regret this circumstance, because the quantity of water evaporated each month throughout the year, may always be found from the mean monthly temperature and point of deposition.*

* See the second part of the 5th volume of the First Series of the Society's Memoirs, p. 588.

Observations

ON

THUNDER STORMS AND LUMINOUS METEORS.

Intimately connected with the state of the aqueous vapour in the atmosphere, are the electrical phenomena of thunder and lightning. By evaporation the electric fluid is silently raised and accumulated in the air, and by condensation, or the conversion of steam into water, its intensity is increased; till, under favourable circumstances, it manifests itself in those energetic and sublime displays which are witnessed in thunder storms. From the annexed comparative view, founded on the experience of the five years, commencing with 1821, and terminating with 1825, it is plain, that those months in which the dew-point is highest are most liable to thunder and lightning.

Mean Monthly Point of Deposition.

Jan.	Feb.	Mar.	April.	May.	June.
34°.2	35°.1	36°.9	40°.4	44°.7	49°.6

Number of Thunder Storms observed.

Jan.	Feb.	Mar.	April.	May.	June.
0	0	4	10	15	16

Mean Monthly Point of Deposition.

July.	Aug.	Sept.	Oct.	Nov.	Dec.
53°.0	53°.6	51°.3	45°.4	41°.0	36°.0

Number of Thunder Storms observed.

26	27	8	6	5	3

On the evening of the 10th of June, 1822, this neighbourhood was visited by one of the most remarkable thunder storms I remember to have seen: numerous and vivid electrical discharges, from almost every point of the compass, illuminated the sky; indeed, for the space of more than an hour, there were, on an average, sixteen distinct flashes of lightning in a minute. The point of deposition, at the time, was 57°.5, and, on the evening of the preceding day, it was 61°. When the dew-point is unusually high for the season of the year, but more especially in summer, thunder frequently ensues on the same or following day.

Of eighteen appearances of the aurora borealis, which have been registered from 1821 to 1828, inclusive, one occurred in 1821, one in 1826, seven in 1827, and nine in 1828; it is evident, therefore, that this interesting phenomenon has been visible much more frequently, in England, during the last two years of the series, than for a considerable period antecedent to them. On three occasions, namely, December the 27th, 1827, and December the 1st and 26th, 1828, the aurora, in the form of a rainbow-like arch, was seen to cross the magnetic meridian, by the plane of which it was bisected, at right angles. That the true height of the luminous arches of the aurora must be great, is certain from contemporaneous observations which have been made upon them, by persons situated in distant parallels of latitude. According to Mr. Dalton, who has recently determined their altitude trigonometrically, it is about one hundred miles above the surface of the earth.* The periodical occurrence of this splendid meteor, and the motion from north to south, which the luminous arches are usually perceived to have, well deserve the attention of the scientific meteorologist.

With regard to the meteors known by the appellation of shooting stars, I have little to observe, except that their motions do not appear to be influenced either by currents of air, or by the earth's magnetism; and that their elevation is probably considerable. The great velocity with which they pass through that portion of their path in which they are visible, and the various and opposite directions they pursue on the same

* Transactions of the Royal Society for 1828, Part II.

night, and even at the same moment of time, sufficiently establish the accuracy of the first remark; and the fact that they must have an elevation of at least several miles, is proved by their never being seen within the region of the clouds. Of more than 260 shooting stars, above the medium size, observed in the course of the eight years, not one was perceived to pass beneath a cloud; it is desirable, however, that the altitude of these meteors should be correctly ascertained by exact measurements.

On the 7th of September, 1828, at half-past eight, P.M., a large meteor appeared which was visible over a great extent of country. According to numerous accounts of this phenomenon, which, with the assistance of Mr. Peter Barrow, I collected from newspapers and other periodical publications, it was seen at Glasgow and Dumfries, in Scotland; at Newcastle-on-Tyne, Carlisle, the village of Bolton in the north riding of Yorkshire, Whitby, Scarborough, York, Hull, Preston, Horton in Ribblesdale, Blackpool, Manchester, Matlock, Northampton, Chelmsford, Bristol, Brighton, and Plymouth, in England; at several of which places it is remarked, that its altitude above the horizon was very considerable; it was seen also by a passenger on board a steam-boat a few miles to the north of the Isle of Man.

Some individuals describe this meteor as having a diameter equal to that of the sun or moon, but they must have greatly overrated its apparent size, misled, in all probability, by the extreme brilliancy of its light; for a very intelligent lady of my acquaintance, who saw it at Manchester, informs me, that it appeared to her somewhat larger than the planet venus when at its greatest elongation.

Observers at York, and to the north of that city, state, that the direction in which the meteor moved was south-easterly; while those at Manchester, and to the south of that town, remark that it was north-easterly.

This difference of opinion was occasioned, no doubt, by mere optical illusion; due allowance for which being made, it is very probable that the true path of the meteor was nearly from west to east, and that it was vertical somewhere between York and Manchester.

Though these observations do not supply data from which the exact height and magnitude of this meteor may be determined, yet they clearly establish the fact, that its true altitude and size must have been very considerable; and taken in conjunction with former observations upon similar phenomena, powerfully support the opinion, that the luminous meteors, denominated shooting stars and fire-balls, occur in a very elevated region of the earth's atmosphere.

REMARKS

ON THE

STATE OF BRITAIN,

AT THE

TIME OF ITS CONQUEST BY THE ROMANS.

BY MR. THOMAS HOPKINS.

(Read March 25th, 1827.)

THE system of education pursued in modern Europe, has made its inhabitants familiarly acquainted with the classical writers of ancient Greece and Rome. At the period of the revival of a taste for literature, the productions of those writers were felt to be so superior to what was then produced, that the veneration for them appears to have been almost unbounded: and the same feeling has prevailed, in greater or less strength, down to the present day. This veneration no doubt contributed to improve the taste of the European world. The study, the analysis, the imitation of such admirable models have had, it must be admitted, a beneficial influence on modern literature.

But some disadvantage has also attended this course. The Greeks, and after them the Romans, were accustomed to treat all other nations as barbarians, and in the study of Grecian and Roman literature, the unjust prejudices of those people against other nations seem to have been imbibed by the student. Hence, the neglect of, and contempt for, other ancient writings and systems, until a comparatively recent period, when our possession of an extensive territory in the eastern part of the world made it necessary that the languages of that part should be studied. Attention is, therefore, now no longer exclusively directed to Greece and Italy; nor would the contemptuous style in which their writers indulged, when speaking of other ancient countries, be imitated by a modern author. Yet, on some points, our common forms of speaking and writing indicate that we retain the associations of ideas given to us by the classical writers of antiquity: insensibly imbibed perhaps, and retained only because they have not been put to the test of an impartial examination.

The ancient Britons are, by Tacitus, called *Barbarians,* and they forthwith are assigned a place in our memories with the North American Indians, or the savages of New Zealand. Adam Smith, speaking of ancient Britain, says 'its inhabitants were, at the time of the invasion of Julius Cæsar, in the same state with the savages of North America!' See 'Wealth of Nations,' Book 2nd. Chap. 3rd. page 25.

Of the actual state of the Britons at the time of the Roman invasion we have no direct accounts, except those furnished to us by their conquerors, the evidence is therefore neither of the best nor the fullest kind. The Romans, however, appear to have been too well satisfied with their own superiority to permit themselves to descend to the making of false statements respecting their enemies. Falsehood and misrepresentation are the expedients of weakness, and the Romans felt themselves to be strong. We find, therefore, in the writings of Cæsar and Tacitus, facts stated which, if exhibited in just connexion, will furnish us with a body of direct evidence respecting the state of Britain at the period of the Roman conquest, from which we may draw such inferences as the facts will warrant.

Julius Cæsar, when speaking of what he could himself ascertain, and not from the reports of others, as he must often have done, says, in book 5th of his Commentaries, that 'the island is well peopled, full of houses, built after the manner of the Gauls, and abounds in cattle.' And from his way of speaking of foraging, it is sufficiently evident, that in the part where he was, the land was extensively cultivated. Indeed he plainly says, a little further on, that 'the inhabitants of Kent,' that is almost the only people he had seen, 'differed little in their manners from the Gauls.'

It is true, he also says, that 'the greater part of those who live in the interior, never sow their lands, but live on flesh and milk and go clad in skins,' but this statement must be considered as resting on the report of others, Cæsar himself having merely passed through Kent and Middlesex and to near St. Albans, only twenty-one miles from London. And we shall see that this statement, made by Cæsar, on the reports of others, respecting the interior, is not confirmed by the Romans themselves, when they afterwards penetrate into it. What Cæsar thought of the inhabitants of the parts which he had himself seen, of their numbers, intelligence, courage and resources, is clearly proved by the preparations he made for his second expedition.

Of those veteran troops, formed in his struggles with, or rather his conquest of, the Gauls, he thought it necessary to bring over five legions and two thousand horse, or about twenty-three thousand men! Now, is it to be supposed, that Cæsar, with the knowledge he had obtained in his first expedition, would have brought with him so formidable an army, if he had not had a very high opinion of the intelligence, spirit, resources, and numbers of the Britons? Are we to believe that he, whose master-mind crushed the giant spirits of Rome, who dared to contend for the empire of the world, with only about one half the number of troops his opponent had, though those opposed to him were Romans, and the great Pompey their commander,—are we to believe, I say, that this Cæsar took with him five legions and two thousand horse to conquer a few wretched barbarians, 'in the state of the savages of North America?' It is impossible to believe this. Cæsar knew whom he had to contend with, from the experience he had had in his first expedition; and he tells us what he knew, by the formidable preparation he thought it necessary to make for his second expedition. And the small extent of his conquest justified the foresight he had exhibited. He commenced his campaign as early as the season would permit, and he found it expedient, at the end of that season, to withdraw from the country, without leaving a single garrison in the Island.

In his account of the campaign, he speaks of the numbers, discipline, and spirit of the natives, as making them truly formidable. There never seems to have been any thing like the attacks of a mob of men. All appear to have been under the controul of discipline; attacking and retiring in order. Their numerous horse and chariots must have required abundant magazines, and no mention is made of a deficiency of supplies. The superiority of Roman arms and discipline, superiority in the arts of war alone, appears to have enabled the Romans to prevail to the extent they did over the Britons. The Romans too, it should be observed, had to contend with the forces of but a small part of the island, and that small part was disunited. If then, the great Cæsar, with a formidable army of veterans, was obliged to expend a whole summer in fighting with the disunited power of Kent and Middlesex, and some of the adjoining parts, and then found it necessary to abandon the country, we may conclude, that the inhabitants were really numerous, and possessed of means of subsisting themselves and defending their country, which should place them among populous and wealthy states, instead of enrolling them among savage or barbarous tribes.

From the period of the invasion by Cæsar, fifty-four years before the Christian era, the Britons were left in the possession of their independence, until the reign of Claudius, who sent Aulus Plautius with four legions with auxiliaries and cavalry amounting to fifty thousand men. This was in A. D. 43. This commander, with his immense army of Romans, fought a battle with the Britons which lasted two days, in which the latter were defeated. But a Roman army of 50,000 men, was considered equal to the subjugation of the most formidable states. It appears, however, to have been thought necessary, for Claudius himself to follow with another large army, of horse, foot, and elephants, to subdue the few barbarians.

After this, we find Vespasian, who was second in command, fighting no less than thirty battles. He took, also, twenty towns, and conquered the isle of Wight, and the whole country from Kent to Cornwall. But, do these struggles of the natives, disputing the progress of the invaders at every step, shew that the country was poor and barbarous? Certainly not. The inhabitants appear to have been divided into a number of small states, and their invaders attacked them separately, and in succession; but if they had not been numerous and powerful, they could not have made the resistance they did, and their conquerors would have spoken of them in very different terms to what they have done.

While Vespasian was fighting in the west, Plautius made war upon the inland Britons, and was, as usual, successful: for which he obtained on his return to Rome, in the year 47, an ovation of twenty days. We find, however, that three years afterwards, it was necessary to send Ostorius Scapula against Caractacus, (said to have been previously conquered by Plautius.) In describing the battle, which ended in the defeat of that celebrated British commander, the historian, (Tacitus) says, Book 12th, speaking of the Roman troops, 'In approaching the bulwark, while the encounter was yet managed by flights of darts, there were more of our men wounded, and many began to fall; but after they had formed themselves into the military shell, demolished the rude and shapeless structure of stones, and encountered hand to hand, on ground equal to both, the barbarians took themselves to the ridges of the mountains, and thither also mounted our soldiers after them, both the light, and the heavy armed. Here, also, was begun an unequal fight, by ours in close order, against the Britons, who only fought by a discharge of arrows, and as they cover themselves with no armour, were, thence, straight broken in their ranks. Where they resisted the auxiliaries, they were slaughtered by the swords and javelins of the soldiers of the legions, and by the great sabres and pikes of the auxiliaries, where they faced those of the legions,—signal was this victory. The wife and daughter of Caractacus were taken prisoners, and his brothers surrendered to mercy.'

Caractacus himself was, shortly after this battle, delivered up to the Romans, having opposed them for nine years. But the surrender of their chief did not subdue the spirit of these inland Britons, who, from the statement of Julius Cæsar, on the report of others, have been represented as a few barbarians, for it appears, from the account of Tacitus, that they shortly afterwards 'assailed, by surprise, the camp marshal and legionary cohorts left to rear forts among the Silures, and but for sudden succours from the circumjacent garrisons, our troops had been cut in pieces. As it was, the marshal and eight centurions were slain, with the most resolute soldiers. Soon after, they entirely routed our foragers, and even the troops left to guard them.'

And a little further on, the historian says, 'they surprised and carried off two auxiliary cohorts, who were, without due circumspection, plundering the country, to satiate the avarice of their officers; and by distributing the spoil and captives among the neighbouring nations, they were drawing them also into the revolt, when Ostorius, sinking under the weight of his anxieties, expired.'

These Iselwyr, (as the Britons called them, or Silures, according to the Romans) it appears, inhabited South Wales and some of the adjoining counties, and formed but a small part of the whole British people. They had neighbouring nations who did not assist them, but whom they were only drawing into the revolt. And yet, this small portion of the Britons, continued to make head against formidable Roman armies, for eleven years, that is, nine years under Caractacus, and two years afterwards, when Ostorius sunk under his anxieties.

The Romans had now been eleven years in attempting the conquest of the Island, without having subdued one half of it; and what they held was preserved by the presence of an overawing military force. And four years after this, it seems to have been thought necessary to send a commander of superior reputation, when, to use again the language of Tacitus, 'Seutonius Paulinus obtained the government of Britain: a competitor with Corbulo in the science of war, and in the voice of the populace, who to every man of renown are sure to create a rival. He (Seutonius) hoped too, by subduing that fierce enemy, to reap equal glory to that which the other derived from the recovery of Armenia. He therefore prepared to fall upon the island of Anglesea, powerful in inhabitants, and a common refuge to the revolters and fugitives. He built for that end boats with broad flat bottoms, the easier to approach a shore full of shallows and uncertain landings.'

As usual, the Romans were successful in a general battle, but it is worth while to observe, that it took Seutonius three years to conquer the island of Anglesea; for he landed in the year 58, and the island was taken in 61. While, however, this was going on, the Icenians under Boadicea, had revolted and destroyed seventy thousand of the Roman troops and partizans, and collected an army of the extraordinary number of two hundred and thirty thousand fighting men! The historian, in speaking of this army on the day of battle, says, 'The British army were every where exulting and bounding in great separate bands, some of horse, some of foot, and exhibited in all a multitude so vast as hitherto was not paralleled! In conclusion, he says, 'signal was the glory gained that day, and equal to the victories of the ancient Romans, for there are authors who record that of the Britons were slain almost 80,000.'

In this, as in former instances, the Romans were indebted to their superior skill and discipline for their victory, but, though the Britons knew not how to marshal large armies, it does not follow that they were barbarians, much less that they were few in number. The immense army of Boadicea was collected from a part only of the island, and as it was kept together for some time, we must believe that the country was very populous and had abundance of supplies. And does not the language of the historian shew that he thought it so? He speaks of the number of the British army as being 'hitherto unparalleled;'—and he pronounces the glory obtained to be 'equal to the ancient Romans,' meaning of course, that it was superior to that obtained by any of the modern Romans. And are we to look upon these people as being in a state similar to the savages of North America?

But what was the fruit of this victory? Did the whole island submit to the Roman arms after this terrible defeat and slaughter of the natives? So far from this being the case, the Romans do not appear to have extended their conquests, for we find, that in the year 78, that is, in 17 years after the victory of Seutonius, the celebrated Agricola had to reconquer the island of Anglesea, and to subdue the Ordovices of North Wales and Cheshire. This commander, is represented by Tacitus, as possessed of talents of the highest order, either for peace or war; yet this great man, with all the advantages arising from the Roman arms and discipline, and his own talents, took six years to push his conquests into Scotland, and defeat Galgacus. Where we find the Romans, fearing that from the surpassing multitude of the enemy, they would be beset at once in the front and on each flank.

Thus, then, we see, that from the year 43, A. D., when Aulus Plautius was sent by Claudius with an army of fifty thousand men, that the Romans had to send successively, their most able generals, with formidable armies, during a period of forty-one years, or until the year 84 when Galgacus was defeated. Must not Britain then have been both populous and powerful? Can any other supposition account for the lengthened resistance made to the Roman arms? Gaul was conquered by Julius Cæsar, in a few campaigns, yet it was known to be populous; and so powerful had the Gauls been considered, by the Romans themselves, a short time before, that they had made a special provision against a Gaulish invasion. But it took forty-one years to conquer Britain! And if we admit Tacitus to be a good authority on the point, we must acknowledge that, even then, the success of the Romans was attributable principally to the disunited state of the Britons. He says, in his life of Agricola, 'The Britons were formerly subject to kings, they are now swayed by several chiefs, and rent into factions and parties, according to the humours and passions of those their leaders. Nor against nations thus powerful does ought so much avail us, as that they consult not in a body for the security of the whole. It is rare that two or three communities assemble and unite to repulse any public danger threatening to all. So that while only a single community fought at a time, they were every one vanquished.' See Gordon's Tacitus, page 81.

Here Tacitus plainly attributes the success of the Romans, not to any deficiency of numbers, or strength, on the part of the Britons, but to their want of union. He speaks of them as 'powerful nations' made weak by disunion. We should recollect too, that the writer who speaks thus, was familiarly acquainted with the great power of the Roman state, and the populous countries that then composed the empire, and would not be likely to use the language he has done, unless the Britons were really numerous and powerful. Had Tacitus been a native of some small state, his language might be construed to mean, that the Britons were powerful compared with his own petty nation. But Rome was then at the height of her greatness, and really potent nations might well have appeared weak in the eyes of a Roman. The language of the writer may, therefore, be understood to mean fully as much as the expressions will convey: and, together with the forty-one years that the Romans took to conquer Britain, sufficiently prove that it was, at that period, both populous and powerful.

So far, we have proceeded on the direct evidence furnished by Cæsar and Tacitus, but there are other facts which lead to a strong presumption in favour of the same conclusion. The internal systems of economy of Britain and Gaul were very similar, and Gaul was known to be extremely populous. And to an overflow of its population, the invasion of Italy, by the Gauls, was attributed. Appian states, that there were four hundred nations in Gaul, and Diodorus

Siculus (lib. v.) says, that the largest of these nations consisted of 200,000 men, besides women and children, and the least of 50,000. Calculating, at a medium, we must, consequently, admit of a population of from 180 to 200 millions of people in that country, the population of which does not, at present, exceed 40 millions.

Nor will this number appear so extraordinary, if we consider what was the state of society among these people. Cæsar says, Book 6th, after speaking of the Gaulish Druids, ' the other order of men is the Nobles, whose whole study and occupation is war. Before Cæsar's arrival in Gaul, they were almost every year at war, either offensive or defensive, and they judge of the power and quality of their nobles, by the vassals and the number of men he keeps in his pay, for they are the only marks of grandeur they make any account of.'

In such a state of society, and it appears to have been common to both Gaul and Britain, there would naturally be a dense population. Up to a recent period, the comparatively barren highlands of Scotland teemed with population, because each noble, or land proprietor, endeavoured to have as many men as he could keep, and to that essential point every other consideration was sacrificed. (See the Earl of Selkirk on Emigration.) In the early times of the Roman Republic, Italy itself appears to have been divided into a great number of small states, all of them extremely populous. So that the whole of this extensive range of country, Italy, Gaul, and Britain, was very populous, and became so from the same causes. Why then should it be said, that while Gaul and Italy were very populous, the Britons were few in number, and scattered over the face of the country. The Roman writers calling them barbarians seems scarcely sufficient to account for it. And yet to what other cause can we attribute it?

Perhaps a part of the description of the country by Cæsar may have contributed to form the erroneous impressions which exist on this subject. He says, 'a town among the Britons is nothing more than a thick wood, fortified with a ditch and rampart to serve as a place of retreat against the incursions of their enemies.' But this proves nothing against the populousness of the country; for he himself, had before said, that the island was well peopled and full of houses. What Cæsar calls a town, seems to have been merely a place of temporary security to retire to, in the event of a neighbouring tribe making an incursion into their territory.

With the Britons the land was so extensively divided among the people, that it became necessary that each owner should reside upon his portion, which he cultivated for a subsistence; and as no rent, and few taxes were paid, there was no opulent class to congregate together to form towns. David Williams in his history of Monmouthshire, page 17, when speaking of the manner in which land was disposed of among the ancient Britons, says

'Supposing the laws of Howell, the good, to have been principally collected from the ancient customs of the country, the political constitution may be thus summarily described. In every district or principality the Prince was the depositary of the general property, or the trustee of the territory, which was divided into portions of twelve acres to every man in public employment, eight to every father of a family, and four to a stranger becoming a member of the community. All the unoccupied lands remained with the Prince, whose privileges were transferred in succession to the eldest of his kindred possessing the best accomplishments of body and mind.'

And further on he says, 'that on the country becoming too full of people, they made war on their neighbours to procure lands, and this was one of the reasons of the general state of warfare in which the Britons were found by the Romans.'*

It is not easy to conceive a system more likely to raise up a great population than the one here described. It is well known, that dividing land thus among numerous occupants, is extremely favourable to an increase of the number of human beings to the utmost limit of subsistence. Four acres we find were considered sufficient for a stranger, and on the increase of a family, necessity would frequently induce the natives to be content with the same extent. But taking the eight acres as an average of the quantity for one family, throughout the country, and supposing there to be four and a-half persons to each family, and we shall have more than one human being to every two acres of land. As the number of acres capable of cultivation in the island is about forty millions, this would give a population of twenty millions, being nearly double the number returned in the census of 1801, which was 10,942,611.

* Gildas says, that when the Britons were at peace, their lands produced abundance of grain. One of their laws prohibits ploughing with horses, mares, or cows, (oxen only were to be used.) Six or eight persons formed themselves into a society for fitting out a plough. Eighty-six laws were made to protect and regulate agriculture.—See *Leges Wallicae,* page 28—298.

This does not, however, make Britain so populous as Appian and Diodorus Siculus represent Gaul to be. These authors exhibit Gaul, as from four and a half to five times as populous as it is at present, while the above calculation, shews ancient Britain, to have been not quite twice as populous as modern Britain was in 1801. It is not, however, pretended, that the data which we are in possession of, are sufficient to enable us to come to any definite conclusion, respecting the actual numbers of the ancient Britons: but such as we have, are in favour of the supposition that they were great, and certainly do not warrant the conclusion, that they were few in number, and savages.

The small number of remains of buildings, prior to the Roman invasion, does not militate against what has been advanced respecting the population. Durable buildings are not erected (except for public purposes) by a people spread over the land, as small proprietors. The Gauls, like the Britons, have left few remains of that kind. The great population of China, is now established by undeniable evidence, yet we are told, there are no buildings in China likely to last for many centuries. And should conquest introduce a different system into China, and no records of its present internal economy and great population survive, a future age might be led to doubt the existence of its present state. The land in China is much divided, and a moderate portion of its produce is paid to the sovereign. A similar system prevailed in Britain, and a great population seems to be the natural consequence. China, however, is large and united, and her canals and her great wall may remind a distant age of what China once was. But, although Britain was divided into so many small states, there are some remains yet in being, which shew that a considerable population must have existed, at the time they were formed. Of these, the great Druidical temples have the most frequently been described. But the Wansdyke is not so well known. This great work commences at Portishead, near Bristol, and passing by Bath is carried across Marlborough downs to Andover, in Hampshire, a distance of eighty miles. Near to Marlborough downs, the Vallum has been thrown in to construct a Roman road, which shews it to have been formed, prior to the invasion by the Romans. It probably was a frontier defence, and it indicates a considerable population in a small state.

The Welsh, and the Scotch Highlanders, as the descendants of the ancient possessors of the island, may be presumed to have retained, in some degree, the spirit of their institutions; and it is fair to infer, that the effects of those institutions, would be traceable in their condition. From the fifth and sixth centuries, when the Saxons acquired possession of the greater part of England, the Welsh maintained a constant struggle in defence of their country, against the Saxons, Danes, and Normans, until the 14th century. Now there must have been something in their institutions, calculated to keep up the population to its full complement, and that population must have been considerable, or such a defence, for, from eight to nine hundred years, could not have been made against the great power of England. And it is reasonable to conclude that those institutions, (founded on the ancient customs of the country,) which kept up the population of Wales during this period, made ancient Britain both populous and powerful.

Even down to a comparatively recent period, Wales seems to have been very populous. Malkin, in his account of South Wales, says, ' one of the reflections, which will most forcibly strike an observing traveller in Wales, and scarcely meet with credit, from those who have not visited this country, is the height of improvement and grandeur to which it had attained, at an era looked back upon as barbarous, through the delusive medium of modern pride. The style of Castle architecture, the style of Cathedral architecture, the style even of the cottages that still remain, evince the flourishing state of those arts, which infer a corresponding convenience in others, whose evanescent nature precludes us from more direct evidence of their perfection.' 'The continual recurrence of Castles, some built for strength, and others for magnificence, but all with demonstrations of skill extorting the acknowledgments of inferiority from the candid workman of these days, is of itself sufficient proof, how considerable must have been the population, how great the warlike force of the district.'

'In England, our ancestors have left us, dispersed in various places, splendid remains of their greatness, but in Wales you can scarcely travel ten miles without coming upon some vestige of antiquity, which in another you would go fifty to trace out. Nor is it alone in the palaces of the lords, that these features of civilzation are to be found. The ruins of ancient farms and barns, are particularly to be noticed, as unquestionable evidence of opulence and fertility.'

These remarks of the traveller, have reference to a period comparatively recent, and when the ancient system had been considerably departed from, but they shew some of the later effects of that system, which in spite of numerous obstacles, kept up the race of the ancient Britons.

In the Highlands of Scotland too, we find similar causes producing similar effects. The Romans found it necessary to build strong barriers to secure their conquests on the northern frontier. Was not this of itself an acknowledgment, that the people beyond this frontier were powerful? And when, at the end of more than three centuries, the Romans abandoned the island, forth issue the northern warriors in swarms of the most formidable description, shewing that the same causes that had formerly made Britain populous, had fully peopled the cold and barren hills of the north. And from that time until the year 1745, when the system of economy which, with some modifications, had existed so long, was finally destroyed, the highlanders, with their celtic manners and customs, were always numerous and often proved themselves to be powerful.

Every view, then, that we take of the subject, shews that the ancient Britons, at the time of the Roman invasion, were not a few savages or barbarians, but a great people, divided into many states, and inferior to their conquerors in the art of war. They had a digested system of morals, and a firmly established religion, which was common to their country and to Gaul. Their priests spent a considerable part of their lives in study. Into the mazes of metaphysics, they seem to have penetrated deeply; and they acquired a degree of command over their followers, not surpassed by any of those who have succeeded them. To the present day our manners are tinged with their routine of observances, more particularly in rural superstitions and festivities. The Druids sunk long since, and their system is now seen only through the mist of obscurity, as we recede from them floating down the stream of time. But so have the systems which formerly prevailed on the banks of the Tiber, of the Nile, and of the Euphrates; and Druidism would probably appear as venerable as any of those other systems which flourished in ancient days, but are now no more, if we had had their doctrines handed down to us in writings. The Gauls, we are told, looked up to Britain as more enlightened in their common religion! Is it not a fair inference from this, that the Britons were considered a more enlightened people than the Gauls?

Until lately it was a generally received opinion, that the Southern Britons, upon the retirement of the Romans from the island, were the almost unresisting victims of the Northerns and the Saxons: but although Britain, in all probability, participated in the weakness which was apparent throughout the Roman empire, a living historian has shewn, that its inhabitants bravely defended themselves against their new enemies, as they had before done against the Romans. And it was to their want of union, that they once more had to attribute their defeat after a century of struggles; when they made a stand in Wales, where they kept their assailants at bay for nearly nine hundred years. But full justice has not yet been rendered to them, if it had, they would not be represented as they commonly are, but would have a respectable place assigned them, in the list of the celebrated nations of the ancient world.

REMARKS

ON THE

STUDY OF ENTOMOLOGY,

AND ON AN

HYMENOPTEROUS INSECT WHICH DEVOURS THE LEAVES OF THE GOOSEBERRY BUSH.

BY JOHN MOORE, ESQ. F.L.S.

(Read April 6th, 1827.)

AN eminent writer observes, that "the best "way to learn any science is to begin with a "regular system, or short and plain scheme of "that science, and for this reason, young stu-"dents should apply themselves to such systems "rather than to pamphlets."

At the period when this excellent advice was given, and, indeed, before the time of Linnæus, several branches of Natural History, and especially Entomology, could hardly be said to have assumed any generally acknowledged systematic arrangement.

Although much of the instruction contained in the Systema Naturæ may be said to have been collected from writers of our own country, yet a great part lay scattered in the works of learned foreigners, which were very little read here. Under these circumstances, the labours of that great man will always be considered as forming an important æra in Natural History.

The warmest admirers of more modern systems confess, that the beautiful arrangements of Linnæus were wonderfully adapted to the knowledge of his day; whilst the student, who still wishes to adhere to them, should remember, that any elementary work to be at all perfect, must, of necessity, keep pace with the progress of discovery.

When we recollect, that Linnæus comprised insects under seven orders, and that recent authors, after throwing out all such as have more than six legs, have already extended these orders to sixteen, we are sometimes led to fear that a fondness for new names occasionally outruns the necessity for them ; and, if we also call to mind the small portion of classical knowledge which falls to the lot of a majority of students, we cannot help regretting the increasing difficulties which the fashionable orismology of this interesting science is daily heaping upon the unlearned.

Young entomologists are well aware of the advantages they derive from the modern division of insects into the two classes, Mandibulata and Haustellata; and few persons will deny, that, for a great proportion of students, simple arrangements are very desirable; how far they are consistent with a proper knowledge of the subject is a more difficult question.

In the study of Ornithology, much convenience has arisen from separating birds into the two grand divisions of land and water birds, with the intermediate tribe of waders; I am not aware that any similar attempt has been made in entomology, but, if it be practicable, I submit, with much deference, that it would be of great use.*

* Since this paper was read, I have discovered that Aldrovandus, in his voluminous work published in 1602, divides insects into terrestrial and aquatic; and Wolfgang Fantzius, in his Historia Animalium Sacra, has a distribution of insects into äerial, aquatic, and terrestrial. Scopoli, in 1777, divided his proboscidea into terrestrial and aquatic, and the coleoptera into those inhabiting water, and those the land; but I am not aware that any modern author has adopted similar divisions.

Birds, which frequent water, are not more beautifully, nor more peculiarly organized for that purpose, than those interesting insects which inhabit, or undergo any of their charges in that element.

When we observe, that the foot of a strange bird is webbed, we immediately conclude it is intended to have the power of resorting to water; and, analogous to this provision, we find, that those insects which are found in or upon water have some of their legs flat or ciliated, for the purpose of swimming or diving.

A division of insects into such as appertain chiefly to land or to water, could not, I presume, be so made as exactly to comprehend the whole of any number of the linnean or modern orders, but it would not, however, interfere with them. We might, perhaps, safely denominate, as land insects, all such as deposit their eggs in various substances and places on dry land; and as water insects, all such as deposit their eggs in water or marshy places.

With the hope of drawing the attention of some of our distinguished entomologists of the present day to the subject, I would name the following as subdivisions of water-bred insects.

1st.—Such as undergo the whole of their changes in water, and are enabled to seek their food in it, or upon it, throughout their lives.

This subdivision would comprise, of coleopterous insects, the carnivora aquatica (the hydrocanthari of Latreille, and Dyticidæ and Gyrinidæ of Leach) and also the families Parnidæ, Helophoridæ, and Hydrophilidæ of Leach. It would likewise embrace the Hemipterous families Nepadæ and Notonectidæ.

2nd.—Such as undergo all their changes in water, but to which, as perfect insects, water is destructive.

I presume the whole of the modern order, Trichoptera, (the Phryganea of Linnæus;) and most of the genera of the family Tipulidæ would fall under this sub-division.

3rd.—Such as undergo all their changes, excepting the last, in water, but to which, as perfect insects, water is destructive.

The beautiful families of neuropterous insects, Ephemeridæ and Libellulidæ, are of this description.

As this hasty and imperfect sketch of water-insects may serve to shew the small space they occupy in the sixteen modern orders, it may, perhaps, hold out an encouragement to the young entomologist to pay more attention to their investigation.

The manner in which they deposit, and the very high specific gravity of some of, their eggs, seem to have escaped the notice of our best authorities.

Swammerdam states, that "the eggs of the "female Ephemera drop into water, and there "impregnated, gradually sink to the bottom." A little attention to this interesting family will, however, convince any enquirer that this is not the case with the Ephemeridæ of the Derbyshire rivers.

I apprehend, there is also a remarkable circumstance connected with the separation of the eggs from the female, which, I am not aware, has been any where noticed, viz. they are not ejected, as by most insects, from the caudal extremity, but between the second and third rings above it; and, I believe, invariably in her up and down flights over the water. With respect to their sinking gradually, it may also be remarked, that the eggs of the ephemera vulgata, when dropped into a glass of water, sink to the bottom with surprising speed.

The coats of these eggs being entirely membranaceous, and containing no calcareous matter whatever; and the interior fluid being perfectly transparent, we are the more surprised that this should be the case; but the uncommon weight of the eggs is no doubt intended to enable the female to select, even in rapid streams, a proper resting place for her infant progeny. A similar provision will most likely be found to extend to all those insects which are born in water, but which, in a perfect state, cannot safely come in contact with it for the purpose of depositing their eggs.

Swammerdam supposed the ephemeræ to live three years, at least, in the water, in a larva state; scientific anglers have, however, long been satisfied, that the eggs of one year become perfect insects the next.

This acute observer did not fail to remark, that the femur and tibia (especially of the posterior pair of legs) are, in the larva of the ephimeridæ, flat and fin-like, evidently for the purpose of swimming; whilst, in the fore legs, they are round, and, of course, better fitted for penetrating the bottoms or sides of rivers in search of food.

Considerable advantage, I am of opinion, would result from laying down certain general rules after the manner of the beautiful, but much overlooked, classification of larvæ, by Bergman, which would enable the student, on examining the more important organs of water-bred larvæ, to predict the family to which the perfect insect will belong. Thus, whenever we observe in the water an insect travelling about, in a larva state, in a pipe or tube of any kind open at the end, we may, I think, safely decide that its wings will be recumbent, in some form or other, when reposing.

Such a rule, if correct, would long since have settled the point as to that fly, so interesting to anglers, the ephemera vulgata or May-fly, springing from a caddis or straw-worm, as most of our fishing books, from Father Walton downwards, as well as many distinguished writers on entomology, have erroneously described it; for it will be remembered, that its wings are always vertical when at rest.

Again, whenever the larva of an insect travels about in the water, in search of food, unprotected by a pipe or case, it will, I believe, be found to have the femur and tibia of some of its legs, flat and fin-like, for the purpose of swimming or diving.

Where the nidus of an insect is its proper food in a larva state, as is the case with the musca vomitoria or blue bottle, and many other land-bred insects, which deposit their eggs in stercoraceous matter, such larvæ will, most probably, be found without feet or walking processes of any kind, and to have annular motion only. And, pursuing the analogy still further, it will be matter of curiosity to ascertain whether any, or what number of, water-bred insects are deposited in cells, and fed by their parents after the manner of the social insects apidæ, vespidæ, &c. on land, and, therefore, need not legs in a larva state.

The division into land and water-insects, whilst it would shew the comparatively little attention which, in this country at least, has been paid to the latter division, might render important services to the neglected study of Ichthyology, by exhibiting the bountiful provision which is made, for various kinds of fish, at different periods of the year; and the zealous disciple of the excellent Walton would likewise be enabled better to account for his occasional disappointments on days apparently favourable for fly-fishing, by learning, that trout are frequently too much engaged in seeking their food amongst the various kinds of larvæ at the bottom of the river, to pay any attention to the more active flies upon its surface.

Organization being the only safe ground-work of any system of Zoology, and diversity of structure being no where more apparent than in the wonderful adaptation of some insects for land and others for water, I apprehend a better defence of the outline I have ventured to recommend can hardly be required than this, that, whilst it is in strict conformity to those rules which govern the more learned naturalist, it would, at the same time, afford a resting-place to such as, with great enthusiasm in the pursuit, are not sufficiently versed in etymology to enable them easily to unravel or remember the more refined derivations.

Considering the interesting variety of insects which are indigenous to Great Britain, a very small portion only are noxious; and the real lover of rural life will not often murmur at the moderate degree of labour and attention which is necessary to secure to him an abundant share of the fragrant flowers and delicious fruits of his garden, as well as of the more important and substantial produce of his farm.

As the safest way to ascertain the parents of destructive larvæ is to secure the animal in its infant state and watch its progress to maturity, it should, as often as possible, be resorted to; where, however, there is considerable difficulty in breeding insects, much advantage might be derived by entomologists noting down the prominent characters of the larvæ only; as this registering would enable the students of one neighbourhood to compare such larvæ with those which attack useful fruits and vegetables in other places, and frequently to suggest to each other the best remedies and securities against them.

Thus, I find, at Sale, in the county of Chester, where I reside, that the larva of a phalæna, which is very injurious to the jargonelle pear when grown up to walls, folds itself in the leaves and attaches them to the young fruit, to enable it to devour the fruit in security; and has, in addition to its six proper horny, eight abdominal and two anal spurious feet.*

The larva of a circulio, which, with us, is most destructive to the blossom of the apple, by forming to itself a close canopy of the petals under which it consumes the seed vessels in security, has, besides its proper ones, only two abdominal and two anal spurious feet. These abdominal feet are furnished on their outward edges only with numerous small horny claws, hooked inwards, whilst, on the anal feet, these claws are set all round, excepting at the heel, in the shape of a horse shoe. The use of these claws is obvious; for in walking, larvæ of this description first draw their hinder parts as near

* I often see the sparrows, early in the morning, busily engaged in picking out these grubs, which I consider as some reparation for the attacks which this mischievous bird makes upon our fruit and corn.

The moths, in their perfect state, seldom take wing before twilight, when the swallows, and other fly-catching birds are gone to rest; and, therefore, they are comparatively little kept under by these active and useful strangers. I have generally a good crop of jargonelle pears, which, next to a rich soil and healthy condition of the trees, I attribute to a pair of Bats that annually attend my garden, and usually make their first flights in the evenings of June and July, in front of the wall-trees, clearly for the purpose of intercepting these moths as they spring from the leaves: the gardens attached to more modern houses are seldom frequented by these solitary, night-flying quadrupeds.

as possible to the thorax by one movement, raising their backs in a loop or bridge-like form: they then hold fast by these spurious feet, whilst they advance their fore parts with a similar movement; and, adhering securely by these claws to the leaf or branch, they are enabled to elevate and project their heads in almost any direction; the horse-shoe form, in which the claws are arranged on the anal feet, giving them the power of pressing forward with greater rapidity.

In these examinations of larvæ, I apprehend we shall almost always find that the number and arming of the prolegs or spurious feet, afford regular specific indications, and that they are seldom accidental varieties of any one species; and, therefore, that it is, generally speaking, unsafe to say, that the larvæ of any species of insect have from one number to another number of prolegs or spurious feet; and, considering that the spurious feet are almost invariably furnished with more or less of these horny claws, the term membranaceous, which has been usually given to them in contradistinction to the six proper or horny feet, is a little incorrect.

In counting the spurious feet of hundreds of larvæ of the tenthredo, which we are about to consider, I do not remember ever finding an exception to the number, being twelve ventral and two caudal.

Fortunately, the researches of our more distinguished entomologists are daily unfolding the habits and characters of these little depredators, and pointing out the means of reducing their numbers; thus affording the most useful information to the farmers, as well as gardeners, of our highly favoured country.

With this view, it is particularly desirable, that the study of entomology should be rendered as easy as possible, in some such way as I have ventured to suggest; but, however much we may wish for greater plainness and simplicity, if we consider the wonderful variety in the "appropriate and approximating forms" of the curious little animals with which we have to do, we shall be prepared to expect, that the more obvious characters alone will require an almost endless accumulation of terms to describe them.

Few persons, now alive, can remember a year to be compared with 1826, in respect of the number and variety of insects which were brought to perfection in this kingdom. The more beautiful kinds assumed an unusual brilliancy and lustre, and many fine specimens were collected which are seldom to be met with in the north of England.

It was natural to expect, that a season so productive of the more splendid insects, would also occasion those few kinds which are noxious, to be more than commonly prolific, and the fact, I believe, was so.

Turnips never suffered more from the jumping beetle, (haltica nemorum) than in 1826. In warm showery weather, the growth of this useful vegetable is exceedingly rapid. No sooner are the two seed leaves developed, than the first rough leaf springs out and spreads itself with a luxuriance which bids defiance to this little ravager. On the contrary, if the weather be very dry, the plant remains, for the first day or two, almost stationary, and the fly seizes the opportunity to fix itself upon the two diminutive smooth leaves, boring holes into them in the first instance, apparently to arrest their growth, and afterwards devouring them entirely.

In many parts of Cheshire, turnips are sown after potatoes, and, of course, much later than the usual period of sowing them; but they are still, though, perhaps, not in an equal degree, liable to be attacked by this fly.

In order to ascertain the early existence of the haltica nemorum, in a perfect state, I have sown turnips, in very small patches, the first week in April, when I was well assured there were no others sown in the parish. I found the plants just as keenly beset by these flies as at a later period of the year; and I apprehend this circumstance proves, that this beetle has other food in store, when turnips are not to be found; and also that a good or bad turnip year may, in some degree, depend upon the abundance or scarcity of these other resources, as well as on the weather being suitable for a rapid or slow developement of the leaves of the turnip.

On a future occassion, I hope to lay before the society some facts concerning this insect, which I have not seen noticed in any of our books; at present I must confine myself to the attempt to describe the habits and character of the tenthredo, the larvæ of which are frequently found in almost overwhelming numbers upon the leaves of the gooseberry bush.*

* I apprehend it may be proved, that many families of insects, as well as birds, avail themselves of high winds in their migration and distribution. Farmers and gardeners are well persuaded, that the prevalence of strong easterly gales, in the spring, greatly increases the numbers of destructive flies. That these nipping winds are very injurious to the health of trees and plants, is clear;

This insect, as might naturally be expected, was very injurious in the year 1826.

Considering the gooseberry as one of the most valuable of our garden-fruits, it is remarkable, that more attention has not been paid to the insects which infest the tree: practical gardeners know very little, and entomologists of eminence have differed much, respecting them.

The sesia tipuliformis of Fabricius, was formerly considered as the parent of these grubs.

In the folio edition of Merian, published at Amsterdam, in 1730, chap. 25, there is an engraving of a phalæna, and also one of a tenthredo, in the same plate, both of which are stated to be destructive to the gooseberry. In the first volume of the celebrated work of Kirby and Spence, page 195, the larvæ of the phalæna grossulariata are said to have been very destructive to the foliage of the gooseberry.

At Sale, I have never found these larvæ with less than twelve spurious feet, and, therefore,

and, I apprehend, it is equally evident, that diseased vegetables, like unhealthy animals, are most liable to the attacks of their respective parasites.

referring to the classification of Bergman, I have always concluded, as the fact has turned out in breeding these insects in boxes, that a tenthredo, and not a phalæna was the parent of our caterpillar.

The Tenthredines are of the order Hymenoptera, which forms the fifth of the Linnean, and the thirteenth of the modern, system; and belong to the class Piezata of Fabricius, whose arrangement, founded on the instrumenta cibaria chiefly, is more useful when we are considering the voracious perfect insects, than when attending to those, which, as in the case before us, are destructive in a larva state only.

The characters of the Tenthredinidæ of Dr. Leach and Latreille are—Abdomen sessile, or attached to the thorax in its whole length, oviduct composed of two lamellæ, which are serrated, mandibles more or less long, terminated by two strong teeth: wings with the marginal cells complete: labrum distinct: larva with membranaceous feet.

Following Dr. Leach, the insect we are considering may then be thus described.

Order,—Hymenoptera.

Section,—Terebrantia.

Division,—First.

Genus,—Tenthredo.

Species,—Tenthredo.*———?

Superior wings, with but one marginal cell: head, antennæ, thorax and wings, brown and shining: body yellow and shorter than the wings: the female larger than the male, and the body of a brighter yellow; the antennæ with seven articulations, decreasing in size towards the apex; and, as well as the body and wings, slightly ciliated.

Following the classification of Bergman, the larvæ may be thus described.

Head,—horny, with projecting teeth.

Feet,—six proper horny, twelve ventral, and two caudal membranaceous; metamorphose into nymphæ, which give birth to tenthredines.

I apprehend, it will be found, that, in warm weather, the larvæ are generally hatched on the third day after the eggs are deposited. They feed together for the first day, but by the time they have consumed the leaf on which they were born, they will have attained a sufficient size to detach themselves singly on the work of destruc-

* Grossulariæ folii?
Rebesii? Vide Scopoli Entomologia Carniolica, p. 734.

tion. In about ten days, they come to their full growth; about half the time being of a light green colour, and afterwards gradually growing darker, and variegated by shining black tubercles, with black bristles proceeding from them.*

When at maturity, they become lethargic, and cease eating, preparatory to stripping themselves of their outer skin, which they leave attached to the foliage or branches of the tree, in the form of a dry brown-looking slough.

After this change, the larva assumes a bright yellow colour; and, as it is seldom seen by gardeners in this state, I believe the stripping generally takes place in the night, during which period the insect also betakes itself to the ground, where it undergoes its other changes, and becomes a perfect fly.

At Sale, the first broods of larvæ make their appearance, usually, about the last week in April, or the first week in May, as the season is early or late; and about the 7th or 10th of July, the second brood will be seen at work, preferring the young and tender midsummer shoots to the

* See Memoires pour servir a L'Histoire des Insectes, par Reaumur, Tom 5, part 1, p. 119 and 120.

then tougher leaves of spring. By the 20th or 25th of that month, this family will also have disappeared, leaving the same unsightly memorials of their voracity. If the grubs be suffered to remain unmolested on the branches, the fruit will not only be rendered tasteless, but the tree will be injured for years to come. The last hatches hibernate in the ground, and I have ascertained that frequent digging about the trees, especially in the winter and spring, has much reduced their numbers.

When I have placed the larvæ in boxes, with leaves of the tree to feed upon, I have always found, that, on stripping off their green clothing, for want of earth to retire into, they have attached themselves to unconsumed leaves, or to the sides or bottoms of the boxes, in a gummy envelopement of silk, which they have spun round themselves for the purpose of assuming the nympha state; and, by placing them in warm rooms, I have been able to produce them as perfect tenthredines, even during the winter months.

The parent fly may be found in our gardens on sunny days, in May and June, and occasionally in July, making short solitary flights near the gooseberry bushes, or creeping underneath the leaves; and is best met with about ten o'clock in the morning. The antennæ have a constant tremulous motion up and down, which is the case with the whole family of tenthredinidæ. On turning up the leaves of the gooseberrry bushes at these periods, we shall find that many of them have the ribs studded over with transparent ova, which swell out gradually until the little animal makes it escape; whilst larvæ, of various sizes, are consuming other parts of the foliage.

In this stage, finely powdered lime, soot, or dry dust of almost any kind, scattered upon the leaves, and especially immediately after rain, will stop the grub from eating, and render it feeble. If the bushes be then well shaken, the insects may be gathered in large quantities from the ground underneath; and, perhaps, this is the speediest way of reducing their numbers when they have been previously neglected; but the fruit and the tree will be rendered very unsightly by this process. Such persons as attach proper importance to neatness in their garden have no remedy so effective as the tedious one of picking the larvæ carefully from the leaves, when they make their first appearance, to prevent their retiring into the ground, to undergo their changes, and produce a far more numerous progeny.

When we consider the thousands of these

grubs which, in ordinary seasons, may be gathered in a small garden, it is remarkable, that it would be a difficult task to find ten perfect flies in any one day. How they are destroyed, I have not been able to ascertain. I have never observed that any of the nimble little fly-catching birds feed upon them. With me, poultry of all kinds refuse eating them; and even young partridges and pheasants, which, till they are half grown, feed chiefly upon insects, can seldom be induced, however hungry, to taste them.

I have very recently been informed, by two respectable persons in my neighbourhood, that the cuckoo sometimes clears a garden in a very short time; and I have also lately learnt that salt dug into the ground near the roots, in spring, has been used with success.*

* In Reaumur, tom. 5, part. 1, there is an engraving of an insect of this family hibernating within the pithy part of the midsummer shoot of the gooseberry bush. If this was the usual resting place of these animals, digging round the roots of the plant would not disturb them. Apprehending that the difficulties attending the selection of such a hiding-place would at once account for the very few perfect tenthredines we meet with, I have caused a very extensive and careful examination to be made with a view to corroborate this accurate entomologist, but hitherto without success; and, after considerable attention paid to the habits of these insects, when I have bred them in boxes, I have still little doubt that their general winter-quarters are in the ground.

ACCOUNT OF ANTHELIA,

OBSERVED in ASCENDING SNOWDON, on the 17th of May, 1826.

BY JOHN BLACKWALL, F. L. S.

(Read October 5th, 1827.)

NOTWITHSTANDING the production of anthelia and halos has engaged the attention of many eminent philosophers, it is pretty generally admitted, that no satisfactory theory of these singular phenomena has hitherto been promulgated. The most prevalent opinion, relative to their formation, appears to be, that they are occasioned by the refraction and reflection of light, by minute particles of ice floating in the atmosphere; but, although Descartes, Huygens, Newton, Marriotte, Young, and other distinguished individuals have either supported this doctrine by their writings, or have sanctioned it by their approval, the facts I am about to relate plainly demonstrate, that water, in a state of congelation, is not at all essential to the production of these meteors.

On the 17th of May, 1826, I ascended Snowdon, the loftiest mountain of the Caernarvonshire chain, from Nant Colwyn, in company with my brother, Mr. Thomas Blackwall; and, in passing over Clawdd Goch, or the red ridge, which leads directly to the summit of the Wyddfa, the highest of its peaks, we perceived our shadows, extremely well defined, on a white cloud, at a short distance from us, to the east. The head of each shadow was encompassed by a glory, or broad disk of yellowish white light, which formed the centre of a small white halo, whose diameter did not exceed five degrees. Both shadows were distinctly visible to each individual, but it was otherwise with the luminous appearances which accompanied them; the glory and halo with which the shadow of my brother's head was encircled being seen by him alone, while those surrounding the shadow of my own head, though very conspicuous to myself, were not perceptible to him.

As we stood admiring this unusual and amusing spectacle, we were suddenly enveloped in a dense cloud, brought over the ridge by a gentle breeze from the west: fortunately, I had a couple of thermometers with me, one attached to a mountain barometer, and the other detached; both of which, on being exposed to the vapour composing the cloud, indicated the same degree of temperature, namely 47°. Now, it is evident, that this vapour could not possibly exist in a state of concretion at a temperature elevated 15° above the freezing point, and I may add, that it did not exhibit any symptom of congelation whatever; yet, when it had passed to a short distance from us to the east, our shadows, with their attendant anthelia and halos, were displayed upon it with remarkable distinctness. The hour at which these observations were made was about 6, P. M., and the sun was rapidly declining towards the western horizon. It is deserving of notice, that, more than an hour afterwards, the temperature of the atmosphere, at the summit of the mountain, was as high as 45°.

From these facts we may safely conclude, that congealed vapour is by no means an indispensable requisite in the formation of anthelia and halos; all hypotheses, therefore, founded on this supposition, must be abandoned or modified.

Since the above was written, my friend, Mr. Peter Barrow, has directed my attention to a brief notice of a memoir by M. Ramond, on the

Meteorology of the Pic du Midi, given in Dr. Brewster's Edinburgh Journal of Science, No. IX. p. 180; in which it is stated, that "when M. Ramond was on the Pic du Midi, he observed his own shadow, and those of his two companions, projected on a cloud, situated a little distance above them, with a distinctness, and an accuracy of outline, quite surprising; but what was more astonishing, these shadows were encircled with glories, shining with the most brilliant colours."

"M. Ramond considers it as certain, that the cloud on which his shadow was projected could not, from the temperature of the Pic, have then held any icy particles in suspension."—It does not appear, however, that he ascertained the temperature of the cloud itself; indeed, as his inference, that it did not contain any frozen particles, seems to have been deduced solely from the temperature of the Pic, it may be presumed, that he did not determine it experimentally; but, as I have had no opportunity of consulting his essay, I cannot positively affirm that this was the case.

There exists a possibility, therefore, that, in the demonstration of this interesting fact, I may have been anticipated; still it is one of so much importance, especially in a theoretic point of view, that, whoever may be entitled to the claim of having established it, the foregoing particulars, which, at least, serve to corroborate its accuracy, will not, it is hoped, be deemed altogether superfluous.

ACCOUNT

OF A

WHITE LUNAR RAINBOW,

Seen at Manchester, on the 14th of January, 1827.

BY JOHN BLACKWALL, F. L. S.

(Read October 5th, 1827.)

ON the evening of the 14th of January, 1827, I had an opportunity of witnessing a white lunar rainbow, a meteor of such rare occurrence, that I have been induced to draw up a succinct account of the principal circumstances under which it was seen. In the early part of the day, there was a violent gale from the west, with a little rain; the temperature of the air, as indicated by a thermometer in the shade, being 50°: the barometer was going down at the time, and, at 11h. 30m. A. M., had sunk to 28.78 inches, the mean height of the instrument, in the situation it then occupied, being 29.54 inches, on an average of six years' observations. Towards noon, the wind changed to the north-west, from which quarter it continued to blow with considerable, though decreasing force, through the remainder of the day, accompanied with occasional storms of snow and hail. The mercury in the thermometer descended gradually to 34°, where it continued stationary during the greater part of the evening; and the barometer, as might be expected, rose rapidly.

About 8h. 40m. P.M., my attention was arrested by a remarkably well defined arch of white light, beautifully displayed on a dark cloud extending from the north to the south-west: the moon, which shone brightly, was nearly 39 hours past the full, and pretty high above the eastern horizon.

The phenomenon resembled a solar iris, in every particular, except the absence of prismatic colours: this dissimilarity, however, must not be regarded as a distinguishing characteristic; for lunar rainbows generally are coloured; and a description is given in the New Series of the Manchester Memoirs, vol. III. p. 176, et seq., of an achromatic solar bow.

How long the arch was visible, I am unable to state, as I did not see its commencement; but the interval comprised between the time at which it was first perceived, and the period of its final disappearance, did not exceed five minutes. A part of the cloud on which it had been exhibited was speedily brought overhead by the high wind, and discharged a large quantity of hail, with a little snow intermixed. The hail was quite opaque; but whether the phenomenon is to be attributed principally to this circumstance or not, I leave for those, whose knowledge of optics is more profound than my own, to determine.

It would be interesting to inquire into the atmospherical peculiarities which have attended lunar and solar bows, devoid of colour, observed on former occasions.

REMARKS

ON THE

TWINKLING OF THE STARS.

BY JOHN BLACKWALL, F.L.S.

(Read October 5th, 1827.)

So numerous and contradictory are the hypotheses in which men of science have indulged, relative to the cause of the twinkling of the stars, that hasty and superficial inquirers are frequently induced to regard them merely as the fanciful conjectures of speculative theorists; but this conclusion, if correct in particular instances, will not, I am persuaded, be found so in all.

The ingenious Robert Hooke, so long since as the year 1665, attributed the phenomenon of twinkling to the unequal and inconstant refraction of the rays of light, occasioned by the trembling motion of the air and interspersed vapours, in consequence of variable degrees of heat and cold in the air, producing corresponding variations in its rarity or density;* and this opinion appears to be well founded; several facts, directly tending to corroborate it, having fallen under my own observation, when engaged in meteorological pursuits.

A moderate share of attention to the subject is sufficient to convince every thinking man, that the tremulous motion, so evident in the light of the stars, must depend upon mutations which take place in the atmosphere immediately surrounding our globe; for it is allowed to be much more conspicuous at some periods than at others, and in stars elevated from ten to fifty degrees above the horizon than in those nearer the zenith: the planets also, particularly Venus and Jupiter, when their altitude does not exceed fifteen or twenty degrees, are sometimes seen to twinkle very perceptibly. It is acknowledged likewise, that, under similar circumstances, stars of the first magnitude, as Sirius, Arcturus, &c., always twinkle more than smaller ones; stars whose light is white, than those whose light is red; and that changes in the colour and intensity of their light, frequently accompany this phenomenon.

* Micrographia, p. 231i et seq.

Now, these well-known facts do not admit of a satisfactory explanation on any principle with which I am acquainted, but that insisted on by Mr. Hooke, namely, a perpetually varying refractive power in the medium through which the rays of light pass; and this principle, in consequence of the unequal distribution of heat, and its constant tendency towards a more uniform diffusion, we know to be ever active in the earth's atmosphere.

What powerfully contributes to establish the accuracy of this opinion, is the effect produced by changes in the direction of the wind, particularly if attended with a considerable increase or diminution of temperature. On such occasions, and on the near approach of clouds, especially of that modification denominated cirro-stratus in the nomenclature of Mr. Luke Howard, I have remarked, that the stars generally twinkle much; but that this vibratory motion of their light is usually greatly reduced when the wind has continued to blow long and steadily from the same point of the compass.

That fluctuations, in the direction of the aërial currents, facilitate the intermixture of warm and cold air, and contribute materially to promote the condensation of vapour into clouds, in the formation of which a large quantity of caloric is evolved, cannot be questioned; and it is a fact, familiar to almost every one, that the rarefied air which, in serene, sunny weather in summer, ascends from the heated surface of the ground, and mingles with higher strata of inferior warmth and tenuity, gives to objects viewed through it, a tremulous appearance, somewhat analogous to that of twinkling; indeed, the planets, whatever their altitude may be, when seen through the hot air which issues from the chimney of a furnace, do positively twinkle very visibly: we may, therefore, reasonably infer, as like results necessarily ensue from the operation of the same causes, that the twinkling of the stars, agreeably to the laws of dioptrics, is occasioned by the variable refraction produced by the blending together of volumes of air of different temperatures and densities.

Regarded, almost universally, as utterly inapplicable to any purpose of practical utility, the twinkling of the stars attracts little attention, except as a matter of mere curiosity; yet, to the intelligent meteorologist, it sometimes affords the earliest indication of changes, which are constantly occurring in regions of the atmosphere to which he has no immediate access; and it enables the attentive astronomer to ascertain, with tolerable certainty, the most favourable opportunities for making telescopic observations. These circumstances, if the subject had no other recommendation, are alone sufficient to entitle it to some consideration; but, as an interesting inquiry in physical science, the cause of this phenomenon is certainly deserving of minute investigation.

OBSERVATIONS,

CHIEFLY CHEMICAL,

ON THE

NATURE OF THE ROCK STRATA,

IN

Manchester and its Vicinity.

BY JOHN DALTON, F.R.S., &c.

(Read December 28th, 1827.)

HAVING lately had occasion to examine some specimens of the rocks in the neighbourhood of Manchester, and some of the mineral and fossil productions, as far as regards their chemical constitution, I thought it might be, at least, of some local interest, to make known the results.

1. *Collyhurst Sand-Stone.*

The colour of the rock at the top, is of a *pink hue*; but, at the bottom, or at 30 or 40 feet below the surface, it becomes a *yellow drab*. The specific gravity of the bottom rock, I found to be 2.58 (water being 1). That of the middle rock, was 2.476; and that of the top rock was 2.256 only. The two last specific gravities, however, are not correct; for the rock is so porous, that it imbibes a considerable quantity of water, by being immersed in that liquid for one or two minutes. I found, that 924 grains of the middle rock, when put into water, and thus submitted to the vacuum of an air-pump, for ten minutes, acquired 29 grains in weight: it was then dried in a heat of 100°, when it lost 37 grains. Afterwards, being immersed in water for two minutes, in order to find its specific gravity, it thereby gained 27 grains in weight. Hence, it varied 4 per cent. in weight; and this variation might, no doubt, have been increased. An expert geologist will, perhaps, account for the different specific gravities of the rock at top and bottom, by ascribing it to pressure; but another, with greater plausibility, may maintain, it is owing to the percolation of water through the upper rock, carrying the finer particles down into the pores of the under rock, where they become fixed. Chemical analysis is in favour of this last notion. The bottom rock, I find, contains 4 or 5 per cent. of *carbonate of lime;* the top contains none. The chief constituent of both is sand, or siliceous earth, combined, probably, with a little alumine. The bottom rock, boiled in muriatic acid, exhibits carbonate of lime, and a trace of iron; the top rock, treated in like manner, loses only 1 or 2 per cent. in weight, and the acid extracts only a trace of iron and of alumine. The colour of the residuary sand, in both cases, is not materially altered; which makes it probable, that the sand is ***combined*** with the alumine.

The bottom rock is ***harder***, and better cemented together, than the top, as might be expected from what has already been stated; it must, I presume, be much better adapted for buildings, than the latter, which, when imbued with water, is softened so as to fall almost by its own weight.

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2. *Sand-Rock, at Mr. Knowles's Coal-Pit, near Agecroft Bridge.*

This sand-rock is found 15 yards below the surface, and is many yards thick. Its specific gravity is 2.47; it acquired 2 or 3 grains per cent. by being immersed in water. It agrees, very nearly, in its constitution, with the upper part of the Collyhurst rock; it has some horizontal *streaks* in the fracture, which are said to be characteristic of vicinity to coal.

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3. *Another Sand-Rock, at same place, but 39 yards below the surface, and below the uppermost Bed of Coal.*

The specific gravity of this is 2.6; it is closer grained, and does not imbibe water readily. It is more remarkably streaked than the upper rock; it effervesces a little with muriatic acid, and yields some iron oxide. In chemical constitution, it agrees nearly with the bottom rock at Collyhurst.

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4. *Red Sand-Rock.*

This kind of rock, I believe, abounds in this district. I shall not attempt to decide whether the samples I have examined are from what geologists call *old* or *new* red sand-stone, but merely state where they were obtained, and what characters they possess.

The specimens were from,

1. Red Rock, opposite the New Bayley.
2. Red Sand-Rock, from the Tunnel, Garrett.
3. Red Sand-Rock, Tinker's Garden.
4. Bank Bridge Rock.
5. Holt Town Rock.
6. Sand-Rock, near Agecroft Bridge.
7. Red Sand, in a granular state, from various other places.

These rocks differ a little in colour; but in other respects they might all be from the same place, though they are probably separated by intervening strata, as well as *distance*, horizontally. They consist of particles of sand weakly agglutinated, and coloured with an indefinitely fine coating of oxide of iron. They are easily reduced into sand, which, being heated to redness, loses 1 or 2 per cent. of weight from the inherent moisture. After this, if the residue be treated with dilute, pure, colourless muriatic acid, about 1 per cent. of oxide of iron is abstracted, and there remains a fine *white* sand, shewing, that the iron only coats the surface of the sand, though it gives it the appearance of a body of the red oxide of that metal.

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5. *Ardwick Limestone.*
2.71 Specific Gravity.*

A bed of limestone is found, at a considerable depth under the ground, to the east of the town.

* The specific gravity of pure white marble, and all or most of the compact mountain limestones, is nearly 2.7.—The specific gravity of the true magnesian limestone, from Yorkshire, is usually much less; being only about 2.1 or 2.2, agreeing with that of soft chalk nearly: the hard chalk of Lincolnshire, I find nearly 2.6 specific gravity. All these kinds, of low specific gravity, however, it must be observed, are porous, and imbibe less or more of water.

It has some peculiar properties, as that of forming a good cement under water. Geologists call it ***Magnesian Limestone,*** because it is found in a certain position, relatively to other strata. In appearance, it agrees pretty nearly with one of 24 specimens of mountain limestone (so called) which I have from a district of 20 miles circuit, in Cumberland; but it has very little resemblance to the true magnesian limestone from Yorkshire. When analysed, it appears to be *pure carbonate of lime,* united or mixed with two per cent. of clay, and a considerable trace of iron, perhaps half of a grain per cent. It is probably owing to the two last ingredients, that its peculiar qualities are to be ascribed. It contains no more magnesia than chalk and mountain limestone in general do, which is a slight trace. Whereas, the true magnesian limestone, found in some parts of Yorkshire, and, perhaps, in other counties, contains the two elements of lime and magnesia, united, nearly or accurately, in atomic proportion, or the lime to the magnesia as 24 : 17. This is the case with the two specimens now presented to the society. Their specific gravity, too, is generally below that of mountain limestone.

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6. *Tinker's-Brow Limestone.*

2.55 Specific Gravity.

There is some limestone found at Tinker's brow, which may be supposed an edge of the Ardwick stratum. The analysis of this specimen shews it to be a compound or mixture of carbonate of lime and clay. The two earths of lime and clay, are nearly in atomic proportions; but this may be merely accidental. The lime only is combined with carbonic acid. It contains rather more iron than the Ardwick stone. Exclusive of the iron, I found the compound to be nearly

36	clay,
36	lime,
28	carbonic acid.
100	

7. *Yellow Clay Stratum.*

A portion of a stratum of yellow clay, from Broughton-grove, was treated with dilute pure muriatic acid, and heated to boiling. The acid extracted 4 or 5 per cent. of oxide of iron; but the residue still retained some colour, though it was evidently whitened. This sort of clay, therefore, is rather abundant in iron, and might, in certain circumstances, become clay iron stone.

ON THE

INJURY

DONE TO THE

FOLIAGE OF THE OAKS,

In the NEIGHBOURHOOD OF MANCHESTER, in the SPRING of 1827.

BY JOHN BLACKWALL, F. L. S.

(Read January 11th, 1828.)

INSECTS, though diminutive in size, and insignificant in appearance, when associated together, in large numbers, frequently become exceedingly formidable and destructive. A striking illustration of this fact is supplied by the appalling devastation which is sometimes occasioned by extensive bodies of locusts; a circumstance thus emphatically described in the bold figurative language of the prophet Joel, ii. 2—6. "A day of darkness and of gloominess; a day of clouds and of thick darkness; as the morning spread upon the mountains: a great people and a strong; there hath not been ever the like, neither shall be any more after it, even to the years of many generations. A fire devoureth before them; and behind them a flame burneth: the land is as the garden of Eden before them; and behind them a desolate wilderness; yea, and nothing shall escape them. The appearance of them is as the appearance of horses; and as horsemen, so shall they run. Like the noise of chariots, on the tops of mountains, shall they leap, like the noise of a flame of fire that devoureth the stubble, as a strong people set in battle-array. Before their face the people shall be much pained: all faces shall gather blackness." From this dreadful scourge, and from other plagues of a similar, though less distressing, character, the inhabitants of the British Isles are, fortunately, in a great measure, exempt. Still they do occasionally experience much inconvenience, both as regards their persons and property, from noxious animals of this class. A multitude of examples, confirming the truth of this remark, might easily be adduced; but as lengthy details, relative to a matter of such general notoriety, would, in all probability, be deemed superfluous, I shall, in the present instance, limit my observations to a case of recent occurrence; in which the oaks, in the vicinity of Manchester, were nearly stripped of their foliage, by two minute species of insects.

Early in May, 1827, the green weevil, Curculio argentatus, appeared in unusual numbers in this neighbourhood; and, by its extensive ravages, greatly disfigured many of our most ornamental trees and shrubs; the copper-beech in particular, in some situations, suffered severely. Towards the termination of the month, this indiscriminate feeder attacked the young leaves of the oak, which were then expanding, and the effects of its depredations soon became very conspicuous in the gnawed and withered foliage.

To this pest quickly succeeded another, the larva of a small moth, Tortrix viridana, which completed the devastation commenced by the green weevil; and the monarch of the grove, nearly destitute of verdure, and loathsome with numerous caterpillars, stood almost leafless, wearing a wintry aspect, even in the middle of June. These caterpillars, in common with many others provided with an apparatus for spinning, on being disturbed, hastily quit their retreats among the convoluted leaves, and descend towards the earth by a fine line, formed of a viscous secretion, which hardens on exposure to the atmosphere. So extremely abundant were they at the period alluded to, that, during a brisk wind, thousands might be seen thus suspended; some carried out by the breeze far beyond the widest spreading branches of the tree to which their threads were attached; others, with violent contortions, slowly ascending their silken filaments; and all, as they were wafted to and fro, fantastically dancing in the agitated air, without any visible support; their lines being too attenuated to be discerned by the unassisted eye; except when they occasionally reflected, with a silvery lustre, the vivid light of the unclouded sun. The spectacle, as may be supposed, was at once highly singular and interesting.

During the continuation of these insects in the larva state, various species of the feathered tribes feasted upon them luxuriously. The willow-wrens, white-throats, and, indeed, the warblers generally, were among the most vigilant and destructive of their enemies, and must have reduced their numbers greatly. The finches also, particularly the chaffinch and house-sparrow, were indefatigable in quest of them; and even the domestic poultry sought with avidity for those which, by design or accident, descended to the ground.

In the month of June, they underwent their second change, or were converted into chrysalides; and in this almost inactive stage of existence, in which several of the animal functions are suspended, and others are only imperfectly exercised, they displayed an instinct deserving particular notice. Concealed within the cavities which they had formed when caterpillars, by folding down the edges of the leaves, and securing them in that position with a little of the glutinous secretion discharged by the spinners, they awaited their final transformation; - but, as if aware that so confined a situation would present too many obstacles for a delicate and newly disclosed moth to overcome, without incurring a great risk of sustaining injury, at the important crisis, they made their way to the mouths of their retreats, and protruding themselves as far as they could consistently with security, their exterior covering ultimately gave way, and, in July, the insects made their appearance in the imago or perfect state.

Having procured some of the larvæ of this moth, for the purpose of observing the metamorphoses they undergo, and identifying their species, I put them into clean phials of transparent glass, the perpendicular sides of which they readily ascended by means of lines of their own spinning, after the manner of the caterpillar of the goat moth.* This circumstance induced me

* Mr. Curtis, in his British Entomology, vol. II. plate 60, has given an excellent figure of this caterpillar, representing it in the act of climbing.

to try the experiment with the larvæ of other insects. Capturing, indiscriminately, such as came in my way, I soon collected a considerable number; and, on introducing them into the phials, found that several of them made their way up the glass without any apparent difficulty, while others were totally incapable of doing so. These ascents, in many instances, were effected by spinning lines, which were made to answer the purpose of a ladder, as noticed above; in some, by the assistance of a slimy or glutinous secretion which left a sensible trace on the glass; and in others, by a method which I cannot satisfactorily explain; the caterpillars, in this case, neither spinning lines, nor leaving any perceptible trace behind them. At first, I was disposed to think, that their spurious legs, or prolegs, (propedes,) as they are denominated by Messrs. Kirby and Spence, in their Introduction to Entomology, acted as suckers; and that they were held to the sides of the phials by atmospherical pressure. It soon occurred to me, that the accuracy or inaccuracy of this supposition might be ascertained by means of the air-pump. Under this impression, I applied to Mr. Dalton, who was so obliging as to allow me the use of his instrument, and to lend me his assistance in conducting the experiment. The result, however, proved the reverse of what I had anticipated;

for, notwithstanding the pressure was very greatly reduced, the caterpillars were still capable of ascending the phial in which they were enclosed: it is probable, therefore, that some adhesive matter, which, perhaps, is not liable to leave a stain upon glass, may be secreted, in small quantities, by the spurious legs of these larvæ; and that they are thus, in opposition to the attraction of gravitation, enabled to climb up the vertical sides of bodies with smooth and even highly polished surfaces. A minute examination of the structure of the false legs, under a powerful microscope, might possibly throw some light on this curious subject, which, it must be acknowledged, merits further investigation.

I hope, at some future period, to have an opportunity of resuming my researches respecting this singular property of the prolegs of several species of larvæ. In the mean time, should the foregoing imperfect attempt to solve this difficult physiological problem, by directing the attention of naturalists to the inquiry, induce a single individual to engage in the undertaking, it will not have been made in vain.

The injury sustained by the oaks on this occasion, was not limited to those which grow in this particular district. I am well informed, that in other parts of the county, and in Yorkshire, Cheshire, Derbyshire, Shropshire, Middlesex, &c., many were similarly affected; and it is probable, that the mischief extended much further. The damage done to the first leaves was, in a considerable degree, repaired by the developement of a second set, about the close of June, and the beginning of July, the lively tints of which gave to our oak woods, at that season of the year, the appearance of spring; but the bloom, as well as the early foliage, having been nearly destroyed, the crop of acorns, which had promised to be unusually abundant, proved remarkably defective.

Various were the opinions entertained as to the cause of this blight, as it was generally termed; it being severally ascribed to disease; to lightning; to the cold winds which prevailed in the spring of the year; and to the ravages of insects. The last conjecture happens to be correct; but few persons gave themselves the trouble to establish its accuracy by actual observation, and still fewer endeavoured to determine the species of these depredators. Their vast multitudes may, with much plausibility, be attributed to the high temperature of the preceding year, 1826, having been extremely favourable to their increase; for, in the same season, many other insects were also very numerous; especially the various species of Aphis, and their natural destroyers the Coccinellæ. Among the latter, C. 7-punctata;—C. 4-pustulata; and C. 2-punctata; greatly predominated. The two last are considered to be distinct, and, accordingly, have had different specific names assigned to them, by entomological writers; but that excellent botanist, and attentive observer of the economy of insects, Mr. Edward Hobson, of Manchester, assures me, that they are opposite sexes of the same species; C. 2-punctata being the male, and C. 4-pustulata the female. Some observations of my own, made since I have been in possession of Mr. Hobson's communication, had disposed me to regard C. 4-pustulata as the male, and C. 2-punctata as the female; but I am now convinced, that the colours of the sexes are liable to vary.

NOTE.

Through the kindness of my friend Mr. Peter Barrow, I have been favoured with a sight of the fifty-second number of Mr. Curtis's work on British Entomology, which has been published since the above paper was read before the society.

In treating upon Coccinella Ocellata, the author observes, that the genus Coccinella "is at once a remarkable example of the value of structure in the combination of groups, and of the little importance of the distribution of colour, when employed to distinguish species. As a genus, Coccinella is so natural, that its appellation has never been disturbed; whereas, the species composing it are so variable, that many of them have been described under a great variety of names." Mr. Curtis, without alluding to sexual distinctions, brings together the following synonimes, under the specific name dispar. "Pantherina and annulata Linn. Don. 7.243.2.—Ci-punctata and 6-pustulata Linn. Don. 2.39.3.—unifascia and 4-pustulata Fab. Don. 7.243.3.—perforata and 7-pustulata Mar.—4-punctata Don. 16.542." Recent researches have induced Mr. Hobson to coincide with me in the opinion, that the distribution of colour affords no criterion which will serve to distinguish the sexes of C. dispar.

NOTICES,

CHIEFLY

BOTANICAL,

RESPECTING THE NATURAL HISTORY OF LLANDUDNO PARISH, CAERNARVONSHIRE.

BY WILLIAM THOMSON, A.M.

(Read April 6th, 1827.)

"Facile est inventis addere."——BAUHINUS.

SOME apology may be deemed requisite for occupying the time of this Society with a subject not much connected with science,—which has not even the recommendation of aiding the establishment of any theory of vegetable action, production, or arrangement, nor the still humbler praise of adding to the list of domestic conveniences, to the enlivening of our parterres, or the improvement of our pastures.

The student of our Native Flora walks in a beaten track. The labours of his predecessors, in every branch of the study, have made the most minute and concealed productions of the vegetable world as well known to him, as the gay and conspicuous ornaments of the flower-border and the conservatory. So little room, indeed, is there for discovery, that, in despair of immortalizing themselves by the useful or the elegant in the objects of their search, even the most respectable modern writers on botany have not disdained to assign a barbarous Latin version of their names, to the most simple and insignificant moss.

Amidst this want of novelty in the advanced study of plants, and the paucity of philosophical induction which it affords, there is still one inducement to its continuance in the perpetual vicissitudes of the vegetable kingdom, by which it almost atones for the absence of fresh discoveries. Plants bud, blossom, and decay with such pleasing gradations of order and beauty, that they present a separate charm and lesson in each stage of their existence. And, unlike all other living things, which but once exhibit the phenomena of dissolution, these annually retire into a mysterious and incomprehensible torpidity that they may renew, continually, the pleasure and instruction they have already afforded.

It is this peculiarity that renders the study of vegetable life perennial, alluring its votaries periodically to the same haunts, with a freshness of interest derived from the simple renovation of the same colours and forms. This also gives a value to every local Flora;—for, however little it may contribute to the stock of acquired knowledge, by extending the means and sphere of observation, it seems to enlarge the dominions of nature.

The object of the following botanical notices, therefore, is merely to exhibit the productive nature of one among the many districts in our island, still untouched by the hand of agriculture. And if, in adding one page to the labours of topographers, I shall have pointed out a new or little-trodden field, for the frequent gratification of taste and quiet contemplation, this paper will not be wholly useless.

The classical author of the "Natural History of Selborne," has afforded a cheering example of patient research and careful observation, by bringing together as many curious and valuable facts in Natural History from the annals of one little district, as most men have been able to produce from the records of whole kingdoms. He has left us, likewise, his testimony to the value of individual examination. In that invaluable work, which may justly be said to have given a more popular, as well as a more philosophical shape to the studies it recommends, than they ever before possessed, he assures us, from personal experience, that every spot of ground, in its natural state, has individual beauties of the vegetable kind, which are fit to occupy considerable attention;—and that those seem to have been productive of the greatest number of species, which have been most explored.

The thought is not new with naturalists. It scarce reaches the extent of a favourite speculation indulged by the interesting but fantastical St. Pierre. With him, the vegetable world had scarcely any limits. For, as he asserts, "if the surface of this globe were examined, each square mile of territory would be found to contain, besides many varieties, some one species peculiar to itself."

This assertion carries the encouragement to exertion too far; and, while it bears the aspect of romance, has a still greater objection to oppose it—that it would swell the catalogue of our Flora to the extent of sixty millions of species, far beyond the adjustment of the most ingenious and diligent classifier.

Yet, fanciful as this hypothesis may be, it may, without hesitation, be admitted, that many of the most valuable additions to our Floras have arisen from this belief in the inexhaustible variety of nature; and that some latent expectation, springing from the same source, still animates the almost daily researches of a Hooker and a Smith.

With a similar taste, and desire for discovery, "*sed haud passibus aequis*," I spent a part of July and August, last year, in examining the vegetable productions of Snowdonia; and found no part of that romantic district so luxuriant in scarce or common plants, as the parish of Llandudno, in Caernarvonshire.

This little retired corner of the county, once, as tradition tells us, possessed of many well cultivated acres now occupied by the Irish Sea, is composed of a narrow tract of marshy meadow land, bounded on the E. and W. points by the promontories of Pen Bach and Pen Mawr, (the greater and the lesser Orme's-Head,) and skirted on the S. by Bodscallan and Gloddaeth, the well-wooded estates of Sir Thomas Mostyn, which run down on the southern side to the river Conway, opposite the town and castle. The territory lying between the fine bay of Llandudno and the Conway seems to suffer annual encroachments from the waters on both sides; and, at some period not distant, may, like the old northern part of the parish, yield to the dominion of the ocean; when the greater Orme's-Head may become an island. The greatest extent of the parish is not more than three miles; its least, one; the soil is chiefly formed of sand or crumbled limestone and decayed vegetable matter.

The village of Llandudno, situated on the larger rock (which, as I have been told, bears no small resemblance to the fortress of Gibraltar,) contains a school and dissenting chapel, together with 200 inhabitants, whose subsistence is derived entirely from the rich copper mines near the village. The church stands about a mile from the village, on the brink of a precipice, overlooking the sea, although it is said to have been built originally in the middle of the parish.

From want of a resident clergyman, (although the living is understood to be valuable,) much ignorance and immorality prevails, and more than the usual share of selfishness, too prevalent among the lower orders in North Wales. The natives seem destitute of the information and habits of inquiry which are generally met with among artisans in large towns, without having retained the simplicity and innocence more frequently the characteristics of the country. Labour, of the severest kind, in a noxious atmosphere, alternating with hours of sluggish idleness or sottish dissipation, is not the most favourable soil for the growth of virtue or the useful sciences. But miners are not shut out from the Elysian fields of knowledge, beyond all other men who are destined to a life of labour. A happy example of the contrary is afforded in a similarly situated district in Scotland. The miners of Leadhills, like those of Llandudno, were once a poor, dissipated, and ignorant colony. Allan Ramsay, the poet, established among them a little library, as a memorial of affection for his native village;—and a moral change commenced. The library was increased, and is now considerable; the miners became scientific in their own and other pursuits; and a well-informed, moral, and happy community, has ever since tenanted that dreary district.

The contiguity of this parish to the island of Anglesea, and its retired position, have probably made it a retreat of the Druids, in the latest of their times. Near Gloddaeth, is a considerable barrow, and, on Pen Mawr, more than one circle may be distinctly traced. The most interesting remnants of antiquity in the place, are, the rocking-stone, the trilithon, and the remains of a British town on the peaked summit of Pen Monnedd. The rocking-stone is a long flat mass of lime-stone, called, from its form, St. Tudno's cradle, and may be moved by a gentle force. The trilithon is composed of irregular masses of rock, like some of these relics often met with in Britain, and seems to have served the purpose of an altar. The upper stone has been thrown from its place, and lies shattered at a distance. Of the town may still be seen the foundations of nine circular huts, and a small portion of the fortified wall. They are less distinct than those of the famous Old Sarum, which I have examined, but in no other respect differ from them.*

The chief natural curiosity of this district, is the cave of the lesser Orme's-Head. This is a long narrow cleft in the rock, approachable only at very low water. Its name Eglwys wen, or white church, has been derived from a singular phenomenon, periodically exhibited in it. At the seasons of the higher spring tides, the waves successively approaching the mouth of the ca-

* The situation of this place, which is called by the natives Dinas, or fortified place, agrees nearly with Cæsar's description of a British town. Oppidum, autem, Britanni vocant, quum silvas impeditas vallo atque fossâ munierunt quò, incursionis hostium vitandae causâ, convenire consuêrunt.—Lib. V. c. 11.

vern, close it up entirely; and, compressing the air within the cavern, are forced up through an aperture in the middle of the roof in a magnificent column of foam, not less than a yard in diameter, and reaching the height of about thirty feet.

Among the rare marine productions may be mentioned, the pearls of the river Conway; the only native pearl fishery in our island. The animal by which they are generated, is a muscle of the usual dark blue colour. The pearls themselves are very small, of the species called seed pearl, and are sold at from 1s. 6d. to 3s. or 4s. per ounce. The existence of these pearls seems to have been known by Tacitus. In his life of Agricola, he thus speaks of the productions of Britain. "Gignit et oceanus margaritas sed *subfusca et liventia.* Quida martem abesse legentibus arbitrantur, nam in mari Rubro viva et spirantia a saxis avelli, in Britanniâ prout expulsa sint colligi. Ego facilius crediderim naturam margaritis deesse quam nobis avaritiam."

The ornithological notices of this little rock would require more skill and knowledge than I possess to do them justice. Few spots, near our island, south of the Shetlands and St. Kilda, can boast an equal number and variety of the feathered tribes. Cormorants, hawks, owls, gulls, choughs, and doves, are the constant tenants of the rocks; and the periodical visits of the starling, solan goose, puffin, razor-bill, and guillemot, swell the catalogue of interesting birds, and break the stillness of these wild precipices. That rare and beautiful bird, the peregrine falcon, is often found here; and Pennant assures us, that a cast of hawks, from Orme's-Head, was, in days of yore, no mean present.

The unsightly cormorant may here be found in uncommon abundance, and might be observed in all its habits and conditions. One circumstance I had frequent opportunities of noticing, which displayed strikingly, that nature had not bestowed on these birds many advantages of structure or mechanical contrivance. After a stormy day, vast numbers of them were found at the foot of the rocks, probably killed by being dashed by the wind against the projecting cliffs; while not a single gull or guillemot had suffered from a similar cause. During the heaviest part of the gale, I had observed these birds cowering in the crevices of the rocks, insensible to our shouts, and even to the report of a fowling-piece. Stones repeatedly hurled at them, even with effect, could not drive them from their place. They seemed conscious of their inability to use their wings, while the gulls appeared to have absolute enjoyment in the power of theirs, resisting and even sporting in the wildest eddies of the tempest.

The mineralogical curiosities of this district are fewer than those of mines, which traverse quartzose rocks. I have observed, indeed, that varieties of mineral productions are less numerous in lime than in granitic or basaltic mines. Thus, Cornwall, Scotland, Hungary, and Norway, whose mines are chiefly found in the latter species of rock, present many more species of minerals than Pary's Mountain, Orme's-Head, or the Peak. A good vein of arseniate of copper, in some places six and in one nine feet thick, has been worked at Llandudno for several years. The produce is equal in quality to that of the best Anglesea ore, being 45 per cent. generally. This I have on the testimony of the late William Jones, the company's mine agent, at Llandudno. Asbestus, of a coarse quality, malakite and varieties of zeolite are the only other minerals found in these mines.

The geological structure of the parish is very uniform. Lime, of the primitive kind, seems to be the only rock, and in both the promontories, lies at an angle of 40°, with the horizon. This rock, being easily reducible to soil, forms an excellent and a varied habitation for vegetables, and, accordingly, I anticipated, at first sight, as well from the quality of the rocks themselves, as from the varied appearance of the whole district, that not a few of the more showy specimens might be found here.

The following catalogue is the result of a careful examination of this spot, during two successive visits; some of the names being added from a catalogue kindly furnished me by Mr. Wilson, of Warrington.*

In the classification, those plants are omitted which are common to almost every piece of uncultivated ground, although there was no deficiency of them, even amidst the profusion of the rarer species.

Class I.—MONANDRIA.

Salicornia *herbacea*, marsh sampire, is not properly a native of Llandudno, being found most abundant on the opposite shore of the Conway. A stray specimen or two may be seen near Diganwy.

* A few names are copied from a Catalogue in the Magazine of Natural History, by Mr. Winch.

Hippuris *vulgaris,* common marestail. Diganwy. The fructification of this plant exhibits a contraction and expansion produced by changes in the state of the atmosphere. Similar phenomena are exhibited by the awns of the walking oat, (avena fatua) and of the gerania; and in the seeds of ferns, especially the pteris aquilina, or common brake.

Callitriche *verna.* Vernal water starwort. Common.

C. *autumnalis.* Autumnal water star wort. Llanrhos. Botanists do not seem to agree in admitting this species. I have not observed any specific differences between it and the common starwort, unless time of flowering and submersion be considered as distinctions.

Class II.—DIANDRIA.

Ligustrum *vulgare.* Common privet. This shrub, so valuable both as an ornament and a fence to our meadows, is not common in a native state. It grows plentifully at Llandudno, on the S. and S. E. sides of the rock. From its elevated native habitat, the privet frequently retains its leaves during the winter, when it is planted in warm low situations.

Fraxinus *excelsior.* Common ash. Found in the same situations as privet. From its stunted appearance, this plant is probably produced in this locality from seed brought by birds from the neighbouring woods.

Veronica *spicata*. Spiked speedwell. Rocks above Llandudno and Gloddaeth.

V. *hybrida*. Welsh speedwell. Same places.

Pinguicula *vulgaris*. Common butterwort. On the S. side of the village.

Lycopus *europæus*. Water horehound. Near Gogarth.

Salvia *verbenaca*. Wild English clary. Under the shelter of rocks in good soil.

Class III.—TRIANDRIA.

Pennant had not minutely examined this place, when he praised the fine pastures of Orme's-Head. There are few species of grass in the parish; and those chiefly belong to the genera festuca, poa, and carex. Besides these there were,

Agrostis *spica venti*. Silky bent grass, abundant.

Aira *cespitosa*. Turfy hair grass.

Briza *minor*. Small quake grass.

Festuca *ovina*. Sheep's fescue grass.

Arundo, *phragmites*. Common reed. Common in the marshes near Llanrhos. This grass is found near Manchester, 10 feet high.

A. *arenaria*. Sea reed. Along the banks of the Conway. The usefulness of this grass, in keeping together the sand banks of a low coast, is well known. Its abundance in such situations is evidently a provision of nature.

A. *epigejos*. Wood reed. Gloddaeth Woods. *Mr. Wilson.*

Hordeum *pratense*. Meadow barley. Common near the village.

Scirpus *glaucus*. Glaucous club rush. Between Llandrillo and Bodafon.

Aira *cristata*. Crested hair grass. Orme's-Head. *Mr. Wilson.*

Poa *rigida*. Gloddaeth. *Mr. Wilson.*

Avena *pubescens*. Downy oat grass. Sandy places below Orme's-Head.

A. *pratensis*. Narrow-leaved oat-grass. Orme's-Head.

A. *flavescens*. Yellow oat-grass. Orme's-Head.

Iris *fœtidissima*. Gladwyn. Properly a native of Anglesea.

Fedia *dentata*. Oval fruited Corn Sallad. Rocks above Llandudno. *Mr. Winch.*

Class IV.—TETRANDRIA.

Dipsacus *fullonum*. Fuller's teasel. Llandudno. Common.

Scabiosa *columbaria*. Small Scabious. Rocks near Pen Hyfrydd. This plant is a native of lime rocks.

Rubia *peregrina*. Wild madder. Gloddaeth and Llanrhos.

Plantago *maritima*. Sea plantain. Abundant on the shore. The miners sometimes pickle the leaves, when rock sampire is scarce.

P. *coronopus*. Buck's horn plantain. Abundant.

Parietaria *officinalis*. Pellitory of the wall. Orme's-Head. The cuticle of this plant exhibits a remarkable power of resistance to heat. At Caernarvon, it grows luxuriantly over the face of a slate rock, which, in the summer-sun, becomes almost too hot to be touched by the finger.

Ilex *aquifolium*. Holly. At Pen Hyfrydd, common.

Sagina *maritima*. Sea pearl wort. Between Llandrillo and Bodafon.

S. *apetala*. Annual small flowered pearl wort. Llandudno. *Mr. Winch.*

Class V.—PENTANDRIA.

Lithospermum *officinale*. Grey millet. Rocks above Llandudno. *Mr. Winch.*

Lithospermum *maritimum*. Sea Gromwell. Llandudno bay. *Mr. Wilson* and *Mr. Winch.*

Borago *officinalis.* Common borage. Diganwy.

Echium *vulgare.* Common viper's bugloss. Diganwy.

Lycopsis *arvensis.* Small bugloss. Common.

Primula *elatior.* Oxlip. On the N. side of Orme's-Head.

Convolvulus *arvensis.* Small bindweed. Llandudno.

Convolvulus *soldanella.* Sea bindweed. On the banks of the Conway. I am not aware, that any author has noticed the early formation of the cotylydons in the unripe seed of this plant. Spallanzani took considerable pains to shew, by the assistance of powerful microscopes, that the seeds of spartium junceum, the Spanish broom of our shrubberies, were discernible in the ovary of that plant immediately after the flower was expanded. In the sea bindweed, proofs of impregnation are as distinctly afforded to the naked eye. Each perfect seed, while scarcely of full size, contains a pellucid glutinous juice, like the white of an egg, in which are inclosed two perfect seed-lobes, of a full green colour, folded up like the unexpanded petals of a flower in its calyx.

C. *sepium.* Great bindweed. Common at Eglwys Rhos.

Viola *hirta.* Hairy violet. Orme's-Head and Gloddaeth.

V. *lutea.* Yellow pansy. On the shore near the Conway.

Hyoscyamus *niger.* Common henbane. Near Llandudno. Abundant.

Erythræa *littoralis.* Dwarf tufted centaury. Shore between the Orme's-Heads.

Euonymus *europæus.* Common spindle tree. Orme's-head.

Hedera *helix.* Common Ivy. Several tufts, of a variety of ivy, with all the leaves simple, are found growing over the rocks at Orme's-Head. This is the most common rock ivy throughout N. Wales.

Beta *maritima.* Sea beet. Shores near the village.

Ulmus *campestris.* Common elm. Pen Hyfrydd.

Eryngium *maritimum.* Sea holly. Shore of the Conway.

Scandix *pecten veneris.* Shepherd's needle. Fields near Llandrillo.

Daucus *maritimus.* Sea-coast carrot. Orme's-Head, near the church.

Conium *maculatum.* Common hemlock. Llandudno.

Torilis *infesta.* Spreading hedge parsley. Corn fields at Llandudno.

T. *anthriscus.* Upright hedge parsley. Llandudno. *Mr. Winch.*

Crithmum *maritimum.* Rock sampire. This plant is rare in the N. of England, but common at Orme's-Head on the barest and most precipitous rocks. It is to this plant that Shakspeare alludes, in his description of Dover cliff.

> " ——How fearful
> And dizzy 'tis, to cast one's eyes so low!
> The crows, and choughs, that wing the midway air,
> Show scarce so gross as beetles: Half way down
> Hangs one that gathers samphire; dreadful trade!
> Methinks, he seems no bigger than his head."—LEAR.

This dreadful trade at Llandudno has cost several lives.*

Anethum *fœniculum.* Common fennel. Plentiful at Llandudno.

Sison *amomum.* Hedge honewort. Between Gloddaeth and Marle. *Mr. Wilson.*

Smyrnium *olusatrum.* Alexanders. Corn fields. *Mr. Wilson.*

Viburnum *opulus.* Guelder rose. Gloddaeth.

Sambucus *nigra.* Common elder. Bodscallan.

S. *ebulus.* Dwarf elder. Shores at Orme's-Head.

* Phillips, in his History of Cultivated Vegetables, mistakes the salicornia, or marsh sampire, for this plant; which, he says, erroneously, is found in abundance on the muddy shores of Sussex. Salicornia, in every respect, differs from Crithmum; in structure and taste as well as in locality. The first is salt and sickly, and a small plant of the First class. Crithmum is of the Fifth class, and of a warm agreeable flavour and aromatic odour. Salicornia is confined to muddy shores, Crithmum to dry lofty rocks.

Statice *armeria.* Common thrift.

S. *reticulata.* Sea lavender. Rocks at Llandudno.

Class VI.—HEXANDRIA.

Allium *vineale.* Crow garlick. Rocks above Llandudno. *Mr. Winch.*

Scilla *verna.* Vernal squill. Llandudno Bay, &c. *Mr. Wilson.*

Narthecium *ossifragum.* Lancashire asphodel. Goggarth.

Alisma *ranunculoides.* Lesser water plantain. Near Eglwys Rhos.

Class VIII.—OCTANDRIA.

Chlora *perfoliata.* Com. yellow wort. Hedges near Eglwys Rhos.

Epilobium *hirsutum.* Great hairy willow herb. Ditches near Llandudno.

Class IX.—ENNEANDRIA.

Butomus *umbellatus.* Flowering rush. In the marsh near Orme's-Head. *Mr. Wilson.*

Class X.—DECANDRIA.

Saxifraga *tridactylites*. Rue leaved Saxifrage. Rocks at Orme's-Head. *Mr. Wilson.*

Dianthus *deltoides*. Maiden pink. Near Diganwy Castle.* *Mr. Wilson.*

Saponaria *officinalis*. Common soapwort. Near the Mine. Llandudno.

Silene *anglica*. English catchfly. Orme's-Head and Diganwy.

S. *maritima*. Sea catchfly. Ditto.

S. *nutans*. Nottingham catchfly. Ditto.

Arenaria *verna*. Vernal sandwort. Little Orme's-Head. *Mr. Wilson.*

Cotylydon *umbilicus*. Common navelwort. Rocks of Orme's-Head.

Sedum *telephium*. Common orpine. Abundant.

S. *anglicum*. White English stonecrop. Rocks above Llandudno. *Mr. Winch.*

S. *forsterianum*. Welsh rock stonecrop. Orme's-Head. *Mr. Wilson.*

Cerastium *semidecandrum*. Little mouse-ear chickweed. Llandudno. *Mr. Winch.*

C. *tetrandrum*. Four-cleft mouse-ear chickweed. Llandudno. *Mr. Winch.*

* This was once a place of great strength. The fortifications, which owed their origin to Danish pirates, were destroyed by Llewellen ap Jorweth, the last King of Wales.—Pennant.

Lychnis *dioica*. Hedge pink. White variety. Orme's-Head. Abundant.

Class XI.—DODECANDRIA.

Agrimonia *eupatoria*. Common agrimony. Fields near Eglwys Rhos.

Reseda *luteola*. Woad. Shores.

Euphorbia *cyparissias*. Cypress spurge. Corn-fields near Llandrillo.*

Class XII.—ICOSANDRIA.

Mespilus *oxycantha*. Com. hawthorn. Rocks.

M. *cotoneaster*. Dwarf quince leaved medlar. This species, first found native by *Mr. Wilson*, in 1826, has no other locality in Britain than Orme's-Head. The plant is accurately described in the last vol. of the English Flora.

Pyrus *aria*. White wild pear tree. Goggarth and Bodafon.

Spiræa *filipendula*. Dropwort Meadow sweet. Rocks at Orme's-Head and Bodscallan.

Rosa *spinosissima*. Burnet rose. Variety (γ) in the English Flora. Shore.

* At Llandrillo, Maelgwyn Gwynedd, King of N. Wales, in the 6th century, had a palace, some ruins of which remain. He removed from this residence to Llanrhôs, to escape the plague, but caught it, and died there. Pennant relates several interesting particulars of his exploits.—Vol. 3.

Potentilla *verna*. Spring cinquefoil. Orme's-Head, &c. *Mr. Wilson.*

Rubus *cæsius*. Dew berry. Abundant.

R. *saxatilis*. Rock bramble. Abundant.

Class XIII.—POLYANDRIA.

Glaucium *luteum*. Yellow horned poppy. Shore, among dry stones.

Papaver *hybridum*. Round rough-headed poppy. Cornfields. *Mr. Wilson.*

Cistus *marifolius*. Hoary dwarf rock rose. Near the rocking-stone. Abundant.

C. *helianthemum*. Common rock rose. Common.

Thalictrum *minus*. Lesser meadow rue. Near the mine on Orme's-Head.

Aquilegia *vulgaris*. Common columbine. Gloddaeth. *Mr. Wilson.*

Ranunculus *parviflorus*. Small flowered crowfoot. Orme's-Head. *Mr. Wilson.*

Class XIV.—DIDYNAMIA.

Verbena *officinalis*. Common vervain.* Common near Llandudno.

* Called by the Welsh, "Fiends' Aversion," from its use by the Druids in their religious ceremonies.

Ballota *nigra.* Black horehound. Common.

Marrubium *vulgare.* White horehound. Common.

Leonurus *cardiaca.* Common motherwort. Near Bodafon.

Nepeta *cataria.* Catmint. Near Bodafon.

Origanum *vulgare.* Sweet marjoram. Very common under the rocks. Always found on the harder lime-rocks.

Thymus *calamintha.* Com. calamint. Orme's-Head. *Mr. Wilson.*

T. *acinos.* Basil Thyme. Rocks above Llandudno. *Mr. Winch.*

Scrophularia *vernalis.* Yellow figwort. Gloddaeth. *Mr. Wilson.*

Orobanche *minor.* Lesser broom rape. Orme's-Head. Diganwy.

Digitalis *purpurea.* Foxglove. I cannot help noticing here, although out of place, a curious native variety of this plant, found near Caernarvon. The petal was divided into five deep segments, and the stamens, instead of conniving, as usual, stood apart from each other. The buds, on being opened, showed the same appearance. This fact might afford a curious illustration of the sexual system. A similar instance occurs in another native plant, of the same class and order, Antirrhinum peloria.

Class XV.—TETRADYNAMIA.

Hutchinsia *petræa*. Rock hutchinsia. Orme's-Head. *Mr. Wilson.*

Cochlearea *danica*. Danish scurvy grass. Llandudno bay, &c.

Crambe *maritima*. Sea kale. Llandudno bay and along the shore to the West. This plant is the original of our excellent culinary vegetable.

Arabis *hirsuta*. Hairy wall cress. Orme's-Head. *Mr. Wilson.*

Brassica *oleracea*. Sea cabbage. Common on the rocks. This plant, which is the original of all our varieties of cultivated cabbage, must have been always abundant at Orme's-Head, as I have found the perfect seeds in bog-soil taken from considerable depth near Llandrillo.

Class XVI.—MONADELPHIA.

Erodium *maritimum*. Sea stork's bill. On the hill near the church, Orme's-Head.

E. *cicutarium*. Hemlock stork's bill. Abundant. The white variety is very common in Llandudno bay. When dried quickly, between the leaves of paper, the petal becomes red. It is probable, that the red colour has existed in the leaves, and run into the petal during the process of drying. I have observed this in several white-flowered varieties of plants, whose usual colour is blue or red.

Geranium *columbinum*. Long-stalked cranesbill. Orme's-Head rocks.

G. *sanguineum*. Blood-red cranesbill. Common. This plant is stronger and more abundant on basaltic rocks than on lime. On the coast of Ayrshire, it is seen in beds of several hundred yards; and, on Arthur's Seat, it is stronger and more bushy than it is ever found at Orme's-Head or St. Vincent's rocks.

Malva *sylvestris*. Common mallow. Llandudno.

M. *rotundifolia*. Round-leaved mallow. Llandudno.

Class XVII.—DIADELPHIA.

Fumaria *capreolata* var. *flore albo*. White-flowered variety of ramping fumitory. In hedges near the little Orme's-Head. *Mr. Winch.*

Lotus *diffusus*. Slender bird's foot trefoil. Abundant.

L. *maritima*. Sea bird's foot trefoil. This seems to me a new species. The inversely heart-shaped leaves, which are fleshy and pubescent, distinguish it from L. *corniculatus*. It grows plentifully on the eastern side of the promontory.

Hippocrepis *comosa.* Tufted horse-shoe vetch. Orme's-Head.

Medicago *minima.* Little bur medick. Near the ruins of Gogarth. *Mr. Wilson.*

M. *maculata.* Spotted medick. Same place. *Mr. Winch.*

Class XVIII.—POLYADELPHIA.

Hypericum *montanum.* Mountain St. John's wort. Common at Orme's-Head, in thickets.

Class XIX.—SYNGENESIA.

Cichorium *intybus.* Wild succory. A beautiful plant, common near Llandudno.

Picris *echioides.* Bristly ox tongue. Near Gloddaeth.* *M. Wilson.*

Carduus *tenuiflorus.* Slender flowered thistle.

C. *marianus.* Milk thistle. Llandudno.

Artemisia *absinthium.* Wormwood. Very common near Llandudno. The miners distil an intoxicating liquor from the leaves and flowers.

Gnaphalium *dioicum.* Mountain cudweed. Orme's-Head, in pastures. *Mr. Wilson.*

* Pennant justly extols the scenery in the neighbourhood of Gloddaeth, as "among the first-rate in this island."

Hypochæris *maculata.* Spotted cat's ear. Rocks above Llandudno. *Mr. Winch.*

Conyza *squarrosa.* Plowman's spikenard. *Id.*

Class XX.—GYNANDRIA.

Orchis *pyramidalis.* Pyramidal orchis. Thickets at Gloddaeth. *Mr. Wilson.*

Epipactis *latifolia.* Var. *rubra.* Helleborine. Rocks at Orme's-Head. *Mr. Wilson.*

Class XXI.—MONOECIA.

Carex *muricata.* Greater prickly carex. Below Orme's-Head. *Mr. Wilson.*

Poterium *sanguisorba.* Common salad burnet. Near the rocking-stone.

Carpinus *betulus.* Common hornbeam. In clefts.

Betula *alba.* Common birch. Probably not indigenous at Orme's-Head.

Class XXII.—DIOECIA.

Humulus *lupulus.* Common hop. Hedges near Llanrhos.

Tamus *communis.* Wild vine. Llanrhos. The trivial name of this plant is exceedingly appropriate. Its size, and manner of growth, together with the arrangement and colour of the fruit, present a striking likeness to the vines on the continent.

Juniperus *communis*. Common juniper. Rocks on Orme's-Head and at Gloddaeth.

Taxus *baccata*. Common yew. Gloddaeth.

Class XXIV.—CRYPTOGAMIA.

Polypodium *vulgare*. Var. *cambricum*. Polypody. Rocks at Bodscallan.

Asplenium *trichomanes*. Common maiden's hair. This plant is always found growing on lime, in a state of decomposition.

A. *marinum*. Sea spleenwort. Rocks on the N. side of Orme's-Head.

A. *ruta muraria*. Wall-rue spleenwort. Rocks. Abundant.

Scolopendrium *vulgare*. Common hart's tongue. Abundant in every cleft of the rocks throughout the parish, and of every variety. Those growing in the ruins of Marle, and of Conway castle, are very luxuriant.

Pteris *aquilina*. Common brakes. On the G. Orme's-Head, very dwarfish, often not exceeding six inches in height. In woods, near Caernarvon, this species grows to the height of ten or twelve feet.

Encalypta *vulgaris*. *Mr. Wilson*.

Lecidea *vesicularis*. *Mr. Winch.*

Lecanora *gelida*. *Mr. Winch.*

Lichen *geographicus*. This minute vegetable presents, on the rocks of N. Wales, many resemblances of the outlines of a map. In the ascent to Snowdon, from Llanberis, we found an almost exact outline of Europe, with the African coast, traced by this lichen, on a flat mass of grey wacké.

A singular sponge-like substance, is found growing from decayed wooden spars in the copper mine. It nearly resembles Alectoria *jubata*, but I had no opportunity of examining its fructification. Similar substances occur in the Cumberland mines.

The sea-weeds of Orme's-Head are not numerous nor plentiful. The tide occasionally throws up, on the shore of the Conway, Fucus *coccinea*, and F. *rubra*, with several of the Ulvas. These, however, are common on the Lancashire coast. Fucus *flagelliformis* is very abundant in Llandudno bay.

DESCRIPTION

OF A

NEW METHOD OF DETERMINING

THE

WEIGHTS OF GASES.

BY MR. JOHN POTTER.

(Read March 9th, 1827.)

HAVING understood that an apparatus, capable of determining the weights of gases to a considerable degree of nicety, was a desideratum in chemical science, I am induced to propose the following contrivance, which I conceive to be tolerably well adapted for that purpose. This instrument, in its construction and mode of use, is much like that of the hydrometers, and instruments of a like nature, used to determine the specific gravities of fluids. It differs from them, however, in some essential particulars, as will be seen from the annexed drawing and the following description. A, A, is a hollow globe of glass, from the lower part of which projects a tube B, B; the lower extremity of this tube is terminated by the cock C, which opens or cuts off the communication with the interior of the globe A, A. This cock has on it a screw, by means of which it may be connected with any description of pneumatic apparatus. When the instrument is placed in water, a ball of lead, D, is attached to this screw, for the purpose of balancing the instrument, and sinking it to a proper depth. From the upper part of the globe, A, A, projects a stem, E, E, consisting of a piece of wire, upon the top of which is fixed the cup, F. A small hole is likewise perforated through the stem E, E, as is shewn at G.

The method of using the instrument is as follows: it is first exhausted, and then, the ball of lead, D, being screwed upon the cock, C, it is placed in water, and small weights, consisting of bits of lead, or of any other suitable material, are placed in the cup, F, and increased or diminished until the small hole, G, through the stem, E, E, is just level with the surface of the water. The instrument is now to be taken out of the water and filled with the gas or air the density of which is to be ascertained. The ball of lead, D, being replaced, it is again put into the water, and sunk, by additional weights, to the small hole in the stem, E, E, as before. The weights employed in this weighing are now to be compared with those used in the former weighing; their difference being the weight of the air or gas contained in the instrument.

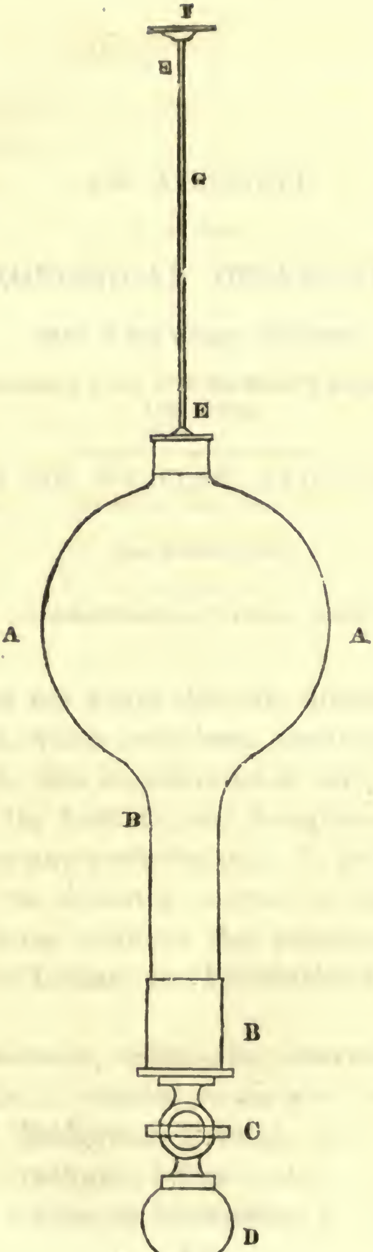
Since the above paper was read before the society, an instrument, similar to the one above described, has been constructed, with a view of ascertaining, experimentally, what good or bad qualities it would be found to possess.

When the instrument, in an exhausted state, was tried in water, much difficulty was experienced in preventing the water from entering. The experiments, in this respect, required at all times the greatest care, as the smallest drop getting by any means into the instrument would vitiate the whole experiment. The balancing of the instrument in the water was likewise found to be a tedious operation, as it always made a considerable number of oscillations, upwards and downwards, before it came to a state of quiescence. The smallest of these oscillations were made in pretty nearly the same time as the largest. The friction occasioned by the instrument passing through the water, was likewise found to be considerable. Upon the whole, it appeared, that the instrument was capable of determining the weight of air or gases to a considerable degree of nicety, though, perhaps, not superior, in this respect to the ordinary method, which has likewise the advantage of being more expeditious.

One circumstance occurred, during the experiments, which deserves notice, and that was, the extreme delicacy with which the instrument indicated variations in the density of the water, occasioned by alterations in the temperature; from which it appeared, that the instrument might be very properly used to determine the density of water, for all degrees of temperature below the boiling point.*

* Soon after the reading of the above paper, it was pointed out to the author, that an instrument on a similar principle was described in Dr. Desagulier's translation of s'Gravesande's Mathematical Philosophy. This instrument consisted of a large globe of glass, having a long neck furnished with a stop cock. The method of using it was as follows:—the instrument being exhausted, was suspended to the end of one arm of a balance, the globe itself being immersed in water, with a part of the neck and the stop cock remaining above the surface. The centre of the balance was now raised and lowered until the balance itself stood in an horizontal position, and then the cock being opened the globe became filled with atmospheric air, and the equilibrium of the balance consequently destroyed. Weights were now placed in the opposite scale until the equilibrium was restored, and these weights consequently shewed the weight of the air admitted into the instrument.

The similarity of this instrument, with that invented by the author, will be readily seen; the original idea of both appears to have been the same, viz:—that of floating the receptacle of the gas in water, and by that means to relieve the balance from as much of the weight as possible.


F
E
G
E
A
A
B
B
C
D

AN ACCOUNT

OF SOME

ASTRONOMICAL OBSERVATIONS

Made at Ivy Cottage, Cornbrook,

IN THE YEARS 1823, 4, 5, 6, & 7, TO DETERMINE ITS LATITUDE AND LONGITUDE.

BY MR. WILLIAM HADFIELD.

(Read March 9th, 1827.)

COMMUNICATED BY PETER CLARE.

As I am not aware that any observations are published, which have been made by persons residing in this neighbourhood, with a view to ascertain the Latitude and Longitude of Manchester, or any particular place in its immediate vicinity, the following account of observations which I have made for that purpose, I beg to offer to the Literary and Philosophical Society.

My residence, where the observations have been made, is situated on the west bank of the Duke of Bridgewater's canal, and the south side of Cornbrook bridge:—its direction from St. Mary's spire, in Manchester, is 42° 46′ west of south, and is two thousand three hundred and seventy-nine yards distant from it.

It may be satisfactory to state what sort of instrument I have used, and how it has been applied in pursuing my inquiries.

The instrument employed for ascertaining the Latitude, is a theodolite, with vertical and horizontal circles, six inches diameter; divided into 20′ of a degree, with verniers, to read off to 15″:—the vertical screw on which it is placed, is fixed in a stone that is set in a brick wall.

Having first ascertained that the vertical axis was placed so as to allow a true horizontal motion to the instrument, and that the horizontal axis would allow a true vertical motion to the telescope, I proceeded to find the meridian in the following manner.

The altitude of the sun's lower limb was taken, when he was as far eastward as circumstances would allow, his lower limb touching the horizontal wire, and his western limb the vertical wire, in the telescope:—the situation upon the horizontal circle was then noted down, in degrees, minutes and seconds; and letting the telescope remain at the same elevation, the instrument was moved horizontally towards the west, until the sun's lower limb again touched the horizontal, and his western limb the vertical wire:—having read off, on the horizontal circle, the distance which the instrument had been moved, and placing the telescope at half that distance, I directed it towards the ground, and placed a temporary mark in the situation to which it pointed, until I had made some further observations on the sun, and also on several fixed stars.

Being at length satisfied that a true situation had been found, I placed a permanent meridian mark, in a field, at the distance of 563 feet from the instrument.

Of the various methods given to find the latitude of a place, perhaps the most easy is to take the altitude of the sun when he is in the equator at noon; but, although he is in the equator twice every year, yet it very rarely occurs, that he is at the same time on the meridian of the place of observation; and there being very few stars in the equator upon which observations can be made, the opportunities to find the latitude in this way, are of very rare occurrence. There are, however, other means presented to us, for attaining this object; such as observations on circumpolar stars, when on the meridian, both above and below the pole; also, by observing, when on the meridian, the altitude of the sun, or a star, whose polar distance is known.

As no opportunity has been presented to make an observation on the sun, when in the equator at noon, I have availed myself of the other methods alluded to, viz. the circumpolar stars, and the sun and stars, whose polar distances are laid down in the Nautical Almanac.

It may perhaps be proper further to remark, that the altitude of the sun or stars, given in the tables, is the true altitude, the necessary allowance for refraction having been made from the apparent altitude.

	°	′	″
Mean Latitude of Cornbrook, from 164 observations on the Sun, in the years 1824, 25, 26, and 27.	53	28	2.6
Greatest Difference			34

Mean Latitude, from observations of the following Stars made in the years 1823, 24, 25, 26, and 27.

		°	′	″
1	Observation on Stella polaris	53	28	1.5
Mean of 7	ditto on α Arietis	53	28	7.7
	Greatest Difference			19
—— 13	ditto on Aldebaran	53	27	53.5
	Greatest Difference			20
—— 13	ditto on Rigel	53	28	3.3
	Greatest Difference			16
—— 4	ditto on α Orionis	53	27	58.7
	Greatest Difference			10
—— 6	ditto on Sirius	53	28	0.1
	Greatest Difference			6
—— 4	ditto on Procyon	53	28	4.3
	Greatest Difference			16
—— 3	ditto on Regulus	53	28	1.9
	Greatest Difference			15
—— 9	ditto on Spica Virginis	53	28	7
	Greatest Difference			19
—— 8	ditto on Arcturus	53	27	58.3
	Greatest Difference			10
—— 4	ditto on Antares	53	27	58.8
	Greatest Difference			6
—— 21	ditto on α Aquilæ	53	28	2.8
	Greatest Difference			20
—— 1	ditto on Fomalhaut	53	27	57.6
—— 6	ditto on α Andromedæ	53	28	3.2
	Greatest Difference			29
	Mean of the above	53	28	1.2

	°	′	″
Mean Latitude of Ivy Cottage, Cornbrook, from 164 observations on the Sun, taken in the years 1824, 25, 26, and 27.	53	28	2.6
Ditto from means derived from 100 observations on Stars, in the years 1823, 24, 25, 26, and 27.	53	28	1.2
Mean of the above, or the true Latitude of Ivy Cottage.....	53	28	1.9
From which the Latitude of St. Mary's spire, Manchester, calculated from the foregoing observations will be.	53	29	4

The observations that I have made on the Eclipses of Jupiter's Satellites, with a view to ascertain the Longitude, are but few in number, as opportunities but seldom occur when the sky is sufficiently clear, and the eclipse happens at the time the planet is above the horizon.

I have, for these observations, made use of an achromatic telescope, with an object-glass, $2\frac{3}{4}$ inches diameter, and $3\frac{1}{2}$ feet focal length, and a magnifying power of 180 times.

The time was ascertained, on each of the nights of observation, by some known star, passing the meridian as near the time of observation as possible.

OBSERVATIONS ON THE ECLIPSES OF JUPITER'S SATELLITES.

Date.	Eclipses of Jupiter's Satellites.	Time at Greenwich.			Time at Cornbrook.			Longitude in time West.		Longitude in parts of the equator, West.		
		H	′	″	H	′	″	′	″	°	′	″
1824.												
Jan. 24.	Emersion of Jupiter's 1st Satell.	9	26	50	9	17	46	9	4	2	16	0
April 7.	—— — — —	10	10	47	10	1	53	8	54	2	13	30
Dec. 5.	Immersion — 2nd —	13	5	47	12	56	51	8	56	2	14	0
1825.												
Jan. 4.	—— — 3rd —	12	35	53	12	26	58	8	55	2	13	45
March 4.	Emersion — 1st —	9	9	28	9	0	32	8	56	2	14	0
„ 20.	—— — — —	7	27	22	7	18	19	9	3	2	15	45
„ 27.	—— — — —	9	22	8	9	13	8	9	0	2	15	0
1826.												
March 30	—— — 2nd —	10	44	33	10	35	40	8	53	2	13	15
May 1.	—— — 1st —	8	51	18	8	42	18	9	0	2	15	0
Mean from the above observations....								8	57.4	2	14	28.3
The Longitude of St. Mary's Spire, Manchester, calculated from the above observations								8	51.5	2	12	52.5

EXPERIMENTS AND OBSERVATIONS

ON

Diverging Streams

OF

COMPRESSED AIR.

BY MR. T. HOPKINS.

(Read Nov. 28, 1828.)

ON the eleventh of October, in the year 1824, Mr. Roberts affixed a valve to the aperture of a pipe, used as a waste-pipe, for the purpose of regulating or equalizing the force of a blast of air which was blowing a furnace. To his surprize, however, he found that the valve, instead of being readily blown off by a strong blast, remained at a small distance from the aperture of the pipe, and was removed to a greater distance only by a considerable exertion of the power of the hand. This singular phenomenon was witnessed by many gentlemen, members of this society, in the same week,

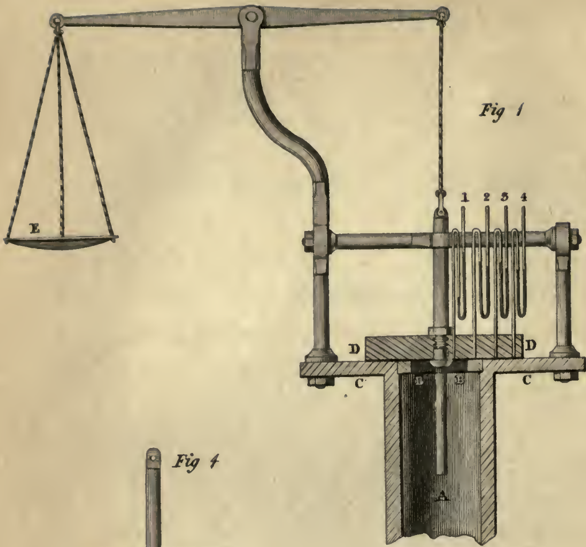


Fig 1

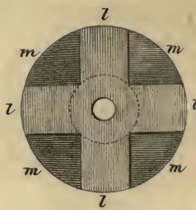


Fig 6

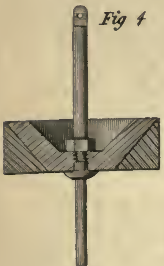


Fig 4

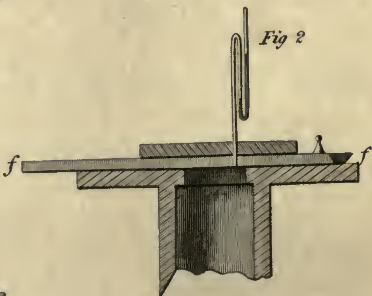


Fig 2

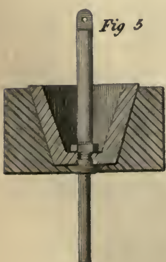


Fig 5


Fig 7

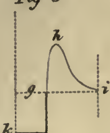


Fig 3

and appeared to be viewed by them all, as equally new and extraordinary.*

Mr. Roberts made some experiments on his air-valve at the time, and various theories were then suggested to account for the adherence of the valve to the pipe. It was not, however, until the month of September in the present year, that I agreed to join him in making further experiments, a part of which, I now proceed to give.

A vertical section of part of the apparatus used is given in figure the first, where A is a pipe 3 inches diameter, with the aperture contracted to $2\frac{3}{8}$ diameter; B B surrounded by a flange C C, $10\frac{3}{4}$ diameter, to form a seat for a valve. On this seat was placed a circular disk or valve D D, 6 inches diameter, with a pin in its center, by means of which it was left at liberty to rise or fall freely, and kept at the same time perpendicular to the aperture.

The valve was attached to one end of a scale

* Monr. Clement, of Paris, was, I understand, in Manchester at this period, and saw the air-valve adhere to the pipe, yet he afterwards, it appears, represented the discovery to have been made in France long subsequent to the time he saw it at Mr. Roberts' works.

beam by a string, and balanced by weights placed in a scale E, attached to the opposite end of the beam. The valve being thus placed on the seat without any weight of its own to press downward, the stream of compressed air was admitted into the pipe A, when the valve D, rose from the flange or seat C, 1-32nd of an inch, and there remained stationary. Thirteen ounces, avoirdupoise weight, were now put into the scale E, which raised the valve to 1-12th of an inch above the seat. Twenty-six ounces raised it to 1-8th of an inch, and thirty-two ounces raised it to 1-4th of an inch, but any weight beyond this last caused the valve to fly abruptly off.

It thus appeared, that when the valve was raised from its seat a quarter of an inch, there was the greatest difference between the force of the issuing current of air pressing against the *under* side of the valve, and of atmospheric pressure on the *upper* side of the valve. The pressure of the atmosphere was greater than the force of the issuing stream of previously compressed air, a weight of thirty-two ounces being requisite to establish an equilibrium.

That we might ascertain what was the state of the stream of air under the valve, in different parts of it, four double syphon tubes were procured, and proper quantities of mercury being put into them, they were inserted in holes made through the valve at certain distances from each other, as shown in Figure the first, 1, 2, 3, 4. The inserted limbs of these tubes being thus left exposed to the action of the stream of air, the compressed air was again admitted into the pipe A, and the valve rose as before, 1-32nd of an inch.

The tube No. 1, in that part of the valve D, which was over the aperture B, had the mercury in it 1½ inches higher in the *outer* than in the inner limb, and consequently shewed a pressure from the compressed air below it, above atmospheric pressure, equal to 1½ inches of mercury. The tube No. 2, which was near to the aperture B, but over the inner edge of the seat C, shewed a rise of the mercury of 3-10ths of an inch in the *inner* limb of the tube, and consequently a pressure from the air below it *less* than atmospheric pressure by 3-10ths of an inch,—or a partial vacuum of 3-10ths of an inch of mercury. The tube No. 3, at the same time shewed a similar vacuum of 1-8 of an inch of mercury. The mercury in the tube No. 4, was undisturbed.

The valve with the four tubes in it was now raised above its seat from 1-32nd of an inch until it was 1¼ inches above the seat, by gradations of 1-32nd of an inch each, and the heights of the mercury in the tubes were noted at each step, distinguishing by a P, or a V, whether they shewed pressure from below, or a partial vacuum, and thus a table of five columns was formed. The first column shewed the height of the valve above the seat, and the other four columns, the heights of the mercury in the four tubes, and whether they indicated pressure or vacuum.

This table shewed, that the pressure from the stream below, on No. 1 tube, continued at 1½ inches of mercury, until the valve was raised from its seat to 1-16th of an inch above it; but from that elevation until it was raised to 1¼ inches from the seat, the mercury shewed a gradually diminishing pressure, and at that height the pressure was only 6-10ths of an inch.

No. 2 tube, shewed its greatest degree of *vacuum*, which was one and 8-10ths inches of mercury, when the valve was raised 3-32nds of an inch; from which point, as the valve was further elevated, the vacuum became less, until at a height of 3-8ths there was no vacuum,—the mercury in the two limbs of the tube being at the same level. On raising the valve from 3-8ths to 1¼ inches, this tube shewed an increasing *pressure* from the stream of air below, and at the last named height, the pressure was 4-10ths of an inch of mercury.

The tube No. 3, shewed its greatest degree of vacuum to be 7-20ths of an inch of mercury, and it was when the valve was up 11-32nds of an inch. As the valve was raised higher, the vacuum became less, until at the height of 1½ inches it was nothing.

In tube No. 4, the mercury began to shew a small degree of vacuum when the valve was raised 3-32nds of an inch; when it was up ½ an inch the vacuum was ¼ of an inch, being its greatest degree; from this point the vacuum diminished, and when the valve was 1½ inches high, there was very little difference in the levels of the mercury in the two limbs.

A similar course of experiments was gone through with a valve 8 inches diameter, with some small variations in the results, which were noted in another table; but the only one worth mentioning is, that while the 6-inch valve required a little more than 32 ounces in the scale E, to detach it from its seat, the 8-inch valve required 48 ounces.

From a general view of the results thus

obtained, it appeared that while the valve adhered to the seat, and remained at but a small distance from it, a circular stripe or flat ring of attenuated air was found between the valve and its seat, and near to the aperture B, the air at the same time in the parts further from the aperture becoming more dense, until close to the periphery it became nearly of common atmospheric density; but as the valve was raised, the ring of attenuated air approached the outer part or periphery of the valve.

To find the form and nature of this ring, it now appeared desirable that the different heights of mercury in the same tube, indicating degrees of vacuum should be ascertained at small and equal distances, beginning at the edge of the aperture, and proceeding along a radial line to the periphery of the valve. To accomplish this, a moveable slide was dovetailed into the valve, and in this slide was inserted the lower limb of one of the double syphon tubes with mercury in it as before, as seen in Fig. 2, where the tube is placed over the aperture, and shews a pressure from the compressed air of 1¼ inches of mercury.

This valve being placed on the seat, the slide *f*, *f*, was moved until the tube came over the seat, and the distance of the tube from the edge of the aperture was noted when the mercury first indicated a slight degree of vacuum. From this point the slide, and consequently the tube, was drawn outward 1-32nd of an inch, and the height of the mercury indicating vacuum again noted. In this way, by stages of 1-32nd of an inch each, the tube was drawn to the outer edge or periphery of the valve, and the height of the mercury noted at each stage. The different heights of the mercury in all these stages, with the exact places of the tube at the times, were then marked by dots on paper, and these dots being connected by lines, we obtained the curve represented in Fig. 3. In this diagram, *g* shews the point at which a vacuum was first indicated, and the line from *g* to *h*, represents the increase of the degree of vacuum, until at *h* it is $1\frac{3}{4}$ inches of mercury. From this point the reduction of the degree of vacuum is seen by the curve from *h* to *i*. The straight line *k*, a little lower down, represents the pressure which the mercury shewed when the tube was over the aperture.

The valve was now raised higher from its seat, and the tube moved as before, and data obtained for the formation of other curves. When the valve was 3-16ths above the seat, the tube being placed over the aperture shewed a pressure of only one and 4-10ths of an inch of mercury; but the tube being brought over the seat at a distance of 5-32nds from the edge of the aperture, shewed a vacuum of one and 8-10ths of an inch of mercury. From that point proceeding outward, the vacuum became less.

These experiments shewed, that until the valve was raised to a certain height above its seat, the under side of that part of the valve which was over the aperture, was exposed to a pressure of 1½ inches of mercury more than atmospheric pressure; and the under side of all the rest of the valve, forming an outer stripe or ring, was exposed to a pressure less than atmospheric, or had a partial vacuum varying from one and 8-10ths of an inch of mercury up to atmospheric pressure. The superior pressure against the under side of the center of the valve, must then have been counterbalanced by the inferior pressure against the under side of that part of the valve which is nearer to the periphery,—and more than counterbalanced, for atmospheric pressure on the top of the valve was still so superior as to admit of a weight of 32 ounces being applied, before that pressure could be overcome and the valve raised.

Valves of various smaller sizes were now tried, and it was found that one of $4\frac{1}{4}$ inches diameter, was what may be called the neutral size over an aperture of $2\frac{3}{8}$ diameter; as, when it was balanced it would just adhere to the seat when the air was admitted, but the least weight placed in the scale raised it. Valves of any size smaller than this did not adhere to the seat, and would therefore be proper valves for such a pipe.

A conical valve was now procured, the greatest diameter of which was 6 inches on the upper side, and its least diameter was $2\frac{3}{8}$ inches, the same as the aperture, and its thickness $1\frac{1}{2}$ inches. This valve being fitted into a proper seat, required as many ounces to raise it from its seat as the flat 6-inch valve did. See Fig. 4.

Another conical valve, whose greatest diameter was the same as the flat neutral valve, $4\frac{1}{4}$ inches, its least diameter $2\frac{3}{8}$, and its thickness 3 inches, was fitted like the preceding one, into a seat of equal thickness with itself. This valve however if less than six ounces in weight, was blown off by the blast. And thus it appeared, that a conical valve may be less disposed to adhere to the seat than a flat valve, the diameter of the upper sides of both being the same. See Fig. 5.

A phenomenon, singular in appearance, was exhibited while using these conical valves. It became necessary to fasten a seat with a hollow cone to the flange, and, in the experiments, the issuing stream of air was made to pass between the cone and its seat. But when this seat was liberated from the flange, and the stream of air suffered to flow, one stream rushed between the cone and the seat, and another between the seat and the flange. And thus the seat of the cone was held in its situation by the two streams of air, without being in contact with any thing else.

During the experiments, burning paper was placed on the valves, that the flame and smoke might shew whether there was any atmospheric current rushing down upon it. But it was only at the periphery that the flame was drawn down until it came in contact with the stream of air issuing from under the valve, which cut off the flame as abruptly as it could have been cut through with a knife, apparently from its force and coldness. On the valve the flame blazed in the way in which it ordinarily does, when there is no current of air acting upon it.

In endeavouring to account for these phenomena, it appeared, that the air in the aperture was projected or driven from the aperture, as from a centre, in radiant lines in every direction through enlarging circles, and thus became attenuated as it was thrown off from the centre, in the way that light is diminished according to its distance from its radiating point. For the purpose of ascertaining whether this was a correct view, or not, another experiment was made.

Instead of a circular valve, one of the form of a cross was used, 6 inches in diameter, of which Fig. 6 is a plan. The centre of this cross valve just covered the aperture B, in Fig. 1. And the four arms *l*, *l*, *l*, *l*, extended to the diameter of six inches. The four angular spaces between them left on the seat of the valve were covered with pieces of wood *m*, *m*, *m*, *m*, fitted to the spaces and fastened to the valve seat, leaving the cross valve at liberty, to be raised up between them. By this contrivance, the compressed air, on issuing from the aperture, was confined to four separate streams of equal and uniform breadth, which could not diverge, but passed under the cross until they escaped at the ends of its arms. The tubes with mercury, as in Fig. 1, having been inserted in the arms shewed not more than 1-8th of an inch vacuum in any part of the arms, and less towards their outer extremities. And this small vacuum probably was the result of some air making its way under the angular pieces *m*.

The cross was now raised enough to leave considerable spaces for the stream to expand from its previously compressed state, and to become rarefied, but no greater attenuation was indicated by the mercury. And thus it appeared, that when there was but little space, only 1-32nd of an inch, under the circular valve for the air to be projected into, there was an attenuation, or partial vacuum, of $1\frac{3}{4}$ inches of mercury, but when the cross valve was gradually raised from 1-32nd to the height of half an inch from the seat, and when of course there was ample room for expansion, not more than 1-8th of an inch vacuum was indicated.

From these various phenomena it appeared that the vacuum under the circular valve was produced by the spreading of the air from a smaller to a larger circle, immediately after it left the aperture. For on the air being prevented from spreading by the pieces of wood, *m*, Fig. 6, when fastened to the seat of the valve, the vacuum nearly disappeared in the streams under the arms of the cross valve; but by attaching the angular pieces to the cross valve, and suffering both to rise together, the full vacuum of $1\frac{3}{4}$ reappeared as with the circular valve.

When the circular valve *b*, in Fig. 1, is placed on the seat, there is stagnant atmospheric air within the aperture *b*. On the condensed air being admitted into the pipe *a*, the stagnant air is put into motion, and before it can overcome the inertia of the valve, is forced between the outer parts of the valve and its seat. The air, while being thus forced is, however, compelled to diverge from a circle, whose diameter is $2\frac{3}{8}$ths to one of a larger diameter, and is consequently dilated and attenuated. The impulse given by the compressed air on its first admission, to the stagnant air in the pipe, causes the stagnant air to commence the process, but the compressed air follows instantaneously, and through the force with which it is impelled by the original moving power, is projected under the valve, and there forced to diverge with a velocity proportioned to the amount of the projectile force.

The projectile force acting through the stream of compressed air, and the peculiarly shaped and confined space through which the air is driven, are then the causes of its dilatation, until its degree of rarity is beyond that of the atmosphere, when atmospheric pressure on the upper side of the valve preponderates.

This view will, perhaps, be illustrated, by supposing the compressed air at the edge of the aperture, to be an elastic ring of $2\frac{3}{8}$ths diameter, and that every part of this ring shall be struck with equal force from the centre, in a radiating direction to the circumference. By the time that the ring is projected to a sufficient distance to be a diameter of, say 4 inches, it will be stretched from a smaller to a larger circumference, and every part of the ring will be equally stretched or attenuated. A part of such a ring may be supposed to be represented in Fig. 7. It is not however necessary that the substance projected should be elastic, for if the ring were made of lead, the effect would be the same; or if grains of sand, or small lead shot, could, in like manner, be thrown from a centre, in all directions around, it is clear that as they were removed farther from the centre, the grains or shot would be more distant from each other, or the stream of them would be more attenuated.

By a reference to the curve, Fig. 3, representing the degrees of vacuum, it will be seen that the circle of greatest vacuum is near to the aperture; and it may be inferred, that this fact is opposed to the theory of forced divergence, as on that theory it may be thought that we ought to have the greatest vacuum where the divergence was the greatest, and consequently near to the periphery of the valve. But it should be borne in mind, that the issuing stream of air has to overcome atmospheric resistance; and when, by diverging, it has become rarer than the atmosphere against which it is acting, the momentum requisite to keep it so is soon expended, and the stream under the outer parts of the valve, not having sufficient force to overcome atmospheric resistance from without, yields to it, and is brought to common atmospheric density. If the velocities of the stream under the different parts of the valve could have been ascertained by stages of thirty-second parts of an inch, in the same way that the degrees of vacuum were found by the heights of the mercury, it is presumed, that this point would have been established by experiment, instead of being left dependent on an inference.

The moving of the circle of greatest vacuum outwards, as the valve was elevated, does, however, exhibit evidence of the justness of the inference. When the valve was but little raised, the force of the stream was expended in diverging a part of itself, near to the aperture; but when the valve was considerably raised, the superior density of the stream was not confined to that part immediately over the aperture, but shewed itself also between the valve and a part of its seat. When it was raised half an inch, the same point, *h*, which in Fig. 3, shews the greatest vacuum, indicated a pressure of a quarter of an inch of mercury, while the circle of greatest vacuum, had removed farther from the aperture.

It has been suggested, that the formation of the vacuum may be accounted for from the known tendency of a compressed spring, when liberated, to fly beyond the point at which it will finally settle. But this action of a spring is only one instance of the operation of a general law of nature which is applicable to all bodies. When any body elastic or non-elastic is put in motion, its inertia causes it to continue in motion in the direction in which it has been impelled until its force is expended. The force of a liberated metallic spring is expended in the effort to overcome the tenacity of the substance of which it is composed, while the force of a cannon ball, fired into an earthen bank, is expended on the resistance presented by the earth; but it is projectile force that is expended in both instances.

ADDENDA.

In a short time after the phenomenon of the adherence of the air-valve was observed by Mr. Roberts, he ascertained, by experiment, without knowing that it had been done before, that *water*, when forced through a conical pipe, with considerable velocity, will draw out other water, placed below in an open vessel, if one end of a small tube is inserted in the conical pipe, and the other end is immersed in the water, in the vessel below: thus showing that water, an inelastic fluid, produced the same effect that air did, when rushing out in a stream, confined in a peculiar manner. And at the time this paper was going to press, water was, by pressure from a column of considerable height, made to issue from a pipe with a valve placed over it, similar to what is exhibited in Fig. the 1st, when the valve, instead of being forced off by the issuing stream of water, was found to adhere to the seat, at a small distance from it. And when the apparatus was inverted, and the valve consequently placed below the seat, upon the water being permitted to flow, the valve, instead of obeying the law of gravity and falling by its own weight, or of being driven off by the force of the stream of water, adhered, with considerable firmness, to the seat.

REMARKS

ON

"AN ACCOUNT OF A

FLOATING ISLAND

AT

NEWBURY-PORT:

BY MR. AMOS PETTINGALL."

See Dr. Brewster's Journal, July, 1827; *or Dr. Silliman's Journal, No.* 25.

BY MR. JONATHAN OTLEY.

(Read December 26th, 1828.)

MR. PETTINGALL commences with the observation, "that a few floating reeds, upon a pond, should collect together and adhere with sufficient compactness to sustain small pieces of earth, and decayed shrubs and plants, and thereby exhibit small clumps of vegetables moving on the water, is not surprizing; but that islands of any magnitude should be found in this vagrant state, has ever been considered an object of considerable curiosity." In the sequel he states the island to be situated in a pond, the dimensions of which are not given; but of the island, "its length averages about 140 feet, and its breadth 120, containing nearly half an acre. Its surface is thickly studded with Dogwood, although not a bush of it is found beyond the limits of the island, as though it were an enemy to the water that surrounds it. There are upon it, six large trees, two of which measure in girth three feet and upwards, besides several clusters of willow trees of small growth. These rise and fall with the island. The pond is usually dry during the summer months, and at these seasons the island has been found so low, that you would descend *perceptibly*, in passing to it from the dry bed of the pond. I visited it yesterday, and found it elevated about eighteen inches above the level of the pond's bottom, owing to the rains that have recently fallen."

"The customary rise of the pond in the fall and spring, is about eight feet, although it has been known to rise *twelve:* the island preserves the same elevation above the surface of the water in the different periods of its rise. I have been told, by a man of unequivocal veracity, that he has forced a pole, ten feet long, down through the centre of the island, and with this as far as he could extend with his arm, he has been unable to meet with a solid and permanent bottom. He also informed me, that, when the pond was very high, these large trees standing upon the margin of the island, overhang the water with considerable obliquity; owing, probably, to the roots being brought to a great degree of tension, and preventing the exterior part from rising with the center. It is not entirely detached from the bed of the pond, but seems to be a kind of stratum peeled off, from the solid parts below. In passing across its surface, the whole island is considerably agitated, and presents a waving appearance, like the sea; you are toiling continually to ascend, as though it were a surface of flexible ice."

I think the foregoing account of the floating island of Newbury-Port, is somewhat defective, in not giving the extent of the pond, in which it is situated. I understand from travellers, that what are called ponds in America, are some of them as large as most of our English lakes; but from the description of its situation, and the circumstance of its being dry in the summer months, it may be inferred, that this is not of very large dimensions. It would also have been more satisfactory, had it stated the thickness of the stratum of earth, forming the island, and whether any water is to be found underneath it in the dry season; as well as at what stage of its altitude it was that a pole of ten feet was thrust through it, without meeting with the bottom. Phenomena to which the name of floating islands has been given, are found in different places; and are of various kinds, as owing their origin to different causes: but I think it rather improbable, that any thing deserving the appellation of an island should rest its foundation upon a few floating reeds, as suggested in the preamble before recited.

In the investigation of phenomena of this kind, it may be proper to premise, that most, if not all, living vegetables (even such as grow wholly under water) contain a sufficient quantity of air to cause them to float, when detached from the earth, in which they have fixed their roots. When deprived of the vital principle, and exposed to the action of water, the air is gradually discharged, and the decayed vegetable matter sinks at last to the bottom: but in some cases the dead vegetable fibre is so far comminuted, and its specific gravity so little exceeding that of water, that by help of a small portion of aerial fluid, it remains suspended in the water, forming a pulpy or semifluid mass.

In some places a stratum of peat earth, strongly matted with roots of grass, &c. may be found to rest upon a clayey, gravelly, or even rocky substratum. In rainy seasons the water from higher grounds being filtrated through the more porous soil, insinuates itself underneath, and not easily finding vent, raises up the lighter stratum to a certain extent; while the surrounding parts being of a less extensible nature, or more strongly attached to the substratum, suffer themselves to be overflowed. Of this kind seems to be the floating island of Newbury-Port:* but it appears rather extraordinary, that it should be able to support "six large trees," and yet yield so considerably to the weight of a man.

When a congeries of decayed vegetable matter is deposited in a basin, formed in the earth, from which the water has not sufficient drainage, the lower parts are kept in a spongy state, while the surface is compacted together by the roots of growing vegetables. When, in a rainy season, this spongy matter receives an additional supply of water, it expands and bears up the superior stratum; while the circumjacent more solid parts are overflowed. Of this kind I conceive are some small islands, appearing in wet seasons, in a meadow on the margin of Grasmere Lake.

* Something of a similar nature are the *bursts* upon the sides of mountains, where the peat earth rests upon a declivity with dryer grounds above it.

When a basin of the kind above-mentioned is only of small extent, it generally has a deep pool or a slow stream of water passing through it: or else a deep pond in its centre; if the basin extends over a whole valley, it commonly forms a lake. The lake has its margins shallow, and gradually sloping down into deeper water; the narrow pool or pond formed in swampy ground, being less affected by waves, has its banks more abrupt and perpendicular; from which the pulpy matter underneath is gradually washed out, leaving the more compact and grassy surface stratum to be supported solely by the water; by the rising and falling and agitation of which, a portion of light earth is sometimes torn off, and floated about at the mercy of the winds. Of this kind is a floating island on a pond, called Priest-Pot, lying at a small distance from the head of Esthwaite-Water, near Hawkshead. This pond is of an oval form, covering an extent of two or three acres, in the middle of a spongy meadow; the island is about 24 yards in length, by 6 in breadth, and has probably been at some time of larger dimensions: it supports several trees of alder and willow of considerable size, and its surface is covered with rough grass, chiefly of the *carex* or sedge kind.

Vegetable matter in a decomposing state under water, always generates a quantity of air; and if the decomposition takes place where it is much diluted with water, the air makes its escape almost as quickly as evolved; but when the stratum of peat is more compact, the air is longer retained. Peat earth is frequently found to exist in the bottom of lakes; but I am not exactly of opinion with Dr. M'Culloch, that it has been originally formed under water. I am more inclined to think, that the water has subsequently been brought to cover it. However, when a stratum of peat earth under water becomes so much impregnated with air, as to render the whole mass of less specific gravity than water, and having little adherence to the clayey substratum, it may emerge to the surface, and continue buoyant till the quantity of air is reduced. Of this kind I consider the floating island in Derwent Lake, which has been observed on the surface, seven times since the year 1800. Rising generally towards the latter end of a warm summer; and dissappearing before the close of autumn. A more particular description of this island, with my observations thereon, has for some time been before the public.

JONATHAN OTLEY.

Keswick, Decr. 8th, 1828.

SUMMARY

OF THE

RAIN, &c. AT GENEVA,

AND

AT THE ELEVATED STATION

OF THE

PASS OF GREAT ST. BERNARD,

FOR A SERIES OF YEARS,

From the Bibliothèque Universelle, for March, 1828,

WITH OBSERVATIONS ON THE SAME.

BY JOHN DALTON, F.R.S.

(Read Oct. 17, 1828.)

GENEVA is situated in Latitude 46° 12′ N. and about 6° E. Longitude from London; its elevation is 450 yards above the sea: its distance from the Atlantic is 360 miles, and from the Mediterranean 160 miles. The high mountains of the Alps form an immense amphitheatre from Geneva, extending more than 100 miles to the eastward.

The mountain Great St. Bernard is one of the higher Alps, over which is a public road or pass into Italy. It is about 60 miles to the S. E.

of Geneva, There is an Inn or Convent at the Pass, for the convenience of travellers; in summer the road is practicable without much danger; in winter it is impassable; in spring and autumn the traveller is often in danger, and sometimes perishes, by the sudden and unexpected falls of snow, by the descent of masses of ice and snow from the sides of the mountains, or by extreme cold. The height of the Pass above the level of the sea is 2720 yards, which is between two and three times the height of Snowdon.

The scientific gentlemen of Geneva have very laudably availed themselves of the opportunity which the situation at St. Bernard afforded them, of ascertaining the meteorological phenomena at the latter place. A series of daily observations on the Barometer, Thermometer, Hygrometer, quantity of Rain, &c. at the convent, has been made for the last 10 years; and a summary of the observations was given in the Bibliothèque Universelle for March last, together with those of the like kind made at Geneva for 32 years.

The observations at Geneva do not appear to present any thing of peculiar interest. The *annual means* and the *general means* for the period of 32 years are all that are given in the

the summary; the mean temperature is 49°$\frac{2}{3}$; this is low, considering the Latitude; but the elevation of the place, its inland situation, and its proximity to the Alps, conspire to reduce the temperature. The annual rain is 30.7 inches (English measure.)

The observations on St. Bernard are given much more in detail. The monthly means for each year are given, and the averages for each month, for the Barometer, Thermometer, Hygrometer and Rain; from which general averages for the whole 10 years are obtained.

It appears that the mean height of the Barometer at St. Bernard is nearly 22 English inches: the mean temperature is 30°$\frac{1}{4}$ Fahrenheit; the mean quantity of Rain and Snow is 60 inches annually; and the mean state of the hygrometer (Saussure's) is 83°$\frac{1}{2}$, only $\frac{1}{2}$ a degree more moist than at Geneva.

From the accounts furnished, I have calculated the mean monthly averages of rain at St. Bernard for 12 years,* and find them as under:—

* Since the paper was read, I have incorporated two more years rain; namely, 1828 and 1829, into the averages for St. Bernard: so that the Table here presented is for twelve years.

	RAIN. Inches.	
January	5.95	Wet period.
February	6.85	
March	6.98	
April	5.65	
May	2.76	Dry period.
June	3.20	
July	4.16	
August	4.31	
September	4.79	Average period.
October	5.47	
November	4.51	
December	5.42	
	60.05	

The most striking circumstance with regard to the rain is the great excess of it, compared with that at Geneva. Though the average rain at Geneva for the 32 years, was 30 inches annually; yet the average quantity for the same 10 years as those which were observed at St. Bernard, was only 26 inches annually. So that the rain at St. Bernard is nearly 2½ times as much as that at Geneva.

From the observations made in Great Britain, it appears to be an established fact, that more rain falls in the hilly parts of the country than in the plain; but it also appears, that the quantity of rain in a low situation is greater than that in an elevated situation in the vicinity. Hence it might have been imagined, that the great elevation of St. Bernard would reduce the quantity of rain below that at the plain of Geneva. The fact however appears to be far otherwise; and it may demand a little consideration.

High mountains produce rain, I think unquestionably from their obstructing the horizontal currents of the air, and causing them to ascend into the higher regions of the atmosphere, by which airs of different temperatures are mixed together. Now it is well known, that two portions of air, saturated with vapour at their respective temperatures, when mixed together, are incapable of retaining the whole of the vapour: a part of it is precipitated in the form of a cloud or of rain. This is the case too if the portions of air be *under* saturation, within certain limits.

The physical principles on which the above statement is supported, are, 1st.—When two portions of air of different temperatures are mixed, the temperature of the mixture is the ***arithmetical*** mean of the two temperatures: 2nd.—When two portions of air saturated with vapour are mixed together, the quantity of vapour found in the mixture must also be the *arithmetical* mean of the quantities found in each; but it is only a quantity proportional to the *geometrical* mean that can be supported in the state of vapour, by the mean temperature: and as the geometrical mean is always less than the arithmetical mean, the excess must needs be precipitated.

This accounts for more rain falling in mountainous countries than on plains;—but the question at present is, how does it happen that more falls on great elevations amongst the Alps than on the plains below.

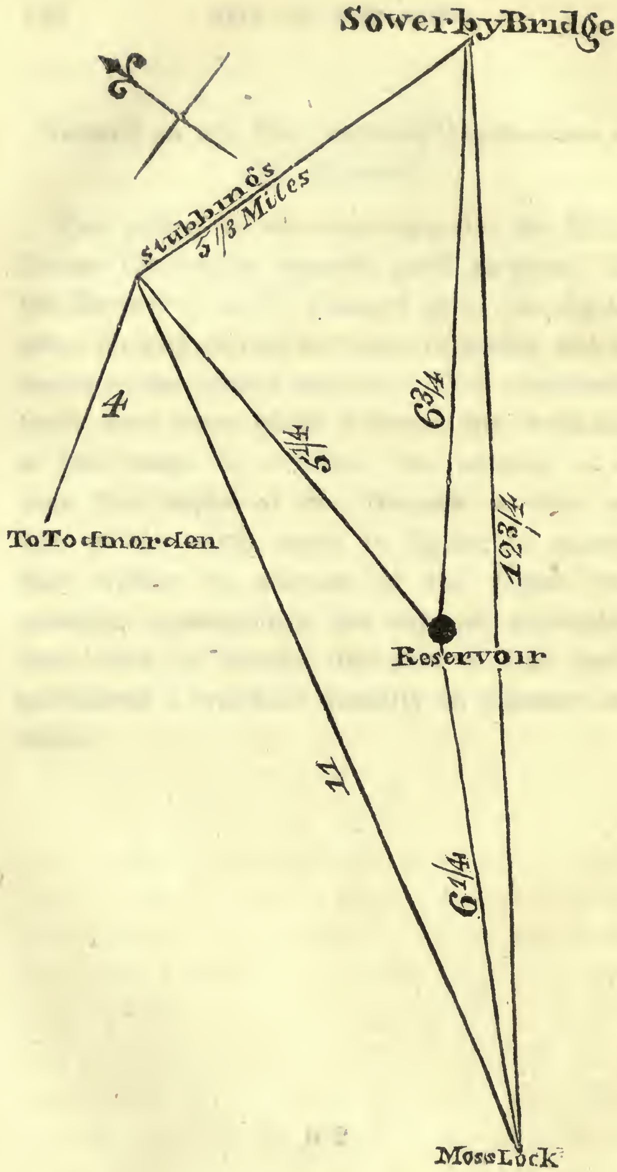
To this it may be answered, that the Pass on St. Bernard is not the *highest* point of land in the vicinity, but rather the *lowest*, at least of the ridge over which the road passes. Hence the fall of rain, even in that elevated station is still under the influence of superior currents of air over the higher summits, and may still exceed in quantity what falls on the distant plains. The quantity of rain which falls at the *foot* of the mountain, either on the Swiss or Italian side, I have little doubt, will be found to be still greater than that which falls at the Hospital as related above. It would be very desirable however to ascertain the fact; and more especially on the side of Italy, where the greatest quantity may be expected from the west winds.

How far a ridge of hills extends its influence over a plain in regard to the weight of water precipitated, it is not easy to form a decided opinion. It can scarcely be doubted, that the greatest influence will be confined to two or three miles from the ridge; but some influence may be found in all probability at the distance of ten or even twenty miles or more, according to the greater or less elevation of the mountains.

It is matter of curious observation, that the falls at St. Bernard for the four first months of the year, are all greater than for any other months; and that the falls for the next four months, are all less than for any other, thus leaving the four last months to yield about the average monthly quantity. A series of twelve years can scarcely leave a doubt as to the general accuracy of the fact. Possibly there may be some uncertainty as to quantity in regard to the snow; the observers estimate one foot in depth of snow to be equal to one inch in depth of rain; and the weight of the falls for six or eight months in the year, is snow.

Remark on the Barometrical Observations at St. Bernard.

The author of the summary in the Bibliothèque Universelle remarks *with surprise,* that the Barometer at St. Bernard gives the *highest* mean for August and the hottest months, and the *lowest* in the coldest months. This observation must have been made without due reflection, as the cause is evident; the stratum of air from the height of St. Bernard to the surface of the earth, must be lighter in summer than winter on account of the higher temperature; consequently the superior atmosphere must then be heavier, the sum of both being considered a constant quantity in summer and winter.



Sowerby Bridge
Stubbings
5 1/3 Miles
4
To Todmorden
5 1/4
6 3/4
12 3/4
Reservoir
11
6 1/4
Moss Lock

ACCOUNT OF THE RAIN

Which Fell on different Places

ON THE

LINE OF THE ROCHDALE CANAL;

The Rain Gages being kept under the superintendence of Mr. ROBERT MATTHEWS, Engineer for the Canal:

COMMUNICATED BY THOMAS FLEMING, ESQ.
One of the Committee of the Canal.

(Read January 22, 1830.)

IT may be proper to give some account of the situation of the gages.—Blackstone-Edge gage is kept at the reservoir of the canal, near the summit of the mountain, separating Lancashire from Yorkshire; around it is an extensive area of moderate elevation, which supplies the waters of the reservoir. The gage is 1500 feet above the sea, and is probably the highest of any that has been kept in Great Britain, at least for any regular series of years. The mountain range is from the S. E. to the N. W.; and is consequently flanked on the Lancashire side, by the S. W. wind, and on the Yorkshire side, by the N. E. wind, which two may be called the wet and dry winds of this country. The gage at Moss Lock is near Rochdale, about 6 miles to the S. W. of that on Blackstone-Edge, and is 510 feet above the sea; and the country to the S. W. is flat. The gage at Sowerby Bridge is about 7 miles to the N. E. of that on Blackstone-Edge, at considerable distance from the mountain, and is 268 feet above the sea. The gage at Stubbins, is about 5 miles to the N. of the line of the other three gages; it is 364 feet above the sea, and is situated in a deep, narrow, and tortuous valley, surrounded by mountains from 300 to 1200 feet of elevation above its level. It is nearly in the middle, between Sowerby Bridge and Todmorden, and about 6 miles from the latter place.

RAIN ON THE SUMMIT OF BLACKSTONE-EDGE,

1500 *feet above the Sea.*

	1819.	1820.	1821.	1822.	1823.	1824.
	Inch.	*Inch.*	*Inch.*	*Inch.*	*Inch.*	*Inch.*
January	2.9	1.5	2.05	1.12		1.47
February.....	3.5	.75	.30	4.00		0.82
March	3.2	2.5	2.48	2.8		2.25
April	3.2	1.0	2.26	4.45		2.75
May	3.5	2.9	1.56	1.4		1.25
June............	0.25	2.0	.70	1.03		3.09
July	2.22	1.2	.90	7.94		0.95
August........	1.5	5.62	3.0	2.7		2.48
September. .	2.0	1.5	4.5	2.0		4.5
October.......	1.25	2.95	5.0	2.2		6.275
November...	3.0	1.39	4.1	2.1		5.53
December...	2.1	4.60	6.0	2.0		6.83
	28.62	27.91	32.85	33.74	35.68	38.195

Rain in 1825.

	Summit of Blackstone-edge.	Moss Lock, near Rochdale.	Stubbins, near Todmorden.
	Inch.	*Inch.*	*Inch.*
January	1.40	1.55	2.46
February.....	1.20	1.05	1.66
March.........	0.55	.80	.97
April	1.69	.95	1.85
May............	4.45	3.35	4.35
June...........	2.77	2.75	2.30
July	0.61	.80	.20
August........	4.69	3.25	4.09
September...	2.60	1.60	1.71
October.......	3.05	4.10	5.42
November...	5.48	4.78	6.20
December ...	2.43	1.80	3.63
	30.92	26.78	34.84

RAIN IN 1826.

	Summit of Blackstone-edge.	Moss Lock, near Rochdale:	Stubbins, near Todmorden.
	Inch.	*Inch.*	*Inch.*
January	.65	0.85	.70
February	2.93	2.20	4.82
March	0.40	0.05	0.15
April	0.85	1.30	1.62
May	0.70	0.15	0.50
June	0.40	0.55	0.65
July	2.25	2.75	1.22
August........	2.15	1.95	1.84
September...	3.04	1.55	2.76
October.......	3.12	3.35	3.30
November ...	1.20	1.80	2.15
December ...	2.00	2.20	2.85
	19.69	18.70	22.56

Rain in 1827.

	Summit of Blackstone-edge.	Moss Lock, near Rochdale.	Stubbins, near Todmorden.
	Inch.	*Inch.*	*Inch.*
January	1.92	2.15	2.83
February	1.62	0.60	1.51
March	3.40	3.30	5.98
April	0.67	0.40	1.36
May	1.50	1.05	1.95
June...........	1.93	2.05	1.62
July	1.80	1.15	1.80
August........	2.59	2.75	2.86
September. .	2.28	2.05	2.25
October..	4.94	3.55	5.05
November...	2.80	3.15	3.20
December....	5.48	4.15	6.32
	30.93	26.35	36.73

Rain in 1828.

	Summit of Blackstone-edge.	Moss Lock, near Rochdale.	Stubbins, near Todmorden.	Sowerby Bridge.
	Inch.	*Inch.*	*Inch.*	*Inch.*
January......	2.70	2.70	3.07	3.31
February.....	1.33	1.25	1.52	1.11
March........	1.42	0.90	1.68	1.29
April.....	3.93	3.50	3.50	3.51
May...........	0.60	1.00	1.35	1.40
June...........	2.18	1.15	1.36	1.09
July	8.01	9.40	6.66	6.75
August........	4.69	4.95	3.65	2.71
September..	2.35	2.40	2.85	2.07
October.......	2.25	2.40	2.67	1.15
November...	4.84	4.30	4.30	3.82
December....	4.97	4.45	7.14	3.07
	39.27	38.40	39.75	31.28

RAIN IN 1829.

	Summit of Blackstone-edge.	Moss Lock near Rochdale.	Stubbins, near Todmorden.	Sowerby Bridge.
	Inch.	*Inch.*	*Inch.*	*Inch.*
January	——	.31	.31	.42
February	1.39	1.20	1.22	1.53
March	.13	.05	.15	.14
April	2.86	2.70	4.18	3.74
May	.55	.70	.80	.21
June.	2.41	2.20	2.60	3.10
July	2.75	2.75	3.59	3.35
August	8.10	6.15	8.24	7.10
September. .	3.91	4.45	3.97	3.05
October	2.39	2.75	2.99	1.79
November. .	2.40	2.35	1.84	1.49
December . .	0.53	.35	0.47	.48
	27.42	25.96	30.36	26.40

Mean monthly and annual Rain, at the four places on an average.—For Blackstone-Edge the monthly means are for 10 years, and the annual for 11 years; for Moss Lock, and Stubbins for 5 years; and for Sowerby Bridge for 2 years.

	Blackstone-edge.	Moss Lock.	Stubbins.	Sowerby Bridge.
	Inch.	*Inch.*	*Inch.*	*Inch.*
January	1.57	1.51	1.87	1.86
February	1.77	1.26	2.15	1.32
March	1.91	1.02	1.79	.71
April	2.37	1.77	2.50	3.62
May	1.84	1.25	1.79	.80
June	1.67	1.74	1.71	2.09
July	2.86	3.37	2.69	5.05
August	3.74	3.81	4.14	4.90
September...	2.87	2.41	2.71	2.56
October	3.34	3.23	3.89	1.47
November...	3.28	3.28	3.54	2.65
December....	3.69	2.59	4.08	1.77
	31.38	27.24	32.83	28.80

The establishment of these rain gages was no doubt undertaken with reference to the interest of the canal proprietors. However, if they had been influenced solely by a desire to promote the science of Meteorology, they could scarcely have devised places for Rain gages, better adapted to that purpose.

Four gages are placed within the compass of 12 miles distance. They include all the varieties of situation that could be expected or desired in that compass.

Moss Lock gage has an extensive plane to the west and south of it, from which quarters the rains chiefly proceed. The currents of air come to it steady and unruffled by obstructions. There is no peculiar incentive to rain. Soon after, the currents gradually ascend and get into a colder region; a portion of vapour is precipitated in rain, and this cause of precipitation continues to act till the air arrives at the summit, when it is a maximum; but the falls there are not a maximum, because a current of air moving at 30 or 40 miles per hour, will not suffer the rain to reach the ground at the same place where it is formed: on the descent of the hill and probably about the foot of it, the heaviest rain will fall.

This the gage at Stubbins sufficiently demonstrates. In every year the greatest fall of rain was found there; and it is also shewn in a general way by a comparison of the monthly quantities in the different gages.

The gage at Sowerby Bridge shews, that the effect of the current of air and vapour having passed the mountain, has nearly if not entirely ceased. Though two years are too short a period to establish a fair comparison, yet it appears very probable, that the rain at the Bridge will not maintain, on an average, a quantity equal to what will be found at Moss Lock. Thus the increased quantity of rain given out on passing the hill has gradually fallen off from some point of the descent, where it was a maximum, till in about 6 miles it is reduced to that at Moss Lock, or probably rather below it.

It is to be hoped that the Committee will continue their observations, when future experience will either confirm or correct the preceding inferences deduced from too limited a series.

ON THE

INSTINCTS OF BIRDS.

BY JOHN BLACKWALL, F.L.S.

(Read January 23rd, 1829.)

THE manners and economy of the inferior orders of animals form one of the most interesting subjects of investigation which can engage the attention of the philosophic naturalist. An acquaintance with this important but greatly neglected branch of zoology, conduces to the correction of numerous erroneous opinions and groundless prejudices, and opens an inexhaustible source of valuable information and rational amusement. It throws also much light on the operations of that mysterious agency, which regulates those actions of animated beings that, although attended with consciousness, do not result from observation, instruction, experience, or reflection, and have, therefore, generally been termed *instinctive actions*.

When we consider how many creatures are

objects of superstitious dread or veneration, and what multitudes, even in this enlightened age and country, are sacrificed annually to mistaken notions of their mischievous properties, reason and humanity are alike shocked; and we deeply deplore the prevalence of errors, which the zealous promulgation of more correct ideas and liberal sentiments can alone effectually remedy.

That useful bird, the white owl, which, on account of the great number of mice it destroys, ought to be carefully protected by the farmer, is frequently looked upon with terror as a forerunner of death, which it is supposed to announce by its loud and dissonant screams; and a small coleopterous insect, the *Anobium tessellatum* of entomologists, has obtained the appellation of death-watch, from a fancied connexion between the ticking sound it produces, and that awful event. The raven and magpie are imagined, by persons of weak intellects and timid dispositions, to prognosticate evil, and this notion has been extended and perpetuated by the allusions made to it in numerous legendary tales, and in the writings of our poets. To take the life of the swallow or martin, or to disturb their nests, is regarded as an unlucky event, portending disaster to the unfeeling aggressor; and the redbreast and wren owe much of their security to popular prepossessions equally without any rational foundation. Many birds, which subsist almost entirely on insects, as the cuckoo, redstart, and flycatcher, are shot by ignorant gardeners and nurserymen, indiscriminately with those species which feed principally on the seeds of plants and other vegetable productions. The goatsucker and the hedgehog are falsely accused of sucking the teats of animals, and a price, usually paid out of the parish rates, is still given for the latter in many parts of England;* and those beautiful and harmless reptiles, the common snake and blind-worm, are destroyed without pity, upon the groundless supposition that they are venomous.

These are a few instances only, selected from many that have fallen under my own observation, of the pernicious consequences which result from an ignorance of that useful portion of natural history, which at present engages our consideration.

* Sixpence a head, I am well informed, has been recently obtained for hedgehogs in this parish. Now it is truly disgraceful, that any portion of the public money should be expended to encourage the destruction of an inoffensive animal, which derives its support from insects and vegetables, because, in the absurd opinion of the vulgar, it is injurious to cattle.

We will now proceed to notice briefly, some of the numerous advantages to be derived from a successful cultivation of this delightful study; and a correction of the above mentioned errors and abuses, with the needless waste of life which it would prevent, is not among the least of them. For the preservation of our persons and property from those creatures, by which they are liable to be injured; for the best methods of promoting the increase, improving the condition, and effecting the subjection of such as contribute to our benefit or amusement; and for the skilful management of our valuable reclaimed and domestic animals, which supply us with so many comforts and luxuries, we must depend, in a great measure, upon our knowledge of their habits, manners, and propensities. To this knowledge also, the practical physiologist is indebted for a means of enlarging his acquaintance with the phenomena of life; the scientific naturalist, and particularly the ornithologist, for an excellent mode of distinguishing species, under circumstances in which the ordinary rules for determining them are of little or no avail; and the physico-theologist, for a more comprehensive view of the power, wisdom, and goodness of the Creator, as manifested in his living works.

Having thus succinctly adverted to the great importance of accurate information in this extensive department of zoology, I shall now limit my remarks exclusively to the feathered tribes, and whoever attentively considers the diversified operations of the various active powers, with which the interesting beings that compose this pleasing division of the animal kingdom are endowed, cannot fail to receive a high degree of mental gratification.

It frequently happens, that the experienced observer is enabled to discriminate birds with the utmost certainty by their notes, manner of flight, or some other peculiarity, when he has no opportunity of procuring specimens of them, or of ascertaining the colours of their plumage. Indeed, in this last particular, distinct species, as the willow wrens, several of the larks, finches, &c., so nearly resemble each other; and individuals of the same species, as many of the falcons, gulls, sandpipers, ducks, &c., are so very dissimilar, and vary so greatly with age, change of season, and other circumstances, that colour cannot always be relied upon as affording sufficient evidence of specific identity. A much surer criterion will be found in the uniformity so conspicuous in the manners and economy of birds of the same kind; a coincidence which can only be accounted for by supposing that their actions are instinctive. That this is actually the case I shall attempt to shew, though it must be admitted that they are occasionally modified, in a considerable degree, by the exercise of the intellectual faculties.

I will not occupy the time of the Society in examining the many vague and contradictory opinions, which have been entertained with regard to the nature of instinct by the various authors who have written on the subject, being convinced, that they are purely speculative, and tend to retard, rather than advance the progress of science. We must not, however, pass unnoticed, the sophistical doctrine, so ingeniously maintained by Dr. Darwin, in Zoonomia,* that what is usually termed instinct in animals, has reference to the powers of intellect solely; since the feathered tribes, notwithstanding the highly curious and unequivocal examples of instinctive actions which they exhibit, have furnished him with some of his most plausible arguments in support of it.

Depending on the assertion of Kircher,† that young nightingales, when hatched by other birds, never sing till they are instructed, and

* See the Section on Instinct. Vol. 1.

† De Musurgia. Cap. de Lusciniis.

confiding in the remark of Jonston,* that the nightingales which visit Scotland have not the same harmony as those of Italy; Dr. Darwin was hastily led to conclude, that the songs of birds, in general, are artificial. Having observed also, that poultry readily obey their usual summons to be fed; and that young ducks hatched under the domestic hen soon appear to understand her calls; and giving credit to the mistaken idea, that wagtails and hedge warblers feed the young cuckoos they bring up, long after they leave the nest, whenever they hear their cuckooing, which, on the authority Linnæus,† he states to be their cry of hunger, he was induced to adopt the same opinion respecting their calls. Now, whether the song of the nightingale results from education, as Kircher maintains, or whether it is wholly independent of tuition, I have never had any direct means of deciding, as the bird is only an accidental visitor in this part of the kingdom. From unexceptionable experiments, however, made with the greatest care, on several other species of British singing birds, I have no hesitation in affirming, that the peculiar song of each is the natural consequence of an instinctive impulse

* Pennant's British Zoology.

† Systema Naturæ.

combined with a suitable state of the vocal organs. This latter condition deserves particular attention, for it is a fact, which has been very generally overlooked, that most of our songsters are absolutely unable to continue their melodious strains beyond the latter end of July, or the beginning of August; the strenuous but unavailing exertions they make to prolong them, sufficiently proving their silence not to be a matter of choice, but of necessity. This circumstance, together with the extreme difficulty they experience in recommencing their songs in spring, clearly demonstrates, that their delightful warblings depend upon the energy of those muscles which contribute to form the voice; an energy which appears to be influenced chiefly by food, temperature, and the exercise of the reproductive functions; for by due attention to the regulation of these particulars, the vocal powers of caged birds may be called into action, or circumscribed at pleasure. Of this, persons who have the management of breeding canaries may easily satisfy themselves; and female birds, in a state of captivity, when brought into high condition, are known, occasionally, to assume the song of the male. That Jonston must have been deceived in supposing he heard the nightingale in Scotland is evident, as it is well known, that this warbler is never found north of the

Tweed, in Great Britain. It has been ascertained too, contrary to the opinion of Linnæus, that young cuckoos, before they come to maturity, utter a feeble cry only, they cannot, therefore, acquire the calls of their species while they remain in this country. No wonder then, that the conclusion Dr. Darwin arrived at was erroneous, when the premises on which his reasoning is grounded are so inaccurate.

It is not, let me remark, intended to insinuate, that birds are incapable of attaining any knowledge of each others' notes, since our domestic fowls, in many instances, are certainly enabled, by observation and experience, to connect vocal sounds with the ideas they are designed to convey.* The martin also, readily learns to distinguish the swallow's call of alarm; and the ringed plover, sanderling, and dunlin, when associated together, evince, by the promptitude and exactness with which they perform their various aërial evolutions, that they comprehend one general signal. All that is meant to be insisted upon is, that the notes peculiar to every

* When our domestic cock gives notice to his mates, that he has discovered some choice morsel of food, the turkey hens always hasten to secure the delicacy, which the gallant chanticleer suffers them to take, even out of his beak, without the least molestation.

species, in a state of nature, are instinctive. This I have endeavoured to prove in an essay, read before the Society in 1822, and printed in the fourth volume of the new series of Memoirs, by shewing, that even such individuals as are brought up in situations where they have no opportunity of being instructed in their appropriate notes, do, nevertheless, utter them naturally.

The pairing of wild birds, and the period at which they prepare to perpetuate their species are determined, according to Dr. Darwin, by the acquired knowledge, that their joint labour is necessary to procure sustenance for a numerous progeny, and that the mild temperature of the atmosphere in spring is suitable for hatching their eggs, and for producing a plentiful supply of that nourishment which is wanted for their young. This opinion he attempts to support by the fact, that poultry which have an abundance of food throughout the year, and are protected from the inclemency of the weather, lay their eggs at any season and never pair. But it should be recollected that this is not the case with pigeons placed under similar circumstances, which do pair, though they produce only two young ones at a time; and that the pheasant among our naturalized, and the black grouse among our native birds, though they have both large families to provide for, are, in their wild state, polygamous. Indeed, it is evident from the anatomical and physiological researches of Mr. John Hunter and Dr. Jenner, that the sexual connexions of birds, and the season at which they breed, depend upon certain conditions of their organization, and not upon any information derived from experience or instruction.

The propensity to propagate their species, in this class of animals, is well known to be of periodical occurrence; and dissection clearly proves that it is always accompanied by a very perceptible alteration in the reproductive system. Besides, reclaimed birds, under the influence of a plentiful supply of nourishing food, shelter from the inclemency of the weather, and the various stimuli with which domestication is usually attended, may be kept in this state of sexual excitation for several years, with comparatively, little interruption. A check to the greatly increased activity of the reproductive powers, so induced, is speedily given, however, by a diminution of sustenance and exposure to cold, at the same time also, a visible change takes place in the physical condition of the organs of reproduction.

In the selection of their mates, the feathered tribes are undoubtedly governed by instinct, as there is reason to believe that different species in a state of nature never pair together, however near their affinity or general resemblance may be. The rook is not observed to breed with the crow, the titlark with the lesser fieldlark or rocklark, the sedge warbler with the reed wren, or the cole titmouse with the marsh titmouse. Now, were every individual left to the unrestrained exercise of its own discretion in a matter of such essential importance, the utmost confusion might be expected to ensue; an unprolific, hybrid progeny would be speedily produced, and the total extinction of many species might be the ultimate consequence. But the allwise Author of nature has not suffered the reproduction of his creatures to be liable to such a contingency, but has implanted in the mind of each a powerful predisposition to form sexual unions with its own kind exclusively. Thus the evils which would unavoidably result from the indiscriminate intercourse of various species are effectually prevented.

It must be admitted that an intermixture of distinct species does sometimes occur among our domesticated birds; but this deviation from their ordinary instinct is rare, and may, with great probability, be ascribed to a change in their organization, occasioned by the artificial mode of life to which they have been subjected. Now as it is a maxim in physiology, that the exercise of every animal function is dependant upon its appropriate material organ, any display of new instinctive phenomena, in birds which have long been under the controul of man, may also be attributed to the operation of the same physical cause. The singular propensity of the cropper pigeon to inflate its craw with air, and the still more remarkable disposition of the tumbler to turn itself over backwards when on wing, which are permanent characters in these varieties of of the rock-dove, being transmitted by generation, can be satisfactorily accounted for on the foregoing supposition only. How unsafe it must always be to draw general conclusions from the habits and propensities of domestic fowls alone, whose instincts are frequently changed, almost as much as their plumage, by the unnatural state in which they are kept, needs scarcely to be insisted on.

Dr. Darwin conjectures that birds learn how to build their nests from observing those in which they are educated, and from their knowledge of such things as are most agreeable to their touch in respect to warmth, cleanliness, and stability; but the undeniable fact, that birds when taken very young, even before they can see, and brought up in confinement, do sometimes construct nests, is alone sufficient to refute this opinion.

The sparrow-hawk and kestril often make use of the deserted habitation of the magpie as a receptacle for their eggs, and the sparrow frequently takes forcible possession of the rustic dwelling of the house-martin for the same purpose. Why then are they never known to build nests similar to those which they thus appropriate to themselves? and why does not the cuckoo, which is always brought up in the nest of some other bird, construct one itself?* The reason is obvious: the act of nidification is not regulated by observation or instruction, but is under the immediate direction of instinct.

Guided by this mysterious power, individuals of the same species, under like circumstances, always adhere to the same stile of architecture. Thus, some of the smaller birds, which produce a large number of eggs, constantly make the

* I have pointed out the errors into which Dr. Darwin has fallen in his remarks on the cuckoo, in my observations on that bird, printed in the fourth volume of the new series of the Society's Memoirs.

entrance to their nests very narrow, and line the interior with an abundance of such materials as conduct heat slowly; while the ring-dove, which lays two eggs only, forms so slight a structure, that they may frequently be seen through it. The partridge, land-rail, and those birds whose young are able to run almost as soon as they are hatched, generally give themselves very little trouble in providing nests for their progeny; and some species of waterfowl do not make any, but deposit their eggs in the crevices, and on the projecting shelves and ledges of lofty rocks, or upon the bare ground. The sociable grosbeak builds in society under a common roof. The pensile, Abyssinian, and Phillippine grosbeaks construct curious nests which they suspend from the slender twigs of trees, particularly such as grow over water; by this means, securing their offspring from the predatory attacks of their numerous enemies; and the taylor-bird frames its temporary abode by sewing two leaves together with the flexible fibres of plants, and lining the cavity with the lightest and softest animal or vegetable down.

It is true, that in preparing their nests, birds occasionally accommodate themselves to some circumstances, and take advantage of others, in a manner which seems to indicate a large share of intelligence. The wren, for example, usually adapts the exterior of its compact fabric, to the situation in which it is placed. When built against a haystack, hay is almost invariably made use of, and green mosses, or withered leaves and fern are employed, as green or the various shades of brown prevail in its vicinity. Nor let it be imagined, that these substances, which from their contiguity are often most easily procured, are selected as a matter of convenience merely; for I have known this minute bird bring long pieces of straw from a considerable distance with much toil, and with incredible perseverance mould the stubborn material to its purpose, solely because its colour approached that of a garden wall, a hole in which, occasioned by the giving way of a loose brick, it had chosen to place its nest in.

A lady who keeps canaries was obliged to separate a young brood from their parents, having observed that the male bird stripped off the soft feathers from their necks and wings for the purpose of lining a newly constructed nest with them, notwithstanding a supply of old feathers had been put into the cage. From this remarkable fact, for which I am indebted to Dr. W. Henry, it is evident, that canaries do not collect materials for their nests indiscriminately, but that they make a selection, in which they are directed by powers of a higher order than those of a merely instinctive character.

Mr. White, in his Natural History of Selborne, page 59, informs us, that in Sussex, where there are very few towers and steeples, the jackdaw builds annually under-ground, in deserted rabbit burrows. The same author remarks also, p. 175-6, that many sand-martins nestle and breed in the scaffold-holes of the back-wall of William of Wykeham's stables, which stands in a very sequestered enclosure, facing a large and beautiful lake, near the town of Bishop's Waltham in Hampshire; and some birds, as already represented, frequently spare their own labour by taking possession of the nests of others.

In these instances there certainly appears to be a great display of sagacity; yet there are facts which seem to render it doubtful whether the feathered tribes are capable of deriving much benefit from experience, or of exercising any remarkable degree of intelligence. Thus, birds when engaged in the performance of their parental duties, expose themselves without hesitation to dangers, which at another period they would carefully avoid. Many species also, while under the incitement of appetite, are readily snared by the most simple contrivances directly after witnessing the capture of their companions; and rooks continue to breed in those rookeries, where the greater part of their young is destroyed every spring.* For three successive seasons a pair of redstarts persisted in making their nest in the upper part of our pump, on that end of the lever which is connected with the rod of the piston, and, of course, always had it disturbed when that engine was used. Mr. White observes too,† that in the neighbourhood of Selborne, martins build year by year, in the corners of the windows of a house without eaves, situated in an exposed district; and as the corners of these windows are too shallow to protect the nests from injury, they are washed down every hard rain; yet the birds drudge on to no purpose from summer to summer, without changing their aspect or house.

These actions, it cannot be denied, seem to indicate a more limited degree of sagacity in birds than might be inferred from those immediately preceding them. This apparent contra-

* I am assured by T. Legh, Esq., that many thousands of young rooks are shot every breeding season in his extensive rookery, at Lyme Park, in Cheshire.

† Natural History of Selborne, p. 160.

diction, however, may be easily reconciled, by admitting, what in all probability will be thought sufficiently obvious, that the dictates of the understanding are frequently too feeble to resist the powerful influence of instinctive impulse. Several examples illustrative of this view of the subject will be found interspersed through the remainder of the essay. There is not any necessity therefore for entering into a more detailed consideration of it here.

After the business of nidification is completed, parturition commences, which is succeeded by incubation, and as birds will frequently continue to deposit their eggs in the same nest, though all except one or two should be removed as fast as they are laid, or exchanged for others of a different size and colour; and as they will sometimes, after having produced their appointed number, sit upon a single egg, on the eggs of other birds introduced for the purpose of experiment, on artificial ones of chalk, or even upon stones of any irregular figure; it is plain that the acts of depositing and incubating their eggs can be ascribed to instinct only. The parental offices of birds to their young, are also regulated by instinctive feeling, as is evinced by their bestowing the same attention on the offspring of other species, when committed to their care, as they do upon their own. Thus the titlark and hedge-warbler manifest the warmest attachment to the young cuckoos, their foster nurslings, though they suffer their own progeny, ejected by the intruders, to perish from neglect within a short distance of the nest; and this affection continues with little diminution till their suppositious offspring have nearly attained their full growth. Yet, under other circumstances, they would pursue and persecute them with the utmost rancour.

The instinctive nature of these actions is likewise satisfactorily established by the fact, that birds when taken very young, and brought up in confinement, not only construct nests occasionally, but also lay their eggs in them, which they will sit upon till hatched, should they prove prolific, and will then carefully attend to the young. An anecdote or two serving more fully to corroborate the opinion advanced above, will not, it is hoped, be unacceptable.

In the beginning of May, 1812, having found a buzzard's nest containing a single egg, the egg was taken and a light-coloured stone substituted for it, over which a rat-trap was set. The buzzard sat upon the trap a day and night, when it was discovered, that the iron ring which confined the spring had not been withdrawn. The ring was then removed, and on visiting the nest afterwards, the female was found caught by the feet. This change of character in so watchful and quicksighted a bird as the buzzard, is certainly very surprising, and must baffle every attempt to connect it with any intellectual process.

A highly interesting anecdote illustrative of the attachment of the raven to its eggs, is thus admirably related by Mr. White.* "In the centre of a grove there stood an oak, which, though shapely and tall on the whole, bulged out into a large excrescence about the middle of the stem. On this a pair of ravens had fixed their residence for such a series of years, that the oak was distinguished by the title of the Raven-tree. Many were the attempts of the neighbouring youths to get at this eyry: the difficulty whetted their inclinations, and each was ambitious of surmounting the arduous task. But, when they arrived at the swelling, it jutted out so in their way and was so far beyond their grasp, that the most daring lads were awed, and acknowledged the undertaking to be too hazardous. So the ravens built on, nest upon nest, in perfect

* Natural History of Selborne, p. 6.

security, till the fatal day arrived in which the wood was to be levelled. It was in the month of February, when those birds usually sit. The saw was applied to the but, the wedges were inserted into the opening, the woods echoed to the heavy blows of the beetle or mallet, the tree nodded to its fall; but still the dam sat on. At last, when it gave way, the bird was flung from her nest; and though her parental affection deserved a better fate, was whipped down by the twigs, which brought her dead to the ground."

That ardent affection which most birds feel for their young seems to awaken their dormant energies and to inspire them with a degree of courage and address that is called forth on no other occasion. Nor is the violence of this affection, to use the language of Mr. White, more wonderful than the shortness of its duration. Thus, every hen is in her turn the virago of the yard in proportion to the helplessness of her brood, and will fly in the face of a dog or a sow in defence of those chickens which in a few weeks she will drive before her with relentless cruelty. The partridge will tumble along before a sportsman, in order to draw away the dogs from her helpless covey; and a very exact observer (the Rev. John White) has remarked, that a pair of ravens nesting in the rock of Gibraltar would suffer no vulture or eagle to rest near their station, but would drive them from the hill with amazing fury; and that even the blue thrush, at the season of breeding, would dart out from the clefts of the rocks to chase away the kestril or the sparrow-hawk. Indeed, so regardless of danger are some species while their nestlings are small, that I have known the redbreast, whinchat, great titmouse, &c., when introduced to their nests, after having been forcibly removed to a distance from their unfledged young, remain quietly upon them as if they had not been molested. Yet, although this instinct, the transient effects of which depend most likely on a temporary excitation of the parental feelings by some physical modification of the corporeal organs, thus for a time powerfully predominates, its manifestations are nevertheless frequently influenced by the active cooperation of the intellectual faculties, as in the following examples.

"The flycatcher," says Mr. White,* "builds every year in the vines that grow on the walls of my house. A pair of these little birds had one year inadvertently placed their nest on a

* Natural History of Selborne, p. 151.

naked bough, perhaps in a shady time, not being aware of the inconvenience that followed. But a hot sunny season coming on, before the brood was half fledged, the reflection of the wall became insupportable, and must inevitably have destroyed the tender young, had not affection suggested an expedient, and prompted the parent birds to hover over the nest all the hotter hours, while with wings expanded, and mouths gaping for breath, they screned off the heat from their suffering offspring."

"A further instance," continues the same author,* "I once saw of notable sagacity in a willow-wren, which had built in a bank in my fields. This bird a friend and myself had observed as she sat in her nest; but were particularly careful not to disturb her, though we saw she eyed us with some degree of jealousy. Some days after as we passed that way we were desirous of remarking how this brood went on: but no nest could be found, till I happened to take up a large bundle of long green moss as it were carelessly thrown over the nest, in order to dodge the eye of any impertinent intruder."

Actuated by a similar motive, old birds, which have had their young frequently handled, use

* Natural History of Selborne, p. 151.

every art to induce them to desert the nest as early as possible; and I have known the redbreast, on such occasions, take off her nestlings long before they could make the slightest use of their wings. That this mode of proceeding must be referred to intelligence, cannot, I think, be doubted, as the danger of allowing their progeny to remain in a state of insecurity is evidently perceived, and the surest means of avoiding it is deliberately adopted in consequence.

Many birds, under particular circumstances, manifest a natural inclination to fight. This disposition is remarkably conspicuous in the ruff, the quail, and the domestic cock. That the feeling is innate and dependant upon organization, is clearly proved by the established fact, that careful breeding and training, exercise a powerful influence upon the last species with regard to this propensity.

Dr. Darwin states that pheasants and partridges teach their young to select and take up their food; and hence he seems disposed to infer, that all birds receive instruction in these particulars; but that they are impelled by instinct, independently of education and experience, to exercise the functions of their various corporeal organs, whose structure is admirably adapted to the several offices they have to perform, admits of such numerous and decisive proofs, that it is truly amazing how a person of so much observation as Darwin could so entirely overlook them.

Those young birds which do not acquire the use of their eyes for several days after they are hatched, open their mouths for food as soon as they are stimulated by hunger, not only when the old ones bring it to them, but when anything approaches the nest. Nestlings too as soon as they are grown sufficiently large, mute over the edge of the nest, though the parent birds carefully convey to a distance whatever drops from them that they do not succeed in ejecting. These actions occur also when birds are brought up in confinement, however young they may be when taken, and therefore must be instinctive.

The common duck has its toes connected by a strong membrane which enables it to swim with facility; and the young of this species, though hatched under birds which instinctively avoid committing themselves to the water, rush to it with avidity almost as soon as they are extricated from the shell, notwithstanding the utmost exertions of the foster mother to divert them from it.

Young swifts are rarely, if ever, observed to perch; and as they cannot easily be distinguished from old ones by their flight, they must display a considerable command of wing the very first time they quit the nest.

Many of the gallinaceous tribe scratch up the earth with their feet in search of food; and they will frequently repeat this action, when fed on a stone or boarded floor, where it can answer no useful purpose. Now, as they do not correct this error, it is plain, that the action itself does not originate in observation, experience, or reflection. Neither can it be attributed to education; nor is this particular misapplication of it to be ascribed to the force of habit, as it may often be observed in very young chickens, which have never associated with others of their kind. But, what is still more to the purpose, and, indeed, decisive of the general question, even pheasants and partridges, as well as ducks, chickens, turkeys, and guinea fowls, which have been hatched by artificial heat, possess the instincts peculiar to their respective species, as I have had several opportunities of ascertaining. How young birds by their struggles in the egg can at all facilitate the use of their legs, as Dr. Darwin conjectures, is to me inconceiveable, especially when the position in which they lie is taken into consideration. But even supposing this notion to be correct, it does not in the least affect the instinctiveness of the act; unless we conclude with Darwin, that instinct has nothing to do with any of those actions which result from the repeated efforts of the muscles under the conduct of the sensations or desires; an opinion so manifestly erroneous that it does not require a formal refutation.

The habits and manners of birds are sometimes so greatly modified by the exercise of the intellectual faculties, that, in many cases, it becomes extremely difficult, if not impossible, to determine what is due to their influence; but that no small portion of intelligence is exhibited in the following instances will scarcely be denied.

The white-headed eagle and several of the gulls, which prey upon the finny inhabitants of the waters, frequently save themselves the trouble of fishing, by robbing their more expert and less powerful congeners of the fruits of their industry, occasionally compelling the objects of their violence, even to disgorge their undigested food.*

* John James Audubon, Esq., the celebrated author of the splendid work on American Ornithology now publishing in London, informs me that when the white-headed eagle pursues

The pied and yellow wagtails run close to the legs and noses of cattle which are grazing, in pursuit of the insects disturbed by them: the same motive also, induces these and many other birds to follow the husbandman when he is busy with the plough or harrow; and the redbreast attends the gardener in his labours, and seizes the worms which he turns up with his spade.

Mr. White states* that the great titmouse, in severe weather, frequents houses; and in deep snows, as it hangs with its back downwards, draws straws lengthwise from the eaves of those buildings which are thatched, in order to pull out the flies that are concealed between them; and I have seen hooded crows, on the eastern coast of Ireland, after many unavailing efforts to break with their beaks some of the muscles on which they were feeding, fly with them to a great height in the air, and by letting them fall

the fish-hawk or asprey, for the purpose of depriving it of its prey, it does not attempt to rise above it, as stated by Wilson in his Ornithology of the United States of America, vol. iv. p. 90—1, but following it closely, urges it from below to as great an elevation as possible, in order that when the hawk quits its prize, it may be able to secure the fish before it reaches the water. As the fish-hawks are not capable of contending individually with the white-headed eagle, they sometimes combine together in considerable numbers to expel the marauder from their haunts.

* Natural History of Selborne, p. 106.

on the stony beach, fracture their shells, and thus get possession of the contents. Perhaps it would not be easy to select a more striking example of intelligence among the feathered tribes than this, where on one expedient proving unsuccessful, after a sufficient trial had been made of it, another was immediately resorted to.

Chickens in their early attempts to catch flies and other winged insects shew little or no address, but repeated failures teach them to use more circumspection; and they soon learn to distinguish between an active, vigilant prey, and the inanimate substances on which they likewise feed. This cautiousness of proceeding is clearly the effect of information obtained by experience, and affords an example of an instinctive power being excited to activity by the intellect; but a still more extraordinary instance of acquired knowledge is given by Montagu in the supplement to the Ornithological Dictionary. This gentleman observed two crows by the sea shore employed in removing some small fish (the refuse of a fisherman's net) from the edge of the flowing tide. They carried them one by one just above high water mark, and there deposited them under large stones or broken fragments of rocks, after having amply satisfied the immediate calls of hunger. Now it must be conceded that these birds were aware that the advancing flood would sweep away their prize, unless they conveyed it beyond the limit of its usual rise, or their conduct is quite inexplicable. It is equally plain that this knowledge, in the practical application of which they manifested so much foresight and sagacity, could be derived from observation and experience only; because, if it originated in a blind instinct, it would be common to every individual of the species, and consequently often displayed; whereas, although I have seen hundreds of crows feeding in situations similar to that above described, I never perceived any of them resort to this effectual means of preserving their prey from the encroaching waters, and I believe the instance related by Montagu is solitary in the records of ornithology.

This propensity to hide the food it cannot devour is not however peculiar to the crow. I have noticed it in the raven and magpie; and rooks, in the autumn, frequently bury acorns in the earth, probably with the intention of having recourse to them when their wants are more urgent; but sometimes forgetting where they have concealed them, they germinate, and not unfrequently excite surprise by the singularity of the situations in which they grow, far distant from any trees by which they could have been produced, and where it is very evident that they have not been planted by man.

It may be proper to remark here, in order to obviate misapprehension, that notwithstanding the circumstances attending this seemingly provident mode of securing a supply of food against a future occasion, sometimes afford unequivocal evidence of an intelligent and discerning agent, yet the act of hiding is induced by a purely instinctive propensity. This will be admitted by every one who considers that the species of birds which are remarkable for this peculiarity, practise it, however well they may be fed, when brought up from the nest in a state of domestication.

In addition to the numerous proofs of the intelligence of birds already given, I may mention their susceptibility of receiving instruction by education. Thus, eagles, falcons, and hawks have been trained to limit the effects of their instinctive propensity to kill, to a particular species of game; and to return to the call and line of the falconer after having struck down the quarry. The cormorant too was formerly employed with success in taking fish. Here then, not only great attachment to their keepers and much docility of disposition are evinced by birds which are naturally wild and voracious, but a considerable share of memory is displayed, and a surprising degree of controul exercised over some of their most active instincts.

Several birds of the finch, grosbeak, and warbler genera acquire the art of piping long and difficult tunes with facility and precision; and it is well known that some of the parrots, and also the jay, starling, jackdaw, and magpie readily learn to pronounce single words, and even short sentences, with tolerable exactness. Yet, although I have excellent opportunities of observing the last species, and have been almost in the daily practice of investigating its habits, I never knew it display any unusual exertion of its capacity for imitation in a state of nature, though when domesticated, it appears to have this faculty more highly developed than almost any other British bird.

The congregating of gregarious birds, which takes place in autumn, when they have finished breeding, is perhaps intended to promote their mutual security, as they are much less liable to be surprised by enemies when associated together in large numbers, than they are when separate. What tends to strengthen this opinion is the fact, that some species provide for the general safety, by appointing sentinels to give notice of approaching danger. This social disposition, which (with the well known exception of rooks) usually continues no longer than the next pairing season, seems, from the uniformity of the actions that result from it, to be of instinctive origin; though it certainly would be difficult to bring any direct proof that such is the case.

In treating of the migration of birds, Dr. Darwin observes, that as all species are capable of remaining throughout the year in those countries in which they were bred, any departure from them must be unnecessary, and therefore cannot be instinctive. This reasoning, however, is extremely fallacious, inasmuch as it restricts the operations of instinct solely to what is necessary; whereas we have seen that the singing of birds, and the practice of concealing their superfluous food, though not absolutely indispensable, are, nevertheless, decidedly instinctive. It is moreover built on the gratuitous assumption, that several of the periodical summer birds, as the swallow, flycatcher, cuckoo, goatsucker, &c., which feed almost entirely on insects, and consequently would not be able to procure a sufficient supply of nourishment in the winter months, have the property of passing the cold season in a state of torpidity; an hypothesis directly at variance with well established facts. Indeed, how very defective and unsatisfactory the arguments advanced in support of the hybernating system are, does not require insisting upon, as those who have considered the subject impartially must be well aware, that they are almost wholly founded on the hearsay reports of ignorant and credulous persons.

The history of the cuckoo proves, most incontrovertibly, that the propensity to migrate in this species is instinctive, since nearly all the young ones brought up annually in the north of Europe, quit it without receiving the least instruction that such a proceeding is requisite, and without any guide to direct them in their novel undertaking. But I forbear to dwell on the instincts of this extraordinary bird, partly on account of their being so very anomalous, but chiefly because I have considered them at length on a former occasion.* The highly curious fact, that the swallow, house-martin, sand-martin, and puffin sometimes leave their last hatched broods to die of hunger in the nest, in order to accompany their species in their autumnal migration, is alone sufficient to establish the instinctiveness of that

* See observations conducive towards a more complete History of the Cuckoo, printed in the fourth volume of the new series of the Society's Memoirs.

inclination which can thus overcome their parental affection, a feeling so energetic as frequently to counteract one of the most powerful laws of nature, self-preservation. No theory, in short, which is not founded on the opinion that birds of passage, in undertaking their annual journeys, are influenced by an instinctive desire to migrate, liable to be called into action by various exciting causes, can satisfactorily account for the remarkable phenomena which result from this periodical disposition to wander.

The certainty with which the carrier pigeon directs its course towards its accustomed home, from distant places where it has never been before, after every precaution has been taken in its conveyance to prevent it from obtaining any knowledge of the way by observation, must, as well as the act of migration, to which it bears a striking resemblance, be likewise attributed to instinct.

It appears then, from the foregoing observations, that the principal actions of birds, though liable to be considerably modified by the operations of the intellectual powers and changes of organization, as well as by various external circumstances, are contrary to the opinion of Dr. Darwin, decidedly of instinctive origin.

Many additional arguments might be advanced, and a multitude of highly respectable authorities quoted in support of this doctrine, but conceiving that sufficient evidence has been already produced, I shall only add, that I am not aware of any serious objection which can be urged against it.

PHYSIOLOGICAL INVESTIGATIONS

ARISING FROM THE

MECHANICAL EFFECTS

OF

ATMOSPHERICAL PRESSURE

ON THE

ANIMAL FRAME.

BY JOHN DALTON, F.R.S.

(Read January 8th, 1830.)

A PERIOD of a century and a half has elapsed since the inventions of the Barometer and the Air-pump. In this time the weight of the atmosphere, its elasticity, its specific gravity, and many other properties have been ascertained experimentally with almost mathematical precision. The *weight* of the atmosphere, that quality we have more particularly to consider in the present essay, is not constantly the same, as is proved by the rising and falling of the barometer. It varies in this part of the earth from 1-12th to 1-15th of the whole weight at certain times; but those variations are gradual, so that it requires some days or weeks before the weight passes from one extreme to the other. On an average the weight or pressure of the atmosphere amounts to $14\frac{1}{2}$ lbs. on each square inch of surface of the earth; and, as fluids press equally in all directions, every square inch of surface, whatever may be its position, must be subject to the same pressure. The surface of the human body, as well as that of animals in general, has to sustain this pressure; and it will be found by calculation, that the whole surface of a middle sized person will have to support from 15 to 20 tons of pressure, all acting inwards and having no other mechanical tendency than that of squeezing or compressing the materials of which the body is composed into a less compass.

The above is a statement of facts, all of which I believe are allowed to be incontrovertible. But a very difficult question arises out of them,—how is it that the animal frame is utterly insensible of the whole, or of any part of this enormous pressure upon it. In ordinary we feel no pressure on the surface of our bodies, either external or internal, neither when the barometer is stationary nor when it is in a most fluctuating state. I have never met with a satisfactory answer to this question, and I doubt whether such a one has ever been given; yet it must be allowed to be one of importance, both as it affects the physiology of the Animal and Vegetable kingdoms.

Having had occasion for a few weeks past to ruminate on this subject, some new views have occurred to me; and it is the object of the present essay to unfold them, in order to elucidate the phenomena arising from aerial pressure on the animal economy more especially.

It is pretty well known that the *specific gravity* of living men in general, is less than that of water.* Mr. Robertson, formerly librarian to the Royal Society, procured an apparatus for the purpose of determining the specific gravity of the human body. He chose ten men promiscuously for the purpose. Of these, three were found very nearly of the same weight as water, one being a little heavier, and the other two a little lighter than water: two others were found only about .8 the weight of water; but the other five were of intermediate specific gravities. The average of the ten was—height, 5ft. $6\frac{2}{3}$ins.—weight, 146lbs—specific gravity, .891—bulk, 2.618 cubic feet. From this I think we may safely infer that the body of a full grown living man, when plunged

* Phil. Trans. vol. 50.—Hutton's Dict. Sp. Gr.

over head in water, will be found upon the average to be nearly .9 the weight of an equal bulk of water.

It is remarkable that all the component parts of the animal frame, at least of the human subject, are severally specifically heavier than the whole body, with the exception of air.—Bone, muscular flesh, blood, membrane, &c. are all heavier than water: animal fat is perhaps the lightest of the components, but even this is heavier specifically than the whole man upon the average.—Bone from the leg of a calf I found to be 1.24 specific gravity. The lean of beef (raw) I found 1.045 specific gravity.—Blood is from 1.03 to 1.05 specific gravity according to circumstances:—on the whole, the solid and liquid parts of the body, examined after life is extinct, would appear on an average to be somewhere about 5 per cent heavier than water.

That part of the volume of man which is exclusively occupied by air, and which may therefore be considered as adding nothing material to the weight of the body, consists of the air tubes and air cells of the lungs, the trachea or wind-pipe, the mouth and other appendages. It is not easy to ascertain the medium volume of air in the lungs of any individual. Messrs. Allen and Pepys found the air remaining in the lungs of a man after death somewhat exceeded 100 cubic inches. I found formerly that after a full inspiration I could blow out 200 cubic inches of air from my lungs, but was then quite exhausted. My ordinary inspirations and expirations amounted each to about 30 cubic inches.*

Judging from the above facts and considerations I should be disposed to conclude that the medium volume of air in the lungs of a middle sized person would not be less, but rather more than 100 cubic inches. Besides the lungs there are no other receptacles for air, I believe, in the body except the stomach and bowels, which are occasionally more or less inflated with portions of air either from the atmosphere or from other sources. If we allow 150 cubic inches for the volume of air, contained in the whole man when entirely immersed in water, it will be as fair an estimate perhaps as can be made. But it may be imagined by some that the whole substance of the body is pervious to air; that the skin, the flesh, the blood and even the bones, may be imbued with air, somewhat in the same manner that water is, and yet have no cavities or cells in which the air is collected into a visible volume. Whether such an idea has ever been entertained

* Memoirs, vol. II. (new series, p. 26.)

or discussed I am not aware; but I presume no one has succeeded in determining either the nature or the quantity of the air so enveloped in the system. We shall now examine how far such a notion is countenanced by the preceding statement of facts.

According to the preceding table of Robertson the average bulk of the ten men was 2.618 cubic feet = 4500 cubic inches nearly; but of this volume 150 inches according to the above estimate were air, and the remainder 4350 inches were solid and liquid parts of the body. Now the average specific gravity of those parts of the body has been estimated above at 1.05 when examined as dead matter: this would make their weight equal to 4567 cubic inches of water; whereas it was found by actual weighing to be 146lbs. as per table = 4044 cubic inches; hence the observed weight was less than the calculated weight, a portion equal to the weight of 523 cubic inches of water, or more than 1-9th of the whole weight of the body.

Here is a discrepance that demands an investigation. Can Robertson's table of the specific gravities of men give too low an estimate? This is not likely; every one knows that the human subject generally floats in water till the lungs become filled with that element, a proof that the body is lighter than water. And many persons are observed to swim with the whole head constantly above the surface of the water.

Have we overrated the specific gravities of the component parts of the body? I think not: bones, and flesh, and blood are certainly all heavier than water, some more some less.

Has the capacity of the lungs for air been underrated? I cannot imagine that any one will contend that the lungs of a middle sized man will hold at a medium state of inflation six times the volume of air we have assigned.

Upon the whole I am inclined to believe the true explanation of the difficulty will be found in this, that the whole substance of the body is pervious to air, and that a considerable portion of it constantly exists in the body during life, subject to increase and diminution according to the pressure of the atmosphere, in the same manner as it exists in water: and further, that when life is extinct, this air in some degree escapes and renders the parts specifically heavier than when the vital functions were in a state of activity.

The facts that water absorbs air of all kinds, that the quantity of air absorbed is proportional to the pressure and density of the gas, whether it be alone or mixed with other gases, and that certain laws of equilibrium take place, by which water acquires that state in which it is disposed neither to give out nor take in any more gas, have been abundantly proved by Dr. Henry and myself. M. Saussure has shewn the like for other liquids, and for a great number of solid bodies. It may be seen too in my Chemistry, vol. 1st, page 236, that a bladder, which is generally considered as an animal membrane least pervious to air, may be filled with one gas, and being some time exposed to the atmosphere, it will be found to continue full blown as at first, but the contents will be chiefly atmospheric air. Messrs. Allen and Pepys in their ingenious and excellent essays on respiration, have proved that when a Guinea pig, or a pigeon is confined, for an hour, more or less, in a mixture of hydrogen and oxygen gases in proportion as 78 to 22, a large portion of azotic gas is found in the residue and an equal portion of hydrogen disappears. They ascribe this change to effects of respiration; but it appears to me more probably due to the principle we are advocating; namely to the egress of azotic from the whole body and the ingress of hydrogen in lieu of it, in consequence of withdrawing the external pressure of the former and substituting that of the latter.

When the palm of the hand is placed over the top of the receiver of an air-pump, and the air is exhausted, the pressure of the air on the outside is scarcely felt, but the inside is swollen and feels as if it was drawn or sucked into the receiver. Thus the sensation is on the inside and not without; the reason is, there is no change of pressure on the outside; but there is within, and the consequence is a tendency of the air in the hand to escape into the receiver, which occasions the pain and swelling. It is thus also that the issuing of blood in the surgical operation of cupping is effected.

Though it does not seem of much consequence what the pressure of the air may be on the animal frame, within certain limits, yet sudden changes must always be accompanied with uneasy sensation. Climbing mountains, or ascending in a balloon, removes a part of the atmospheric pressure from the body; this causes the air in the body to tend outwards, and sometimes occasions bleedings. To supply oxygen to the lungs a greater volume of air must be breathed, and this seems to produce an acceleration of the pulse. On the other hand, by descending 30 or 40 feet deep into water in a diving bell, the pressure of the air upon the body is increased inwards; pains in the ears are felt from the difficulty of suddenly restoring a disturbed equilibrium; but if the descent is slow and interrupted, time is given for the air to enter the pores, and the pain is less sensible. To what limit warm blooded animals could bear the rarefaction of air so as to subsist, has not, that I am aware of, been determined with much precision. Ascents in balloons have been made till the atmospheric pressure was reduced more than one half. Formerly I found that a mouse could subsist in air of $\frac{1}{4}$ of atmospheric density, and seemed not to have suffered much; but upon reducing the density below $\frac{1}{4}$, the animal was convulsed and expired immediately, notwithstanding the air was instantly admitted.

If the view we have expanded in this essay, in regard to the action of aerial pressure on the animal frame, be correct, it may be inferred, that the pressure admits of great latitude; perhaps an animal could subsist under the pressure of $\frac{1}{2}$ an atmosphere, or of 3 or 4, or more atmospheres. The uneasiness and danger would be found in the quick transition. If time is allowed for the air to enter the body, and to escape from it, the transition is gradual, and the sensation arising from it imperceptible.

The animal economy would be adapted to it, like as in the transition from a cold to a warm climate. It may hereafter be found, what length of time is sufficient to adjust the equilibrium; and whether this subject is any way connected with certain diseased states of the body. As far as regards the absolute pressure on the body, and our insensibility of it generally, this question will be met by the argument, that the air within the body, by its elasticity, sustains a corresponding pressure from without; but this only accounts for our alleviation from a small fractional part of the whole exterior pressure. The greater part must still be supported by the body; and we must have recourse to the great incompressibility of matter, to account for our insensibility of pressure. Canton found that water, pressed by one atmosphere more than ordinary, only exhibited a reduction of 1-21740th part of the whole; if the same rate, applied to the compression of the human body, the reduction or compression of the size of a man, 4500 cubic inches, would only be 1-5th of a cubic inch, for the weight of an additional atmosphere. Now as the body consists of solids and liquids of almost incompressible matter, and there is only a small part of the volume consisting of elastic fluid that is compressible, no material change of volume can take place, but on the sudden transition from one atmospheric pressure to another; and unless a change of volume take place, we cannot feel any pressure, either inward or outward. The phenomena of the water-hammer shew, that the particles of water are hard, as they strike each other like flint and steel; and it is exceedingly probable that other bodies, solids as well as liquids, are constituted in like manner. A general pressure on the system then, only increases in a small degree the attraction of the ultimate particles, and it is met by a corresponding increase of repulsion from the atmosphere of heat; so that the system remains, as nearly as possible the same, and unaffected by such pressure.

I can scarcely forbear observing on the present occasion the absurdity of those who remark, that all people might swim, and that it is only from fear or ignorance of the art, that some fail in the attempt. When we see that some persons are heavier than water, and others only .8 of that weight, it would be just as plausible for a piece of deal to upbraid a piece of *lignum vitæ* with the inability to swim, from fear, or from want of skill in the art, which the deal considered of easy acquisition.

A SERIES OF EXPERIMENTS

ON THE

QUANTITY OF FOOD,

Taken by a Person in Health,

COMPARED WITH THE QUANTITY OF THE DIFFERENT SECRETIONS DURING THE SAME PERIOD;

WITH

CHEMICAL REMARKS ON THE SEVERAL ARTICLES.

BY JOHN DALTON, F.R.S.

(Read March 5th, 1830.)

DURING my residence at Kendal, nearly 40 years ago, I had at one time an inclination to the study of medicine, with a view to future practice in the medical profession. It was on this account chiefly, but partly from my own personal interest in knowing the causes of disease and of health, that I was prompted to make such investigations into the animal economy as my circumstances and situation at the time would allow. I had met with some account of Sanctorius' weighing chair and of his finding the quantity of insensible perspiration compared with the quantity of aliment; and it occurred to me that the differences of constitution and of climate might occasion very considerable modifications which it would be desirable to ascertain. The following train of experiments were accordingly instituted for the purpose.

It may be proper to observe that my habits, daily occupations, and manner of living were exceedingly regular; my health during the time was uniform and good; and that the weight of my person has never been subject to much change since grown to maturity.

The first series of experiments was made in the month of March, for 14 days successively. I had three meals each day, breakfast between 7 and 8 in the morning, dinner between 12 and 1, and supper about 7 in the evening; except on two days in which I had tea to breakfast, and again in the afternoon. The usual breakfasts consisted of boiled milk with bread and a little oat-meal, and suppers were of the same, with the addition of bread, cheese and beer. The dinners consisted of butcher's meat, potatoes, pies, puddings, bread and cheese. About one-third part of the bread used consisted of a thin oat-cake common in Westmoreland and Cumberland. I drank no water, seldom wine, and no fermented liquor, except common table beer.

The weight of the individual articles were taken at each meal separately, and entered in a journal, distinguishing fluids from solids.

It will be quite unnecessary to give a detail of the articles and their weights just as they were entered in the journal, because it would be found little more than a repetition of names and quantities. A very short time shewed that the daily demand for food, both solid and fluid, was nearly uniform as to quantity; and that the supply might have been made absolutely so without any inconvenience. But the diurnal evacuations were by no means so near uniformity.

An aggregate of the articles of food consumed in the fourteen days is given below; and the mean proportions for one day are also given, neglecting small fractions.

	Consumption in 14 Days.	Consumption in 1 Day.
Bread, ...	163 *oz. avoird.*	12 *oz. avoird.*
Oat-cake, ...	79	6
Oat-meal, ...	12	1
Butcher's Meat,	54½	4
Potatoes, ...	130	9
Pastry, ...	55	4
Cheese, ...	32	2
Total,	525½ Solids	38 Solids

	Consumption in 14 Days. oz. avoird.	Consumption in 1 Day. oz. avoird.
Milk,	435½	31
Beer,	230	16½
Tea,	76	5½
Total,	741½ Fluids	53 Fluids

Thus it appears that the average daily consumption of solid and fluid articles was 91 ounces, or a little short of 6lbs. avoirdupoise. The distribution of the aliments into solids and fluids as above is evidently to be understood in a popular sense; as it is well known that all the solids contain a greater or less portion of water, and all the fluids a greater or less portion of solid matter. In fact water must be considered as the basis of all the fluids.

During all this period, a daily register was kept of the urinary secretion and of the evacuation of the bowels. The total quantity of urine for the 14 days was 680 ounces; and the total quantity of fæces was 68 ounces.

The daily average was, Urine 48½ oz.—Fæces 5 oz., a greater disproportion than was anticipated, being nearly in the ratio of 10 to 1; they amount together to 53½ oz. or 3½ lbs. nearly; but the quantity of food taken daily was 91 ounces; there remains a balance 37¼ oz. to be accounted for, which must have been spent by the insensible perspiration from the skin, and that from the lungs conjointly, on the supposition that the weight of the body remained stationary.

I have already observed that the daily evacuations were not so nearly uniform as was the quantity of food. The urinary secretion was greatest when tea was substituted for milk, and on one day was 15 oz. above par. On another occasion finding a greater defalcation than I had before observed, I could discover no cause for it, unless a tea-spoonful or two of vinegar taken at dinner could account for it. To be satisfied of this, I took, some days after, an ounce of vinegar in four equal portions during one day; and the effect was a greater diminution of urine on that day than on any other during the two weeks, the quantity being 15 oz. below the average, and 4 oz. less than on the former day when vinegar had been taken. There did not appear to be any increased effect in any other secretion as a compensation for this diminution.

In order to try the effects of different seasons I resumed these investigations in the month of

June the same year, and continued them for one week successively. The results were what might have been anticipated nearly. A less consumption of solids, and a greater consumption of fluids, were observed. The evacuations were somewhat diminished, and the insensible perspiration was increased.

The following were the results:

	Solids Consumed in 7 Days.		Fluids Consumed in 7 Days.
	oz.		oz.
	236	....	391
per Day	34	....	56=90 Total,

being four ounces per day less in solids, and three ounces in fluids than in the former trial.

The daily averages in the evacuations were, Urine 42 ounces;—Fæces $4\frac{1}{3}$ ounces; leaving a balance of nearly 44 ounces for the daily loss by perspiration, being an excess of about 6 oz. above that in the former season, or one-sixth more, owing no doubt to the higher temperature of the weather.

Another trial of one week's continuance was made in September the same year. The results were so nearly alike to those in June as to render an enunciation of them unnecessary. The daily consumption of food was 93½ ounces, and the perspiration one half of that quantity.

I may now be allowed perhaps to subjoin one day's experience of the effect that taking a large dose of carbonate of potash (Salt of Tartar) has upon the secretions. This was suggested by a similar experiment made by Dr. Alexander, and published by him in a small volume of medical essays. His results I do not at present recollect; but my notes at the time imply that I expected the alkali to act as a diuretic. My experiment was made on a fine day at the end of March after the two weeks series; the thermometer ranged from 40° to 60°. In the morning I had a bason of tea prepared for breakfast, with the usual quantity of sugar and cream; into this I infused four drams avoird. (100 grains) of dry carbonate of potash; after it was dissolved I proceeded to my repast as usual, apprehending the diluted alkali would be so far qualified in its taste by the sugar, as to be rendered tolerably palatable, but in this I was mistaken; the nausea was unbearable; and I was obliged to drink it off as fast as I could, and then eat my toast to an additional cup in the ordinary way. This done, I felt nothing amiss; took a moderate walk and returned. On sitting down I perceived small drops of fluid on the backs of my hands, without any sensation of heat above common. My appetite was rather keener than usual during the day, and I felt uncommon agility in the evening. The secretion by the kidnies, was not at all disturbed. But on retiring to bed I burst into a profuse perspiration, which continued through the night, and was felt in degree during the succeeding night. By taking care, the effects went off without any perceptible detriment.

Being satisfied, by the preceding trains of experiments, that no more information was to be expected in this way than was already acquired, I varied the process, with a view to obtain the quantity of perspiration, and the circumstances attending it more directly. I procured a weighing beam, by which I could weigh my body, so that the beam would turn with one ounce. Dividing the day into periods of four hours in the forenoon, four or five hours in the afternoon, and nine hours in the night, or from ten o'clock at night to seven in the morning, I endeavoured to find the perspiration corresponding to those periods respectively.

My method of proceeding was, to weigh myself directly after breakfast, and again before dinner, observing neither to take or part with any thing during the interim, besides what was lost by insensible perspiration; the difference in the weights, in this case, was the loss by perspiration. The same procedure was adopted in the afternoon and in the night.

I continued this train of experiments for three weeks in November, the same year. I then took the aggregate of the morning observations, next that of the afternoon observations, and lastly that of the night observations, and divided each of those three aggregates by the number of hours in the several periods, in order to find the hourly perspiration in each period, apprehending there might be some differences owing to the time of day, or being awake or in sleep.

The mean hourly losses, by perspiration, were as under:

	Oz. avoird.
Morning	1.8
Afternoon	1.67
Night....................	1.5

During twelve days of this period, I kept an account of urine, corresponding in time with that of perspiration. The ratio was, urine : perspiration :: 46 : 33, or 7 to 5 nearly; which is somewhat greater disproportion than that observed in March; owing, probably, to the temperature of the weather being lower in the latter season.

So far I have given the facts and observations made 40 years since; I made no deductions from them at the time; indeed the knowledge of animal and vegetable chemistry was at that time in its infancy. Since then the progress of this branch of philosophy has been very considerable, and we are now enabled to approximate, in a good degree, to the quantities of the several chemical elements to be found in the great variety of products of the two kingdoms.

By combining this knowledge with that obtained from the preceding facts, we may possibly discover or establish some physiological principles important to be understood in the animal economy, more especially in regard to the acquisition and preservation of health.

From the table we have given, it will appear, that bread and farinaceous vegetables constitute the greatest part of ordinary food. About the time of the above experiments I found that 5 lbs. of flour would make 7 lbs. of bread. Now from the analyses of flour that are given in our systems of Chemistry I think we cannot estimate the carbone in flour at less than 42 per cent.; hence we have 30 per cent. of carbone in bread. Twelve ounces of bread (the daily average in the first set of experiments) must then contain 3.6 ounces of carbone. Seven ounces of oat-cake and oat-meal may be estimated, I think, = 1.8 ounces of carbone, or half the quantity that 12 ounces of bread have. Four ounces of pastry can scarcely contain less than one ounce of carbone. Nine ounces of potatoes must contain nearly one ounce of carbone. Four ounces of butcher's meat and two ounces of cheese would have together somewhere about three ounces of carbone, if Gay-Lussac's experiments be nearly correct. Thirty-one ounces of milk, estimating the carbone at three per cent. gives eleven-twelfths of an ounce. Twenty-two ounces of tea and beer would contain only a small fraction of an ounce of carbone, not easily estimated, but of little account by reason of its smallness.

From this it would appear, that about 11½ ounces of the element carbone is taken into the stomach by one kind of aliment or another in the course of the day in some state of combination.

Chemical analysis has been applied with considerable success to the animal product, urine. According to Berzelius the urine of healthy persons differ materially according to circumstances. Upon the average it may be reckoned to consist of 93 or 94 per cent. of water, and the rest is a complication of a great many articles. The carbone contained in these ingredients cannot be estimated at more than 1 or 1¼ per cent. from the analyses hitherto made. This will give .5 or .6 of an ounce of carbone upon 48½ of urine per day. Berzelius has not neglected the analysis of the fæces; of 100 parts, three-fourth may be estimated as water, and the rest do not seem to contain more than ten parts of carbone. This would give half an ounce of carbone in five ounces. Hence we may infer that one ounce, a little more or less, of carbone, is carried off from the body daily through these two channels. The remainder 10½ ounces must therefore be spent in the insensible perspiration.

The quantity of insensible perspiration from the skin cannot be easily determined by direct experiment. That from the lungs may be approximated from known facts.

I have shewn (see Manchester Memoirs, vol. 2nd. new series, page 27,) that I produce by breathing in the space of 24 hours, 2.8lbs. troy, of carbonic acid gas. This is equivalent to .78

parts of a lb. troy of carbone = .642 parts of a lb. avoirdupoise = 10¼ ounces, nearly. Now when I estimated the quantities of carbone in the several articles of food, &c. just related, I had no recollection of this quantity of carbone expended in breathing; it may well be supposed then, that I was highly gratified to find by the calculation that the difference of the two quantities, found by such different modes of investigation, was only one quarter of an ounce.

With respect to the aqueous vapour exhaled from the lungs I have determined in the essay quoted above, (page 29) that the highest estimate of the quantity I exhale cannot exceed 1.55lbs troy = 1.275lbs. avoir. = 20½ ounces avoir.; if to this we add 10¼ ounces of carbone, we have 30¾ ounces for the carbone and water expended from the lungs in one day, and this taken from 37½ leaves 6¾ ounces per day, for the insensible perspiration from the skin, which, if the above estimate be allowed, must consist of 6½ ounces water, and one quarter of an ounce carbone. According to this, the matter perspired from the lungs is five times as much as that from the whole surface of the body.

If, instead of carbone, we trace the element azote into and out of the body we shall find from our data that from butcher's meat, cheese, and milk, about $1\frac{1}{4}$ ounce of azote is taken into the stomach daily, and nearly as much passed off by urine and fæces.

Upon the whole we may observe, that of the 6lbs. of aliment taken in a day, there appears to be nearly 1 lb. of carbone and azote together; the remaining 5lbs. are chiefly water, which seems necessary as a vehicle to introduce the other two elements into the circulation, and also to supply the lungs and other membranes with moisture. Very nearly the whole quantity of food enters into the circulation; for, the fæces constitute only one-18th part, and of these a part, bile, must have been secreted; one great portion is thrown off by means of the kidneys, namely about half of the whole weight taken, but probably more or less according to climate and season, &c.—another great portion is thrown off by means of insensible perspiration, this last may be sub-divided into two portions, one of which goes off by the skin, amounting to one-6th part, and the other five-6th, are discharged from the lungs in carbonic acid and in water or aqueous vapour.

Such are the deductions I have drawn from my early experiments, and from the light which modern chemistry has diffused over the animal and vegetable products. This branch of science belongs more peculiarly to the physician. What the profession may have done in it of late years I am not aware, my studies not having been in that line. But it must be allowed to be a subject worthy the attention of professional characters, and not uninteresting as a branch of general physics.

A BRIEF MEMOIR

OF

SAMUEL CROMPTON;

WITH A

DESCRIPTION OF HIS MACHINE CALLED THE MULE,

AND OF THE

Subsequent improvement of the Machine by others.

BY JOHN KENNEDY, ESQ.

(Read February 20th, 1830.)

SAMUEL CROMPTON was born on the 3rd December, 1753, at Firwood, near Bolton in Lancashire, where his father held a farm of small extent; and, as was customary in those days, employed a portion of his time in weaving, carding, and spinning. During the infancy of Samuel Crompton, the family removed to Hall-in-the-Wood, which was the scene of his early inventions.

His parents were very respectable in their station of life, and taught him to read and write.

His father died when he was very young; his mother was a prudent and virtuous woman; and this circumstance, together with the sequestered situation in which they lived, induced a contemplative turn of mind. He had taken various views of the Christian religion, but finally preferred the Swedenborgian faith, without adopting the restrictions it imposes on certain kinds of food. In all his dealings through life, he was strictly honest, patient, and humane. In politics he took little interest, but regretted the waste of life and property which war occasions.*

When about 16 years old, he learnt to spin upon a *jenny* (of Hargreaves's make) and had occasionally woven the yarn which he had spun. This being but indifferent work, led him to reflect how it might be improved, and set him to construct the machine which we are about to describe. He was only 21 years of age when he commenced this undertaking, which took him five years to effect; at least before he could bring his improvements to maturity. As he was

* At the time of Bonaparte's marriage with the Archduchess of Austria, a lady observed to Mr. Crompton, that she hoped he would have a family, that it might humanize him; when he quietly replied, "Do you want a breed on 'em:" an instance of his cool and deliberate way of thinking.

not a regular mechanic, and possessed only such tools as he purchased with his little earnings, acquired by labour at the loom or jenny, and as he had also to learn the use of those simple tools, we may be justly surprised that even in five years he succeeded so far as to make his machine practically useful.

He often said, that what annoyed him most was, that he could not get leave to enjoy his little invention to himself in his garret; for, the product of his machine obtaining a better price than other yarns of those times, a report soon got abroad that he had constructed a new machine, for the purpose of improved spinning, and people from the neighbourhood, for miles round, came and climbed up at the windows to see him at his work. He erected a screen to prevent this, but the annoyance was so great, that he could not proceed advantageously with his ingenious labour; and finally he was induced to lay the whole thing before a number of gentlemen and others, who subscribed a guinea each to look at it. On this as on every other occasion, the late Mr. Pilkinton, of Bolton, gave him his steady and friendly support. These sums amounted to about £50, which enabled him to construct another machine still farther improved and of larger dimensions.* When relating this little history to Mr. G. A. Lee and myself, Mr. Lee having observed "it was a pity he had not kept the secret to himself," he replied, "that a man had a very insecure tenure of a property which another could carry away with his eyes."

In 1784 or 5 he made a carding machine, the working of which was a little different from those in common use: the main or large cylinder was made to turn in an opposite direction, thereby carding or combing the cotton downward from the rollers, and of course upwards from the doffing cylinder. His object was, to get an easier egress for the waste or dirt that was in the cotton, and to save the trouble of stripping, &c., but this was not followed up so as to be practically useful.

About the year 1802, Mr. Lee and myself set on foot a subscription for him, which amounted to about £500; and with this he was enabled to increase his little manufacturing establishment in Bolton, namely of spinning and weaving. He was prevailed upon also to sit to a London artist for his portrait, which is now in my possession. He was left a widower when his children

* The first Machine consisted of not more than 30 or 40 Spindles.

were very young, and his only daughter kept his little cottage in King-street, Bolton, where he died and where she is now living. Being a weaver he erected several looms for the fancy work of that town, in which he displayed great ingenuity. Though his means were but small, his economy in living made him always in easy circumstances. He was fond of music, and built for himself an organ, which he had in his little cottage. In 1812 he made a survey of all the cotton districts in England, Scotland, and Ireland, and obtained an estimate of the number of spindles then at work upon his principle, which amounted to between four and five millions.* On his return he laid the result of his enquiries before Mr. Lee and myself, with a suggestion that parliament might grant him something. With these data before him, Mr. Lee, who was a warm friend to genius of every kind, with his usual energy entered fully into his merits, and made an appointment with the late George Duckworth, Esq. of Manchester, who also took a lively interest in the scheme, and gratuitously offered to draw up a memorial to parliament in behalf of Mr. Crompton. This was signed by most of the principal manufacturers in the kingdom who were acquainted with

* Now in 1829 about seven Millions.

his merits. He went to London himself with the memorial, and obtained an interview with one of the members for the county of Lancaster. He remained there during the session, and was in the House on the evening that Mr. Percival was shot, and witnessed the catastrophe. A short time before this disastrous occurrence, Mr. Percival had given him a promise to interest himself in his behalf, and in accordance with this assurance had brought in a bill, which was passed, for a grant of £5,000 in full, without fees or charges.

Mr. Crompton was now anxious to place his sons in some business, and fixed upon that of bleaching; but the unfavorable state of the times the inexperience and mismanagement of his sons, a bad situation, and a misunderstanding with his landlord, which occasioned a tedious lawsuit, conspired in a very short time, to put an end to this establishment. His sons then dispersed, and he and his daughter were reduced to poverty. Messrs. Hicks and Rothwell of Bolton, myself and some others, in that neighbourhood and in Manchester, had in 1824 recourse to a second subscription, to purchase a life annuity for him, which produced £63 per annum. The amount raised for this purpose was collected in small sums, from one to ten pounds, some of which were contributed by the Swiss and French spinners, who acknowledged his merits and pitied his misfortunes. At the same time his portrait was engraved for his benefit, and a few impressions were disposed of: he enjoyed this small annuity only two years. He died January 26th, 1827, leaving his daughter, his affectionate housekeeper, in poverty.

With respect to the invention of Mr. Crompton, I am aware of the difficulty of delineating from memory the features of a machine, the original of which has been long destroyed, but will state them to the best of my recollection.

In describing Mr. Crompton's machine, I shall preface the subject by observing, that a wheel has been employed in spinning ever since the abandonment of the distaff and rock. When the spindle was first put into motion by a revolving wheel, the contrivance thus formed was called simply a spinning wheel, which contained only a single spindle. The worsted wheel, the flax wheel,* &c. are instances of this, and the introduction of the Jersey wheel as well as of the distaff,

* The flax wheel is a German invention, and is called the Saxony or Leipsic wheel. In some instances there were two spindles attached to the same wheel, and the spinner was by this means able to form a thread with each hand.

in all probability was derived from India.* But on the introduction of Hargreaves's invention, which was a machine containing a number of spindles, it assumed the name of a *Jenny* from its performing the work of a female .Crompton's machine was called the Hall-in-the-wood wheel, or muslin wheel, because its capabilities were rendered available for yarn for making muslins: and finally it got the name of the mule, from its partaking of the two leading features of Mr. Arkwright's machine, and Hargreaves's spinning jenny. Mr. Crompton's invention consisted in erecting his spindles on a moveable carriage, which at the same time turned on their axes and centres, whilst the moveable carriage was receding from the beam or rollers which measured out the rove to a certain length. His first suggestion was to introduce a single pair of rollers, viz. a top and a bottom, which he expected would elongate the rove by pressure, like the process by which metals are drawn out, and which he observed in the wire-drawing for reeds used in the loom. In this he was disappointed, and afterwards adopted a second pair of rollers, the latter pair revolving at a slower speed than the former; and thus producing a draught of one

* The jersey wheel and distaff are implements, which the Hindoos have used from a remote period of antiquity, and are those which without any alteration, they still employ in spinning.

inch to three or four. These rollers were put in motion by means of a wooden shaft with different sized pullies, which communicated with the rollers by a band. This was certainly neither more nor less than a modification of Mr. Arkwright's roller-beam; but he often stated to me, that when he constructed his machine he knew nothing of Mr. Arkwright's discovery.* Indeed we may infer that he had not, otherwise he would not have gone thus rudely to work, and indeed the small quantity of metals which he employed, proves that he could not have been acquainted with Mr. Arkwright's superior rollers and fixtures in iron, and their connection by clock-work. Even the rollers were made of wood and covered with a piece of sheep skin, having an axis of iron with a little square end,

* There was a patent taken out in 1738, by Lewis Paul, who, in conjunction with Wyatt, had a manufactory of cotton at Northampton, for spinning with rollers at different velocities; and it is quite as probable that Mr. Crompton was unacquainted with Mr. Arkwright's patent, as that Mr. A. was with that of Paul. Lewis Paul was also in 1748, the patentee of the invention of revolving cylinders for carding cotton. This machine is the original of the machine for carding now used. After the breaking up of Wyatt and Paul's establishment at Northampton, it was purchased by a hat manufacturer of Leominster, and by him applied to the carding of wool for hats; and about 1760 it was introduced into Lancashire, and re-applied to the carding of cotton, by a gentleman of the name of Morris, in the neighbourhood of Wigan.

on which the pullies were fixed. Mr. Crompton's rollers were supported upon wooden cheeks or stands. His tops were constructed much in the same way, with something like a mouse-trap spring to keep the rollers in contact. His first machine contained only about 20 or 30 spindles. He finally put dents of brass reed-wire into his under-rollers, and thus obtained a fluted roller. But the great and important invention of Crompton was his spindle carriage, and the principle of the thread having no strain upon it, until it was completed. The carriage with the spindles could by the movement of the hand and knee recede, just as the rollers delivered out the elongated thread in a soft state, so that it would allow of a considerable stretch before the thread had to encounter the stress of winding on the spindle This was the corner stone of the merits of his invention. The frame of his machine was composed entirely of wood, and extended to the length of the draw or stretch: the roller-beam was supported at each end by the frame; and the wheel-end, and wheel-frame, and roller-beams all stood breast high. The rim or wheel had a small pulley upon its axis, with a band for the purpose of communicating power or motion to the back shaft which had pullies upon it to give motion to the rollers. There were also from the same shaft, bands to each head or pair of rollers, as I have before stated, which had pullies of different sizes for the purpose of varying his draught, having sink-weights and pullies to keep the bands of an equal tightness, and the rollers in corresponding motion. The band that gave motion to the shaft was connected with a loose pulley, having a catch upon the rim, so that it might be disengaged at pleasure, and stop the rollers. This was done by a treadle with the foot; the other band was conducted from the rim or wheel by two carrying pullies, at the extreme end of the frame, containing the carriage or railway. The band made a turn upon a pulley round the wooden or tin roller, which roller being supported by two centre points or bearings, gave motion to the spindles by means of separate bands.* He had a faller or wrapper (like that of Hargreaves's jenny, only Hargreaves's were placed behind the stationary spindles, and that of Crompton's on the spindle-carriage as it was called) nearly on the same principle as that now in use. His carriage was a long box with wooden pullies or carriage wheels, running upon a wooden railway, very much inclining downwards from the beam

* The wood or tin roller and vertical wheel were not of Hargreaves's invention nor Crompton's, but were introduced into Hargreaves's jenny by a person of the name of Haley, of Houghton Tower, a few years after Hargreaves's invention.

which enabled him to draw it out by his knee with greater steadiness, as at that time he had no method of drawing out the carriage by machinery.

Having thus given a brief sketch of the "short and simple annals" of Mr. Crompton's life, and described the construction of his spinning machine, I think it proper to mention those persons, whose exertions have been conducive to the successive improvements of the mule, together with the description of those parts of the machine which are attributed to them. I shall narrate the circumstances, partly from Mr. Crompton's verbal communications, and partly from those of others on whose testimony I could rely, including the period from 1779 to 1786 or 7. Although a few, a very few machines were made as described in the account of Crompton's spinning machine, the first deviation according to Crompton's own statement, was by an ingenious mechanic, Henry Stones of Horwich, a few miles from Bolton, which might be considered as the seat of the manufactories of the finer fabrick of cotton cloth, in Lancashire, at that time. The improvement of Stones was the introduction of metal rollers (he having probably become acquainted with Arkwright's method of rollers for spinning) and clockwork to his rollers, with a chain conveying motion to the rollers from the wheel, with some contrivance, self-acting, to stop the rollers when it was required. I ought to observe here, that the roving then spun was all prepared and roved as if for the common jenny, by hand or stock cards, that being thought the best method of making superfine carding; after which it was roved by hand by the single spindle, and few but the adepts were entrusted with these processes. Hargreaves's spinning wheel had spread through a circuit of forty miles in extent, including Blackburn, Bury, Oldham, Ashton, and Stockport, and had superseded the use of the single spindle which before was common in these distincts; but after Crompton's invention which took the same range of places, the use of Hargreaves's jenny was in a great degree superseded by the former. Henry Stones was probably the first who constructed Crompton's machine, either for the use of himself or others. But, up to 1783, I do not think there were a thousand spindles in existence of Crompton's construction, namely upon a moving carriage, when Arkwright's patent for preparing roving, &c. was suspended by a trial of its validity, which was ultimately decided against him in the Court of King's Bench. About this time Crompton's machine was every thing, and as Arkwright's contrivances for making rovings became available to the public and were of the utmost importance to Crompton's machine, the door was opened wide for the exertions of ingenuity in preparing the cotton roving, as all could now avail themselves of the advantage displayed by Arkwright's method of preparing the cotton for spinning; and the consequence was, that every one who had the slightest talent for constructiveness or appropriativeness fell to work to improve this process.*

Among the appropriations of this period, 1786, was the introduction of the machine called the Billy,† which was a modification of the mule, and was invented to make the rovings for Hargreaves's jenny, and combined the principle of the jenny and the mule. This machine consisted of Crompton's spindle carriage with a clasp, like the jenny, but stationary, and a feeding cloth

* This was done by arranging the card slivers and slivers belonging to the drawing frame, so as to form the fibres of the wool into parallel lines. The renowned Bishop Blaize, of Germany, gave the idea of laying fibrous woolly substances, in a parallel direction, by inventing the wool-comb. Tradition says, that he was torn to pieces by his own implement, a sacrifice to the prejudices of the vulgar.

† By a person at Stockport who received a premium for the ingenuity he thus displayed; it was voted to him by the Jenny Spinners.

which had motion given to it by a band from the rim; the cardings were placed upon this cloth, with a boy or girl to piece one carding to another as they were taken up by this cloth, the clasp falling upon, and holding the cardings, by a treddle, (which was worked either by the foot or some mechanical contrivance) after a certain quantity had been given out. The carriage receded like the mule to elongate the carding into a rove, which was afterwards to be spun by Hargreaves's machine, which was at this time in general use, and had become of the utmost importance in the spinning, not only of cotton but of wool; for which latter process, it is the machine still employed. The substitution of the roller beam with rollers to give draught to the drawn sliver, made it applicable to the mule; and also to Arkwright's method of spinning, to which it was of great importance, as it enabled rovings to be made at a small expense, and of any fineness that might be required. Thus the essence of Crompton's invention, which was the carriage, became of the greatest importance to the inventions of others; as also (when thus combined) to the original machine itself, though not primarily intended for this purpose. The introduction of metal rollers and clockwork soon enabled the machine to be extended to a considerable length, up to 100 or 130 spindles, but this extension again was soon at its limit. The tin rollers which were difficult to make, being ponderous and of great vibration, another contrivance was produced to obviate this inconvenience, viz. by placing vertical cylinders or drums in the carriage. The first attempt was made by a person at Bury, of the name of Baker, also an ingenious man. He placed upright pullies in the carriage with nicks to carry six or eight spindles, with the rim-band passing over a pulley upon the vertical shaft, so placed as to give motion to them; this was soon extended to a cylinder or drum as it is now called, (first made in wood, then in tin) to embrace 24 to 30 spindles, the wharves being put on like the strings of a harp to embrace the whole breadth of the drum. By this means the carriage was soon extended to a much greater length, and the better construction of the rollers and their fixtures on the beam, facilitated the enlargement of the whole machine. The greatest improvement was the giving motion to the rollers by a diagonal shaft from the rim to the rollers, which dropped out of gear at the rim when the rollers were to stop. This was also a contrivance of Baker's.* By this time, (1786,) there was a great variety of methods

* The bevelled gear was at this time made of wood, probably cut by his own pocket knife.

for measuring the number of revolutions of the front rollers, in order to give out the required length before the stretching commenced. James Hargreaves of Toddington, contrived the first method of bringing out the carriage, by a very ingenious invention. It consisted of a parallel scroll, with a small conical one attached to the same, for the band, connected with the carriage, to wind upon; the whole deriving its motion from the wheel axis. Of course there were many contrivances to effect the same purpose, such as a wheel with a pulley upon it, which was forced into a toothed wheel upon the front roller, with a band upon the pulley connected with the carriage, which produced a similar effect, and was disengaged when the rollers were stopped. This was continued for some time; the spinner completing the second draw by the hand and knee, which was more or less, according to the fineness he was spinning. The difficulty of obtaining rollers,* spindles, in short all the metal parts of these machines, and the preparing machinery for rovings, added to the want of experienced workmen of every kind, retarded the progress of the spinning trade much less than might be supposed. The fear of over-production then existed,

* Spindles were obtained from the manufacturers of wool-combs, and heckles for dressing flax, for the machines of both Hargreaves and Crompton.

and did exist afterwards from time to time, which caused a suspension of increase of means, and sometimes even a diminution of produce by the means that were in existence. This is the case with every infant trade or manufacture; an obstinate resistance to a reduction of prices existing, until some enterprising spirit attempts to meet the market by some simplification, and better arrangement of the means of production, so as to enable the individual to offer the article produced at a lower price. This principle will hold in all our manufactures, and in such seasons of depression, the greatest improvements have always been made. The art of spinning on Crompton's machine was tolerably well known from the circumstance of the high wages that could be obtained by those working on it, above the ordinary wages of other artisans, such as shoe makers, joiners, hat makers, &c. who on that account left their previous employment;* and to them might be applied the fable of the town in a state of siege. For, if in the course of their working the machine, there was any little thing out of gear, each workman endeavoured to fill up the deficiency with some expe-

* By their industry, skill, and œconomy, these men first became proprietors of perhaps a single mule, and, persevering in habits so intimately connected with success, were afterwards the most extensive spinners in the trade.

dient suggested by his former trade; the smith suggested a piece of iron, the shoe maker a welt of leather, &c. all which had a good effect in improving the machine. Each put what he thought best to the experiment, and that which was good was retained. But with all these exertions, there was still very much to learn, for the principle on which the rovings were prepared, had little chance of being known, being confined to the principal mill-owners of Mr. Arkwright's patent process of spinning, &c. But the demand for these machines after the decision of the Court of King's Bench, in 1783, (which I consider very questionable) soon found makers, and the perseverance of the mule spinner soon acquired the art.*

It would be vain to enumerate all the little additions to Crompton's original machine; also, as they arose so much out of one another, it is impossible to give to every claimant, what is exactly his due for improvements. It is therefore only necessary to mention those who have well authenticated claims to the addition of parts of

* The roving-making then became a distinct business, and in this state the cotton was sold to the little spinners. This was common till power was applied to the turning of the mule. Mills were then built of a suitable width, and in the course of a few years the hand-mule was entirely superseded.

great importance to the machine. But the circumstance of the interval being very short, in making the machine tolerably correct, shews that many heads must have been at work. What led to the enlargement and the forming of the parts of the mule, with additional strength and accuracy, was the application of artificial power, which was first introduced by Mr. Kelly, of Glasgow,* 1790, formerly of the Lanark Mills. The way in which Kelly applied this artificial power to the usual hand-mule, was simply by a loose pulley, to which a catch was attached, which could be made at pleasure to seize another catch fixed to the axis; on this axis was placed a screw, which worked into a wheel, the number of whose teeth governed the number of revolutions of the rim, by disengaging the rope from the fast to the loose pulley. Immediately after the introduction of this power, Mr. Wright an ingenious machine maker of Manchester, an apprentice and workman of Sir R. Arkwright's, constructed the double mule, embracing the advantages of Kelly's application of artificial power. The double mule was constructed by placing the rim in the middle of the frame or rollers. I believe he had four hundred spindles

* Two years after this, he took a patent for a self-acting mule.—See his letter to me, 8th of January, 1829.

in this mule, and his experiment of its success was with a horse-gin or mill, so that Wright's double mule gradually superseded the use of the single mule; as, by his manner of placing them, the spinner could superintend and operate upon four times the quantity of spindles, compared with the former method.*

A few years after this, Benjamin Butler, of Bolton, dispensed with the framing of the rim or wheel, extended the axis to the middle of the roller-beam, and connected it by gearing with a little coupling shaft, which the front roller coupled each way. The shaft or axis of the rim was engaged and disengaged every stretch, to enable the rim to effect the necessary revolutions of the spindle to complete the thread. To put up the spun thread, he attached a small rim to the carriage about the middle of it, and brought the drum-band over it; thus the little rim was connected with that band which gave motion to the spindles, and had a handle upon it, by which the spinner could govern the spindles in the act of wrapping up the thread. This was called the Fanny wheel or mule, but since that time various modifications of this kind have

* The squaring band though insignificant in itself was of no little importance to the mule. It acts like a parallel rule in guiding out the carriage.

been constructed by successive artizans. About 1790, the muslin trade received a great stimulus at Stockport, from the efforts of the late Samuel Oldknow, whose spirit of enterprise extended this branch of our manufacture. He took new ground by copying some of the fabrics imported from India, which at that time supplied this kingdom with all the finer fabrics, and which the mule spun yarn alone could imitate. He was very successful in carrying on the ingenious processes which he had devised; but the French revolution creating a panic and general stagnation for a time, he abandoned this branch of the trade, and betook himself to his large water-mill at Mellor, which was built in the year 1790. On his retiring from the manufacturing of fine muslin, Messrs. Horrocks who had just established themselves at Preston as mule-spinners, took up what he had laid down. They became extensive manufacturers of cloth, similar to that made by Oldknow, and supplied the same market, London. This gave a new stimulus in that district, and immediately upon the subsiding of the panic caused by the French revolution, a market sprung up on the Continent for yarns of all kinds, but principally for muslin yarns, up to the highest numbers that could be produced. This gave a general stimulus all through the kingdom, and Watt's and ~~Savary's~~ New steam-engine supplied power for the mule spinner, which was soon generally embraced instead of Kelly's application of water power, the use of which can only be local. The mule spinning now took the lead, and became important and extensive. The profits being very considerable the increase was rapid. It was not until 1793 that any attempts were made in spinning fine yarns, say from 100 hanks upwards, by power, when I observed the process very carefully. The rollers, according to the fineness of the thread, would only admit of a certain velocity per minute, for instance, with 200ds. the rollers could only go at the rate of 25 or 26 per minute, and the spindle about 1,200. But when the rollers ceased to move, then the spindle was accelerated by the spinner to nearly double its former speed. In what manner the acceleration of the speed of the spindle might be effected by machinery without the aid of the spinner, was suggested to me, by observing in Mr. Watt's steam engine, that one revolution of the beam (if I may use the expression) acting upon the fly wheel by means of the sun and planet wheel produced a double velocity. The difficulty however of making the necessary apparatus at that time, induced me to use the more complicated method of four wheels of unequal sizes for producing the same effect. The description is as follows:—Two of the wheels were less and two larger; upon the rim-axis, were placed one of the small and one of the large, and the two others were fixed in a frame which carried the axis upon which they were placed, and which had a shank or axis growing to it. This was placed in a vertical position, so that when the carriage was put up, an arm projecting from this vertical shank was connected by a wire with a catch which kept the lying shaft that turned the rollers in gear. In the elongating process the smaller wheel was in contact with the larger wheel upon the rim, but when by the disengagement of the catch, the rollers became still or stationary, at that moment the larger wheel by means of a weight came in contact with the lesser wheel upon the rim or axis, to which it communicated a double velocity. The shaft with its large and small wheels working alternately, had a pulley with a catch upon it, and was driven by the mill work, and was forced into a corresponding catch upon the said little shaft when the mule was to be set in motion by the steam power, (the power in this instance was Savary's.) There was a worm upon the rim axis with a wheel upon it, the number of whose teeth determined the revolutions of the rim, as described in Kelley's single speed. The second drawing which had generally been performed by hand, had also to be performed by the machine itself. This had been done in a few instances before power had been applied. From the simplest of these methods I took the hint; by driving a shaft from the rim, by a strap from a small pulley upon the rim-axis, and a large one upon the little axis, which had a small pinion upon it; so that when the drawing-out wheel and band were disengaged from the front roller, they fell back into the little pinnion whose axis was revolving at a very slow speed, and consequently gave a much slower speed to the second stretch or draw, (as it was called) the speed of which was more or less according to the numbers to be spun. Messrs. A. & G. Murray at that time (like myself and partners) were machine makers, and to a small extent were engaged in fine spinning by hand. They fitted upon the principle described a few pairs of hand mules, which they had previously made, wherein they adopted these contrivances for one of their customers in Derbyshire, who had artificial power. Mr. Drinkwater of Manchester, was the most extensive fine spinner at the time of which I speak. He was one of the early water spinners, and in possession of the most perfect system of roving making. His large mill in Piccadilly, was filled with mules of 144 spindles, each of which was worked by men's hands.

Mr. Owen was then his manager, and they came to see the new machine in 1793. They approved of it and thought it practical. Mr. Humphries of Glasgow, who was a good machanic, and succeeded Mr. Owen as manager also approved of the scheme, and got instructions to apply this system of power to his fine work produced by the mules in Piccadilly mill; and, to make its advantages available, he coupled these 144 together, so that he saved one half of the steam-gearing, and obtained a reduction in the price of spinning, the spinner having double the number of spindles to operate upon. Mr. H. made an improvement in the four wheels already described, by keeping them always in gear with a loose clutch between the two wheels on the rim shaft, which was alternately fastening the little driving wheel, and then relieving it and fastening the larger, which accelerated the speed of the rim with a loose pulley as already described in my first. This prevailed for some years, when I thought that this might be simplified, which was done by adopting three pullies, namely one on the small wheel, and another on the larger wheel, with a loose pulley; and by removing the driving strap, which was on the loose pulley when the mule was at rest, to the pulley on the smaller wheel when the rollers were to work. Then the strap was removed to the pulley on the larger wheel, which accelerated the rim and spindles until the thread was completed, and the strap being removed to the loose pulley, the whole machine came to rest, and the thread was put up by the spinner in the ordinary way. I was now able to construct the sun and planet wheel for the acceleration of the speed of the spindle which was as follows:—the sun and planet wheel had only two wheels and one pulley, with a clutch that fastened the sun wheel, when the accelerated motion was required. Many other modifications were introduced, but the four wheels prevailed, some of which for convenience I constructed by making them bevils, and placing their axes vertically to get motion from an upright shaft, which produced the same effect as the spur wheels. This was suggested to me by Mr. Lee of Salford, and I made him a a model of one in 1800.

Having thus briefly explained the principal modifications of fine spinning by power; I have only to add, that they produced a great change in the value of the fine yarn, and consequently a great extension of its use. The Scotch in Lanarkshire, and Renfrewshire, being long in the habit of weaving fine cambric from flax yarn, and silk gauzes, had also turned their hands to the manufacture of fine cotton fabrics, principally from the fine yarns produced by Hargreaves's, and other subsequent machines. The Lancashire manufacturers followed them in the thicker and finer fabrics, and about 1805 or 6 the Nottingham lace trade sprang up. Mr. Heathcote (originally a whitesmith) invented a machine, by which he could make lace, similar to that of Brussels and Buckingham, which was worked by hand; and he principally if not wholly, at first, used fine flax yarns. Two-fold fine cotton twisted together was found to answer very well as a substitute; and as it required the finest yarns, a great impulse was given towards perfecting the production of fine cotton yarn. It bore a high price, as the lace manufacturer had only to compete with hand-spun thread, and hand-made lace.

Having brought this brief sketch down to the present period, I shall close the paper, by subjoining a brief account of the growth of cotton, communicated to me by an old and intelligent Planter of Sea Island Cotton; premising, that the information was supplied, in consequence of an advertisement which I had caused to be inserted in the Charlestown newspapers.

LETTER.

Sir,

There has been for some months past, a notification in your paper, requesting a communication upon the subject of the introduction of cotton, into Georgia and Carolina.

It has been intimated to me, that possibly, this notification has originated in some *one desirous of correct information,* in order that it might enter into some more general work; and, as I am at present, perhaps, the only person alive, that recollects distinctly, the introduction of the Sea Island cotton, I have addressed this letter to you.

It is known to many, that cotton was cultivated for domestic purposes, from Virginia to Georgia, long anterior to the revolutionary war. Mr. Jefferson speaks of it in his notes on Virginia Bartram speaks of it, in his travels, as growing in Georgia; and I have understood that twenty-two acres were cultivated by a Col. Dellegal, upon a small Island near Savannah, before the revolution. But this was the *green seed,* or *short staple* cotton. Two species of the same family then existed in this country—the real green seed, and a low cotton, resembling it in blossom, both being of a pale yellow, approaching to white; one with the seed covered with fuzz; the other with fuzz only upon the end of the seed. To explore the first introduction of the short staple cotton into this country, would now in all probability, be impossible; but we may very well suppose, it was by one of the southern proprietary governments, and possibly from Turkey; the trade of which country with England was then of much higher consideration, than it has subsequently become. Nor would it have escaped those proprietors, many of whom were enlightened men, that the climate of Asia Minor where cotton grew abundantly, was analogous to the climates of the provinces south of Virginia. Just about the commencement of the revolutionary war, Sir R. Arkwright had invented the spinning jenny, and Cotton spinning became a matter of deep interest;—in England cotton rose much in price;—its various qualities attracted notice, and the world was searched for the finer kinds.

The Island of Bourbon was alone found to produce them, and yet the Bourbon cotton greatly resembled in its growth our green seed cotton, although it cannot be its parent plant, for all attempts to naturalize it in Georgia (which were many and repeated) have failed.—It gave blossoms, but it was cut off by the frost in the fruit, nor would it ratoon, or grow from the root the next year—in which too, it resembles the green seed cotton of our country.—This is all I am able to say, and perhaps all that is necessary to be said, of the short staple cotton.

The Sea Island cotton was introduced directly from the Bahama Islands into Georgia. The revolutionary war that closed in 1783 had been a war, not less of opinion, and of feeling, than of interest, and had torn asunder many of the relations of life, whether of blood or of friendship.—England offered to the unhappy settlers of this country, who had followed her standard, a home, but in two of her provinces; to the provincials of the north, she offered Nova Scotia—to the provincials of the south, the Bahama Islands—many of the former inhabitants of the Carolinas and Georgia, passed over from Florida to the Bahamas with their slaves.—But what could they cultivate? The rocky and arid soil of those Islands could not grow sugar-cane; coffee would grow but produced no fruit. There was one plant that would grow, and that bore abundantly; it was cotton.

The seed, as I have been often informed by respectable gentlemen from the Bahamas, was in the first instance procured from a small Island in the West Indies, celebrated for its cotton, called Anguilla. It was therefore long after its introduction into this country, called Anguilla seed.

Cotton, as I have already stated, had taken a new value by the introduction of the spinning machine into England. The quality of the Bahama cotton was then considered among the best grown—new life and hope were imparted to a colony and a people with whom even hope itself had been almost extinct. This first success, as is natural to the human mind under whatever influence it may act, recalled the memory of the friends they had left behind them. The winter of 1786 brought several parcels of cotton seed from the Bahamas to Georgia; among them (in distinct remembrance upon my mind) was a parcel to the governor Tatnall, of Georgia, from a near relation of his, then surveyor general of the Bahamas; and another parcel at the same time was transmitted by Col. Roger Kelsall, of Exuma, (who was among the first, if not the *very first* successful growers of cotton) to my father Mr. James Spalding, then residing on St. Simon's Island, Georgia, who had been connected in business with Col. Kelsall before the revolution. I have heard that governor Tatnal, then a young man, gave his seed to Mr. Nicholas Turnbull, lately deceased, who cultivated it from that period successfully.

I know my father planted his cotton seed in the spring of 1787, upon the banks of a small rice field, on St. Simon's Island. The land was rich and warm, the cotton grew large and blossomed, but did not ripen to fruit; it however ratooned, or grew from the roots the following year. The difficulty was *now over*, the cotton adapted itself to the climate, and every successive year from 1787, saw the long staple cotton extending itself along the shores of Georgia, and into south Carolina, when an enlightened population, then engaged in the cultivation of Indigo, readily adopted it.

All the varieties of the long staple, or at least the germ of those varieties came from that seed; differences of soil developed them; and differances of local situations are developing them every day. The same cotton seed, planted on one field, will give quite a black and naked seed; while the same seed, planted upon another field, different in soil and situation, will be prone to run into large cotton, with long boles or pods, and with seed tufted at the ends with fuzz. I should have great doubts if there is any real difference in these apparent varieties of the long staple cotton. But, if there is, all who observe, must know that plants, where they have once intermingled their varieties, will require attention for a long series of years to disentangle them.

Subsequently to 1787, as the cultivation of cotton extended and became profitable, every variety of the cotton that could be gleaned from the four quarters of the globe, has been tried, but none of them but *one* has resulted in any thing useful. Mr. James Hamilton who formerly resided in Charleston, and who now resides in Philadelphia was indefatigable in procuring seed, which he transmitted to his friend, Mr. Couper, of St. Simon's. Mr. Couper planted some acres of Bourbon cotton; it grew and blossomed, but did not ripen its fruit, and perished in the winter.

Mr. Hamilton sent a cotton from Siam, it grew large, was of a rich purple colour, both in foliage and blossom, but perished also without ripening its fruits.

The nankeen cotton was introduced at an

early period, the same that Mr. Secretary Crawford distributed the seed of some years back. It was abundant in produce; the seed fuzzy and the wool of a dirty yellow colour, which would not bring over the price of the other short staple cottons, but I know it to produce three hundred weight to the acre, on Teykel Island, in Georgia. The kidney seed cotton—that is a cotton which produces the seed all clustered together, with a long strong staple, extending from one side of the seeds, (and which I believe to be the Brazilian, or Pernambucco cotton) was tried, and was the only new species upon which there could have been any hesitation, but this too was given up, because not as valuable and not as productive. I have given the names of gentlemen, because I had no other means of establishing facts, and now my communication shall close.

Your very obedient Servant,

THOMAS SPALDING,

DARIEN,

GEORGIA.

To the Editor of the Charlestown Courier.

Such then is the brief history of the Mule, a machine of incalculable importance in the commercial relations of this country, and the present improved state of which a concurrence of unlooked for circumstances has contributed to produce. The extent to which the spinning of cotton has been carried by this means, I endeavoured on a former occasion to estimate. Since that period, there has been a still farther increase in the investment of capital, the number of persons employed, and the quantity of yarn produced. The continued substitution of machinery for human labour is however a subject of deep interest, and it is my intention at some future period, to throw together for the consideration of the society, a like sketch of the other machines, antecedent to the one now described.

ON THE

FORMS OF THE CATENARY

IN

SUSPENSION BRIDGES.

BY MR. EATON HODGKINSON.

(Read February 8th, 1828.)

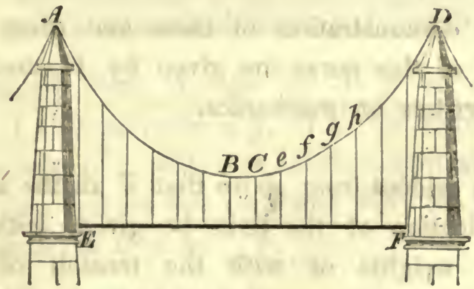
1. THE problem of the catenary has long been interesting; it was attempted by Galileo without success, the science of his time being inadequate to its solution. It afterwards engaged the attention of Leibnitz, the Bernoullis, Maclaurin, Huygens, &c. and was one of the earliest applications of the integral calculus, which was necessary to its investigation. But it is only in the present age, when the results of science are so frequently brought down to the uses of mankind, that this curve forming an element of suspension bridges has obtained an interest, it did not possess before, in becoming an object of practical utility.

But after the labours of the great men above, and the recent ones of Whewell, Gilbert and others, little is left to be done in a scientific point of view. Hence to their successors, it will be like searching over a well reaped field, where scarcely an ear can be gleaned.

The following pages will therefore be in a great measure devoted to the solution of a few problems rising out of this new application of the catenarian curve; seeking principally for the forms of the curves made by the chains under different circumstances.

2. First premising that the general form of the curve A B C D

FIG. 1.



which we will suppose to be the main chain supporting one side of the bridge, (A and D being the points of suspension, and E F the road-way) is a catenarian polygon, some of whose properties are as follow:

1st. Suppose the part or link BC horizontal, the sum of the weights supported by the vertical rods hanging from C e f g, and sustaining part of the road-way with any thing upon it, is as the tangent of the inclination of $g\,h$ to the horizon.

2nd. The weight borne at any joint g is as the difference of the tangents of inclination of the links $g\,h$ and $f\,g$ to the horizon.

3rd. The strain on any link, as gh, in the direction of its length, is as the secant of its angle of inclination to the horizon. The strength of the link ought therefore to be in that proportion.

The demonstration of these and other properties of this curve are given by Hutton and most writers on mechanics.

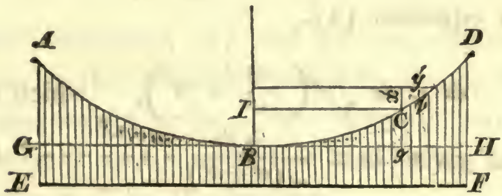
It is evident from above that if all the angles of inclination of the links be given, with one of the weights or with the tension of one link, we shall be enabled to find all the other weights, and the tensions of all the links.

3. These properties too would equally apply to a flexible chain alone, if the weights of its

parts were so disposed as to make it assume the same curve. Hence, when the form of the polygon or curve is one of the data, it is easy to find the rest.—But if instead of the curve we have the weights and the tension of some part of the curve given to find the form of the curve the problem is frequently much more difficult. We will now proceed to the general consideration of this latter case.

4. Let then in the catenarian curve A B C D,

FIG. 2.



suspended from A and D, B I be vertical $= x$, I C horizontal $= y$, B C the curve $= z$, the weight of z with any other weight attached to it (as that of the road-way EF, &c.) $= w$, the tension at B $= a$.

Now the chain B C, whether it have any other weight hanging from it or not, is kept at rest by three forces; by the tension at B in the direction y', by the tension at C in the direction of z', and by gravity in the direction

x'. Therefore by the principles of statics these forces are as the sides of the incremental triangle formed by their directions.

$$\therefore y' : x' :: a : w$$

Whence $dy : dx :: a : w$

and $$\frac{dx}{dy} = \frac{w}{a} \text{ - - - - - - - - } (1)$$

In every curve $dz = \sqrt{\left(dy^2 + dx^2\right)} = dx\sqrt{\left(\frac{dy^2}{dx^2} + 1\right)}$.

Hence substituting for $\frac{dy}{dx}$ its value derived from equation (1).

$dz = dx\sqrt{\left(\frac{a^2}{w^2} + 1\right)}$, which transposed gives $dx = \frac{w\,dz}{\sqrt{(a^2+w^2)}}$ - - - - - - - - (2)

If in equation (2) we substitute for dx its value derived from equation (1) we have

$$\frac{w\,dy}{a} = \frac{w\,dz}{\sqrt{(a^2+w^2)}}$$

$$\therefore dy = \frac{a\,dz}{\sqrt{(a^2+w^2)}} \text{ - - - - - - - } (3)$$

The formula (1) gives $\frac{dx}{dy} = \frac{w}{a}$; and since

$\frac{dx}{dy}$ = the tangent of the inclination of the curve at C to the horizon,

$\therefore$ Tangent of inclination at C $= \frac{w}{a}$ - - (4).

From equation (3) we have $\frac{dz}{dy} = \frac{\sqrt{(a^2 + w^2)}}{a}$, where $\frac{dz}{dy}$ is the secant of the inclination at C to the horizon; and as this quantity is the ratio of the force or tension at C to that at B, and a the tension at B

$$\therefore \text{ tension at C} = \sqrt{(a^2 + w^2)} \text{ - - - - } (5)$$

The conclusions (4) and (5) are equivalent to those in the catenarian polygon above, since here the weight w is as the tangent, and the tension at C as the secant of the inclination of the curve at C to the horizon.

Cor. 1. If $w = z$, or the catenary be the ordinary one in which the curve is of uniform weight, we have a = tension at B in lengths of the curve. Substituting then z for w in the equations above we have:

From equation (2) $dx = \frac{z\,dz}{\sqrt{(a^2+z^2)}}$. Integrating ($x = o$, when $z = o$)

$$x = \sqrt{(a^2+z^2)} - a \text{ - - - } (6)$$

From equation (3) $dy = \frac{a\,dz}{\sqrt{(a^2+z^2)}}$. Integrating ($y = o$, when $z = o$)

$$y = a.\ \text{hyp. log.}\ \frac{z + \sqrt{(a^2+z^2)}}{a} \quad \text{- - (7)}$$

Substituting for z in equation (7) its value derived from (6) gives

$$y = a.\ \text{hyp. log.}\ \frac{x + a + \sqrt{(x^2 + 2ax)}}{a} \quad \text{- - (8)}$$

From equation (4), tangent of inclination at C to horizon $= \frac{z}{a}$ - - - - - - - - - - - - - (9)

From equation (5) tension at C $= \sqrt{(a^2+z^2)}$ - - (10)

Cor. 2nd. The formulæ of the uniform catenary may be rendered rather more simple, by putting some multiple of a for the variable quantity. Thus suppose $ma = z$, the cases of the last corollary give

$$x = a\left(\sqrt{(1+m^2)} - 1\right),\quad y = a\cdot \text{hyp. l.}\left(m + \sqrt{(1+m^2)}\right),$$

$$\therefore \frac{x}{y} = \frac{\sqrt{(1+m^2)} - 1}{\text{hyp. log.}\left(m + \sqrt{(1+m^2)}\right)},$$ tangent of elevation at C $= m$, tension at C $= a\sqrt{(1+m^2)}$.

If $m = 1$, $x = .4142a$, $y = .8813a$, $\frac{x}{y} = \frac{4142}{8813}$, $\therefore y = 2.127\,x$. Tangent of elevation at C = 1, ∴ Angle = 45°. Tension at C = 1.4142 a.

Whence it appears that in the uniform catenary the values of x and y do not depend on the length of the curve alone, but on the ratio $\frac{z}{a} = m$, the tangent of the inclination of the curve at that point to the horizon. And where the inclination of the curve is the same, the ratio of x to y is a constant quantity, whatever the values z and a, and consequently the form of the common catenary may be. And at the point where $a = z$, m being then $= 1$, the angle is always 45°, and the ratio $\frac{x}{y} = \frac{4142}{8813}$.

We will now proceed to the application of the general formulæ above in suspension bridges.

5. In the chain bridge the strain proceeds from three distinct causes,—the weight of the catenarian chain,—the weight of the road-way,—and that of the suspension rods which connect by vertical lines the two former together; the last of these weights being very small comparatively with the others, may probably without much error be neglected, we shall however introduce it.

To find the value of w in the preceding formulæ call:—

The weight of a unity of length in the curve $= b$

The weight of a unity of length in the road-way, which is supposed to be divided transversely into separate parts, and may include any weight uniformly distributed over it, with that of the suspension rods below the horizontal line G B H. (*fig.* 2.) $= c$

The weight of a unity of vertical surface in the suspending rods, (the rods being here supposed to be uniformly distributed, and indefinitely near to one another, and therefore reckoned as a uniform surface.) $= e$

6. In the small triangle formed by $x'\ y'\ z'$, (*fig.* 2,) the weight therefore of the part z' of the chain $= b\ z'$, and the contemporary increment of the road-way being y', its weight $= c\ y'$, and the increment of the suspending rods above G H, being $=$ the area (C g) $\times\ y' = x\ y'$, its weight $= e\ x\ y'$.

Hence increment of w or $w' = b\ z' + c\ y' + e\ x\ y'$.

Ultimately $d\,w = b\,d\,z + c\,d\,y + e\,x\,d\,y$.

Integrating $w = b\,z + c\,y + e\int x dy$. Which needs no correction, since x, y and z vanish together.

Substituting this value for w, in the equation (1) where $\frac{adx}{dy} = w$ we have

$$\frac{adx}{dy} = bz + cy + e\int xdy \text{ - - - - - } (11),$$

which is the general differential equation of the curve when the weights of the chain, road-way, and suspension roads are considered. The road-way being here supposed to be divided by transverse cuts into indefinitely small separate parts, capable of sliding past one another, each part forming a separate weight: which is admissible since the curve must always adjust itself so as to leave the road-way a straight line.

7. The equation (11) being complicated, and therefore not so useful in practice, we will first consider the cases where one or other of the weights are neglected.

Prob. 1st. To find the form of the curve when c and e are each equal to nothing, or the weights of the road-way and suspension rods are omitted.

The equation (11) then becomes $\frac{adx}{dy} = bz$, or $\frac{a}{b} \cdot \frac{dx}{dy} = z$; which is the differential equation of the common catenary, whose tension at the vertex B in lengths of the chain is $\frac{a}{b}$.

Prob. 2nd. Suppose the road-way with its load (if any) to be so heavy that the weight of the chain and suspension rods may be neglected. What is the nature of the curve?

Here b and e are each equal nothing, and Equation (11) becomes $\frac{adx}{dy} = cy$, $\therefore \frac{a}{c} dx = ydy$, integrating ($x = o$ when $y = o$) gives $\frac{2a}{c} x = y^2$. The curve is therefore a parabola, whose parameter is $\frac{2a}{c}$, the vertex being at B.

This Problem is a case which might often occur, particularly where the span or width of the river is very small, and the height of the attachments great. A drove of oxen or a loaded waggon passing over the bridge, (*fig.* 3,) is an example of it.

FIG. 3.

Scholium. The weight of the chain and suspension rods in this instance being considered so small, comparatively with the load, that they might be neglected, it is evident that a small increase of strength, and consequently weight in the higher parts of the chain would not materially alter the form of the curve. Hence if we wished to adapt its strength to the strain upon it, we have from what was said (Art. 2nd.) in the commencement of this essay, only to increase the strength of the different parts of the curve, according to the secant of their angles of inclination to the horizon. To perform which—since $\frac{dx}{dy}$ is the tangent of the curve's inclination, taking from the equation of the curve, the value of this ratio, or $\frac{cy}{a}$, in numbers, we may by a table of natural tangents find the angle at any point, and thence its secant for the strength.

The same observation will apply to the two following problems.—

Prob. 3rd. Suppose b and c Equation (11) are each $= o$, the suspension rods only being considered.

We have then $\frac{adx}{dy} = e\int xdy$. Differentiating, making dy constant and dividing by dy we have $\frac{ad^2x}{dy^2} = e\,x$.

Multiplying by $2\,dx$ gives $a.\frac{2dxd^2x}{dy^{\,2}} = 2\,e\,x\,dx$.

Integrating $a\left(\frac{dx}{dy}\right)^2 = ex^2 + C$. But when $x = o$, the curve being perpendicular to the axis of the abscissæ, dx is infinitely small comparatively with dy $\therefore \frac{dx}{dy} = o$, and Const. $= o$. Hence $dy = \sqrt{\frac{a}{e}} \cdot \frac{dx}{x}$,

$$y = \sqrt{\frac{a}{e}}\ \text{hyp. log.}\ x + \text{Const.}$$

Cor. When $x = o$, $y = -\infty$, when $x = 1$, $y =$ Const., the curve has therefore a horizontal asymptote. When y is positive, x and y increase together.

An analogous problem to this with some inquiries of the same nature, is met with among the excellent investigations on the catenary in Whewell's Mechanics.

Prob. 4th. Suppose the weights of the roadway and suspension rods are considered,............ Here b, only, $= o$, in the formula (11), and $\frac{a dx}{dy} = cy + e\int x\,dy$.

Differentiating making dy constant and dividing by dy gives $\frac{a d^2 x}{dy^2} = c + e\,x$.

Multiplying by $2dx$ we have

$$a.\frac{2dxd^2x}{dy^2} = 2cdx + 2exdx.$$

Integrating $a\left(\frac{dx}{dy}\right)^2 = 2cx + ex^2 + C.$

But when $x = o$, $\frac{dx}{dy} = o$, $\therefore$ Const $= o$.

Whence $dy = \frac{a^{\frac{1}{2}}dx}{\sqrt{(ex^2 + 2cx)}} = \sqrt{\frac{a}{e}} \cdot \frac{dx}{\sqrt{\left(x^2 + \frac{2c}{e}x\right)}}.$

$$\therefore\ y = \sqrt{\frac{a}{e}} \cdot \log. \left[x + \frac{c}{e} + \sqrt{\left(x^2 + \frac{2c}{e} x\right)} \right] + \mathrm{C}.$$

When $y = o$, $x = o$ $\therefore \mathrm{C} = -\sqrt{\frac{a}{e}}$ hyp. log. $\frac{c}{e}$.

$$\therefore\ y = \sqrt{\frac{a}{e}} \cdot \text{hyp. log.}\ \frac{x + \frac{c}{e} + \sqrt{\left(x^2 + 2\frac{c}{e}x\right)}}{\frac{c}{e}}.$$

This equation may be put under a different and rather more familiar form thus: multiply both sides by $\frac{c}{e}$ and divide by $\sqrt{\frac{a}{e}}$ and we obtain

$$\frac{cy}{\sqrt{ae}} = \frac{c}{e}\ \text{hyp. log.}^{*}\ \frac{x + \frac{c}{e} + \sqrt{\left(x^2 + 2\frac{c}{e}x\right)}}{\frac{c}{e}},$$

an equation of the common catenary, whose abscissa is $= x$, ordinate $= \frac{cy}{\sqrt{ae}}$; and tension at the vertex in lengths of its curve $= \frac{c}{e}$.

Prob. 5th. Suppose the weights of the chain and road-way are considered, the suspension rods only being neglected; to find the nature of the curve.

* All the logarithms in this paper are those called hyperbolic, whether mentioned so or not.

Here $e = o$, and the equation (11) becomes

$$\frac{adx}{dy} = bz + cy.$$

Differentiating, making dy constant, and substituting $\surd(dx^2 + dy^2)$ for dz gives

$$ad^2x = bdy\surd(dx^2 + dy^2) + cdy^2.$$

Putting $dx = wdy$, $d^2x = dwdy$. Substituting these in the last equation gives

$$adwdy = bdy^2\surd(w^2 + 1) + cdy^2,$$

whence $adw = bdy\surd(w^2 + 1) + cdy$,

$$\therefore\ dy = \frac{a}{b} \cdot \frac{dw}{\surd(w^2 + 1) + \frac{c}{b}}.$$

And $dx = wdy = \dfrac{a}{b} \cdot \dfrac{wdw}{\surd(w^2 + 1) + \frac{c}{b}}.$

To integrate the latter equation put $w^2 + 1 = v^2$, then $wdw = vdv$. Substituting we obtain

$$dx = \frac{a}{b} \cdot \frac{vdv}{v + \frac{c}{b}}.$$

Integrating gives $x = \dfrac{a}{b}\left[v - \dfrac{c}{b} \cdot \log.\left(v + \dfrac{c}{b}\right)\right] + \text{Const.}$

Substituting for v its value $\surd(w^2 + 1)$,

$$x = \frac{a}{b}\left[\surd(w^2+1) - \frac{c}{b} \cdot \log.\left(\surd(w^2+1) + \frac{c}{b}\right)\right] + \text{Const.}$$

But when $x = o$, $w = \dfrac{dx}{dy} = o$ $\therefore$ Constant $= -\dfrac{a}{b}$

$$\left[1-\frac{c}{b}\log.\left(1+\frac{c}{b}\right)\right]=\frac{a}{b}\left[-1-\frac{c}{b}\times-\log.\left(1+\frac{c}{b}\right)\right].$$

$$\therefore x=\frac{a}{b}\left[\sqrt{(w^2+1)}-1-\frac{c}{b}\log.\frac{\sqrt{(w^2+1)}+\frac{c}{b}}{1+\frac{c}{b}}\right].$$

To find the integral of the other expression $dy=\frac{a}{b}\cdot\frac{dw}{\sqrt{(w^2+1)}+\frac{c}{b}}$. We may make it rational by putting $\sqrt{(w^2+1)}=wv+1$, and finding the differential in terms of v; then $w=\frac{2v}{1-v^2}$, $dw=\frac{2(1+v^2)\,dv}{(1-v^2)^2}$, and $\sqrt{(w^2+1)}+\frac{c}{b}$

$$=wv+1+\frac{c}{b}=\frac{2v^2}{1-v^2}+1+\frac{c}{b}=\frac{1+\frac{c}{b}+\left(1-\frac{c}{b}\right)v^2}{1-v^2}.$$

We have therefore $dy=\frac{a}{b}\cdot\frac{dw}{\sqrt{(w^2+1)}+\frac{c}{b}}$

$$=\frac{a}{b}\cdot\frac{2(1+v^2)\,dv}{(1-v^2)^2}\times\frac{1-v^2}{1+\frac{c}{b}+\left(1-\frac{c}{b}\right)v^2}$$

$$=\frac{a}{b}\cdot\frac{2(1+v^2)\,dv}{(1-v^2)\cdot\left[\left(1+\frac{c}{b}\right)+\left(1-\frac{c}{b}\right)v^2\right]}$$

$$=\frac{a}{b}\cdot\frac{2(1+v^2)dv}{(1-v^2)\,(f+gv^2)},$$ putting $1+\frac{c}{b}=f$,

and $1-\frac{c}{b}=g$.

The co-efficient of dv being a rational fraction, is therefore always integrable, and by known rules.—Omitting then the process as tedious, and afterwards substituting for f g and v we have, since $w = o$, when $y = o$, for the correct integral

$$y = \frac{a}{b}\left[\log.\left(\sqrt{(w^2+1)} + w\right) \mp \frac{c}{\sqrt{(c^2-b^2)}}\cdot\right.$$

$$\left.\log.\frac{\pm\sqrt{(c^2-b^2)}w + c\sqrt{(w^2+1)} + b}{c + b\sqrt{(w^2+1)}}\right].$$

If c be less than b, or the weight of a given length of the road-way with its load be less than the weight of the same length of the chain, $\sqrt{(c^2 - b^2)}$ is imaginary, and the integral found wholly by logarithms will fail; it must therefore in part be expressed by circular arcs as below:—

$$y = \frac{a}{b}\left[\log.\left(\sqrt{(w^2+1)}+w\right) + \frac{c}{\sqrt{(b^2-c^2)}}\times\right.$$

$$\left.\left(\text{arc tang.}\frac{b+c\sqrt{(w^2+1)}}{\sqrt{(b^2-c^2)}w} - \text{arc of quadrant}\right)\right].$$

Cor. If $c = o$, the chain only will be considered, and the equations become

$$x = \frac{a}{b}\cdot\left(\sqrt{(w^2+1)}-1\right),$$

$$y = \frac{a}{b}\log.\left(\sqrt{(w^2+1)}+w\right).$$

The values of x and y above are obtained in terms of w (the tangent of the curve's inclination at any point to the horizon) which is nothing when x and y are each equal to nothing. Assuming therefore different values for w, and substituting them in the expressions for x and y, we shall have so many points in the curve.

8. When the strengths of the chain and road-way continue unchanged, b and c being then constant, the ratio of x to y at any point depends alone on the tangent w of the curve's inclination there. This curious property it appears then is not confined to the common catenary in which (Cor. 2. Art. 4) it was shewn to exist.

Prob. 6. We might find the values of x and y in terms of the sums of the weights of the chain and road-way, and thence deduce the values of z:—For since by equation (3) article 4,

$$adz = dy \sqrt{(a^2 + w^2)},$$

and in the supposition of this Problem $w = bz + cy$, (Art. 6,) $\therefore dz = \dfrac{dw}{b} - \dfrac{c}{b}\,dy$. Substituting this for dz in the preceding equation gives

$$\frac{a}{b}\,dw - \frac{ac}{b}\,dy = dy \sqrt{(a^2 + w^2)}$$

$$\therefore dy = \frac{a}{b} \cdot \frac{dw}{\frac{ac}{b} + \sqrt{(a^2 + w^2)}} \text{ - - - - - } (1)$$

And since, (1) Art. 4, $adx = wdy$,

$$dx = \frac{1}{b} \cdot \frac{wdw}{\frac{ac}{b} + \sqrt{\left(a^2 + w^2\right)}} \text{ - - - - - - } (2)$$

If we substitute in the equation $dz = \frac{dw}{b} - \frac{cdy}{b}$ above, the value of dy obtained from eqnation (1) we have

$$dz = \frac{dw}{b} - \frac{ac}{b^2} \cdot \frac{dw}{\frac{ac}{b} + \sqrt{\left(a^2 + w^2\right)}} \text{ - - - } (3)$$

Of these differentials that in equation (2) may be immediately integrated if we put n^2 for $a^2 + w^2$ and substitute accordingly.—To integrate equation (1) and the latter part of equation (3) make $\sqrt{(a^2 + w^2)} = a + wv$, and by ordering as in the second differential of the preceding Problem we obtain

$$\frac{dw}{\frac{ac}{b} + \sqrt{\left(a^2 + w^2\right)}} = \frac{2b(1 + v^2)\,dv}{\left(b + c + (b - c)\,v^2\right)(1 - v^2)},$$

a rational function which may therefore be integrated by the rules for such quantities; and, that done, the integrals of equations (1) and (3) are easily obtained.

Prob. 7. To find the form of the curve when the weights of the chain, road-way, and suspending rods are all considered. We have here the general equation (11) in which

$$adx = bzdy + cydy + edy\int xdy.$$

To integrate this, we shall assume the weight bz of the curve to be the same as it would have been if the curve were a common catenary from the same span, versed sine, and thickness of chain: an assumption which may be admitted, as the difference of the weights of the two curves, their lengths being so nearly alike, could have had very little influence on the form of the curve sought, when in the bridge. Let then $a'dx = bzdy$ be the equation of the common catenary above, in which a' is the tension at the vertex, and if we substitute for $bzdy$ in the general equation above its value $a'dx$ from this, we have

$$adx = a'dx + cydy + edy\int xdy, \text{ or}$$

$$(a - a')\,dx = cydy + edy\int xdy.$$

Differentiating, making dy constant, and dividing by dy^2 gives

$$(a - a')\,\frac{d^2x}{dy^2} = c + ex\ .$$

This is of the same form as the second equation obtained in Problem 4; ordering then as in that Problem we have

$$y = \sqrt{\left(\frac{a-a'}{e}\right)} \log. \frac{x + \frac{c}{e} + \sqrt{\left(x^2 + \frac{2c}{e}x\right)}}{\frac{c}{e}}.$$

Or multiplying by $\frac{c}{e}$ and dividing by $\sqrt{\left(\frac{a-a'}{e}\right)}$ we obtain

$$\frac{cy}{\sqrt{(ae-a'e)}} = \frac{c}{e} \log. \frac{x + \frac{c}{e} + \sqrt{\left(x^2 + \frac{2c}{e}x\right)}}{\frac{c}{e}}.$$

Which is the equation of the ordinary, or uniform, catenary whose abscissa is x, ordinate $\frac{cy}{\sqrt{(ae-a'e)}}$, and tension at the vertex in lengths of its curve $= \frac{c}{e}$.

The ordinate y is therefore equal to that of the common catenary, above, divided by $\frac{c}{\sqrt{(ae-a'e)}}$.

Cor. If $e = o$, or the weight of the suspending rods is neglected, the preceding equation,

$(a-a')\,dx = cydy + edy\int xdy$, becomes

$$(a-a')\,dx = cydy.$$

Integrating, ($x = o$, when $y = o$,) gives

$$2\,\frac{a-a'}{c}\,x = y^2.$$

An equation of the common parabola, whose vertex is at B (*fig.* 2,) and parameter $= 2\frac{a-a'}{c}$. From which it appears that by a near approximation, in the case of Prob. 5, where the weight of the chain and road-way are considered, the curve is a parabola; as in Prob. 2nd, but with a different parameter.

9. In the preceding problems the thickness of the catenarian chain, forming the main support of the bridge, was uniform—in those which follow the nature of the curve of equilibrium is sought when the strength of the chain is as the strain upon it.

10. Referring then to the general properties of the catenary in Art. 4, and making s to represent the weight of any length z of the variable chain alone, (w representing that of the chain and any other bodies attached to it;) and supposing moreover the weight of any very small length dz at the bottom B to be bdz (Art. 6:) we shall have, when the strength of the chain is as the strain upon it, by equation (5) Art. 4,

$$a : \sqrt{(a^2 + w^2)} :: bdz : ds.$$

$$\therefore dz = \frac{a}{b} \cdot \frac{ds}{\sqrt{(a^2 + w^2)}} \text{ - - - - - } (1)$$

Prob. 8. To determine the curve of equilibrium, and the strength of the chain in every part, when the chain only is considered.

In this case $s = w$, in equation (1) Art. 10.

Hence, $$dz = \frac{a}{b} \cdot \frac{ds}{\sqrt{(a^2+s^2)}} \quad - - - (2).$$

Integrating ($s = o$, when $z = o$),

$$z = \frac{a}{b} \log. \frac{s + \sqrt{(a^2+s^2)}}{a} \quad - - - - (3),$$

an equation of a uniform catenary between a curve s, and ordinate bz.

If in equations (2) and (3) article 4, we substitute for dz, its value derived from equation (1) art. 10, and change w into s we have

$$dx = \frac{a}{b} \cdot \frac{sds}{a^2+s^2},$$

$$dy = \frac{a^2}{b} \cdot \frac{ds}{a^2+s^2}.$$

Integrating these equations, we have

$$x = \frac{a}{2b} \cdot \log. \frac{a^2+s^2}{a^2} \cdot - - - - - - (4),$$

and by = a circular arc, whose radius is a, and tangent s. Or reducing the quantities to radius 1, we have

$$y = \frac{a}{b} \cdot \text{arc} \left(\tan. = \frac{s}{a}\right) - - - - - - (5).$$

Mr. Davies Gilbert has given, in his excellent memoir on suspension bridges, (Philosophical Transactions, 1826,) a solution of this Problem, with tables deduced from it; assuming that the road-way and suspending rods might both be considered as collected in the chain. But it appears to me that, in many cases, the road-way being much shorter than the curve, a near approximation to the form of the curve could only be obtained by considering the road-way as a separate part. The solution of the following problem will therefore include this latter case.

Prob. 9. To find the curve of equilibrium, and the law of variation in the thickness of the chain in a suspension bridge, when the weight of the chain and road-way are considered.

If in the formula of the variable catenary (Equation 3, Art. 4,) where $adz = dy\ \sqrt{(a^2 + w^2)}$, we substitute for dz, its value $\frac{a}{b} \cdot \frac{ds}{\sqrt{(a^2+w^2)}}$, derived from equation (1) above, we obtain,

$$\frac{a^2}{b} ds = dy\ (a^2 + w^2) \quad - - - - - - (6).$$

Now, from the supposition of this problem, $w = s + cy, \therefore dw = ds + cdy$. Eliminating ds, between equation (6) and this, gives,

$\frac{a^2}{b}(dw - cdy) = (a^2 + w^2)\,dy$, which transposed leaves

$$dy = \frac{a^2}{b} \cdot \frac{dw}{a^2(1+\frac{c}{b})+w^2} = \frac{a^2}{b} \cdot \frac{dw}{a^2 n + w^2} \text{ - - - } (7),$$

putting n for $1 + \frac{c}{b}$, as it will often occur. If we multiply both sides of equation (7) by $\frac{b}{a^2}$, and $a^2 n$, we obtain $(b + c)\,dy = \frac{a^2 n\, dw}{a^2 n + w^2}$.

The integral of this is a circular arc $(b + c)\,y$, whose radius is $an^{\frac{1}{2}}$, and tangent w: or if we reduce these quantities to radius unity we have

$$y = \frac{a}{bn^{\frac{1}{2}}} \cdot \text{arc}\left(\tan. = \frac{w}{an^{\frac{1}{2}}}\right) \text{ - - - - - - - - } (8).$$

To find s.—Suppose the quantities a, b and c are given; we shall, assuming different values for w, be enabled to find, by equation (8) and a table of natural tangents, y in terms of w; and thence obtain the values of s, since $s = w - cy$.

To find x.—Since from equation 1, Art. 4, $adx = wdy$; substituting in this for dy its value derived from equation (7) above, we have,

$$dx = \frac{a}{2b} \cdot \frac{2\,wdw}{a^2 n + w^2},$$

whence $x = \frac{a}{2b} \log. (a^2 n + w^2) + \text{Const.}$

But w $(=s+cy)$ and x vanish together, $\therefore$ Const. $= -\frac{a}{2b}\log.\, a^2 n$, and,

$$x = \frac{a}{2b}\log.\ \frac{a^2 n + w^2}{a^2 n} \text{ - - - - - - - } (9),$$

To obtain z. If in $adz = dy\sqrt{(a^2+w^2)}$, (equation 3, Art. 4), we substitute for dy its value derived from equation (7) above, we obtain,

$$dz = \frac{a}{b}\cdot\frac{dw\sqrt{(a^2+w^2)}}{a^2 n + w^2} \text{ - - - - } (10).$$

where $a^2 n + w^2 = a^2\left(1+\frac{c}{b}\right) + w^2 = a^2 + \frac{a^2 c}{b} + w^2$.

Dividing the numerator of equation (10) by its denominator transposed gives,

$$\frac{\sqrt{(a^2+w^2)}\,dw}{a^2+w^2+\frac{a^2c}{b}} = \frac{dw}{\sqrt{(a^2+w^2)}} - \frac{a^2c}{b}\cdot\frac{dw}{\left(a^2+w^2+\frac{a^2c}{b}\right)\sqrt{(a^2+w^2)}}.$$

If $a^2 n + w^2$ be put for $a^2 + w^2 + \frac{a^2c}{b}$, as in equation (10), we have,

$$dz = \frac{a}{b}\left\{\frac{dw}{\sqrt{(a^2+w^2)}} - \frac{a^2c}{b}\cdot\frac{dw}{(a^2n+w^2)\sqrt{(a^2+w^2)}}\right\} \text{ - - - } (11).$$

The first of these differentials is of the same form as the first in Prob. 8, and presents no difficulty. The second may be made rational, and therefore integrable, by putting $\sqrt{(a^2+w^2)} = a + wv$, and finding the value in terms of v, as in Prob. 5.—Neglecting the process, we have, when w is restored and the integrals corrected,

$$z=\frac{a}{b}\left\{\log.\frac{w+\sqrt{(a^2+w^2)}}{a}-\left(\frac{c}{nb}\right)^{\frac{1}{2}}\log.\frac{n^{\frac{1}{2}}\sqrt{(a^2+w^2)}+w\left(\frac{c}{b}\right)^{\frac{1}{2}}}{\sqrt{(na^2+w^2)}}\right\}\text{- - -}(12).$$

Cor. If $c=o$, or there be no horizontal road-way, $s=w$, and the formulæ for y, x, and z, above will become,

$$y=\frac{a}{b}\text{ arc }\left(\tan.=\frac{s}{a}\right),$$

$$x=\frac{a}{2b}\log.\frac{a^2+s^2}{a^2},$$

$$z=\frac{a}{b}\log.\frac{s+\sqrt{(a^2+s^2)}}{a},$$

deductions in accordance with those in Problem 8.

We should express the formulæ of this Problem somewhat more simply if we assumed the variable quantity w to be some mnltiple, m, of the tension at the apex: thus, supposing $w=ma$, and consequently $\frac{w}{a}=m$, gives,

$$y=\frac{a}{n^{\frac{1}{2}}b}\cdot\text{ arc }\left(\tan.=\frac{m}{\sqrt{n}}\right),$$

$s=ma-cy$, (by supposition of this prob.)

$$x=\frac{a}{2b}\log.\left(1+\frac{m^2}{n}\right),\text{ and,}$$

$$z=\frac{a}{b}\left\{\log.\left(m+\sqrt{(1+m^2)}\right)-\left(\frac{c}{nb}\right)^{\frac{1}{2}}\log.\frac{\sqrt{n(1+m^2)}+m\sqrt{\frac{c}{b}}}{\sqrt{(n+m^2)}}\right\}.$$

Though the formula for z is complex, the values of x, y and s, which would be much more frequently needed by persons using them for the purpose of building a bridge, may be obtained without great difficulty; as in the following example to these last found.

Suppose in a suspension bridge $a = 600$ tons, b and c be 2 and 1 ton per yard respectively. To find the values of y, s, x and z, when $w = 300$ tons.

Here $m = \frac{w}{a} = \frac{300}{600} = .5$, $n = 1 + \frac{c}{b} = 1 + \frac{1}{2} = 1.5$,

$$y = \frac{600}{(1.5)^{\frac{1}{2}} \times 2} \times \text{arc}\left(\tan. = \frac{.5}{(1.5)^{\frac{1}{2}}}\right) = 244.9 \times \text{arc of } 22^\circ.\ 12'.$$

But arc of $90 = 1.5708$, and $22^\circ.12' = 22.2$,

$\therefore 90 : 22.2 :: 1.5708 : .38746 =$ arc of $22^\circ.12'$, whence

$y = 244.9 \times .38746 = 94.89$ yards $= 284$ feet 7 inches,

$s = .5 \times 600 - 1 \times 94.89 = 205.11$ tons,

$$x = \frac{600}{4} \log.\left(1 + \frac{.5^2}{1.5}\right) = 150 \times \log.\ 1.16666 = 150 \times 2.3026 \times$$

common log. $1.16666 = 23.124$ yards, or 69 feet; since

hyp. log. $= 2.3026 \times$ common log., and

$$z = \frac{600}{2}\left\{2.3026 \times \text{com. log}\left(.5 + \sqrt{(1 + .5^2)}\right) - \left(\tfrac{1}{3}\right)^{\frac{1}{2}} \times 2.3026 \times\right.$$

$$\left.\text{com. log.}\frac{\sqrt{1.5 \times 1.25} + .5\sqrt{\frac{1}{2}}}{\sqrt{1.75}}\right\} = 98.6 \text{ yds. or } 295 \text{ feet } 9 \text{ inches.}$$

If now other weights be assumed for w we may thus find the values for as many points as we please in the curve.

ON THE

CHAIN BRIDGE,

AT

BROUGHTON.

BY MR. EATON HODGKINSON.

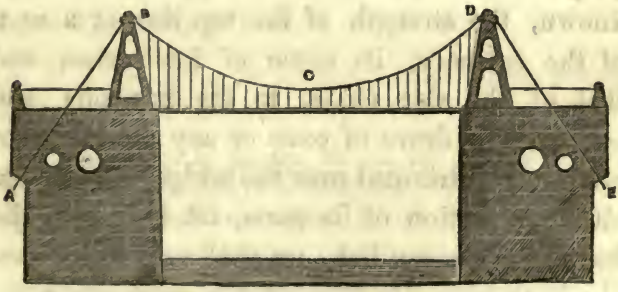
(Read February 8th, 1828.)

1. THIS elegant though comparatively small structure has, as is well known to almost every one in this neighbourhood, been recently erected over the river Irwell, about a mile and a half from Manchester. It is the private property of John Fitzgerald, Esq., and connects Pendleton with Broughton.

It is not the intention of the writer to give a popular description of this bridge, which is only 144 feet 6 inches span; but to estimate numerically the principal strains on the structure, and compare its strength with them. In this respect, the analysis made of the Broughton bridge would mostly apply with equal propropriety to any other, whether great or small; and the reason for fixing on this bridge was, that it was the nearest.

2. The figure below is a representation of the bridge, the road-way of which is suspended from two double wrought iron cables, whose links are straight round bars connected at each end by three small elliptical links and two bolts; the bar and coupling being about 5 feet long. The whole cable on one side, and which sustains half of the bridge, is represented by A B C D E.

FIG. 1.



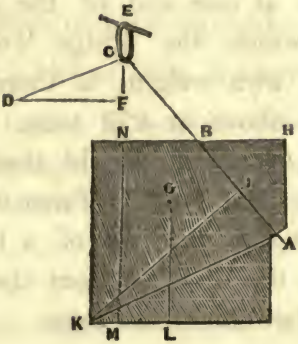
It is firmly fixed at the ends A and E by large disks at the back of the masonry, which it passes through; and is sustained at B and D by a link, at each place, suspended vertically, and hanging within a strong pyramidal support of cast iron with a quadrangular base. The point of suspension is therefore moveable, by the deviation of that link a little from the perpendicular, and thus allows for expansion and contraction in the cable.

The side chains A B and E D, are nearly inclined in equal angles to the horizon, and the same may be said of the top links of the catenary at B and D.

3. To estimate the strength of the bridge.—In an existing structure any of its dimensions can be taken at pleasure, so as best to suit our purpose; we shall therefore first assume as known, the strength of the top link at B or D of the catenary, its angle of inclination, and that of the side chain, to the horizon; and supposing a drove of oxen or any other weight uniformly distributed over the bridge so as not to alter the position of its parts, till it fails by the fracture of the top link, we shall seek as follows.

4. To find the horizontal tension, the weight supported by the bridge, and the tensions of the side chain and vertical link.

FIG. 2.



If DC (*fig.* 2.) be the top link of the catenary, AC the side chain (which here is nearly straight,) and CE the vertical link suspended from the pillar resting on N, and supporting the whole; and if we make DF horizontal, and CF vertical, the angle CDF = 18°. 32′*. Now the whole tension in CD = 351 tons†: resolving this into its horizontal and vertical effects gives

$$351 \times \cos. 18°. 32'‡ = 351 \times .9481 = \overset{\text{tons.}}{332.8} = \text{horiz. tension,}$$
$$351 \times \sin. 18°. 32' = 351 \times .3179 = 111.6 = \text{weight supported.}$$

* The angles of the top link of the catenary with the horizon at the two ends of the Bridge, are 18°. 35′ and 18°. 30′, their mean therefore = 18°. 32′.

† The chain A B C D E (*fig.* 1.) is uniform throughout, and has, taking both cables, 13 square inches in its cross section. The cables would therefore require a direct strain of 13 × 27 = 351 tons to break them; the mean strength of a bar of iron an inch square, being 27 tons, (*Barlow's Essay on the strength of Timber*, Appendix.)

‡ The sines, cosines &c. in this paper are the natural ones.

The weight supported as given above is only what is borne at one end of the bridge: the whole load which the bridge would sustain, including its own weight, is therefore double the quantity above or 223 tons. It appears likewise that the piers would then be drawn toward each other with a force of 332 tons, and this acting at the end of a lever, whose length is CK, the distance from the top of the chain to the bottom of the river.

The inclination of the side chain is 33°. 10′*, and it must sustain the horizontal force of 332 tons as above; therefore, force in CA $\times$ cos. 33°. 10′ = 332; and since cos. 33°. 10′ = .837, $\therefore$ force in CA $= \frac{332}{\cdot 837} = 396.6$ tons = tension of side chain.

Resolving this last into its vertical effect gives $396.6 \times \sin. 33°. 10' = 396.6 \times .547 = 216.9$ tons = vertical pressure from side chain.

If to this last quantity be added the vertical pressure from the catenary, or 111.6 as found above, we have $216.9 + 111.6 = 328.5$ tons = whole force in CF = tension of vertical link. This is borne equally by the two pillars on one side of the river, therefore, weight borne by one

* The angles of the side chains with the horizon are 33°. 30′ and 32°. 50′: mean = 33°. 10′.

$=\frac{328.5}{2}=164.2$ tons: dividing this quantity by 27, the number of tons required to tear asunder a square inch bolt of iron, we have $\frac{164.2}{27}=6.08$, the square inches in section which one of the vertical links must have to support the weight. A strength which is exceeded by the links in the bridge.

From the estimates above, it appears that the tensions of the bottom and top link of the catenary, and of the side chain, are 333, 351, and 397 respectively; and therefore their strength ought to be as these numbers. But the whole chain is uniform, hence the top link is weaker than the middle part in the ratio of 333 to 351, and the side chain will bear less than the middle in the proportion of 333 to 397, or of 5 to 6 nearly. Attention to this subject would cause a saving in iron, which otherwise, besides its expense, does harm by unnecessary weight.

5. If the link CE were not vertical, we might obtain the tensions as above, by equating among themselves all the forces of the same kind whether horizontal or vertical, and thus arrive at two equations from which to take the unknown quantities; or we might proceed as below.

From the principles of statics, when three forces balance each other, any one is as the sine of the angle formed by the other two; hence, when the angles and one force are given, we may easily find the other forces, or the reverse. —Example. One of the links CE in the bridge inclines 6° from the vertical*, the point C being turned toward the river: what would be the tensions in CE and CA, and the effect to rend the pier, during the fracture of the link DC? the inclinations of DC and AC being as in Art. 4.

We have DCF $= 90° - 18°.32' = 71°.28'$, and BCF $= 90° - 33°.10' = 56°.50'$; therefore DCB $= 71°.28' + 56°.50' = 128°.18'$, DCE = supplement of $71°.28' + 6° = 114°.32'$, BCE $= 360° -$ (DCE + DCB) $= 117°.10'$.
Whence sin. DCB = .780, sin. DCE = .910, sin. BCE = .884; and since tension in CD = 351 tons, by Art. 4, we have as below:—
.884 : .910 :: 351 : 361 tons = tension in CA,
.884 : .780 :: 351 : 309 tons = tension in CE.

If we resolve the tension in CE just found into its vertical and horizontal effects, we have $309 \times \cos. 6° = 309 \times .9945 = 307.3$ tons, and $309 \times \sin. 6° = .309 \times .1045 = 32.3$ tons. Hence

* The defect mentioned is being removed at the time of printing this paper.

the corners of the pier, on which the pillars stood, would be acted on by a vertical force of 307 tons, and a horizontal one of 32 tons; the tendency therefore to separate the pier slanting in the form of a wedge must be very great. The deflected link is only at one corner of the bridge, and therefore that corner would be exposed to half the above effect.

6. To find the strength of the piers to resist the forces in Art. 4.

Should failure in the piers take place, it may be in one of the following ways:—

1st. They may slide in a mass toward each other in consequence of the horizontal force, which we have shown would in the extreme case above be 332 tons.

2nd. The fastening of the chain being by means of a large disk of iron at A, the pier might separate in some line KA; the part, whereof K A H N K is a plane, turning round a point K in the foundation.

3rd. The pier might fail by the wedge whose end is B A H being drawn upwards, all the rest remaining undisturbed.

The first of these modes of failure would be obviated by giving sufficient weight to the pier, and thus making its friction greater than the horizontal tension. Suppose the weight of the pier and bridge with supports resting on the pier to be z, and that friction be $\frac{1}{5}$ of the pressure, then $\frac{z}{5} = 332$, $\therefore z = 1660$ tons; a weight which would probably be surpassed in this bridge.

The second mode of failure might be prevented as below; suppose w be the weight of the part whose section is KAHNK, G its center of gravity, w' the pressure on N from the loaded bridge and pillars, L and M points in perpendiculars from G and N to the horizon, and KI perpendicular to CA, then make $w \times \text{KL} + w' \times \text{KM} =$ force in AC $\times$ KI, or $=$ horizontal force $\times$ distance MC. In this respect the pier seems to have more than sufficient strength.

The third mode of failure above is avoided by giving sufficient weight to the wedge, whose end is BAH, or by fastening it well with cramps to the other parts of the masonry.

Where there are no cramps used, let x be the weight of the wedge, and as this weight acts in the direction HA perpendicular to the horizon, its effect in the direction BA is $= x \times \cos. \text{BAH}$. But $\cos. \text{BAH} = \cos. \text{BCF} = \sin. 33°.10' = .547$, and tension of side chain $= 396.6$ tons, Art. 4, $\therefore x \times .547 = 396.6$, whence $x = 735$ tons.

In the piers of this bridge, I understand, there are no cramps, and the solid content of the wedge is 150 yds. and its weight about 280 tons. Hence the weight of the wedge is little more than $\frac{1}{3}$ of what it ought to be, if the surface represented by the line AB were a smooth one, and there were no allowance made for the tenacity of the cement. The resistances from roughness of surface and tenacity cannot be estimated, but considering that perhaps not $\frac{3}{4}$ of the wedge would be removed at all, the rest separating from it, it seems fair to conclude, that the pier would give way in this place when the chains were not more than half loaded to their breaking weight.

From the preceding inquiry it appears, that were the bridge overloaded it would give way,—1st in the pier as above,—2nd in the side chain,—3rd in the top link of the catenary,—4th in the succeeding ones, and 5th at the bottom at C, *Fig.* 1.

ADDITIONS.

In the preceding paper, we have supposed the iron to be of the mean strength of sound metal; but if it should have any unseen cracks or bad weldings in it, there is no saying what would be an adequate compensation for them, for its strength might be reduced in any degree. In the specimens I have seen in broken chains, some of which have been of great dimensions, most of the failures have been through defective welding; hence the necessity of testing all chains, the consequences of whose failure would be serious: they ought I think to be tried with a high test, for if a chain were tried with 9 tons per square inch, there is no proof, further than the probability the defect would have shown itself, that it would bear 12 tons, much less 27.

There may, however, be urged two objections against high tests: 1st. The difficulty of applying such immense strains, and which it is probable leaves some bridges without their chains being tested at all: 2nd. That high tests injure the elasticity of the metal, and consequently impair its strength.

The first of these objections is removed when the parties making the chains are situated near to some public testing machine, as that at Liverpool. The second objection has great plausibility, and I took the following methods to satisfy myself how far it was really the case.

It is well known, that if a body be stretched with a force, such as but little to exceed $\frac{1}{3}$ of its ultimate strength, its elasticity will remain perfect; but if the force be greater, as $\frac{1}{2}$ or $\frac{2}{3}$ of the full strength, the elastic force will be impaired, and the length of the body somewhat increased, as is shown in the following experiment. I took a thin wire 20 feet long, and fastening it firmly at one end to an elevated object, let it hang vertically with a small weight attached to the other end to render it straight: then, hanging weights at the bottom, increasing by 10lbs. at a time, and always taking the preceding weights off before others were laid on, I found that the wire constantly returned to the same length, till 90lbs. had been laid on, when it showed an inappreciable increase of length: with 100 lbs. the increase was .03 inch, with 140lbs. it was .2 inch, and the wire broke with 208lbs. The ratio of the elastic to the breaking force was therefore $\frac{90}{208} = \frac{3}{7}$ nearly.

To see what defect in strength a wire would exhibit when broke several times successively, I took a piece of the same thickness as that above, and suspending it in the same manner as before, broke it six times by weights hung at the bottom, and increased about 4lbs. each time; the results are as below:—

Experiments.		Breaking weight in lbs.		Remarks.
1	....	194½	....	Broke at bottom fastening.
2	....	188½	....	„ ¼ in. from top „
3	....	185	....	„ 1 in. from „ „
4	....	168	....	„ at bottom „
5	....	185	....	„ at top „
6	....	187	....	„ at bottom „

The wire, exclusive of the fastenings, was 8 feet long when the experiments began, and 3 feet 3 inches when they ended.

With another wire of the same thickness, broke six times successively, the results were as below:—

Experiments.		Breaking weights.
1	....	218
2	....	211
3	....	207
4	....	222
5	....	215
6	....	214

From these experiments it appears, that stretching a wire frequently, even to its breaking point, does not materially reduce its power of bearing again. The case in a bolt may not be exactly the same, as in a thin wire, the manufacture of the two being different; but it does not seem probable that any great difference would arise from that cause. Hence the objections against high testing of chains formed of straight bars, must be small when compared with the dangers from defective welding.

These remarks may not be irrelevant, for I once saw in the main chain of a suspension bridge, one of the links with a large crack in it, stopped up with putty.

A FEW REMARKS

ON THE

MENAI BRIDGE.

BY MR. EATON HODGKINSON.

(Read December 12th, 1828.)

In the early part of this year, I laid before the Society, with other matter, some practical observations on a small chain bridge in our own neighbourhood; I had not then seen the magnificent work of art, about which I am going to occupy a few minutes further of the Society's attention.

Last summer, however, induced by that curiosity which has drawn so many thousands, I was led to visit North Wales and the Bangor Bridge; and as the bridge was at that time in painting, I took advantage of a rope ladder, used by the men, and got to the top of the principal pillar on the Anglesea side; when, with a board and a string and plummet, I managed to take, probably without much error, the angle of inclination to the horizon of the top link or bar in the chain, on each side of the pillar: the angle of the internal link was 16°. 10′, that of the external, or link of the side chain, 18°. 3′.

Having these data*, and the area of a section of the catenarian chain (which I believe is uniform), we will seek for the strength of the structure and the strains upon it; and then make a few observations on chain bridges generally.

* As there may be some who would wish to know the dimensions of the bridge in other respects, I will give them from the large plan, published by Mr. William Provis, the resident engineer.

Distance from center to center of the main pillars ..	579 feet.
Span of the catenary 570 feet, versed line	43 ,,
Height from low water of Spring tides to the road-way ..	121 ,,
Height from high water of ditto to ditto	100 ,,
Height of main supporting pillars above ditto	50 ,,
Span of each of the Stone Arches	52½ ,,

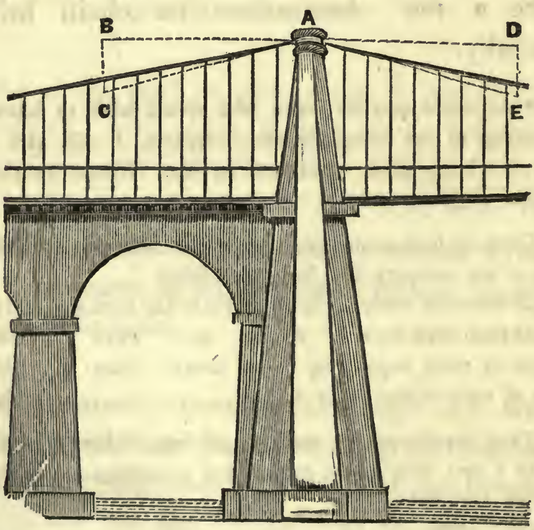
Total breadth of the road-way 28 feet, divided into a foot-way of 4 feet wide in the center, and a carriage-way of 12 feet wide on each side.

Number of suspending cables 16, each composed of 5 bars, and each bar having a section of 3¼ inches of iron.

Number of sets or lines of suspending chains 4.

The chain is attached to a moveable saddle lying on the top of the pillar, the tension of its top link on both sides of the pillar is therefore the same. The utmost tension which the chain might be expected to bear in the direction of its length, is 7020 tons; for a section of all the cables composing it, is 260 square inches, and the strength of a square inch bar of iron, may be estimated at 27 tons.

Let AC and AE be in the direction of the top links of the chain, AB and AD horizontal, BC and DE vertical.



Then, if we suppose the bridge to retain its present form, and to be loaded with the utmost weight the chain will bear; the strain being greatest on the top link, we have, force in AC, or in AE = 7020 tons, angle DAE = 16°. 10′, angle BAC = 18°. 3′.

Resolving the tension in AE and AC into its vertical and horizontal effects gives as below:—

7020 × nat. sin. 16°. 10′ = 7020 × .27843 = 1954½ tons = force in DE = weight of bridge borne by one pier - - - - - - - - - - - - - - (1),

7020 × nat. sin. 18°. 3′ = 7020 × .30985 = 2175 tons = force in B C = vertical pressure from side chain - - - - - - - - - - - - - - - - (2),

7020 × nat. cos. 16°. 10′ = 7020 × .96045 = 6742 tons = force in AD = horiz. tension from catenary - - - - - - - - - - - - - - - - - (3),

7020 × nat. cos. 18°. 3′ = 7020 × .95078 = 6674 tons = force in A B = horiz. draw from side chain - - - - - - - - - - - - - - - (4).

Doubling the number obtained in equation (1), we have 2 × 1954½ = 3909 tons for the whole weight, the bridge would bear distributed over its road-way.

From equations (1) and (2) we have 1954

+ 2175 = 4129 tons for the whole vertical pressure on the pier.

Subtracting equation (4) from (3) gives 6742 − 6674 = 68 tons = horiz. force caused by the inequality of the angles 16°. 10′ and 18°. 3′, and supported only by the pillar which it tends to draw in direction AD. This force however is but $\frac{\cdot 68}{4129} = \frac{1}{60}$th of the vertical pressure upon the pillar.

The deflection of the force down the pillar, from the vertical line, is half the difference of the angles, 18°. 3′ and 16°. 10′, = 57′: therefore the deviation from the vertical at 171 feet from the top is 171 × nat. tan. 57′ = 171 × .01658 = 2.83 feet, or less than a yard at the surface of the water.

The ends of the chains of this unrivaled structure are, at the Anglesea side at least, fastened deeply in a rock that has never been removed. There seems then no probability that it would fail first in that part; and therefore, if the angles are rightly assigned, it appears from above, that the bridge ought ultimately to bear 3909 tons. Now the weight of the chain and its appendages is perhaps little more than 500 tons; the bridge is therefore not loaded by its own weight with more than $\frac{1}{7}$th of what it ought theoretically to bear: the horizontal force too acting at the top of the pier to overturn or derange it is then ordinarily only $\frac{1}{7}$th of what is given above, or $\frac{68}{7} = 9\frac{2}{3}$ds tons. This force is very small compared with the pressure on the pillar, it being only $\frac{1}{60}$ of that pressure; but when it is considered that it is constantly acting at the end of a lever equal in length to the height of the pillar (perhaps 200 feet high), and that frequently by jerks as in windy weather, and when there are heavy weights on the bridge, it may not possibly be without effect. It was not however considered of much consequence by the able Engineer of the bridge, or the tendency might have been easily avoided, by making the angles on each side of the pillar equal.

We have reckoned the strength of the iron in the chains at 27 tons per inch, but with from $\frac{1}{2}$ to $\frac{2}{3}$ds of this weight it generally begins to stretch permanently; which point may be considered as the extent to which it should ever be used in practice. As to the relative strengths of the parts of the chains, the top link should be about $\frac{1}{22}$nd stronger than the bottom one in this bridge. For considering it as a common catenary, with this span and versed sine, the inclination of the top link to the horizon would be about 16°. 49′; and as the strain on the links is as the secant of their inclination, the strength of the top and bottom links ought to be as 10447 to 10000. This difference is usually neglected: whether it has been paid any attention to in this much more perfect structure, I have not particularly noticed.

ADDITIONS.

The effect on the equilibrium of the bridge from expansion and contraction is but small. I was informed by an intelligent person who resided near to it, and had paid particular attention to it, at different times of the year, that the center of the suspended part rises from contraction about 11 inches higher in winter than in summer; and that the saddles on the pillars recede, each about $1\frac{1}{2}$ inches, both together 3 inches, from each other through that cause.

In this structure, the versed sine, though larger than was at first contemplated, is less than $\frac{1}{13}$th of the whole span. Had it been greater, the piers must have been larger, and the bridge would perhaps have been more liable to agitation; otherwise the quantity of iron in the main chains might have been considerably less, as will be seen from below.

Suppose, between two fixed points in a horizontal line, a number of uniform catenaries be suspended of different lengths and thickness, but all having the same quantity of matter in them. That catenary will bear the greatest weight, distributed uniformly along its curve, which has its versed sine about $\frac{1}{3}$rd of its span; and the curve so proportioned would bear, along its length, more than double what it would with the proportion of versed sine to span used in chain bridges generally. For it might be shewn that, if with a span of 200 and versed sine 14.81 (nearly the ratio of the Menai Bridge), the weight borne was 28, other versed sines as below would nearly give for the weights borne those which follow.

ver. sines,	16.82,	25.53,	44.13,	57.67,	65.85,	77.15,
weights,	31.53,	44.3,	60.7,	65.49,	80.2,	65.72.

This last matter, in a different form, has been pursued by Mr. Davies Gilbert, Phil. Trans.

1826, and elsewhere, who has likewise shown that the maximum extent of span for a catenarian curve in wrought iron would be several miles, and dependent on the nature of the catenary.

The ratio of the span and versed sine above would however be some what modified if the weight were considered as uniformly distributed along a horizontal road-way, for then the curve would be a parabola. See paper on Forms of the Catenary in this volume.

THEORETICAL

AND

EXPERIMENTAL RESEARCHES

TO ASCERTAIN THE

STRENGTH AND BEST FORMS

OF

IRON BEAMS.

BY MR. EATON HODGKINSON.

(Read April 2nd, 1830.)

THE very frequent use of iron beams for supporting the floors of factories, and of other places crowded with people, renders it extremely desirable that the best information should be obtained with respect to the strength of this material, in order to insure, without a great superfluity of metal, the requisite stability. In a case so deeply involving the loss of life, and where a failure would be attended with such serious consequences, hardly too much research can be applied. The scientific character of the subject also imparts to it additional interest; and we cannot therefore be surprized, that the strength of materials has engaged the attention of many of the most distinguished Philosophers from the time of Galileo, who was the parent of these enquiries, to the present age; including the names of Bernoulli, Euler, Lagrange, Coulomb, and Robison.

These great men have shewn in its different departments, how the refinements of speculative science can be applied to practical purposes. They have done much—but owing perhaps to a disinclination to the labour and expense of making sufficient experiments, much was left undone for later inquirers. And in the lateral strength of bodies after all the hypotheses that have been employed upon it, the practical man has been till lately without a general and satisfactory theory.

2. But, while it is thus necessary to insure sufficient strength, it is very desirable that the beams should be formed in the manner best adapted to resist the strains to which they are subjected: and if, by an alteration from the usual shape, the same strength could be obtained with a smaller quantity of metal, the expense would be reduced; and the material rendered even capable of bearing more pressure, by

having less to support in its own weight. It may be stated in illustration of this remark, that there are usually upwards of 100 tons of metal in the beams supporting the floors of a factory.

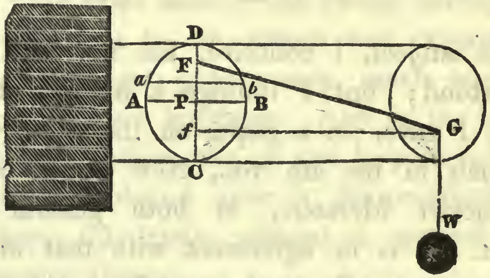
3. To attain these objects as far as lay in the power of the author of this paper, a number of experiments have been made; but before giving an account of them, it may be proper to describe the nature of the strain to which bodies thus acted upon are exposed.

This subject, I conceive, has not been well understood; but I indulge a hope that the theory I gave, in a paper on the Strength of Materials in the 4th vol., New Series, of the Manchester Memoirs, is both general and correct. It is in agreement with that of Dr. Robison, and of Coulomb, properly considered, so far as they have gone; and the following abstract is in pursuance of it.

4. Suppose a beam, placed horizontal, with one end firmly fixed in a wall and a weight hung at the other, it will bend: but, it is evident, that could not take place, except by the lengthening of the top parts, or the compression of the bottom, or by both. Now both of these actu-

ally take place; and, hence, there is some intermediate point or line, between the top and bottom of the beam, where the particles are neither extended nor compressed: all above being in some state of tension, and all below compressed. This line may properly be called the neutral line.

If then, ADBC represent a transverse surface of the beam, and AB the neutral line, ADB must be the surface of tension, and ACB that of compression.



Now, it is evident, that the extensions or compressions of any particles within these surfaces, will be as their distances from the line AB; and the forces, exerted by those particles, must be in the same proportion, so long as the elasticity remains perfect; for then the forces are found to be as the extensions or compressions. Afterwards, the forces of the particles would be as some different functions of their distances from the neutral line.

5. In order to estimate the strength of the piece whose section is ACBD, if F and f represent the points at which the forces rising from tension and compression being collected, would produce the same effects as they do at their respective distances from the neutral line: f will be the fulcrum on which all the forces of tension may be conceived as sustained, and F f one arm of a bended lever, while the length G f is the other, (the points F and G being supposed to be connected by the chain F G, merely to give the lever the appearance of greater strength.) If, further, we call the forces supposed then to be collected in F and f, F′ and f' respectively, we shall have, for the equilibrium in this case, these two equations,

$$F' = f' \text{ - - - - - - - - - - - - - - } (1),$$

$$W \times Gf = F' \times Ff \text{ - - - - } (2).$$

Equation (1) assumes the sum of the forces exerted by the extended fibres to be equal to the sum of the forces from the compressed ones, which is the case: for, the weight W acting in the direction G W, parallel to the surface ACBD, can have no influence in pushing the piece toward or drawing it from the wall; and therefore the pressure on the fulcrum f must be just equal to the resistance from tension in F; or in symbols $F' = f'$.

Equation (2) depends on the equality of the products of the forces multiplied by their leverage. This may be expressed in a different way thus:—if we suppose the neutral line AB to be an axis round which the beam in bending revolves, the weight W, multiplied by the length of the beam to the part ACBD, must be equal to the sum of the products of the forces of tension and compression multiplied by their distances from the neutral line, or

$$W \times \text{length} = F' \times FP + f' \times Pf. \text{ - - - - } (3)$$

In this theorem the form only is varied, for - - - from equation (1), $F' = f'$, and since length $=$ Gf, we have

$$W \times Gf = F' \times (FP + Pf) = F' \times Ff,$$

as in equation (2).

In the foregoing formulæ, adapted as below, equation (1) may be used to find the position of the neutral line; and equation (2) or (3) may either of them be taken at pleasure to determine the strength of a body.

6. To find values for the expressions in the equations above, supposing the forces to be as the extensions and compressions. Call PD $= a$, PC $= b$, any double ordinate ab on the surface of tension $= X$, its distance from AB

$= x$, any double ordinate on the surface of compression $= Y$, its distance from AB $= y$; call moreover the weight sustained by the stretched fibres in a unity of section at the top of the beam $= t$, and the cotemporary resistance of the same quantity of fibres at the bottom $= r$.

We have then $a : x :: t : \frac{tx}{a} =$ force of stretched fibres in a unity of section at distance x from AB, whence $\frac{tx}{a} \times Xdx =$ force in X, when the thickness of X is dx. Integrating the force in X gives $\frac{t}{a} \int X x dx =$ sum of the forces of tension $= \mathrm{F}'$; whence $\frac{t}{a} \int X x^2 dx =$ sum of the forces of tension multiplied by their distances from AB $= \mathrm{F}' \times \mathrm{P\,F}$.

In like manner we should have for the forces of compression, $\frac{r}{b} \int Y y dy = f'$, and $\frac{r}{b} \int Y y^2 dy = f' \times \mathrm{P}f$. But to obtain the value of r in terms of s, suppose the forces necessary to produce an equal extension and compression in a fibre to be as 1 to n; then, if the extension from t be unity, nt would be the force to produce the same compression in the same fibres. But the extension at the top of the beam is to the compression at the bottom as

a to b, hence $a : b :: 1 : \frac{b}{a}$ = compression at bottom; the force to produce that compression is therefore $\frac{bnt}{a}$, which is another value for r. Substituting this for it gives $\frac{nt}{a}\int Yydy = f'$, and $\frac{nt}{a}\int Yy^2dy = f' \times Pf$.

7. If we substitute, in equations (1) and (3), their values from the preceding article we have:

From equation (1), $\frac{t}{a}\int Xxdx = \frac{nt}{a}\int Yydy$,

$$\text{whence} \int Xxdx = n\int Yydy \quad - - - (4)$$

From equation (3),

$$W \times Gf = \frac{t}{a}\int Xx^2dx + \frac{nt}{a}\int Yy^2dy \quad - - - (5)$$

These two equations, when n and t are given, will suffice to find the position of the neutral line, and the strength of any body; but we may put them under a simpler form as below.

8. Call the area ADB = s, the distance from AB of the center of gravity of that area = g, and the distance of its center of percussion from AB = p, call too the area ACB = s', the distances from AB of its centers of gravity and percussion g' and p' respectively. We have

then $\int X\,dx = s$, and by mechanics $\int X\,x dx = gs$, $\int X x^2 dx = gps$, likewise $\int Y dy = s'$, $\int Y y dy = p's'$, $\int Y y^2 dy = g'p's'$. We have therefore, from equation (4),

$$gs = ng's'. \text{ - - - - - - - } (6)$$

Cor. If $n = 1$, $gs = g's'$.

Hence it appears that the neutral line divides the surface of fracture so, that the parts multiplied by the distances of their centers of gravity from that line are in a given ratio;* and when equal extensions and compressions are produced by equal forces, these products are equal. The experiments on cast and malleable iron, further on, give $n = 1$, answering to this latter case, but the mean from my experiments on timber, before the elasticity was destroyed, gave $n = \frac{4}{5}$; see paper on the Strength of Materials, Manchester Memoirs, vol. 4, pages 273 and 4.

9. If we put in equation (5) the values from article 8, we obtain

$$W \times Gf = \frac{t}{a}(gps + ng'p's') = \frac{tgs}{a}(p + p'),$$

since $ng's' = gs$.

* Mr. Barlow, Essay on Strength of Timber, has arrived at the same conclusion; but it is to be regretted, that able Philosopher has mistaken the consequences resulting from it.

If the beam be long, and much bent, the leverage is not the length of Gf, but Gf $\times$ cosine of deflection. Calling G$f = l$, and this cosine $= c$, gives

$$\mathrm{W}\,lc = \frac{tgs}{a}\,(p + p'),\ \text{whence}$$

$$\mathrm{W} = \frac{tgs}{alc}\,(p + p') \text{ - - - - - - - - } (7),$$

which is another expression for the strength.

Cor. 1. If $p = p'$, or the distances of the centers of percussion be the same on each side of the neutral line,

$$\mathrm{W} = \frac{2\,tgps}{alc}.$$

This case occurs principally when the sections are the same on each side of the neutral line.

Cor. 2. If the beam be rectangular as a joist, and the material iron, while the elasticity remains perfect, the neutral line will be in the middle; calling then the breadth b, and the depth d, we have $g = \frac{a}{2} = \frac{d}{4}$, $p = \frac{2a}{3} = \frac{d}{3}$, $s = \frac{bd}{2}$, $c = 1$ nearly, the deflection being small, $\therefore$ from Cor. 1,

$$\mathrm{W} = \frac{tbd^2}{6l}.$$

Cor. 3. Comparing equations (3) and (7), it will appear that PF $= p$, and P$f = p'$.

For examples see my paper on the strength of materials referred to above.

10. These theorems can with propriety only apply so long as the elasticity is but little impaired, and in cast iron must be used only during that time; as afterwards, the shifting of the neutral line would alter the value of s and lead to error; as will be seen from an example to the 2nd Corollary, taken from an experiment of Mr. Tredgold. (Essay on strength of cast iron, section 5, page 83).

A rectangular bar of old park iron, depth 0.65 inches, breadth 1.3 inches, fixed in a horizontal position at one end, broke by a weight of 184lbs. at the other, leverage 2 feet. Now as about $\frac{1}{3}$ of the breaking weight would destroy the elasticity in a rectangular piece, (Tredgold, page 79), quere, what was the strain per inch on the top of this bar, or the value of t, when the weight of 60lbs. was laid on it? Since $w = \frac{tbd^2}{6l}$, $\therefore t = \frac{6lw}{bd^2} = \frac{6 \times 24 \times 60}{1.3 \times .65 \times .65} = 15730$lbs. per inch.—Mr. Tredgold, from the mean of his experiments, makes the quantity that would be required to destroy by direct tension the elasticity of a piece of cast iron, whose section was an inch, 15300lbs.; which is somewhat less than as above.—I have little doubt that these quantities are much too high; the lower parts of a bar would have strength enough to render it straight again when the weight was removed, though the elasticity of the top part was a little injured. Besides, the full cohesion of cast iron, from the experiments of Captain Brown and Mr. Rennie, is but from 16 to 20,000lbs. per inch. Mr. Tredgold was aware of this, and quotes the experiments, and yet supposing the rule* applicable at the time of fracture, he makes the value of t, or the full cohesion of cast iron = 48200lbs. per inch, a strength nearly equal to that of malleable iron; and in the deductions from other experiments, he makes it nearly equal to this. The mistake seems to arise from his having supposed the neutral line to have retained its place in the middle of the bar; but it must necessarily have shifted and gone lower before fracture, on account of the high resisting power of this metal when over compressed, and of the stretching, which no doubt takes place in the top part of the beam then; leaving much more of s in a full state of tension, and increasing the strength far beyond what is given by the rule.

11. It will be proper, before proceeding further,

* Mr. T. uses the same theorem as above for a joist.

to advert to a matter which seems to strike at the root of the theory we have just laid down. M. Duleau, (Essai théorique et expérimental sur la resistance du fer forgé), speaking (page 2nd) of a body fixed horizontally by one end, and bent by a weight at the other, after having observed that the top fibres will be elongated, and the lower ones compressed, proceeds thus: "Coulomb has supposed that when the curvature of the elastic piece is very small, the neutral line is so placed, that the sum of the moments of the tensions of the superior fibres is equal to that which is obtained by adding together the moments of the compressions of the inferior ones. This principle, which has not been demonstrated in a rigorous manner, has been adopted by all the authors* who have treated on this subject." He further mentions, "It is useless to have recourse to it, if the inquiry is respecting a solid, whose transverse section is divisible by an horizontal line into two symmetrical portions. This right line is then, evidently, that in which the passage from tension to compression lies," (the neutral line.) "It is very rare that in practice one has to consider bodies of any other form."

* Mr. Duleau perhaps means in France, those in England have till lately supposed bodies to be incompressible.

12. This last observation of M. Duleau is right, as regards wood or malleable iron; but the case is very different in cast iron. It will be indispensably necessary for us to use some general principle, as that above, if correct; since nearly all the forms in the following pages are different on the two sides of the neutral line.

13. M. Duleau, having (at pages 26 and 27) found that a triangular piece of malleable iron was equally bent by the same weight, whether the edge or the base was uppermost, comes to the following conclusion:—"This piece whether placed upon a face, or an arris, presents the same resistance: it follows from thence, supposing the line of passage from tension to compression to be a right one, that in a transverse section of the piece, the sum of the moments of the compressed fibres, and that of the moments of the extended ones are equal."—This conclusion, if correct, would be a proof of the preceding principle, and is offered as such by M. Duleau.—With great respect for that writer's judgment, I must however draw a different conclusion. It appears to me that the experiment shews the extensions and compressions to have been equal from the same forces, and as the forces; the situation of the

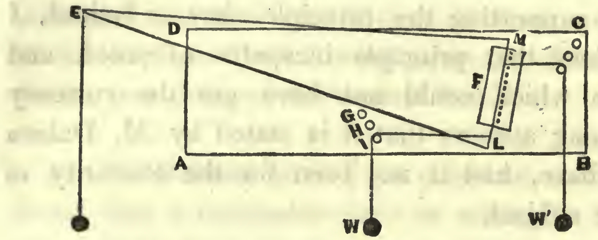
neutral line wherever it was being a fixed one. But, I conceive, it proves nothing as to the equality of the momenta of the forces on each side of the neutral line, which was necessary for supporting the principle above. Indeed, I believe that principle incapable of proof, and one which could not have got the currency among authors that it is stated by M. Duleau to have, had it not been for the obscurity of the subject.

14. It is that principle, in a more generalized form, upon which I animadverted (Manchester Memoirs, vol. 4.) as forming the basis of Mr. Barlow's Theory of the strength of Timber; but it appears from the quotation above that the mistake has not originated with that gentleman.

15. The theory I have offered above, and which has been given in a more general manner, as applied to timber, in the volume of the Memoirs just referred to, depends on the equality of the forces on each side of the neutral line, or $F' = f'$, equation (1). There cannot I conceive be any doubt of that equality; it seems sufficiently evident from the principle of the lever, as in Art. 5; but the following mechanical contrivance, the account of which is extracted from a paper of mine which was published in

the Edinburgh Journal of Science, will, if needed, throw additional light on the matter.

16. I took a board ABCD,



3 feet 6 inches long, and 1 foot broad; and at about half the distance between its two ends, and near to the side AB, there were small moveable pullies affixed, as G, H, I, and similar pullies near the corner C, as represented on the figure; and in the intermediate space between these, as at F, there was a rectangular hole cut through the board, and where the dotted line is seen, there projected the ends of a number of equal equidistant straight springs of iron or steel wire, which were firmly inserted at their other ends into a wooden

frame *a b c d*, and this frame was then fast nailed at its ends *a* and *d* to the back of the board, so that the springs between *a* and *d* might project about an inch through the hole F, and be perpendicular to the plain of the board.

I then got a very light piece of wood, in the form of an isosceles triangle, ELM, whose altitude was about 3 feet 6 inches, (the length of the board,) and its base LM 10 inches. Along the side of LM there was nailed a piece of tin, perforated with a row of holes, so as rather loosely to fit the ends of the springs projecting through F, and this tin was slid upon them. The board ABCD was then raised perpendicularly to the horizon, its edge CD being uppermost, and having the triangular piece ELM sliding along its side, and attached to the board only by the springs: the end AD of the board serving to render the triangle steady, and, (by means of a pointed instrument passing through the latter) to hold it, if necessary, in any position.

I then hung a small weight at the end E, and there being nothing to support it and the weight of the triangle, but the springs, the point was, as might be inferred, carried some distance down, the upper springs being drawn after the base of the triangle, nearly in the direction CD, and the lower ones made to recede in the opposite direction; the whole turning as it were on the central spring, which was not bent, and consequently supported nothing.

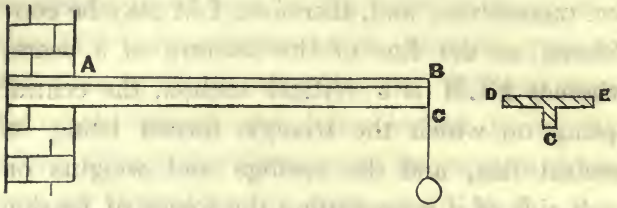
I next attached a weight w to a point of the triangle near to L, 6½ inches from the central spring, by a string passing over the pully I, and an equal weight w' to another point, ½ an inch on the other side of that spring; and increasing the weight at E, so as to bring the base LM perpendicular to the horizon, (which was done in all the experiments,) I found that the whole turned round the central spring as before, though the distances of the equal weights from the central spring were as 13 to 1.

I afterwards put weights to other points, passing over all the pullies, G, H, I, at once, putting sometimes a large one over G, and a small one over I, and sometimes the reverse; and, doing in like manner by the pullies near the corner C, I found that the apparatus always turned round the central spring, without its being bent, when the sums of the weights on each side were equal. But, if the sums of the opposing weights were unequal, the triangle no longer turned round the central spring, but round some other point, at which there was an equality between the negative and affirmative forces.

It is evident that we might have substituted, for the weights in the above experiments, springs, which would have been unbent, when those which are in the instrument were so, and which (when bent so that the triangle might assume the position it was in during the experiments,) would exert equal forces to the weights w, w', &c. themselves; and, therefore, LM may be considered as the line of the fracture of a beam, whereof ELM is a vertical section, the central spring on which the triangle turned being its neutral line, and the springs and weights on each side of it representing the forces of tension and compression; which, from the experiments above, have no particular relation to their distances from the neutral line, but must, under all the circumstances, be equal.

17. My next object was to seek, experimentally, for the laws which connect the extensions and compressions with the forces in cast iron, through their whole range, to the fracture of a bent piece.

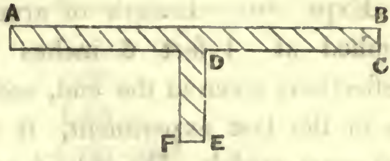
For this purpose, I got several castings moulded of different sizes, but of one general form, such that by bending them, first one way and then the other, the relative resistances to extension and compression in one and the same part might be obtained. Each casting was several feet long, and equal in section throughout, with its parts as slender as could well be run to preserve it even and sound. During the experiment, one end of the casting was fixed in a wall, and weights to bend it hung at the other.



A B C represents an elevation of the castings then, and D C E a cross section of all of them. The end A was wedged fast into the wall, but so as to be easily removed, in order that the casting might be turned the other side upward; as the intention was alternately to compress and extend the vertical rib of D C E at C. In the following description therefore, ⊥ must be understood as indicative that the casting was turned that way upward, and hence the leg at C extended, and ⊤ that it was turned the opposite way, and the leg compressed.

Expt. 1st. In this experiment the length A B (figure above) was about 3 feet 6 inches, and in the annexed figure ABCDEFA, which represents a cross section of all the castings, used for this purpose, (and is merely an enlarged figure DCE above, for the purpose of specifying more particularly the dimensions,) AB = 2.4

inches*, BC = .3, DE = .8, EF = .3. The weights, of the casting when reduced to the end,



and of the scale, were $8\frac{1}{2} + 4\frac{1}{2} = 13$lbs. This last weight is set down in each of the experiments, but not used; the weights given are extra ones above it, and the deflections those that answer to these extra weights.

T		⊥	
Weight.	Deflection.	Weight.	Deflection.
7 lbs.	.34 inches.	7 lbs.	.37 inches.
27 „	1.35 „	27 „	1.56 „
42 „	2.25 „		

Expt. 2nd.—Length of arm 3 feet 10 inches, AB = 4 inches, BC = .2, DE = 1 inch, EF = .2 inches, weight of casting at the end = $6\frac{1}{2}$lbs. scale = $4\frac{1}{2}$, both = 11 lbs. The weights were hung, and the deflections taken, at the end.

T		⊥	
Weight.	Deflection.	Weight.	Deflection.
30	.53	30	.63 .60 } mean .61½
40	.76	40	.90 { Elasticity imperfect.
50	.92	50	.96
60	1.24	60	1.20
70	1.40	70	1.35
80	1.63	80	1.65

* All the dimensions are in inches, except otherwise mentioned, and the weights in lbs.

Expt. 3rd.—Length of arm 5 feet, weight applied at 4 feet 6 inches from shoulder, deflections taken at the end, section nearly same as in the last experiment, it being cast from the same model. Weight of casting at the end with scale $9\frac{1}{2} + 4\frac{1}{2} = 14$ lbs.

⊤

Weight.	Deflection.	
30	.97 } .98 } mean .97½	Unloaded, it had taken a sensible set, but returned in a minute or two to its place.
40	1.29	
50	1.67	Set .05
60	1.94	
70	2.28	Set .14

⊥

Weight.	Deflection.	
30	.96	Perceptible set
40	1.33	
50	1.72	Set .07.
60	broke it.	

Expt. 4.—Length of arm 5 feet—deflections taken at the end—weights hung at 4 feet 6 inches from shoulder—weight of casting at the end with scale $10\frac{1}{2} + 4\frac{1}{2} = 15$ lbs., AB = 4.1 inches, BC = .25, DE = 1.1, EF = .25.

The experiment was commenced with the rib upwards, thus ⊥, but the casting was turned the other side up before the elasticity became imperfect.

⊥

Weight.	Deflection.	
30 - - - - -	.86	
40 - - - - -	1.24	
50 - - - - -	1.60	Set .13.
60 - - - - -	2.03	Do. .17.

70 It broke with this, 6½ inches from the fixed end, it being rather thicker near the end.

⊤

Weight.	Deflection.	
30 - - - - -	.84	
40 - - - -	1.16	
50 - - - - -	1.52	Set .10, after several minutes it became .07.
60 - - - -	1.86	Set .15.

18. The preceding experiments were mostly begun with the broad part of the casting upwards to compress the vertical rib; and when a weight or two had been laid on and the deflexions noted, the piece was turned the other way up, before the elasticity was much injured, and the deflections from the same weights again taken, the rib being now extended. This mode of turning the piece, which was usually done two or three times with the same casting, it being first reduced, if necessary, to its original form, answered better for obtaining the comparative extensions and compressions by equal weights,

than making all the experiments on each side in a series would have done; though it left the tabular results rather more anomalous. I preferred too, obtaining the extensions and compressions from the same rib, to taking each from different ones, on account of the difficulty of getting such small castings precisely of equal size and equal strength of iron. This mode of making the experiment, however, rendered the time when the elasticity became impaired rather doubtful, and I have set the results down with regard to the set as they appeared to me, but I place but little confidence in them, particularly those respecting compression.

19. With regard to the inferences to be drawn from these experiments, we will make the three following suppositions:—

1st. Suppose materials to be incompressible, as has generally been assumed in this country, and the experiment to be begun with the back of the casting upwards, the leg DFE (figure to experiment 1.) will, by supposition, offer an insuperable resistance to a force tending to compress it, and the deflection must arise from the small quantity which the broad part ACB will extend: if the casting be then turned the other side upwards, the part ACB being incompressible, the deflection must arise from the extension of DFE, and consequently would be many times as great as when the casting was the other side upwards, DFE being so much less than ACB.

2nd. Suppose bodies to be inextensible, then if the back of the casting be upwards, the deflection will wholly rise from the compression of DFE, and will be many times greater than if DFE had been upwards, when it must have arisen from the compression of ACB.

3rd. Suppose the extensions to be equal to the compressions, from the same forces; the deflection in this case will be the same from the same force, whether the rib DFE be downwards or upwards.

20. We will now collect in a tabular form, the principal results from the experiments, and see how near they correspond to any of the above suppositions.

Expts.	Weights.	Deflections from Extension.	Deflections from Compression.	
1st.	7	.37	.34	Earlier weights.
2nd.	30	.61½	.53	
3rd.	30	.96	.97½	
4th.	30	.86	.84	
1st.	27	1.56	1.35	Later weights.
2nd.	80	1.65	1.63	
3rd.	50	1.72	1.67	
4th.	60	2.03	1.86	

From these results it appears that in every instance, except one, in the above table, the deflections from extension are greater than those from compression by equal forces, whether the elasticity be perfect or not; the four first having been taken before the elasticity could have been much injured, and the last four nearly at the breaking point. Still the difference is not so great, but that they may in most cases, without material error, be assumed as the same. Hence, the extensions and compressions from equal forces in cast iron, are nearly equal.

21. This is a very interesting fact: it is most likely a property common to tenacious bodies, when not over-strained generally:* it has

* Timber seems to be in some degree an exception; see my Paper above referred to.

often been assumed by writers, but I have not before seen any proof of it, except in an experiment of M. Duleau, which renders it very probable that it is the case in malleable iron. Mr. Tredgold in his "Essay on the Strength of Cast Iron" has assumed it, and made it the basis of his reasonings. He thence concludes (Section 4th,) that in a beam of the strongest form, its section ought to have equal ribs at top and bottom of it: so that the neutral axis being at half the height of the beam, there might be equal resisting powers on both sides of that axis. Another conclusion which might be drawn in the same way, and which he mentions (page 132) is, that a triangular prism [he might from his view have said any other form] is equally strong, whether it be loaded one way up or the opposite, supposing it not strained, so as to injure its elasticity.

22. This is perhaps not a full view of the matter: Mr. Tredgold reasoned on a supposition that the same force, which would destroy the elasticity of a body by tension, would destroy it by compression; but it is highly probable, as will be shewn further on, that in cast iron a much larger force is required in the latter case than in the former. An experiment, which I am going to describe, suggested this to me; it

shewed that, in some forms of section, the forces are widely different at the time of fracture. The experiment was made, with the preceding ones, at Messrs. Hatton's Foundry in Salford.

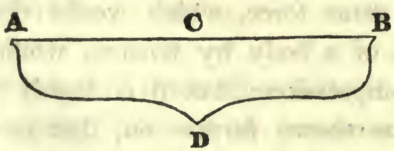
23. I took two castings, apparently precisely alike, whose dimensions in the section were the same as in last experiment (expt. 4), and placing the ends of each of them upon two props, 4 feet 3 inches asunder, broke them by weights in the middle, one with the broad part downwards thus (⊥), and the other with it upwards (⊤).

The first bore $8\frac{3}{4}$ cwt. and broke with 9 cwt.

Deflection in the middle with 4 cwt. .6 inch

" " " $8\frac{1}{4}$ " 1.8 "

The weights were laid very gently on, and the casting shewed no signs of approaching rupture, except a small increased deflection; but when it broke, a piece flew out whole from the compressed side, of the form below, where AB = 4 inches, and CD = .98 inches, the



point D at the bottom being in or near to the neutral line. AB was in the direction of the length of the casting, and the weights were laid on at C. Hence, since the depth of the casting was 1.35 (expt. 4), CD $= \frac{.98}{1.35}$ depth $= \frac{5}{7}$ of it nearly.

This experiment is interesting; it shews the situation of the neutral point, and may from the peculiar form of the wedge, throw some additional light on the nature of the strain.

The other casting bore $2\frac{1}{4}$ cwt. and broke with $2\frac{1}{2}$ cwt.

The strength of the castings was therefore as $8\frac{3}{4}$ to $2\frac{1}{4}$, or as 4 to 1 nearly, according as they were broke, one or the other way upwards.

Those, who suppose the strength to be bounded by the elasticity, and that the same force would destroy the elastic power, whether it tended to extend or compress the body, must have conceived these castings, and indeed those of every other form, to be equally strong, which-ever way upwards they were turned. A conclusion which we see would lead to very erroneous results, if applied to measure the ultimate strength of cast iron.

24. When I had proceeded thus far, I shewed the results to Mr. Ewart, and mentioned to

him my intention of prosecuting the matter on a small scale; when that gentleman, with a degree of kindness, of which I cannot adequately express my sense, suggested that I ought to be relieved from the expense of such an enquiry, stating that the subject was one which deserved the fullest investigation, and that those who were in the iron trade and had foundries, would be quite willing to render every assistance for the purpose. Accordingly at Mr. Ewart's suggestion, Messrs. Fairbairn and Lillie offered, in the most liberal manner, to make the experiments at their expense. It was a matter which they felt an additional interest in, on account of the heavy work they have to do, and for their guidance they had already made some valuable experiments, which will appear in the sequel of this paper; and which are on a larger scale than any I have seen published. From these gentlemen I have met with uniform politeness and kindness, and their promptitude to execute whatever was requested from them, demands my warmest acknowledgments..

25. To Mr. Ewart I am indebted beyond what is mentioned above; he attended the experiments on Beams, made suggestions as to the objects most desirable to be pursued, and

many remarks which his extensive knowledge, particularly practical, rendered valuable to me.

26. I felt desirous of making, on malleable iron, experiments similar to those already given on cast iron, and for this purpose a bar about 6 feet long was made. It was of the same form as those used in cast iron, its section being uniform throughout, and in the shape of a ⊤, of which the top was 5 inches broad, and $\frac{3}{8}$ of an inch thick, and the vertical part or leg $1\frac{1}{8}$ inch deep, and nearly $\frac{1}{4}$ inch thick. In the experiments, one end was fixed horizontally and firmly wedged into an immoveable object, a large stack of pig iron, and weights were hung at 3 feet distance, where the deflections were taken. The experiments were made nearly in the same manner as those on cast iron; only here, we did not subject the same part to both tension and compression, but contiguous parts. Thus, suppose the first experiment was to extend the vertical rib, the next experiment would be to compress it in a part very near to the former place; the piece having been taken out of its fastenings, rendered straight if necessary, and fastened again, the other side upwards, by the part which was most strained before. This mode was used, turning and shifting the piece, throughout its whole length; and toward the

conclusion of the experiments, the weights were increased till the vertical rib exhibited signs of being drawn out and crushed.

27. The results from these experiments were anomalous, partly through defective fixing;* but they left no doubt that the extensions, and compressions, were nearly equal from the same forces, and that through their whole range. And therefore that forces not very unequal would have destroyed the piece, whether the rib was drawn out or crushed; a result very different from what was obtained in cast iron.

The means from the extensions and compressions, with the three greatest weights laid on, were as below.—

Weights.	Deflections from Extension.	Deflections from Compression.
585 - -	1.40 - - -	1.54
697 - -	2.00 - -	2.56
809 - -	4.75 - - -	4.79.

28. In order to determine the same thing, M. Duleau, who is the only writer that I am aware of, who has attended to this matter in malleable iron, has made some very ingenious

* This objection did not exist in the experiments on cast iron, for in those the castings were rendered utterly immoveable, by means of powerful screws.

experiments, (Essai Théorique et Expérimental sur la résistance du fer forgé, pages 26 and 27), which I will give in his own words. " On a courbé par force, à froid, une pièce en fer carrée, de o^{m}.02 ($\frac{4}{5}$ of an inch) de côté, suivant un arc de cercle, de manière à ce que deux faces restassent planes. Sur ces deux faces, on avait tracé des lignes perpendiculaires à l'axe de la pièce, et distantes de o^{m}.025 (an inch). On a donné successivement à la pièce trois courbures, telles que sur une longueur d'arc de o^{m}.30 (a foot), la flèche fût de o^{m}.022, o^{m}.037 et o^{m}.058. Les lignes tracées sur les faces planes sont restées droites et perpendiculaires à la pièce, et l'alongement de la partie convexe s'est trouvé justement égal au raccourcissement de la partie concave.

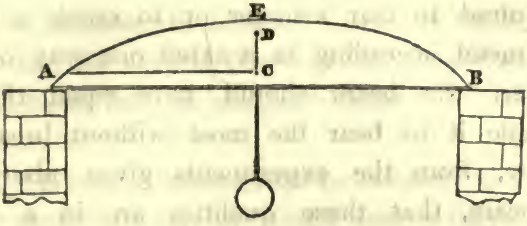
Pour une flèche de $\left\{\begin{array}{l} \text{o}^{m}.022 \\ \text{o }.037 \\ \text{o }.058 \end{array}\right\}$, cet alongement a été, sur une longueur d'arc

de o^{m}.30 (a foot), égal à $\left\{\begin{array}{l} \text{o}^{m}.005 \\ \text{o }.010 \\ \text{o }.0175 \end{array}\right\}$,

ou, par mètre, égal à $\left\{\begin{array}{l} \text{o}^{m}.0167 \\ \text{o }.0333 \\ \text{o }.0583. \end{array}\right.$

Cette expérience prouve que les fibres du fer ont éprouvé un alongement ou un raccourcissement proportionnel à leur distance du milieu de la pièce, et par conséquent que *le même poids qui agit sur une fibre, parallèlement à sa longueur, soit pour la tirer, soit pour la refouler, l'alonge ou la raccourcit de la même quantité.* Ici les fibres avaient perdu leur force élastique; la propriété qu'elles ont présentée existe donc, à plus forte raison, lorsque l'action qu'elles ont éprouvée n'a pas détruit cette élasticité.

29. The intention of the preceding experiments, on the elasticities of cast and wrought iron, being to prepare in some degree the way to an inquiry into the best form of beams of those metals, particularly of cast iron, we will now reconsider the strain, which beams are subjected to, with a view to their proper formation to obtain the utmost strength. If, then, we conceive a beam, supported at its ends, and bent by a weight any where laid upon it, we have seen that all the bottom fibres or parts must be in a state of tension, and the top ones in a compressed state, and if C and D be the centers of tension and compression, the forces in C will be just equal to those in D, where D may be considered as the fulcrum of the bent lever DCA.



In order to increase the arm CD of the lever, and consequently the strength of the beam, the parts should be disposed as far asunder as possible. This would perhaps be best effected by putting two strong ribs, one at top the other at bottom, the intermediate part between the ribs being a thin sheet of metal to keep the ribs always at the same distance, and thereby prevent derangement from irregular strains, as well as to serve another purpose which will be mentioned further on.

30. As to the comparative strength of these ribs, that appears to me to depend upon the nature of the material, and can only be derived from experiment. Thus, suppose it was found that it required the same force to destroy the elasticity of a piece of metal, whether the force acted by tension or compression. In this case, the top rib ought to be equal to the bottom one, supposing it was never intended to strain the beam so as to injure its elasticity. And if it were found that the same weight would be

required to tear asunder or to crush a piece of metal according as it acted one way or the other, the beam should have equal ribs to enable it to bear the most without breaking. Now, from the experiments given above, it appears, that these qualities are in a great measure possessed by wrought iron; and therefore, whether it was intended to strain a beam of it to the extent of its elasticity or even to the breaking point, there ought to be equal ribs at top and bottom.

31. If, however, the metal were of such a nature that a force, F, was needed to destroy its elasticity by stretching it, and another force, G, to do the same by compressing it, it is evident that the ribs ought to be to one another, as F to G, in order that the beam might bear the most without injury to its elasticity. And if it took unequal weights, F', and G', to break the piece by tension and compression, the beam should have ribs, as F' to G', to bear the most without fracture.

Our experiments on cast iron were not well adapted to shew what relative forces would be required to destroy the elasticities; but it appears, by the experiments of Mr. Rennie, that it would take many times the force which would draw it asunder, to crush it. The bottom rib must then be several times as large as the top one to resist best an ultimate strain.

32. This last matter must be considered with some modifications; it would not perhaps be proper to make the size of the ribs just in the ratio of the disruptive to the crushing forces, as the top rib would be so slender that it would be in danger of being broke by accidents.

33. The thickness too of the middle part of the beam, or that between the ribs, is not a matter of choice; independent of the difficulty of casting, it cannot be rendered thin at pleasure, but must have a certain thickness, though in long beams the breaking weight is small, and a very small strength of middle is necessary.

The neutral line being the boundary between two opposing forces, those of tension and compression, it seems probable that bending the beam would produce a tendency to separation at that place. Moreover, the tensile and compressive forces are, strictly speaking, not parallel; they are deflected from their parallelism by the action of the weight, which not only bends the beam, but tends to cut it across in the direction of the section of fracture; and this last tendency is resisted by all the particles in the section. Thus, in the figure to article 4, the force of compression is not in the direction G*f* produced, but in a direction downwards, oblique to that. It is composed of the force in G*f* produced, and that part of the weight W, whose vertical effect is sustained by the lower part of the beam. This compounded force will then tend to separate the compressed part of the beam in the form of a wedge, and this tendency must be resisted by the strength of the part between the ribs, or flanges. We have already given one instance of fracture this way, and there will occur several others in the course of our experiments.

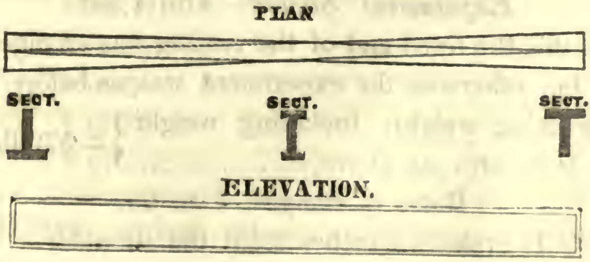
34. We see then that there are three probable ways in which a beam might break, 1st, by tension, or tearing asunder the extended part; 2nd. by the separation of a wedge, as above; and 3rd, by compression, or the crushing of the compressed part. I have not however obtained a fracture by this last mode in cast iron.

35. Before giving a detail of the experiments relative to this subject, made through the medium of Messrs. Fairbairn and Lillie, I will first describe an attempt which I had previously

made at Messrs. Hatton's foundry in Salford. The intention was, to endeavour to ascertain the relative proportion of the ribs in a cast iron beam, to enable it to bear the most without breaking, the thickness of the ribs and of the middle part between them being invariable.

For this purpose I got a model made of wood; the middle part of which was a uniform board, 3 inches deep, $\frac{1}{4}$ inch thick and nearly 8 feet long; to the top and bottom edges of this board there were nailed equal triangular ribs, of the same thickness as the board; the breadths of the ribs being so disposed that the sum of the top and bottom ribs in any cross section of the model was always the same, and the ribs were each just an inch broad in the middle of the model, and varied toward the ends in all proportions from equality to 8 to 1.

The figures below are intended to represent, a vertical plan of the model, its middle and end sections, and an elevation or side view of it.



It is evident that every cross section of this model had the same quantity of matter in it, and might represent that of a beam with any proportion of flanges. From this model I got castings made, and fixing them firmly at one end in a horizontal position, broke them by weights hung at a certain distance from that end, the narrower flange being always at the bottom or the compressed one.

The following five experiments were made on as good a casting as could be procured; it was not quite uniform, but balanced when laid across a thin surface, at $\frac{7}{16}$ of an inch from the middle. Its whole length was 7 feet 8 inches.

Experiment First—Arm 4 feet.

Breaking weight, including the pressure from weight of casting at place where weights were hung, } = 665 lbs.

Ratio of flanges, at place of fracture, 3 to 13,

It broke one inch from the shoulder.

Experiment Second—Arm 4 feet.

In this the fixed end of the casting was changed, but otherwise the experiment was as before.

Breaking weight, including weight from arm as above,...................... } = 579 lbs.

Ratio of flanges, 7 to 23.

It broke $3\frac{5}{8}$ inches from the shoulder.

Experiment Third—Arm 3 feet 9 inches.
Breaking weight = 663 lbs.
Do. reduced to arm of 4 feet, = 621 lbs.
Ratio of flanges, 11 to 21.
It broke two inches behind the shoulder.

Experiment Fourth—Arm 3 feet 2 inches.
Breaking weight = 904lbs.
Do. reduced to arm of 4 feet, = 715 lbs.,
or 721lbs., including weight of arm.
Ratio of flanges, 12 to 19.

Experiment Fifth—Arm 2 feet 9½ inches.
It broke at shoulder, with 932 lbs.
Breaking weight, reduced to arm of 4ft., = 650 lbs.
Do. including weight of arm = 655 lbs.
Ratio of flanges, 13 to 18.

From the above, we may deduce the following approximate ratios of the flanges, and their breaking weights.

Ratio of flanges.	Breaking weights
1 to 4⅓	665 lbs.
1 to 3⅓	579 ,,
1 to 2	621 ,,
1 to 1½	721 ,,
1 to 1⅓	655 ,,

I made several other experiments, in the same way, but the results were so anomalous that no general conclusion could be drawn from them. There was, however, one fact which so often presented itself, that it could not I conceived arise from accident—the piece frequently broke at a distance from the shoulder, shewing clearly the greater weakness in that part where the flanges approached to equality. This I noticed the more particularly, as, according to the reasonings of Mr. Tredgold, the strongest form of cross section was that where the top and bottom ribs or flanges were equal.

36. The indecisive character of these experiments, Mr. Ewart was of opinion, might arise from their having been made on too small a scale, and he suggested to me the propriety of repeating them on a larger one: which was done, and the results shew a degree of uniformity, which forms a striking contrast to the others. The mode however of making the experiments was varied; for, as I had met with difficulty in getting sufficiently good castings from the form of model before used; and as Messrs. Fairbairn and Lillie had a very convenient apparatus, a long lever, for trying or breaking beams, I felt rather more desirous of making

the future experiments on small beams, since the results would be more useful in that form.

37. The form we first adopted was one in which the arc AEB (figure to Art. 29) was a semiellipse, and the bottom rib a straight line; but the sizes of the ribs at top and bottom were in various proportions; the ribs in the model were first made equal, and when a casting had been taken from it, a small portion was taken from the top rib and attached to the edge of the bottom one, so as to make the ribs as 1 to 2; and when another casting had been obtained, a portion more was taken from the top, and attached to the bottom as before, and a casting got from it, the ribs being then as 1 to 4.

In these alterations the only change was in the ratio of the ribs, the depth and every other dimension in the model remaining the same.

38. In most of the experiments the beams were intended to be broke by a weight at their middle, and therefore, the form of the arc AEB was of less importance; in making them elliptical they were too strong near the ends for a load uniformly laid over them; the proper form is something between the ellipse and parabola. It is shewn, by most of the writers

on the strength of materials, that if the beam be of equal thickness throughout its depth, the curve should be an ellipse to enable it to support, with equal strength in every part, an uniform load; and if there be nothing but the rims, or the intermediate part be taken away, it is shewn by Hutton in his Treatise on Bridges, or in my Paper, page 364 of this volume, that the curve of equilibrium, for a weight uniformly laid over it, is a parabola: when therefore the middle part is not wholly taken away, the curve is between the ellipse and parabola, and approaches more nearly to the latter, as the middle part is thinner. Mr. Tredgold states the proper form of the curve to be an ellipse.

39. The instrument used in the experiments was a lever, about 15 feet long, placed horizontal, one end of which turned on a pivot in a wall, and the weights were hung near to the other; the beams being placed between them and the wall. In the first 17 experiments, the beams were placed at 3 feet distance from the pivot in the wall, and in the other experiments, at 2 feet, except otherwise mentioned.

All the beams, in the 22 experiments immediately following each other, were exactly $5\frac{1}{8}$

inches deep in the middle, and 5 feet long, and were supported on props just 4 feet 6 inches asunder. The lever was placed at the middle of the beam, and rested on a saddle, which was supported equally by the top of the beam and the bottom rib, and terminated in an arris at its top, where the lever was applied. The deflections were taken, in inches and decimal parts, at or near the middle of the beam, as mentioned afterwards. The weights given below are the whole pressure, both from the lever and the weights laid on, when reduced to the point of application on the beam. The dimensions of section in each experiment were obtained from a careful admeasurement of the beam itself, at the place of fracture, which was always very near (usually within half an inch of) the middle of the beam, the depth of the section being supposed to be that of the middle of the beam, or $5\frac{1}{8}$ inches.

As the experiments were made at different times, and there might be some variation in the iron, though it was intended always to be the same, a beam of the same length and depth as the others, but of the common form, always from the same model, was cast with each set of castings for the sake of comparison.

40. The first six beams in the first series were cast *horizontal*, that is, each beam lay flat on its side in the sand; all the rest were cast *erect*, that is, each beam lay in the sand in the same posture as when it was afterwards loaded, except that the casting was turned upside down, when in the sand.

In all the experiments the area of the section was obtained with the greatest care; it includes, besides the parts of which the dimensions are given, the area of the small angular portions at the junction of the top and bottom ribs with the vertical part between them.

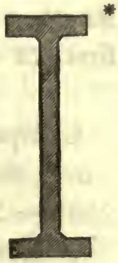
41. The principal objects proposed in the experiments were: first to seek, directed by theory, for the form of section, in which a cast iron beam would be the strongest, up to the time of fracture, its length and depth being given: next to obtain, if possible, a general rule for the strength of such a beam then, since the rules given in the commencement of this paper only apply, in cast iron, so long as the elasticity is perfect: see Article 10.

I. EXPERIMENT.

Beam with equal rib at top and bottom.

Dist. between supports, 4ft. 6ins. Depth of beam, $5\frac{1}{8}$ins.

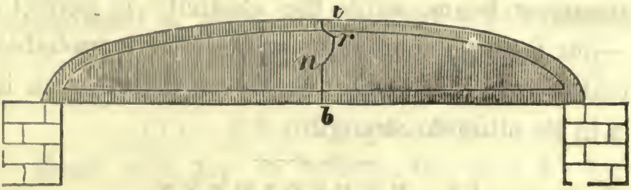
Dimensions of cross section, at place of Fracture, in inches and parts.



Area of top rib $= 1.75 \times .42 = .735$
Area of bottom rib $= 1.77 \times .39 = .690$
Thickness of vertical part, between the ribs, $\} = .29$.

Area of above section $= 2.82$ inches.

Weight of Casting $= 36\frac{1}{4}$ lbs.
Breaking weight 6678lbs. $=$ 59 cwt. 70 lbs.



The form of fracture is represented by the line $b\,n\,r$, where $t\,r = .6$, and $b\,n = 2.5$, the figure being a side view of the beam.

To find the strength per inch of cross section, we have, dividing the breaking weight by the area, $\frac{6678}{2.82} = 2368$ lbs. per inch. This quantity in each

* All the sections in these experiments are laid down of $\frac{1}{4}$ their real lineal dimensions in order to afford ocular comparison.

beam may be taken as an index of its strength, and we shall use it to compare together the strengths of those beams that are of the same length and depth, which is the case in the first 22 experiments.

Comparing this with the result from Expt. 4, where the beam bore 2584lbs. per inch, gives $2584 - 2368 = 216 =$ defect.

$\therefore$ Loss in strength $= \frac{216}{2584} = .083$ or $\frac{1}{12}$ nearly, in parts of what the common beam bore.

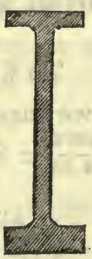
The form of section above is essentially, what Mr. Tredgold has represented to be, that of the strongest beam, while the elasticity is perfect: —our future experiments will render it probable that it is in this respect nearly as defective as it is in its ultimate strength.

II. EXPERIMENT.

Beam with areas of section of top and bottom rib as 1 to 2.

Dist. between supports, 4ft. 6ins. Depth of beam, $5\frac{1}{8}$ins.

Demensions of cross section.



Area of top rib $= 1.74 \times .26 = .45$ ins.

Area of bottom rib $= 1.78 \times .55 = .98$,,

Thickness of the vertical part,........ $\Big\} = .30.$

Area of cross section $= 2.87$ inches.

Weight of casting $= 39$lbs.

Breaking weight = 7368lbs. = 65 Cwt. 88lbs. It broke obliquely about 4 inches from the middle, the top inclining to it.

The form of fracture at the top of the beam was nearly the same as in Experiment 1; here $tr = .55$ inches: see second figure to that experiment.

To find the strength per inch of section, as in the last experiment, we have $\frac{7368}{2.87} = 2567$lbs. per inch.

Comparing this as above with the result of experiment 4, gives $2584 - 2567 = 17 =$ defect.

$\therefore$ Loss in strength $= \frac{17}{2584} = .0066$ or $\frac{1}{152}$.

III. EXPERIMENT.*

Beam with top to bottom rib as 1 to 4.

Dist. between supports, 4ft. 6ins. Depth of beam, $5\frac{1}{8}$ins.

Dimensions of cross section in inches.

Area of top rib $= 1.07 \times .30 = .32$

Area of bottom rib $= 2.1 \times .57 = 1.2$

Thickness of vertical part. $\Big\} = .32.$

Area of cross section $= 3.02$.

Weight of casting $= 40$ lbs.

* At the preceding experiments and at several of the latter ones, Mr. John Kennedy was present, as well as Mr. Ewart who attended them generally.

Ultimate deflection, upwards of $\frac{1}{4}$ inch.

Breaking weight, 8270 lbs. = 73 cwt. 94 lbs. It broke nearly in the middle.

The form of fracture was nearly as in expt. 1, and $tr = .6$: see figure to that experiment.

Dividing the breaking weight by the area gives the strength per inch of section $\doteq \frac{8270}{3.02} = 2737$ lbs. But experiment 4th gives 2584 lbs. per inch.

Hence $2737 - 2584 = 153 =$ excess.

$\therefore$ Gain in strength $= \frac{153}{2584} = \frac{1}{17}$ nearly.

IV. EXPERIMENT.

Beam cast in common form from Messrs. Fairbairn and Lillie's model.

Dist. between supports and depth of beam as before.

Dimensions of section in inches.

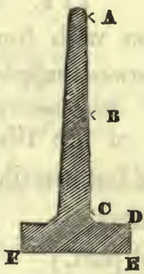
Thickness at A = .32

" " B = .44

" " C = .47

FE = 2.27

DE = .52



Area of section = 3.2 inches.

Weight of casting = $40\frac{1}{2}$ lbs.

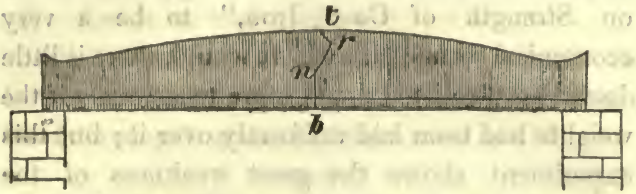
Deflection with 5758 lbs. .25 inches.

" " 7138 " .37 "

Breaking weight = 8270 lbs.

The beam twisted a little before breaking: this however was not usually the case in the other beams from the same model.

Form of fracture as in figure, $tr \doteq .75$.



Hence strength per inch of section $= \frac{8270}{3.2}$ $= 2584$lbs.

V. EXPERIMENT.

This casting had its top rib a parabolic arch and the top and bottom ribs nearly equal in section, with equidistant ordinates only between them.

Dist. between supports and depth of beam as before.

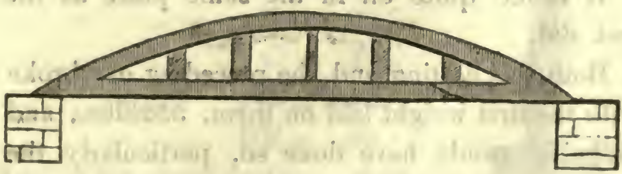
Dimensions of ribs in inches.

Area of top rib $= 2.2 \times .56 = 1.23$.

Area of bottom rib $= 2.2 \times .53 = 1.17$.

Weight of casting $= 41\frac{1}{2}$ lbs.

Breaking weight $= 5528$lbs. or less: see next expt. This weight however is but $\frac{2}{3}$ of what was borne by the common beam in the last experiment.



It broke by separating, near the first ordinate, as in the figure, the top part remaining whole; the weight having been removed as soon as the fracture commenced.

This is represented by Mr. Tredgold, "Essay on Strength of Cast Iron," to be a very economical form of beam. It would, there is little doubt, have resisted much more tenaciously if the weights had been laid uniformly over it; but this experiment shows the great weakness of the beam when the weight is applied at a single point, and therefore the danger of using it in practice.

VI. EXPERIMENT.

A parabolic arch differing from the last, only, in its having a portion taken from the top rib, and added to the bottom, leaving the height as before, and the ratio of the ribs 1 to 2 nearly. Distance between supports and depth as before.

Dimensions in section of the ribs.

Area of top rib $= 2.2 \times .36 = 0.79$ inches.
Area of bottom rib $= 2.2 \times .75 = 1.65$ „

Weight of casting = 43 lbs.
Breaking weight = 5528 lbs.

It broke quite off in the same place as the last did.

Both this casting and the preceding one broke with the first weight laid on them, 5528lbs., and probably would have done so, particularly the former, with several cwt. less. The latter casting was doubtless the stronger, as it had more matter in the bottom rib, and they both broke by tension, or by drawing asunder the bottom part: which had indeed been the case with every beam we had tried.

All the preceding experiments were made on beams cast on their side from iron, of which the following is a description.

Mixture.

$\frac{1}{3}$ of Blaina, No. 2, } Welsh,
$\frac{1}{3}$ of Blaina, No. 3, }
$\frac{1}{3}$ of W S S, No. 3, Shropshire.

This mixture is a strong iron, and therefore well suited for beams.

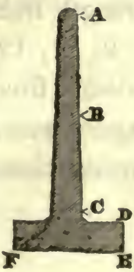
VII. EXPERIMENT.

This was on a beam from the same model as that in experiment 4, it was cast erect, but upside down as usual, and therefore ought not to be compared with the preceding ones.

Distance between supports as before.

Dimensions of section in inches.

Thickness at A = .30.
„ „ B = .37.
„ „ C = .425.
FE = 2.28.
DE = .53.



Area of the above section = 2.98 ins.

Weight of beam = 38 lbs.

Deflection with 6679 lbs. .37 inches.

" " 9495 " .50 "

" " 9279 " .62 "

Breaking weight = 9503 lbs.

It twisted in a serpentine manner before it broke. The form of fracture was nearly as in experiment 4, but here $tr = 1.0$, and $bn = 2.5$.

Hence strength per square inch of section $= \frac{9503}{2.98} = 3188$ lbs.

This beam is not used alone for comparison with the others, as it was conceived to have been cast under the pressure of a superior head of metal. Which, with the different mode in which it was cast, may account for its increased strength.

Remark.—In the future experiments, all the beams, except otherwise mentioned, were cast erect but upside down, as there is an accession of strength from that cause. Those in experiments 8, 9, 11, 12, and 21, were elliptical, and were indeed from the model of the three first experiments, its top and bottom ribs being further changed.

VIII. EXPERIMENT.

Beam from the same model as that of experiment 3rd, the top rib in the casting being to the bottom as 1 to $3\frac{1}{2}$ nearly.

Distance between supports as before.

Dimensions of cross section.

Area of top rib $= 1.05 \times .32 = 0.34$ inches.
Area of bottom rib $= 2.15 \times .56 = 1.20$,,

Thickness of vertical part $= .33$.

Area of cross section $= 3.08$ inches.

Weight of casting $39\frac{1}{2}$ lbs.

Breaking weight 8263 lbs. = 73 cwt. 89 lbs. It broke very near to the middle.

The form of fracture was nearly as in the figure to experiment 1, but here $bn = 2.5$ and $tr = .55$.

Hence strength per inch of section $= \frac{8263}{3.08}$ = 2683lbs. Comparing this with the result from the common beam in experiment 10, which bore 2792 lbs. per inch, we have $2792 - 2683 = 109 =$ defect.

$\therefore$ Loss in strength $= \frac{109}{2792} = \frac{1}{26}$ nearly.

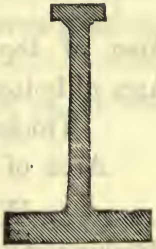
This beam was like all the rest cast erect, but with the broad rib lowest by mistake, which perhaps is the cause of the defect in strength.

IX. EXPERIMENT.

In this the model of the above had one inch in breadth added to its bottom rib.

Ratio of the ribs 1 to $4\frac{1}{2}$ nearly.

Distance between supports as before.



Dimensions of section in inches.

Area of top rib $= 1.05 \times .34 = 0.357$
Do. bottom rib $= 3.08 \times .51 = 1.570$
Thickness of vertical part $= .305$
Area of section $= 3.37$ inches.

Weight of beam $= 44\frac{3}{4}$ lbs.
Breaking weight $=$ 10727 lbs. $=$ 95 cwt. 87 lbs.

It broke by tension, 4 inches from the middle, but slanting toward it; and there seemed to be a small flaw in the bottom rib, at the place of fracture. Here $tr = .6$ inch.

Hence strength per inch of section $= \frac{10727}{3.37}$ $= 3183$ lbs. Comparing this with the result of expt. 10, gives $3183 - 2792 = 391 =$ excess.

$\therefore$ Gain in strength $= \frac{391}{2792} = \frac{1}{7}$ nearly.

REMARK.—Though this beam had a larger bottom rib, it still broke by tension, or by tearing asunder the bottom part first, as was evident since it had neither been crushed nor broke by a wedge, (Art. 34); this I had noticed to be the case in every experiment, (see Expt. 6).

There had been $\frac{1}{7}$ gained in strength, above that of the common beam, by the addition already made, and it was probable, we might add still more to the lower rib without danger of fracture by compression; for in no case except of the common beam, which sometimes twisted a little before it broke, had there been the slightest appearance of over compression. This idea will be pursued in our future experiments.

X. EXPERIMENT.

Common beam, cast upside down, in the usual manner. This like the rest was from the same model as that in experiment 4.

Distance between supports as before.

Dimensions of section in inches, (see section in expt. 4).

Thickness at A = .29
" " B = .425
" " C = .46
FE = 2.3
DE = .53.

Area of section = 3.16 inches.

Note.—The three castings in experiments 8, 9 and 10, were all broke at 4 feet distance between the props, on account of there being defects near the ends of two of the castings, the weight however was laid on the middle of them, 3 inches being taken off each end. The real breaking weights were 9296, 12068 and 9926 respectively; those given above being the reduced ones to a span of 4 feet 6 inches. From the above cause, the deflections are neglected.

Weight of beam $= 40\frac{1}{2}$ lbs.
Breaking weight $= 8823$ lbs.

It broke $1\frac{1}{2}$ inches from the middle. The form of fracture was nearly as in experiment 4; here $bn = 2.25$ and $tr = .8$

Hence strength per inch of section $= \frac{8823}{3.16} =$ 2792 lbs.

In the following experiments, the bottom rib is considerably increased, agreeably to the remarks made on experiment 9, but for fear lest the top rib should be overpowered, and by its compression the point of support thrown lower down the beam, and consequently the beam weakened, the top rib was a little strengthened likewise.

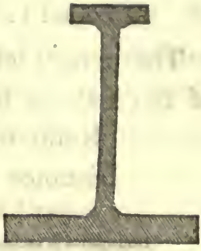
The bottom rib will continue to be increased by small degrees, till such time as the beam breaks by compression, or by the separation of a wedge; at which point, perhaps, we shall have arrived at nearly the strongest form of section, for the same depth of beam and quantity of section.

XI. EXPERIMENT.

Beam from model of experiment 9, only its top and bottom ribs altered as above.

Ratio of ribs 1 to 4 nearly.

Distance between supports and depth as before.



Dimensions of section.

Area of top rib $=1.6 \times .315=0.5$ inches.
„ bottom rib $=4.16 \times .53=2.2$ „
Thickness of vertical part $=.38$ „
Area of section $=4.50$ inches.

Weight of beam $=57$ lbs.

Deflection with	11186	lbs.	.4 inches.
„	12698	„	.45 „
„	13706	„	.52 „

Breaking weight $=14462$ lbs. $=129$ cwt. 14 lbs. It broke by tension 1 inch from the middle; $bn=2{\cdot}5$ inches.

Hence strength per inch of section $=\frac{14462}{4.5}$ $=3214$ lbs. Comparing this with the result of experiment 13, which bore 2693 lbs. per inch,

$$3214-2693=521=\text{excess}.$$

$$\therefore \text{Gain in strength } \frac{521}{2693}=\frac{1}{5} \text{ nearly}.$$

We may seek for the gain by comparing the weights of the two beams, and the quantities they bore: thus, since in experiment 13, the weight of the beam was 41 lbs., and it broke with 8942 lbs, and the weight of this beam 57 lbs., and its breaking weight 14462 lbs. Hence $41 : 57 :: 8942 : 12431 =$ weight this beam should have borne, according to the strength of the common beam; but it did bear 14462. $\therefore$ Gain $=14462-12431=\frac{1}{6}$ nearly.

XII. EXPERIMENT.

The model of this beam differed from that of the last, in having a broader bottom flange.

Ratio of ribs 1 to $5\frac{1}{2}$ nearly.

Distance of supports as before.

Dimensions of section in inches.

Area of top rib $= 1.56 \times .315 = 0.49$

Area of bottom rib $= 5.17 \times .56 = 2.89$

Thickness of vertical part $\Big\} = .34$ inch.

Area of section $= 5$ inches.

Weight of beam $= 67\frac{1}{4}$ lbs.

Weight in lbs.	Deflections.
8288	.24 inches.
12698	.36 ,,
13706	.40 ,,
14210	.42 ,,
15218	.45 ,,
15722	.48 ,,
16226	.49 ,,
16730	.53 ,,

with this last weight it broke, after having borne it some minutes. It broke by tension very near the middle. 16730 lbs. = 149 cwt. 42 lbs.

Hence strength per square inch of section $= \frac{16730}{5} = 3346$ lbs. Comparing this with the result of experiment 13, we have $3346 - 2693 = 653 =$ excess.

$\therefore$ Gain in strength $= \frac{653}{2693} = .242 = \frac{1}{4}$ nearly.

Seeking for the gain, by comparing the weight 67¼ of this beam, and its breaking weight 16730, with the weights 41, and 8942, in experiment 13, we have, as in the last experiment, 41 : 8942 :: 67¼ : 14667.

Whence gain $= 16730 - 14667 = \frac{1}{7}$ nearly: a gain considerably less than that given above, on account of the great weight of the bottom rib; it being uniform in size through its whole length of 5 feet.

XIII. EXPERIMENT.

Beam of the *common form*, from the same model as the others.

Distance between supports as before.

Dimensions of section in inches (see *fig.* to expt. 4.)

Thickness at A = .29.
„ „ B = .425.
„ „ C = .53
DE = .565
FE = 2.34.

Area of section = 3.32 inches.

Weight of beam = 41 lbs.

Weights in lbs.	Deflections in inches and parts.
7598	.4
8494	.43
8942	.47,

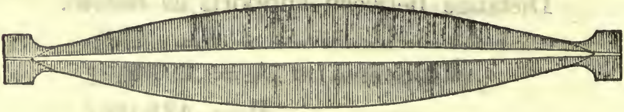
with this last it broke after standing a few minutes. It broke 1½ inches from the middle.

Hence strength per square inch of section $= \frac{8942}{3.32} = 2693$ lbs.

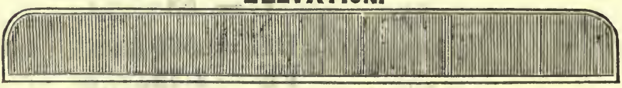
FORM OF BEAM ALTERED.

42. The beams in our future experiments were of equal height, through their whole length, and had their top and bottom ribs uniform in thickness, but tapering toward the ends, the bottom rib being parabolic. They are represented by the vertical plan and elevation below, where the sections of their middle are as in the following experiments; and the sections, from their middle toward the ends, as in experiments 11, 9, 3.

PLAN.



ELEVATION.



This form was adopted to save metal, by reducing the bottom rib, which was likely to become very large. The reasonings by which I was led to suggest these deviations, from the form of the elliptical beam before used, were the following.

The vertical part, which was uniform through the whole length,* being thin comparatively

* The vertical part of the beam ought like the ribs to have been reduced toward the ends, considering the leverage only;

with the bottom rib, the beam might be made of equal height throughout, and consequently its depth, and power of bearing near the ends, increased with little additional metal.

From the form of fracture in the preceding experiments, and the great size which the bottom rib would be of, I was convinced that the neutral line in our future experiments would lie very low,* and, therefore, nearly all the tensile force would be exerted by the bottom rib, whilst the rest of the beam would serve for little more than a fulcrum; the center of resistance to compression, or of that fulcrum, laying very near to the top, it being perhaps at the point *r* in our former experiments.

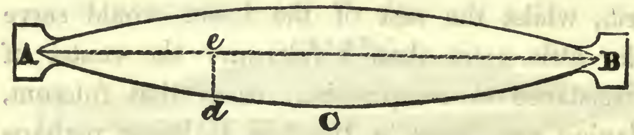
Suppose D to be the vertical distance from the center of compression, at any part of the beam, to the center of tension in the bottom rib; and if T be the direct tensile strength of the bottom rib at that part, T multiplied by some function of D, (perhaps $T \times D$) will represent the strength of the beam there. But

but that was neglected as there might have been too great a tendency in the weight to cut the beam across near to its ends, if it were more reduced there; see Article 33.

* This was verified by the 19th experiment, where the wedge shewed the neutral line to be at $\frac{6}{7}$ of the depth.

D, throughout the same beam, will be a constant quantity, or nearly so; the strength of the beam therefore at any part will be nearly in proportion to that of its bottom rib at that part; and as the strain will be less toward the ends, the bottom rib may be reduced there likewise.

Suppose the bottom rib to be formed of two equal parabolas, the vertex of one of them A C B being at C;



then by the nature of the curve, any ordinate $d\,e$ is as $A\,e \times B\,e$; the strength of the bottom rib therefore, and consequently that of the beam at that place will be as this rectangle. It is shewn too by writers on the strength of materials that the rectangle $A\,e \times B\,e$ is the proportion of strength which a beam ought to have to bear equally the same weight every where, or a weight laid uniformly over it; it would appear therefore that the beam above is rightly devised, and these views will be strengthened by the future experiments.

The length of the parabola AB in the following experiments was just 4 feet 6 inches, or equal to the distance between the supports, and three inches were added to each end of the beam, beyond the points A and B, to lay upon the props; the parabolic bottom was likewise a little strengthened at the ends, as in the figure.

XIV. EXPERIMENT.

Distance between supports 4ft. 6ins. and depth of beam $5\frac{1}{8}$ins. as before.

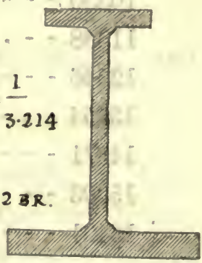
Dimensions of section in inches.

Area of top rib $= 2.3 \times .315 = .72$ $= \frac{1}{3.214}$

Area of bottom rib $= 4.06 \times .57 = 2.314$

Thickness of vertical part $= .33$.

Area of Section $= 4.628$ inches. = 2 8R.



Breaking weight = 15024 lbs. = 134 cwt. 16 lbs. It broke by tension very near to the middle.

Hence strength per square inch of section $= \frac{15024}{4.628} = 3246$ lbs.

XV. EXPERIMENT.

In this experiment the breadth of the bottom rib only was increased as before.

Distance between supports and depth as before.

Dimensions of section in inches.

Area of top rib = 2.35 × .29 = .68 $= \frac{1}{4{\cdot}29}$

Area of bottom rib = 5.43 × .537 = 2.916

Thickness of vertical part = .35.

Area of section = 5.292 inches. = 1·8 B.R.

Weights in lbs.	Deflections in parts of an inch.
6218	.12
7598	.15
8288	.18
9309	.20
10330	.22
11338	.25
12346	.26
13354	.29
14371	.31
15393	.33
16401	.53

Breaking weight 16905 lbs. = 150 cwt. 105 lbs.
It broke by tension.

Hence strength per square inch of section $= \frac{16905}{5.292} = 3194$ lbs.

XVI. EXPERIMENT.

Beam from the same model, but with further increased bottom rib.

Distance between supports and depth as before.

Area of bottom rib $= 6.8 \times .502 = 3.413$ ins. $= \frac{1}{5}$

Weight of beam $= 64\frac{1}{2}$ lbs. Area of Section =

Weights in lbs.	Deflections in parts of an inch.
6218	·16
7598	·18
8288	·19
9309	·21
10331	·22
11339	·24
12341	·26
13351	·28

Breaking weight = 14336 lbs. nearly, and 14336 lbs. = 128 cwt.

This broke by tension and ought to have borne considerably more than the last beam; but its iron must have been of a less tenacious kind than the others; as is evident by comparing their deflections, this beam having bent little more than half what the preceding one did before it broke. The same may be said of the common beam following.

XVII. EXPERIMENT.

Beam of the *common form* from the same model as the preceding ones, (see fig. to expt. 4).

Distance between supports as before.

Weight of casting $39\frac{1}{2}$ lbs.

Weights.	Deflections.
6218	.28 inches.
7138	.33 ,,

Breaking weight = 7598 lbs.

The area of a cross section at the place of fracture in this beam was not taken; but assuming it at 3.08 inches, which is about a mean between the other areas from the same model, we have $\frac{7598}{3.08} = 2466$ lbs. = strength per inch of section.

This beam was cast, with the others preceding it, for comparison; but as there seems to be some difference in the iron, it is not safe to attempt any. The experiments however will have their use, as is mentioned further on.

XVIII. EXPERIMENT.

Beam from the same model as that in expt. 16.

Distance of supports as before.

Dimensions of section in inches.

Top rib $= 2.3 \times .28 = .64$

Bottom rib $= 6.61 \times .54 = 3.57$

Thickness of vertical part $= .34$.

Area of section $= 5.86$ inches.

Weight of casting $68\frac{1}{2}$ lbs.

Weights in lbs.	Deflections in parts of an inch.	Returned to, Weights removed.
9327		0
10017		0
12087	·26	0
12777	·29	0
,, repeated	·30	·05
14345	·33	·07
		.05 in a minute.
15913	·35	·05
16697	·36	
18265	·43	

Breaking weight 19441 lbs. = 173½ cwt.

This beam broke very nearly in the middle, by tension, as before.

Hence strength per square inch of section = $\frac{19441}{5.86} = 3317$ lbs.

43. The preceding beams were intended to be equally strong, in every part, to sustain a load laid uniformly over them; and to see if that was the case in this beam, I made the following experiment.—The larger arm of the beam was placed on two supports, 2 feet 3 inches apart, and had 26497 lbs. laid upon its middle, without breaking there: it failed at its fractured end, or it would have been tried further.

Now from the principle of the lever, (see problem further on) the strength of half of a beam so broken, is to the strength of the whole beam, as 3 to 2.

Hence, as the whole beam broke with 19441 lbs. the half should not have broke with less than 29161 lbs. We have no data to prove whether this would or would not have borne that weight, 26497 lbs. only having been laid on it; it is however probable that it would have borne it or a greater weight.

Another half of a beam of nearly the same dimensions, and which must either have belonged to that in experiment 15 or 16, broke as before in its middle, with 22255 lbs. the breadth of the bottom rib, at the place of fracture, being 4.6 inches.

Now 2 : 3 : : 16905 (expt. 15) : 25357 lbs.

,, 2 : 3 : : 14336 (expt. 16) : 21504 lbs.

Hence, the strength of the end of the beam was a little in defect or excess, according as it belonged to the former or the latter of these experiments.

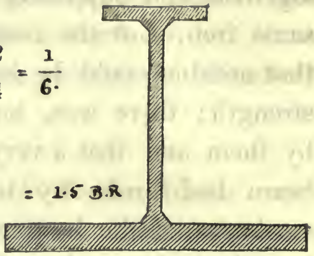
The beams in the preceding four experiments, commencing with experiment 14, were cast together, and supposed to have been from the same iron, but the results were so anomalous that nothing could be learned as to the relative strength; there was, however, one fact proved by them and that a very important one; every beam had broke by tension, or through the weakness of the bottom rib, which though so large had always been torn asunder, while the top part had remained unchanged. It, having been prevented from twisting by the small rib there, shewed no signs of being over compressed.

44. In the following four experiments, and indeed in all the following ones, a good deal of attention was paid to the iron, it was the same as that used in the commencement of this series, and of which a description has been given: it is a strong iron and was considered by Mr. Lillie as best adapted for beams.

XIX. EXPERIMENT.

The last beam having still broke by tension, the bottom rib was again increased, by making it a small portion thicker, but without altering the depth of the beam.

Distance of supports, 4ft. 6ins. and depth of beam, $5\frac{1}{8}$ inches, as before.



Dimensions of cross section in inches.

Area of top rib $= 2.33 \times .31 = .72$ $= \frac{1}{6.}$

„ of bottom „ $= 6.67 \times .66 = 4.4$

Thickness of vertical part $\} = .266$

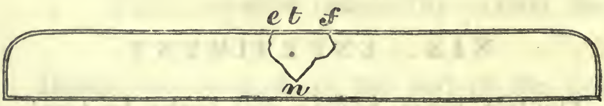
Area of section $= 6.4$, or $6\frac{2}{5}$ ins. = 1·5 B.R

Weight of beam $= 71$ lbs.

This beam broke in the middle by compression with 26084 lbs, or 11 tons 13 cwt., a wedge separating from its upper side.

The weights were laid gradually and slowly on, and the beam had borne within a little of its breaking weight a considerable time, perhaps half an hour.

The form of the fracture and wedge is represented by the figure, shewing a side view of the beam; where *enf* is the wedge, *ef*



$= 5.1$ inches, $tn = 3.9$ inches, angle *enf* at vertex $= 82°$.

It is extremely probable, from this fracture, that the neutral point was at *n*, the vertex of the wedge, and therefore at $\frac{3}{4}$ths of the depth of the beam, since 3.9 inches $= \frac{3}{4} \times 5\frac{1}{8}$ inches nearly.

Hence, strength per square inch of section $= \frac{26084}{6.4} = 4075$ lbs. which is much greater than that in any of our former experiments.

Comparing this result with that of the common beam in experiment 22nd, which was cast with these, and which bore 2885 lbs. per inch, we have $4075 - 2885 = 1190$ lbs. = excess.

$\therefore$ Gain in strength, from the section, $= \frac{1190}{2885} = .41$, or upwards of $\frac{2}{5}$ of what was borne by the common beam.

The quantity of metal saved, through the section, would be represented by the above excess 1190, divided by 4075, the quantity which the beam bore per square inch of section.

$\therefore$ *Saving* of *metal*, from section, $= \frac{1190}{4075} = .292$, or $\frac{3}{10}$ nearly.

If we compare the strengths of this beam, and that in experiment 22, by the weights, as was done in experiments 11 and 12, we shall have the saving in metal, through the section and general form of the beam conjoined, = .377.

As this is the strongest beam we have tried, if it be compared, by weight, with the result from the very strong beam, of the common form, in experiment 7, the saving in metal will be .29.

45. Thus we have, by constantly making small additions to the bottom rib, arrived at a point where resistance to compression could be no longer sustained; but it was not till the bottom rib had considerably more matter in it than double the rest of the beam there, the bottom rib being to the rest as 4.4 to 1.83, and to the top rib as 6 to 1.—Still the top rib was not crushed nor shewed any signs of weakness. The fracture took place by the vertical part of the beam becoming torn, by the opposite forces of tension and compression round the neutral line (see art. 33), as was the case in the experiment in article 23.

The great strength of this section is an indisputable refutation of that theory, which would make the top and bottom ribs of a cast iron beam equal.

XX. EXPERIMENT.

Beam from the same model as that in the last experiment.

Distance between supports as before.

Dimensions of section in inches, (see fig. last expt.)

Area of top rib = $2.3 \times .28 = .64$,

Area of bottom rib = $6.63 \times .65 = 4.31$.

Thickness of vertical part, } = .335.

Area of section 6.5, or $6\frac{1}{2}$ inches.

Weight of beam = $74\frac{3}{4}$ lbs.

Weights in lbs.	Deflections in parts of inch.	Returned to (weights taken off.)
9328	.22	0
11397	.24	0
12777	.25	0
14345	.26	.03
15913	.30	.04
17481	.34	
18265	.36	
19049	.38	
20617	.43	
22185	.47	
22969	.48	
,,	.50	

It broke in the middle of the beam by tension, with 23249 lbs. or 10 tons 8 cwt. nearly.

This is considerably less than what the former beam bore, though its bottom rib, in which the tensile power of this form of section almost wholly lies, was not much different. The iron must therefore have been weaker.

Strength per square inch of section $= \frac{23249}{6.5}$ $= 3576$ lbs.

Comparing this with the result of the common beam in experiment 22, which bore 2885 lbs. per inch, $3576 - 2885 = 691 =$ excess.

$\therefore$ Gain in strength, from section, $= \frac{691}{2885} = .236$, in terms of what the common beam bore; whence saving in metal $= \frac{691}{3576} = \frac{1}{5}$ nearly.

If we compare this beam with the common one, by their weights, the saving in metal will be .26, or upwards of $\frac{1}{4}$th.

The thickness of the vertical part of the beam, in experiment 19, was .266, and in this expt. .335; we might therefore have increased the bottom rib of this beam, in the ratio of 335 to 266, or $\frac{1}{3}$rd nearly, when it is probable the beam would have broke equally soon by tension, or by the rupture of the vertical part as in experiment 19.—And a much greater excess of strength than that above would have been obtained.

XXI. EXPERIMENT.

This was on an *elliptical* beam from the same model as that in experiment 12, and those preceding it, the bottom rib being further increased, and being like as in them of equal breadth through the whole length of 5 feet.

Distance between supports as before.

Dimensions of section in inches.

Area of top rib = $1.54 \times .32 = .493$

" of bottom " = $6.50 \times .51 = 3.315$

Thickness of vertical part, .. } = .34

Ratio of ribs $6\frac{1}{2}$ to 1.

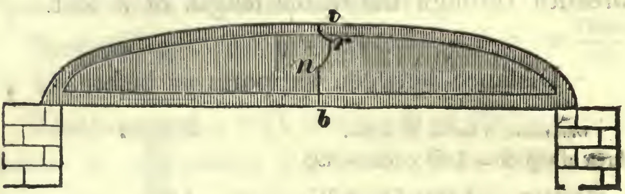
Area of section = 5.41 inches.

Weight of beam = $70\frac{3}{4}$ lbs.

Weights in lbs.	Deflections in parts of inch.	Returned to (weights taken off.)
9327	·26	0
10707	·27	0
11397	·28	0
12087	·30	0
12777	·31	0
14345	·34	0
15913	·35	0
16697	·42	·06
17481	·43	·06
19049	·46	
19833	·50	
20617	·54.	

It broke very near the middle, by tension, with 21009 lbs., or 9 tons 8 cwt. nearly.

Form of fracture nearly as $b\,n\,r$ in figure; $bn = 1.8$ inches.



Hence strength per square inch of cross section $= \frac{21009}{5.41} = 3883$ lbs.

Comparing this with the result from the common beam in experiment 22, which bore 2885 lbs. per inch, $3883 - 2885 = 998 =$ excess.

Hence gain in strength $= \frac{998}{2885} = .345$, in terms of what the common beam bore: or saving in metal, from section, $= \frac{998}{3883} = .257$, or upwards of $\frac{1}{4}$.

If the comparison be made by their weights, the saving in metal will be only .23, which is less than it would have been, had the ends of the beam been formed as in the preceding ones from experiment 13 to this: the bottom rib of this being all of a breadth and thickness and 5 feet long; though the distance of the supports was but 4 feet 6 inches. This remark applies, though in a less degree, to the beams in expts. 11 and 12, and those immediately preceeding them; those beams like this having their vertical part an ellipse, and their bottom rib of equal breadth throughout.

XXII. EXPERIMENT.

This beam was of the common form, from the same model as before, for comparison with the three preceding ones.

Distance between supports as before.

Dimensions of section in inches.

Thickness at A = .30.
" " B = .42.
" " C = .45.
DE = .51.
FE = 2.28.

Area of section = 3.17 inches.

Weight of beam = 40 lbs.

This beam bore 8965 lbs. and broke in the middle with considerably less than 9327 lbs. a man supporting part of this extra weight by a lever. This accident prevented the exact determination, but I believe 9146 lbs. the mean between the numbers above, to be very near the breaking weight, perhaps rather above it.

Hence strength per square inch of section $= \frac{9146}{3.17} = 2885$ lbs.

46. It has been doubtless remarked that, in most

of the foregoing experiments, the vertical part of the beam was made much thinner than the bottom rib; which it was necessary for it to be, otherwise that rib must have been proportionably broader, and this would have endangered the strength of it, by its liability to flexure. The injury from irregular cooling in metal of unequal thickness, I was in some degree acquainted with, and it was soon mentioned to me by Mr. Ewart, whose extensive general knowledge and kindness left few maxims connected with the material uncommunicated. It was felt an objection: the appearance of the surface of fracture was minutely examined, but nothing was elicited from it: there was a difference in the aspect of the bottom rib and other parts, but only such as iron of different thickness unconnected would have shown. There was a powerful argument too in our favour; we had excellent castings; they had no flaws or defects in them which could be attributed to the cause mentioned above, and their progressively increasing strength left it, I conceive, without doubt, that irregular cooling had no mischievous effect on beams cast like ours. The reason of it may possibly be this, the beam being mostly cast erect, and wrong side up, the heavy bottom rib, lying near the surface in the sand, might nearly keep pace in cooling with the thinner part lower down; and in those cases where the beam was cast on its side, it would cool more regularly with the sand in which it was buried.

47. In the preceding experiments, it has been mentioned, that we began with that form of section which a highly ingenious modern writer on the strength of cast iron, was induced to consider as the strongest to preserve its elasticity, the top and bottom ribs in it being equal; and this form we found to be $\frac{1}{12}$th weaker to resist an ultimate strain, than that of the common beam in the iron on which he wrote, though it would, as we have before seen, be perhaps, the strongest in wrought iron. We then, by gradually reducing the top rib in the same model and adding the part taken off to the bottom one, obtained castings in which the strength was found to be regularly increasing, and the form in experiment 3rd somewhat stronger than that of the common beam. It did not now seem adviseable to decrease further the top rib; and as every beam had been found to break by tension, or through the weakness of the bottom part, I thought it best to keep increasing the bottom rib by small degrees till such time as the beam broke by the rupture of some other part. This increase was commenced in experiment 9, the ribs in that being as $4\frac{1}{2}$ to 1; and the result, from the form of section, in this case was a gain

in strength of about $\frac{1}{7}$th: this beam also broke by tension. Now before increasing the bottom rib any further, I thought it adviseable to add a little to the top one, as the vertical part of the beam, or that part between the ribs would be, perhaps, strong enough for much larger ribs. In expts. 11, 12 and 21, the top rib and vertical part of the model were the same, the only difference being in the breadth of the bottom rib: from the first of these the increase of strength in terms of what was borne by the common beam was $\frac{1}{5}$, from the second $\frac{1}{4}$, and from the third $\frac{1}{3}$; and had we added still more to the bottom rib in the same model, it is probable that the gain might have been much greater.

48. In experiments 14, 15, 16, 18, 19, and 20, the top rib of the model was the same, but somewhat larger, than in experiments 11, 12, and 21, and the bottom rib was the only one varying. In the 19th experiment the section of the bottom rib at the place of fracture was more than double the rest of the section, and the ratio of top and bottom ribs was 1 to 6. In this instance the fracture took place by the rupture of the vertical part of the beam, which happened to be thinner than usual; the gain in strength here, from the section, was upwards of $\frac{2}{5}$ of what the common beam bore; and the saving

by it was nearly $\frac{3}{10}$ of the metal. This experiment was repeated in expt. 20, but the beam seems to have been of somewhat weaker iron.

49. The form of section, in experiment 19, is the best which we have arrived at for the beam to bear an ultimate strain. That in expt. 21, if its bottom rib had been a little further increased, would, it is probable, have borne nearly as much per square inch of section; but its narrower top rib, when tapering toward the ends as in our latter beams, might probably have allowed the beams to have twisted there; a tendency which was observed in an experiment further on. If then we adopt the form of beam in experiment 19, I think we may confidently expect to obtain the same strength with a saving of upwards of $\frac{1}{4}$th of the metal; or in other words, that 75 tons of metal will bear more than 100 tons would, if cast in the best models of the usual form.

50. In the first seven of the preceding experiments those deflections which were obtained were taken in the middle of the beam, but, from the mode then used, not with great accuracy. In the succeeding ones a good deal of care was taken, and I imagine they are not very incorrect, though, on account of the smallness

of the deflections, inaccuracies could scarcely be avoided; they were not in these taken in the middle of the beam, but three inches from it, as it was more convenient to take them there than in the middle.

51. If we examine the deflections in experiments 18, 20, and 21, we shall find that in the first and third about $\frac{2}{3}$, and in the second upwards of $\frac{1}{2}$ the breaking weight was laid on without the elasticity being in appearance at all injured. Now this is contrary to former experience, it having been generally found that the elastic force was sensibly injured with about one third of the breaking weight (Tredgold's Essay, Page 79). And as experiments have mostly been made upon rectangular pieces, the above fact, if properly ascertained, will render it probable that change of form may have an influence upon this ratio; and may in some forms remove the point of incipient derangement from $\frac{1}{3}$ to $\frac{1}{2}$ or even $\frac{2}{3}$ of the breaking weight.

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52. In the preceding experiments the beams being short, and the deflections small, there was considerable difficulty in ascertaining the precise point, where the above defect took place;

but the matter seemed to be too important to be allowed to pass without some further investigation, and especially as we should be enabled at the same time to determine what influence a change in the depth of one of our beams would have upon its strength, every other dimension remaining the same. For these purposes, therefore, I made the following experiments.

In these, and indeed in all the future experiments, the same sort of beam was used as that in our last, and which was described immediately before expt. 14; it was broke too in the same manner. There was, however, this slight difference, that there the parabolic base was but just equal in length to the distance between the supports, and the beam had its ends rendered a little wider and longer with matter attached to them to lie on the props; but here the parabola was made 6 inches longer than the distance between the props, in order that 3 inches of it might lie upon them at each end; this was done to render the beam capable of bearing more toward the ends, as there was some doubt whether in the preceding experiments the ends were not a little too weak. The beams were all cast 7 feet 6 inches long, and were supported by props 7 feet asunder; they were from the same model, which varied only in the breadth of its vertical part, the depth of the beam being all that was intended to vary. The depths were nearly 4, 5, 6, and 7 inches; but accurate admeasurements are given with the sections.

The vertical part in the beams was rendered too strong comparatively with the size of the bottom rib; it was desirable it should be so, that they might all break by tension, or in the same manner, to furnish the means of judging correctly of their relative strength. The bottom rib ought otherwise to have been made stronger, or the vertical part thinner.

XXIII. EXPERIMENT.

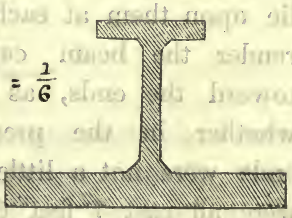
Distance between supports 7 feet, depth of beam 4.1 inches.

Dimensions of section at place of fracture, or middle.

Area of top rib = 2.25 × .33 = .74 ins. $= \frac{1}{6}$
,, bottom ,, = 6.00 × .74 = 4.44 ,,

Thickness of vertical part $\}$ = .40 inches.

Area of section = 6.54 ,, 1·5 B.R.



Weight of casting = 1 cwt. 0 qrs. 2 lbs. = 114 lbs.

Weights in lbs.	Deflections in parts of an inch.	Returned to, (weights removed.)
2764	.25	0
2879	.25	0
2994	.26	0
3109	.26	0
3224	.27	0 doubtful.
3339	.28	.0 ,,
3454		perceptible set.
3569	.29	,,
3684	.31	.04
3914	.32	.05
4029	.32	.05

The beam was now removed; and having been again placed under the lever, the experiments were recommenced, and the deflections taken from the form it had assumed.

Weights.	Deflections.	Returned to.
5180	.40	0
5353	.41	0
5525	.44	0
5698	.44	0
6042	.45	0
6215	.51	0
6971	.55	apparent set.
7349	.57	
7727	.62	
8105	.64	
8483	.70	
8861	.74	

The beam not having been broke at this time, the experiment was resumed two days afterwards, when the beam seemed nearly straight again, and the deflections were those from the form it then had acquired.

Weights.	Deflections.	Returned to.
8637	·75	·03
9327	·76	·03
10017	·80	·03
10707	·88	·03
11397	·95	·04
12087	1·04	·08
12815	1·08	·09

13543. It broke with this within $1\frac{1}{2}$ inches of the middle by tension : 13543 lbs. = 6 tons. 103 lbs.

53. For a beam to support equally through its whole length a uniform load, it is necessary that it should bear the same weight, when applied towards its ends, that it bore in the middle. To ascertain whether the above beam would have done this, the longer half of it was placed upon two supports 3 feet 6 inches asunder; one prop supporting the end as before, and the other lying under the middle of the beam. Weights were then gradually laid on, half way between the supports, till fracture took place. It broke with 23396 lbs. or 10 tons $8\frac{3}{4}$ cwt., 15 inches from the end, and where

the breadth of the bottom rib was 3.85 inches. Now, by the property of the lever, the pressure, which the whole beam bore, is, to the weight which the half beam broke as above, should have borne, as 2 to 3. Hence 2 : 3 :: 13543 lbs. (weight borne by whole beam) : 20314 lbs. = weight, which the half beam should have borne: but it required 23396 lbs. to break it; hence a parabolic beam similar to that above, 7 feet 6 inches long, and broke by props 7 feet asunder, is rather too strong toward the ends; and it would have been too strong there still, if the props had been 7 feet 6 inches distant, or at the ends of the parabolic base of the beam.

54. The other half of the beam was turned the wrong way up, and broke by weights gradually laid on its middle or half way between the props, one prop supporting the end of the beam, as before, and the other placed 3 feet 3 inches from it. It broke in the middle with 13356 lbs. or 5 tons 19¼ cwt.

If we reduce this weight to what it would have been if the distance of the props had been 3 feet 6 inches as above, we have, 3 feet 6 inches : 3 feet 3 inches, or 14 : 13 :: 13356 : 12402 lbs. or 5 tons, 10¾ cwt. which is little more than ½ of what the former half beam bore; and the difference of strength would have been much greater, if we had used two equal whole beams. Hence we see the impropriety of turning beams the wrong side up, as is often done in Factories. But this matter was still more clearly shewn in one of our early experiments (art. 23), where a ⊤ section of cast iron bore nearly 4 times as much one way up as the other.

XXIV. EXPERIMENT.

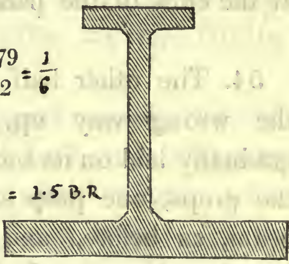
Dist. between supports 7 ft. depth of beam 5.2 ins.

Dimensions of section in inches.

Area of top rib $= 2.25 \times .35 = .79$ $= \frac{1}{6}$
,, bottom rib $= 6.00 \times .77 = 4.62$

Thickness of vertical part $\Big\} = .34$

Area of section = 6.94 inches. = 1·5 B.R



Weight of casting = 1 cwt. 0 qrs. 16 lbs = 128 lbs.

Weights in lbs.	Deflections in parts of an inch.	Retnrned to, (weights removed.)
7257	- - - - - - - - - - - -	perceptible set.
7947	- - - - .35 - - - - -	.08
8637	- - - - .43 - - - - -	.08
9327	- - - - .51 - - - - -	.11
10017	- - - - .53 - - - - -	.13
10707	- - - - .56 - - - - -	.14

Weights.	Deflections.	Returned to.
11397	.58	.16
12087	.63*	.16

15129 = 6 tons 15 cwt. 9 lbs.

It broke by tension very near the middle with this weight, others having been laid on progressively up to it.

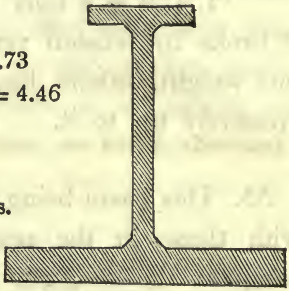
55. This beam being nearly of the same depth with those in the preceding series, we may compare the strength of its iron with that in the last of those experiments.

Hence strength per inch of section $= \frac{15129}{6.94} =$ 2180 lbs. To find what this quantity would have been, if the beam had been only 4 feet 6 inches long between the props, as in our first 22 experiments. 4 feet 6 inches : 7 feet, or 9 : 14 :: 2180 : 3391 lbs. per inch; which quantity is less than that in experiments 19, 20, and 21; the iron is therefore a little weaker.

* After the beam had been bent to .63, it had the weight taken off the middle, and 14345 lbs. or 6 tons 8 cwt. gradually laid on it, half way between the middle and the end. It would doubtless have borne considerably more there, but was not tried further, as the intention was to break it by a weight in the middle.

XXV. EXPERIMENT.

Distance between supports 7 feet, depth of beam 6.0 inches.



Dimensions of section in inches.

Area of top rib $= 2.2 \times .33 = .73$

„ of bottom rib $= 5.95 \times .75 = 4.46$

Thickness of vertical part $\} = .355$

Area of Section $= 7.08$ inches.

Weight of casting $=$ 1 cwt. 0 qrs. $15\frac{1}{2}$ lbs. $=$ $127\frac{1}{2}$ lbs.

Weights in lbs.	Deflections in parts of an inch.	Returned to, (weights removed.)
7257	·27	0
7947	·32	0
8637	·33	0
9327	·34	0
10017	·36	0
10707	·38	0
11397	·42	0
12087	·45	0 doubtful.
13543	·49	perceptible set.
14271	·53	·04
14999	·56	·06
15129	·58	

It broke by tension in the middle with this last weight, 15129 lbs. = 6 tons 15 cwt. 9 lbs., after standing a minute.

I conceive this beam to have been rendered somewhat weaker, by a small twist in the vertical part of the casting.

XXVI. EXPERIMENT.

Distance between supports 7 feet, depth of beam 6.93 inches.

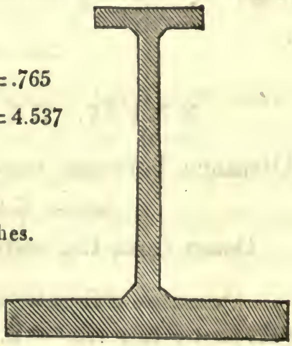
Dimensions of section in inches.

Area of top rib = 2.25 × .34 = .765

„ of bottom rib = 6.05 × .75 = 4.537

Thickness of vertical part } = .38

Area of section = 7.67 inches.



Weight of casting = 1 cwt. 1 qr. 6 lbs. = 146 lbs.

Weights in lbs.	Deflections in parts of an inch.	Returned to, (weights removed.)
8637		0
9327	·20	0
10017	·23	0
10707	·24	0
11397	·25	0
12087	·27	0
13543	·32	0 doubtful.
14271	·35	apparent set.
14999	·37	·03

Weights.	Deflections.
15913	·40
17481	·45
18265	·50
19049	·52
19833	·55
20617	·60
21401	·65

22185 = 9 tons 18 cwt. with this weight it broke by tension exactly in the middle.

XXVII. EXPERIMENT.

Distance between supports 7 feet, depth of beam 6.98 inches.

Beam from the same model as the last.

Dimensions of section in middle, (see fig. last experiment.)

Area of top rib = 2.25 × .32 = .72 inches.

" of bottom rib = 5.95 × .73 = 4.343 "

Thickness of vertical part = .37

Area of section = 7.40 inches.

Weight of beam = 1 cwt. 1 qr. 1 lb. = 141 lbs.

This beam received a shake in the commencement of the experiment, which gave it a sensible set, but did it no other injury.

Weights in lbs.	Deflections in parts of an inch.
9327	.25
11397	.36

Weights.	Deflections.
14345 - - - - -	.44
15913 - - - - -	.47
17481 - - - - -	.50
19049 - - - - -	.58

With this last weight the beam was conceived to be very near fracture; the weights were therefore removed, and the casting was found to have taken a set of $\frac{1}{10}$ of an inch.

56. To ascertain whether the beam was as strong toward the ends as in the middle to bear a uniform load, weights were now gradually laid upon the beam, at a point half way between the middle and the end, in the same manner as in the note after experiment 24. It bore 20225 lbs. for about half-a-minute and then broke near the end, at a place in the under side of the bottom rib, where it was rather unsound. The inference from this experiment, though imperfect, is that the beam would bear a somewhat greater weight near the ends than in the middle.

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57. From these experiments it appears that the ultimate strength, in sections like the preceding, is, cæteris paribus, nearly as the depth; but somewhat lower than in that ratio.

With regard to the elasticity there are some anomalies; experiments 25 and 26 exhibited no defect, when the first beam had borne upwards of $\frac{2}{3}$, and the latter more than $\frac{1}{2}$ the breaking weight. But experiment 23, which was on a beam of very small depth, shews a deviation at an early period, it was however very small till upwards of half the breaking weight was laid on; when perhaps the elasticity of the bottom rib began to be injured. The former defect being attributable to some inconsiderable falling off in the elasticity of the compressed part of the beam; possibly arising from oblique compression, through greater flexure in a shallow beam.

58. There seems, therefore, to be little doubt that the elastic force is longer perfect in these forms than in those on which experiments have generally been made; and the reason may probably be this: the earlier deflections in our best beams are almost wholly caused by compression, on account of the smallness of the compressed part; and it appears highly probable that cast iron would remain perfectly elastic under much greater forces when applied directly to compress it, than would be required to injure its elasticity by tension.

59. The following experiments were made after the paper was read, but are incorporated with it, as they will give additional evidence to some of the preceding conclusions. In these the lineal dimensions of some of the beams were considerably increased, and more varied, agreeably to a suggestion of Mr. Ewart's, and which I was glad to accede to, as Messrs. Fairbairn and Lillie were desirous of affording every means of a full investigation.

The beams were in this instance, cast on their side, in the manner of those in the first six experiments on beams; it being rather more convenient to cast them so, than erect, as has been usually done in the others. The intention of these experiments would perhaps be understood by first taking the three marked 28, 29, 30, and then the next three. I will, however, give the following explanation.

In experiment 28, the model from which the beam was cast was that of experiments 19 and 20, with the bottom rib still further increased; the vertical part of the beam, or that between the flanges, being rendered a little thicker, and tapering upwards from the bottom flange. This was done to endeavour to prevent fracture taking place, by a wedge tearing out from near the neutral line, as was the case in experiment 19.

In experiment 29, the model of the beam had precisely the same section in its middle as that in experiment 28; but the beam was twice the length. If then the strength be inversely as the length, this beam ought to bear half of that in experiment 28.

In experiment 30, the model had in section, in its middle, the same top and bottom rib, as in the two preceding experiments, with nearly the same thickness of vertical part; but this beam was double the depth of the others; it was likewise double the length of that in expt. 28, and had therefore the same length as that in experiment 29. If then the strength be simply as the depth, as we have before concluded, (article 57,) this beam ought to have double the strength of that in experiment 29; and if the strength with the same section is inversely as the length, its strength should be the same as that in experiment 28, it being double the length and depth, and in other respects the same.

The remarks above made respecting experiments 28, 29, and 30, will equally apply to experiments 31, 32, and 33, these having been made with the same view, and only differing from the beams in the preceding ones, by having a larger bottom rib.

60. All the beams (except those of the common form) were made like those described after experiment 13; with this difference however, the parabola of the bottom rib was, in the 4ft. 6ins. beams, 3 inches longer than the distance between the supports, viz. 4 feet 9 inches, and in the 9 feet beams, 6 inches longer, or 9 feet 6 inches, this was done to render the ends of the beams a little stronger, agreeably to the remarks made prior to experiment 23.

XXVIII. EXPERIMENT.

Distance between supports, 4 feet 6 inches.

Depth of beam, $5\frac{1}{8}$ inches.

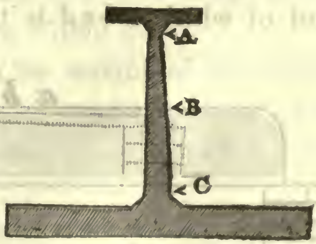
Weight of beam, 81 lbs.

Dimensions of section in inches.

Area of top rib $2.15 \times .27 = .58$

,, bottom ,, $6.74 \times .71 = 4.785$

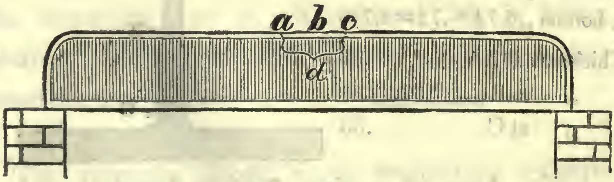
Thickness at A	.25
,, at B (half way between flanges)	.37
,, at C	.53



Area of section 7.20 inches.

Weights in lbs.	Deflections in parts of an inch.	Returned to, (weights taken off.)
11056	·19	0
11746	·20	0
12436	·23	+
13126	·24	+
13816	·25	·03
14506	·27	·03
15196	·29	·04
15886	·31	
16576	·32	
18592	·36	
19600	·40	
20608	·42	
21616	·45	
22624	·49	·13
23128	·50	
23632	·52	
24136	·53	
24640	·55	

25144 with this it broke. It is doubtful whether by tension or compression, a crack shewing a wedge which *broke out afterwards*, and of which *a b c d* is the form.



ac = length of wedge = 4.2 inches.

bd = depth of wedge = 1.7 „

25144 lbs. = 11 tons 4¼ cwt. = breaking weight.

Hence strength per square inch of section = $\frac{25144}{7.2}$ = 3492 lbs.

To compare this with the results from the common beam, we will take the mean between those in experiments 4 and 34, they being both supposed to be from the same sort of iron, and the only ones that were cast on their sides. Experiment 4, gave 2584 lbs., and experiment 31, 3009 lbs. per inch : mean = 2796.

$\therefore$ 3492 − 2796 = 696 = excess.

Hence saving in metal, from section, = $\frac{696}{3492} = \frac{1}{5}$ nearly.

If we compare this beam, by weight, with the mean weights derived from experiments 4 and 34; since in the former, 40½ lbs. bore 8270, and in the latter, 36½ bore 8792; taking the sums, 77 lbs. bore 17062 lbs.

$\therefore$ 77 : 81 lbs. (the weight of this beam) :: 17062 : 17948 lbs. = weight it should have borne; but it did bear 25144, $\therefore$ 25144 − 17948 = 7196 = excess.

Hence saving in metal from section and ends = $\frac{7196}{25144}$ = .286.

XXIX. EXPERIMENT.

Distance between supports 9 feet, depth of beam $5\frac{1}{8}$ inches.

Weight of beam = $170\frac{1}{2}$ lbs.

Dimensions of section in inches, (fig. to experiment 28.)

Area of top rib = 2.2 × .36 = .79

Area of bottom rib = 7.0 × .69 = 4.83.

Thickness at A = .27

„ at B = .33

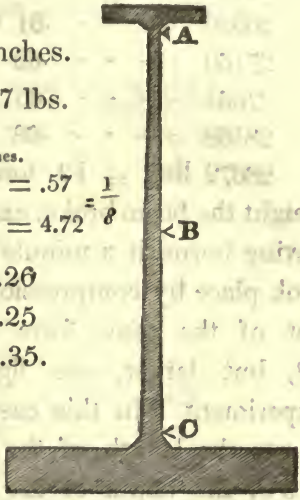
„ at C = .60.

Weights in lbs.	Deflections in inches.	Returned to, (weights taken off.)
8296	1·00	·15
8986	1·12	
9676	1·27	
10366	1·45	

11056 = 4 tons $18\frac{3}{4}$ cwt. With this weight it broke, by tension, 9 inches from the middle of the beam, where there were two small defects in the lower part of the bottom rib: the sectional area of the whole defective part being about $\frac{1}{7}$th of an inch. This experiment is therefore imperfect.

XXX. EXPERIMENT.

Distance between supports 9 feet.



Depth of beam $10\frac{1}{4}$ inches.

Weight of beam 227 lbs.

Dimensions of section in inches.

Area of top rib = 2.1 × .27 = .57

„ bottom rib = 6.14 × .77 = 4.72

$= \frac{1}{8}$

Thickness at A, .26

„ at B, .25

„ at C, .35.

Weights in lbs.	Deflections in parts of an inch.	Returned to, (weights taken off.)
11056	·23	0
12436	·24	0
13816	·26	+
15196	·30	+
16576	·34	·03
18592	·37	·05
19600	·40	·05
20608	·43	
21616	·46	
22624	·49	·06
23632	·51	
24640	·54	·07

Weights in lbs.	Deflections in parts of an inch.	Returned to, (weights taken off.)
25648 - - - -	·55	
26152 - - - -	·58 - - - -	07
26656 - - - -	·61	top of beam a little twisted
27160 - - - -	·62	,,
27664 - - - -	·65	,,
28168 - - - -	·68	,,

28672 lbs. = 12 tons 16 cwt. With this weight the beam broke, exactly in the middle, after having borne it a minute or more. The fracture took place by compression, a wedge being broke out of the same form as that in experiment 28, but larger, see figure of beam in that experiment. In this case,

a c, the length of the wedge, = 13 inches.
b d, the depth of the wedge, = 5.8 ,,

The length of the wedge half way down it was usually longer than at the top; in this case it was 14 inches there, or one inch longer.

XXXI. EXPERIMENT.

Distance between supports, 4 feet 6 inches.
Depth of beam, 5.1 inches.
Weight of beam, 88 lbs.

Dimensions of section in inches, (fig. experiment 28.)

Area of top rib = 2.15 × .24 = .52
Area of bottom rib = 7.60 × .72 = 5.472.

Thickness at A = .27
„ at B = .44
„ at C = .48.
Area of section = 7.90 inches.

Weights. in lbs.	Deflections in parts of an inch.	Returned to, (weights taken off.)
10017	·17	0
10707	·19	0
11397	·21	0
12087	·22	0
12777	·25	+
13561	·25	+
14345	·27	+
15129	·27	·03
15913	·28	·03
16697	·30	·03
17481	·32	·03
„	·33	·04
18592	·35	·07
19600	·36	
20608	·37	·07
21616	·40	
22624	·42	
23632	·47	·09
24640	·52	
25648	·53	
26152	·55	·14
26656	,56	
27664	·58	

28168 lbs. = 12 tons 11¼ cwt. It broke in the middle by tension with this weight, after having borne it about a minute.

Hence strength per inch of section $= \frac{28168}{7.9} =$ 3565 lbs. To compare this with the results from the beam of common form; the mean from experiments 4 and 34, gives 2796 lbs. per inch, (see end of experiment 28).

$$\therefore 3565 - 2796 = 769.$$

Hence saving in metal from section $= \frac{769}{3565} =$ .215 lbs.

If we compare the weights of the same beams, as in experiment 28, the saving will be .307, or upwards of $\frac{3}{10}$ of the metal; which is the whole saving both from the section and the form of the beam near its ends.

XXXII. EXPERIMENT.

Distance between supports, 9 feet.
Depth of beam, $5\frac{1}{8}$ inches.
Weight of beam, 192 lbs.

Dimensions of section, (fig. experiment 28.)

Area of top rib $2.25 \times .3 = .67$ inches.
Area of bottom rib $7.7 \times .76 = 5.85$ „
Thickness at A = .36
„ at B = .42
„ at C = .50.

Weights in lbs.	Deflections in inches.	Returned to, (weights taken off.)
8296	.90	.07
8986	.96	.12
9676	1.05	.15
11056	1.30	
12436	1.52	
13816	1.84	.46
14506	2.04	

15196 lbs. = 6 tons 15¾ cwt. With this it broke in the middle, throwing out a wedge, as in figure second to experiment 28.

Here *a c*, the length of the wedge = 6.9 inches
b d, its depth = 2.25 „

XXXIII. EXPERIMENT.

Distance between supports, 9 feet. Depth of beam, 10¼ inches.

Weight of beam, 244 lbs.

Dimensions of section, (fig. experiment 30.)

Area of top rib 2.2 × .33 = .73 inches.
Area of bottom rib 7.6 × .75 = 5.70 „
Thickness at A, .15
„ at B, .38
„ at C, .35

Weights in lbs.	Deflections in parts of an inch.	Returned to, (weights taken off.)
12436	·22	0
13816	·24	0
17584	·29	0
18592	·32	0

Weights in lbs.	Deflections in parts of an inch.	Returned to, (weights taken off.)
19600	·35	+
,,	·35	·03
20608	,,	·04
21616	·37	·04
22624	·40	·04
23632	·42	·05
24640	·47	·06
25648	·48	
26656	·50	·10
27664	·54	
28672	·55	
29680	·58	·13
30184	·64	
30688	·65	
31192	·70	·15
31696	·76	

32200 lbs. = 14 tons $7\frac{1}{2}$ cwt. With this it broke in about half-a-minute, a wedge separating from it as before. The length of the wedge was 18 inches, and its depth 6.15 inches. This wedge was of the same form as that in experiment 28, but not quite as well defined; approaching to the form of that in experiment 19.

61. The beam had twisted a little, by the last two or three weights, in a serpentine manner through its whole length; which shews that in so deep and thin a beam, the top rib

(2.2 inches broad in the middle, and tapering to about half that width near the ends) was as narrow as was admissible to support the beam.

XXXIV. EXPERIMENT.

Beam of common form, from the same model as before, and cast on its side for comparison.

Distance between supports, 4 feet 6 inches.

Depth of beam in its middle, $5\frac{1}{8}$ inches.

Weight of beam, $36\frac{1}{2}$ lbs.

Dimensions of section in inches.

Thickness at A = .27
,, at B = .40
,, at C = .44
FE = 2.27
DE = .46

Area of section = 2.921 inches.

Weights in lbs.	Deflections in parts of an inch.	Returned to, (weights taken off.)
2078		0
4148	.21	0
4493	.23	0
4838	.24	+
5183	.26	.04
5528	.27	
5873	.29	
6218	.31	
6563	.33	
6908	.35	.05

Weights.	Deflections.	Returned to.
7253	.37	.05
7598	.38	
7943	.40	
8288	.42	
8540	.44	.06

8792 lbs. = 3 tons $18\frac{1}{2}$ cwt. With this it broke after bearing the weight half-a-minute.

The form of fracture was nearly that of *b n r* in figure 2 to experiment 4. The distance of *n* from the top was = 2.3 inches, and of *r* = .8 inches.

Hence $\frac{8792}{2.921} = 3009$ lbs. per square inch of section.

The beam in experiment 4, supposed from the same iron, and the only one cast on its side like this, did but bear 2584 per inch. Taking the mean of the two we have $\frac{3009 + 2584}{2}$ = 2796 lbs. per inch.

XXXV. EXPERIMENT.

A beam of the *common* form, and from the same model and iron, cast *erect*, as usual.

Distance between supports, 4 feet 6 inches.

Depth of beam in its middle, $5\frac{1}{8}$ inches.

Weight of beam, 37 lbs.

Dimensions of section in inches (fig. last experiment.)

Thickness at A = .27
„ at B = .355
„ at C = .43.

D E = 2.26
F E = .47.

Area of section = 2.837 inches.

Weights in lbs.	Deflections in parts of an inch.	Returned to, (weights taken off.)
2078	·09	0
4148	·18	0
4493	·20	+
4838	·23	+
5183	·25	+
5528	·27	·02
5873	·29	·03
6218	·32	·05
6563	·35	·06
6908	·38	·06
7253	·39	·06
7943	·42	
8288	·44	
8540	·45	·08
8792	·46	

9044 lbs. = 4 tons, ¾ cwt. It bore this weight about half-a-minute, and then broke in the middle, by tension, as in every other instance in these beams.

Form of fracture nearly same as in figure to experiment 4.

Distance of point n from top = 2.00 inches.

" r " = .80 "

From this beam we have $\frac{9044}{2.837} = 3188$ lbs. per square inch of section.

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62. In the preceding experiments, there ought, according to supposition (art. 59), to have been an equality of strength between the beams in experiments 28 and 30, and those in 31 and 33; there was however a difference, in both cases, of about $\frac{1}{8}$th of what the larger beam bore, that beam being the stronger.

Experiment 29 was defective, but experiment 32, where the object was the same, and which ought to have given a strength equal to half that in experiment 31 or 33, indicated, as it ought, a breaking weight, whose double was somewhere between what was given by them. The discrepances, in the strength of the beams in the four experiments first named above, are considerable; but not so great as to render it necessary to seek for any other law. They may moreover, perhaps, be accounted for by the ways in which the fractures took place; three out of the four beams having broke by the separation of a wedge; and consequently by the rupture of a part, whose strength was not so well proportioned as that of the bottom rib, which must have been torn asunder first, if the beams had broke by tension.

The superior thickness of the vertical part near the bottom rib prevented, as was intended, the fracture taking place as in experiment 19, by the breaking out of a wedge near the neutral line; but it was only that, as in experiment 28, a wedge of a less size, and of the same form, might break out higher up, where the vertical part was thinner. It seems then probable, from these experiments, that no advantage would accrue from making the vertical part of unequal thickness.

Comparing the weights of the beams in experiments 32 and 33, with the loads they bore, we see that a great increase of strength may be obtained through a small additional weight of metal, when the depth can be increased.*

* It is not always advisable to increase the depth of a beam, when we have the means of doing it; as it would lessen its flexibility, and render it liable to be broke by percussion, through weights falling upon it. Some experiments on the resistance of beams, to impulsive forces, which I commenced on a large scale some time ago, through the liberal views of Messrs. Fairbairn and Lillie, may probably in an extended form be offered, at some future period, to the Public.

This circumstance, which was shown too by experiments 23 and 26, will enhance the value of the latter, and of experiments 30 and 33; especially as they are on a larger scale than any other we have made. We have not in these experiments been able to crush the top rib of the beam; though in both 30 and 33, the bottom rib was to the top one in a higher proportion than had been used in experiment 19, or in either of the former series of experiments; the ratio in experiment 33, being nearly that of 8 to 1. The bottom rib, in both instances, was rather too large for the thickness of the vertical part, as was evident from that part having failed the first. To have made the section then of equal strength every where; and consequently to have disposed the metal in the most economical manner possible (which has been one principal view in these enquiries), the vertical part of the beams should have been rendered a little thicker; and, as we have just seen, perhaps, made uniform. The top rib too, small as it was, would in both instances, possibly, have borne a reduction; only, that it would have rendered the beam liable to have twisted; a tendency which shewed itself in both experiments.

From these experiments, and those commencing with 23, it is evident that, with a given top and bottom rib and thickness of vertical part, we may often beneficially increase the depth of a beam, and that to a considerable extent; but doubtless, though we have had no experiments suitable to shew it, a less thickness of vertical part would have been required, if the depth of the beam were reduced, its length remaining the same. This matter has been touched on in article 33, (see also Robison's Mechanical Philosophy, vol. 1, article 390), and the experiments will throw additional light upon it; but its further consideration must be deferred to some future opportunity.

These experiments shew clearly too, that, in some of our earliest experiments, the weakness of the beams, considering the quantity of section, rose principally from the vertical part being too thick.

Before concluding these remarks, I may further mention, that in all cases where the beam has broke by the separation of a wedge, as in the preceding experiments, its vertical part should be rendered a little stronger, so as just to cause it to break by tension; to which case only, the following rules for the strength will properly apply.

63. We will now proceed to the practical application of the results from the beams I have experimented on generally; especially with respect to those that have been considered as the best.—It gives me great pleasure that they are likely to be adopted, and particularly that the commencement is in such a quarter: Mr. John Kennedy, who felt an interest in the experiments, and favoured me with his presence at many of them, informs me, that he shall employ the beams in some erections he is going to make: and Mr. Stephenson, I understand, will use them in a railway bridge crossing Water-street, Manchester.*

64. The investigations relative to the strength, given in the early part of this essay, are such as apply, and ought to be used, while the elasticity is perfect, or nearly so; but to find the ultimate strength, or that exerted at the time of fracture, in such forms of section as we have found to be the best, the experiments will supply us with the following much easier general theorem. A theorem which I think will be sufficiently simple and correct for all practical purposes.

* Since the above was written, Messrs. Fairbairn and Lillie have used them to a considerable extent, for Factories, Bridges, &c. in different parts of the Country

RULE FOR THE STRENGTH OF BEAMS.

65. Comparing the results of experiments 9, 11, 12, 19, 20 and 21, and allowing for difference of iron, as indicated by the beams of the common form cast with the others for comparison; I find that the strength is nearly in proportion to the size of the bottom rib or flange: a bottom rib of double size giving nearly, but not quite, double strength. And the subsequent experiments show the strength to be as the depth, every thing else being the same. Therefore in different beams, whose length is the same, the strength must be as their depths multiplied by the areas of a middle section of their bottom ribs: and where the lengths are different the strengths will be as this product divided by the lengths.

$$\therefore W = \frac{c\,a\,d}{l},$$

where $W =$ the breaking weight in the middle of the beam, $a =$ the area of a section of the bottom rib in the middle of the beam, $d =$ the depth of the beam there, $l =$ the length or distance between the supports, and $c =$ a quantity, nearly constant in our best forms of beams, and which will be supplied by any of the preceding experiments on them. But if used for other forms, as in our earlier ones, it will be best to deduce it from that beam, which most nearly resembles in section the beam we intend to obtain the strength of.

Cor. If, in different beams of these forms, the ratio of the length to the depth be the same, the strength will be as the area of a middle section of its bottom rib, c being a constant quantity.

66. In the preceding theorem, the quantity c would perhaps be nearly constant for sections of any particular forms such as we have used; but the rule, more strictly speaking, only applies where the whole tensile force exerted by the beam lies in its bottom rib; all the superior part being in some degree of compression, more or less: or exerting so little force by tension that it may be neglected in comparison with what was borne by the bottom rib or flange. This, in the earlier part of our experiments on beams was by no means the case; for in them the bottom flange was so small, that the lower extremity of the vertical part, between the flanges, must have exerted considerable tensile influence, in some cases; and in the earliest experiments, perhaps more than the bottom flange itself did. In the latter ones however, the bottom rib became so large, and the neutral

line was so low, as appeared by the wedge in experiment 19, that, $\frac{3}{4}$ of the depth of the beam was probably compressed.

The neutral line being, therefore, within little more than half an inch of the bottom flange in that experiment, there could have been but little there, besides that flange, submitted to tension; and that little, on account of its proximity to the neutral line, exerting scarcely any tensile influence.

Hence if the formula above be correct in the form of section in experiment 19, it must be very nearly so in forms approaching to it; and c in them nearly a constant quantity.

67. We will seek, by means of the formula, from each of the experiments, for the value of c, when constant; and, for that purpose, confining ourselves to those forms in which the section of the bottom rib in its middle is more than half the whole section of the beam, take the mean from among them all for c. Since then, from the formula, $W = \frac{cad}{l}$, $\therefore c = \frac{lW}{ad}$ $= \frac{l}{d} \times \frac{W}{a}$. If we take the dimensions in inches, we shall have, in many of the experiments, $\frac{l}{d} = \frac{54}{5\frac{1}{8}} = 10.5366$; in those $c = 10.5366 \times \frac{W}{a}$.

Taking then the breaking weight in cwts. we have :—

In Experiment 12,

$$c = 10.5366 \times \frac{W}{a} = 10.5366 \times \frac{149.37}{2.89} = 544.6$$

In Experiment 14, bottom rib too small.

In " 15,

$$c = 10.5366 \times \frac{W}{a} = 10.5\,\&c. \times \frac{150.9}{2.916} = 545.4$$

In Experiment 16, different iron.

In " 18,

$$c = 10.5366 \times \frac{W}{a} = 10.5\,\&c. \times \frac{173.58}{3.57} = 512.3$$

In Experiment 19,

$$c = \text{''} = 10.5\,\&c. \times \frac{232.89}{4.4} = 557.7$$

In Experiment 20,

$$c = \text{''} = 10.5\,\&c. \times \frac{207.58}{4.31} = 507.5$$

In Experiment 21,

$$c = \text{''} = 10.5\,\&c. \times \frac{187.58}{3.315} = 596.2$$

In Experiment 23,

$$c = \frac{lW}{ad} = \frac{84 \times 120.93}{4.44 \times 4.1} = 558.0$$

In Experiment 24,

$$c = \frac{lW}{ad} = \frac{84 \times 135.07}{4.62 \times 5.2} = 472.3$$

In Experiment 25, imperfect.

In " 26,

$$c = \frac{lW}{ad} = \frac{84 \times 198.08}{4.537 \times 6.93} = 529.2$$

The mean from all these values of c, to produce cwts. in beams cast *erect*, is $= 535.9$.

The preceding beams were all cast erect; those which follow were cast on their side, (see definition of erect and on their side immediately before the first experiment on beams.)

In expt. 28, $c = \frac{l}{d} \times \frac{W}{a} = 10.5366 \times \frac{224.5}{4.785} = 494.4$

In „ 29, imperfect.

In „ 30, $c = \frac{l}{d} \times \frac{W}{a} = 10.5366 \times \frac{256}{4.72} = 571.4$

In „ 31, $c =$ „ $= 10.5366 \times \frac{251.5}{5.472} = 484.3$

In „ 32, $c =$ „ $= 10.5\&c. \times 2 \times \frac{135.75}{5.85} = 489.0$

In „ 33, $c =$ „ $= 10.5\&c. \times \frac{287.5}{5.7} = 531.4$

The mean from these values of c, to produce cwts. in beams cast *on their sides*, is $= 514.1$.

68. We will now take an approximate view of what error there will accrue, in each of the experiments above, by adopting these mean values of c. Calling then 535.9 and 514.1, 536 and 514 respectively, we have as below.

	Experiment.	Real value of c, from last article.	Mean value for c Subtracted.	Difference.	Error in parts of breaking wts.
BEAMS CAST ERECT.	12	545	− 536	9	$\frac{1}{60}$
	15	545	„	9	$\frac{1}{60}$
	18	512	„	−24	$-\frac{1}{21}$
	19	558	„	22	$\frac{1}{25}$
	20	507	„	−29	$-\frac{1}{17}$
	21	596	„	60	$\frac{1}{10}$
	23	558	„	22	$\frac{1}{25}$
	24	472	„	−64	$-\frac{2}{15}$
	26	529	„	−7	$-\frac{1}{75}$

Experiment.		Real value of c from last article.	Mean value for c Subtracted.	Difference.	Error in parts of breaking wts.
BEAMS CAST ON THEIR SIDES.	28	494	-514	-20	$-\frac{1}{25}$
	30	571	„	57	$\frac{1}{10}$
	31	484	„	-30	$-\frac{1}{16}$
	32	489	„	-25	$-\frac{1}{19}$
	33	531	„	17	$\frac{1}{31}$

69. In the preceding table, the positive, or negative errors, show what portion of the breaking weight the beam bore, more or less than what would have been assigned to it by the formula, with the mean value, 536 or 514, of c. And we see that excepting in experiment 24, where the error was $\frac{2}{15}$, the positive errors never amounted to above $\frac{1}{10}$, nor the negative ones to more than $\frac{1}{16}$, and both were generally much smaller.

70. Since 536, or 514, is the mean value of c, to obtain the breaking weight in cwts. by the formula, according as the beam has been cast erect or on its side, one twentieth of the above numbers will be the value of c, to produce the breaking weight in tons.

$\therefore c = \frac{536}{20} = 26.8$ to obtain tons, in beams cast erect.

$c = \frac{514}{20} = 25.7$ to obtain tons, in beams cast on their side.

If, in these two last cases, we throw away the decimal, and call the value of $c = 26$, or 25, as the case may be, the negative error in the preceding table will be very small.

71. Hence we may in future take

$$\frac{26 \times a \times d}{l}, \text{ or } \frac{25 \times a \times d}{l},$$

(according as the beams were cast erect or on their sides), for the measure of the ultimate strength in tons, in our best forms of beams, and with the iron we used.

It would perhaps however have been better to have called the multipliers 26 and 24, as I conceive the iron from which the 25 was derived to have been somewhat stronger than that from which the 26 was obtained.

Example.—What weight laid on the middle of one of the main beams, in the rail-road bridge crossing Water-street, Manchester, would be required to break it, supposing it cast erect,* and of the same iron we have used; the dimensions from the model now constructing by Messrs. Fairbairn and Lillie being as follow:—

* The beams were cast on their sides; but there was a little additional matter in their vertical part, which would perhaps make up the small deficiency in strength, arising from that mode of casting.

Distance between supports 26 ft., or 312 ins.

Depth of beam, in middle, $27\frac{1}{2}$ inches.

Area of section of bottom rib, in middle, $16 \times 3 = 48$ inches.

Form of section, of beam, nearly the same as in experiment 30.

Referring to the formula we have, $l = 312$, $d = 27.5$, $a = 48$.

$\therefore$ W, the breaking weight, $= \frac{26 \times a \times d}{l} = \frac{26 \times 48 \times 27.5}{312} = 110$ tons.

These beams are intended to bear the same weight in every part; they will not however, be quite of uniform depth throughout. The load will have to lie upon their bottom rib, through its whole length; it becomes necessary therefore to make that rib somewhat broader, toward the ends, than according to the parabolic form described after experiment 13; and this enables the depth of the beams, near their ends, to be a little reduced.

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72. The views developed in this essay, if correct, must, as appears to the writer, have an influence on the forms of cast iron wherever it is intended for bearing purposes. In all the preceding experiments, the beams were designed to bear the same weight uniformly distributed over them; but were always, except otherwise

mentioned, broke by weights in the middle; the formula given above (art. 65) is therefore for this consideration. But if the weights were otherwise disposed, the beam would have to be modified, and the formula adapted to it, as in the following cases.—

73. Suppose the weight to be applied to one end, instead of the middle, and the beam fixed, by its other end, in a horizontal position in a wall.

It may be easily shewn, (Venturoli's Mechanics, vol. 2, article 551), that a beam, to be broke by a weight in its middle, will bear four times as much as another beam, of the same length and section, to be broke by a weight at its end. Now as the formula for the weight in the middle gave $W = \frac{c\,a\,d}{l}$, that for the weight at the end will give $W = \frac{c\,a\,d}{4\,l}$. Calling then the values of c (articles 70 and 71), 26 and 24, and taking $\frac{1}{4}$ of these numbers, we have, for the strength $W = \frac{6\frac{1}{2} \times a \times d}{l}$, or $= \frac{6 \times a \times d}{l}$; according as the beam was cast erect or on its side. The beam when the weight is at the end must, it is obvious, be turned the contrary way up that the larger rib, whose section is $= a$, may be the tensile one.

74. If the weight be applied at the end, and the strength of the beam be as the strain upon it. Putting c' for the co-efficient $6\frac{1}{2}$, or 6, just found, we have $W = \frac{c' a d}{l}$, where W and c' are constant, and consequently $a d$ varies as l. We may therefore make either a or d the variable quantity, but it will be more economical to make the depth d constant. If we do this, and make the thickness of the rib submitted to tension uniform, that rib will form a triangle whose vertex is at the end, where the weight is applied.—In like manner, where the weight is to be applied at the middle of the beam only, the stretched rib, then at the bottom, may be uniformly tapered from the middle to the ends, forming two triangles, instead of the parabolas before employed. In this case, the lines C A and C B (fig. page 470), and their correspondent ones on the other side, will be straight.

75. We might now point out other modifications in beams, and particularly those of steam engines; which, as appears from the experiments, should have a large equal rib or flange at top and bottom, with, perhaps, a thin solid sheet between them; differing, in the size of the ribs, only, from the form in figure 26, plate 4th of Tredgold's Essay on the strength of cast iron. This is for double engines; but for single

ones, the beam should have a large rib at top, and a small one at bottom, and be formed like that in the conclusion of the last article. But the further consideration of this matter would extend too far the limits of this paper; it would be well, however, if it were subjected to experiment, as that might tend to a reduction in the mass, and inertia, of these beams.

ULTIMATE DEFLECTION.

76. Having, in a variety of cases, obtained the strengths of beams and the laws on which they depend, we will next seek for the ultimate deflection in the different experiments. This may be done without much error, by supposing it proportional to the breaking weight, and comparing it with some other weight, whose deflection was taken; as for instance, the largest in each experiment.

BEAMS 4 feet 6 inches BETWEEN SUPPORTS, AND $5\frac{1}{8}$ inches DEEP.

In experiment 11, 13706 lbs. bent the beam .52 inch, and 14462 lbs. broke it.

$\therefore$ 13706 : 14462 :: .52 : .55 inch = ultimate deflection.

In experiment 12, .53 inch = ultimate deflection.

In experiment 21, 20617 lbs. bent the beam .54 inch, and 21009 lbs. broke it.

∴ 20617 : 21009 :: .54 : .55 inch = ultimate deflection.

In the preceding experiments, the height of the beams was *elliptical*; in all those that follow it was *uniform* throughout.

In experiment 15, 16401 lbs. bent the beam .53 inch, and 16905 lbs. broke it.

∴ 16401 : 16905 :: .53 : .55 inch = ultimate deflection.

In experiment 16, iron of different quality.

In experiment 18, 18265 lbs. bent the beam .43 inch, and 19441 lbs. broke it.

∴ 18265 : 19441 :: .43 : .46 inch = ultimate deflection.

In experiment 20, 22969 lbs. bent the beam .50 inch, and 23249 lbs. broke it.

∴ 22969 : 23249 :: .50 : .51 inch = ultimate deflection.

BEAMS 7 feet BETWEEN SUPPORTS.

In experiment 23, where the depth was 4.1 inches, 12815 lbs. bent the beam 1.08 inches, and 13543 lbs. broke it.

∴ 12815 : 13543 :: 1.08 : 1.14 inches = ultimate deflection.

In experiment 24, where the depth was 5.2 inches, 12087 lbs. bent the beam .63 inch, and 15129 lbs. broke it.

∴ 12087 : 15129 :: .63 : .79 inch = ultimate deflection.

In experiment 25, 6 inches was the depth and .58 inch = the ultimate deflection.

In experiment 26, where the depth was 6.93 inches, 21401 lbs. bent the beam .65 inch, and 22185 lbs. broke it.

∴ 21401 : 22185 :: .65 : .67 inches = ultimate deflection.

The preceding beams were all cast *erect;* those which follow were cast *on their sides.*

In experiment 28, where the distance of the supports was 4 feet 6 inches, and the depth $5\frac{1}{8}$ inches, 24640 lbs. bent the beam .55 inch, and 25144 lbs. broke it.

∴ 24640 : 25144 :: .55 : .56 inch = ultimate deflection.

In experiment 31, where the distance between the supports was 4 feet 6 inches, and depth 5.1 inches, 27664 lbs. bent the beam .58 inch, and 28168 lbs. broke it.

∴ 27664 : 28168 :: .58 : .59 inch = ultimate deflection.

In experiment 32, where the distance between the supports was 9 feet, and depth $5\frac{1}{8}$ inches, 14506 lbs. bent the beam 2.04 inches, and 15196 lbs. broke it.

$\therefore$ 14506 : 15196 :: 2.04 : 2.14 ins. = ultimate deflection.

In experiment 30, where the distance between the supports was 9 feet, and depth $10\frac{1}{4}$ inches, 26152 lbs. bent the beam .58 inch, without twisting, and 28672 lbs. broke it.

$\therefore$ 26152 : 28672 :: .58 : .64 inch = ultimate deflection, if the beam had not twisted.

In experiment 33, where the distance between the supports was 9 feet, and depth $10\frac{1}{4}$ inches, 29680 lbs. bent it .58 inch, without twisting, and 32200 lbs. broke it.

$\therefore$ 29680 : 32200 :: .58 : .63 inch = ultimate deflection, if it had not twisted.

The real deflection however, in both of the last experiments was greater than is given above, rising from the beam having twisted; as may be seen by referring to the experiments.

77. The ultimate deflections in the preceding experiments are, as might be expected, rather anomalous; but to obtain some general conclusions from them, we will confine ourselves to those beams which were of uniform depth throughout, seeking:—

1st. To find the ultimate deflection in terms of the depth. If we take experiments 23 and 26, which were the extremes of a series where the beams differed only in depth, we shall find, that in these, the deflections are inversely as the depths; for, the products of the depths and deflections were equal in the two experiments, since $4.1 \times 1.14 = 6.93 \times .67$, very nearly.

This result seems to be natural, for we may expect that iron of the same quality would stretch the same portion of its length before fracture; and, consequently, the ultimate deflection would be inversely as the distance of the neutral line from the bottom of the beam: and this distance must in the case before us be nearly as the depth.

2nd.—For the ultimate deflection in terms of the length. The mean deflection of the beams in experiments 15, 18, and 20, (in which the span was 4 feet 6 inches, and depth $5\frac{1}{8}$ inches), was .51 inch; and in experiment 24 (where the span was 7 feet, and the depth 5.2 inches, nearly same as the others), the deflection was .79 inch. Hence the ultimate deflection in these was simply as the length; for 4 feet 6 inches : 7 feet :: .51 : .79 inch.

Supposing the ultimate deflections to be inversely as the depth when the length is the same, if we reduce the deflections in experiments 23 and 26, to what they would have been if the depth had been $5\frac{1}{8}$ inches, the deflections from both these would have been .91; for $\frac{1.14 \times 4.1}{5\frac{1}{8}} = .91$ and $\frac{6.93 \times .67}{5\frac{1}{8}} = .91$. The lengths of the beams here being 7 feet, as in experiment 24, the deflections are about $\frac{1}{7}$ higher than .71, the quantity that we have just found they should have had, if the deflections had been as the lengths. Comparing likewise the length and deflection in experiment 28, or 31, with those in experiment 32, where the depth was the same, we find that double the length gave there more than three times the deflection.

From these different experiments we find, that the ultimate deflections are in a higher ratio than as the lengths, but are not as the square of the lengths, as is generally assumed.

3rd.—The ultimate deflections, we see, are in a ratio somewhat higher than as the length; and comparing those in experiments 30 and 33, with that in experiment 32, they appear sometimes to increase faster than the depths decrease. If, however, the ultimate deflections were directly as the length and inversely as the depth, or were higher than in both of these ratios in an equal degree, we should conclude that a beam of double length and depth of a given one would ultimately be deflected the same quantity as it. To see how this accords with the experiments, we will take the short beams, in experiments 28 and 31, and compare their deflections with those from the beams of double their length and depth in experiments 30 and 33; the ultimate deflections from the small beams were .56 and .59 inch respectively, and those from the large ones were .64 and .63 inch. Whence it appears, that the deflections were nearly, but not precisely, equal; there being in both cases a deflection, somewhat greater in the larger beam.

PROBLEM.

78. Suppose a beam supported at its ends, by two props under A and B; and so formed that it would just break with the same weight W on the middle, or any other part, between the supports. If then the prop be taken from under B, and placed under any other point C, what weight W′ laid on, half way between A and C, would be required to break the beam?

If the beam had been uniform, the strength would have been increased in the inverse ratio of the distances between the supports; and we should have had $W' = \frac{W \times AB}{AC}$; but, supposing m and n to be the middles of AB and AC, the strength of the beam is decreased toward the ends (page 470) in the proportion of $Am \times Bm$ to $An \times Bn$. If then the above value be reduced in this proportion, we have $W' = \frac{W \times AB}{AC} \times \frac{An \times Bn}{Am \times Bm}$; but $\frac{AB}{AC} = \frac{Am}{An}$, and $Bm = Am$, $\therefore W' = \frac{W \times Am \times An \times Bn}{An \times Am^2} = \frac{W \times Bn}{Am}$.

Cor. If $AC = \frac{AB}{2}$, $An = \frac{Am}{2}$, and $Bn = 3\,An = \frac{3}{2}\,Am$. Whence $W' = \frac{W \times \frac{3}{2} Am}{Am} = \frac{3}{2}W$.

The conclusion in this Corollary was made use of (pages 475-6 and 494) to ascertain whether the beams were properly proportioned, to bear the same weight toward the ends as in the middle; and consequently a load laid uniformly over them.

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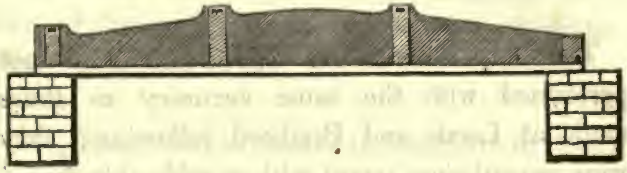
79. The following experiments are on a large scale and adapted to practice; they were made on beams of the usual form, by Messrs. Fairbairn and Lillie, from their models, at different places, where they happened to be supplying beams; and were superintended by Mr. Fair-

bairn. The weights were laid on, without the intervention of a lever, and suspended from a saddle on the center of each beam, where the deflections were taken. The figure below may be considered as a side view of the beams, the ribs at bottom of which were, as usual, uniform.



EXPERIMENTS.

80. 1st. Beam.—Distance between the supports, 14 feet.

Depth of beam in the middle, 15 inches.

Depth of beam near the ends, $9\frac{1}{2}$,,

Weight of beam (taken from the average weights of several beams from the same model), 7 cwt. 3 qrs. 20 lbs.

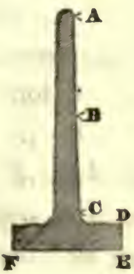
Dimensions of section.

Thickness at A = $\frac{5}{8}$ inch,

,, at C = 1 ,,

D E = 1 ,,

F E = 5 inches.



Weights.	Deflections in parts of an inch.	Remarks.
4 tons 10 cwt. - - -	0.21	
6 ,, - - - - - -	.278	
10 ,, - - - - - - -	.48 -	A little warped.
11 ,, 4 cwt. - -	.537 -	The top edge of beam pressed considerably out of perpendicular.
12 ,, 10 ,, - - -	.665 -	The pressure outward much increased, and danger of breaking.

The experiments on this beam were not performed with the same accuracy as those made at Leeds and Bradford following; they were nevertheless tested with considerable care.

81. Experiments made at Leeds in the month of November, 1824.

2nd. Beam.—Distance between supports, 16 feet.

Depth of beam in middle, 15 inches.

Depth of beam near ends, 10 ,,

Dimensions of section in inches, (see last experiment.)

Thickness at A = $\frac{7}{8}$,

,, ,, C = $1\frac{3}{8}$,

D E = $1\frac{3}{8}$,

F E = 6.

Weights.	Deflections in parts of an inch.
6 tons - - - - - -	0.2
8 ,, 5 cwt. - - - - - -	.28
11 ,, - - - - - - - - -	.4
13 ,, 5 cwt. - - - - - -	.475
16 ,, - - - - - - - - -	.55

Weights.	Deflections in parts of an inch.
18 tons 5 cwt. - - - - -	.675
21 „ - - - - - - - - -	.85

23 „ It broke with this after sustaining the weight two hours.

There were four other beams broken; they were however more or less imperfect, and broke two or three feet from the center, where the flaws happened to be.

82. Experiments made at Bradford in 1825. 3rd. Beam.—Distance between supports, 20 feet 9 inches.

Depth of beam in middle, 18 inches.

Depth of beam near the ends, 11½ „

Dimensions of section in inches.

Thickness at A = 1,

„ „ C = 1½,

D E = 1½,

F E = 6.

Weights in tons.	Deflections in inches.
13 - - - - -	1.16
18 - - - - - - -	1.25

19 with this it broke, after sustaining the weight some time.

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83. We have seen (experiments 1 to 6) that beams of this form of section will bear a greater weight without breaking, than those that have

been offered as an improvement upon them. The form of the common beams has, as might be expected, undergone modifications and improvements since their introduction, which was I believe by Messrs. Boulton and Watt; and they, I understand, were the earliest to apply them to render buildings fire-proof, which was first done by them in 1800, at Messrs. Philips and Lee's Cotton mill, in Salford.

84. We may therefore consider the beams experimented on by Mr. Fairbairn, as a sample of pretty nearly the strongest in section that have hitherto been in use; but if we suppose other beams made of the same length, depth, and quantity of section as these (the section being formed as in experiment 19, where upwards of $\frac{2}{3}$ of its area was in the bottom rib); supposing moreover the strength of the imagined beams to be estimated by the rule in article 71, it would indicate a considerable increase of strength above what is given by the common beams above; leaving a gain indeed much higher than we found in our experiments (19 to 21), arising, perhaps, from some difference in the metal in the two cases.

APPENDIX

TO THE PAPER

ON THE

CHAIN BRIDGE AT BROUGHTON,

(See Page 384,)

BY THE AUTHOR.

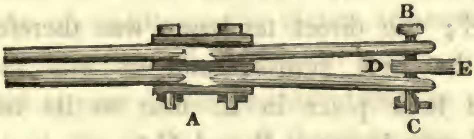
THIS bridge, since the paper on it in the present volume was printed off, has unfortunately broken down; the failure however may afford very useful instruction to future Architects.

A full account of the circumstances appeared at the time (April last), in the Manchester Guardian and Chronicle Newspapers, and was copied from them, chiefly from the former, into the Philosophical Magazine for May: still it may not be improper in this place to give a short account of the fracture and its cause, with some remarks on the structure of the main chain, (in the extremity of which under ground the fracture took place); referring for other details to the able account in the Magazine.

The accident happened through the vibration caused in the structure, by the marching of a troop of Soldiers over it. They were four a-breast, and about 60 of them on the bridge, the foremost being half way across, when they heard a tremendous crash like a continuous discharge of musketry, and in a moment one side of the bridge sunk down, sloping into the river, dragging after it the main pillar, which they had passed on their right, with the stone to which it was attached, and throwing every one upon the bridge into the river or among the chains. Some of the men escaped unhurt, several were injured, but fortunately no lives were lost.

Immediately after the accident I went down, at the request of the parties connected with the bridge, to ascertain the cause of the failure. I found that the principal fracture had taken place in the main chain, near to its termination under ground, and where it appeared there was an error in its construction, which rendered it much weaker than any where else, though being under ground the defect was not previously visible.

The chain, which has been partially described in page 385, is of malleable iron, and is formed, one on each side of the bridge, of a double row of straight round bars, 2 inches in diameter, and about 5 feet long each. These bars, which we may call its links, have eyes in each of their ends to admit of cross bolts 2 inches diameter; and to form a connection between any two of these links and another succeeding pair, similarly formed, there are three small equal square links passing round the ends and middle of the cross bolts.



This mode of coupling, which is represented by the joint A in the figure above, and is a rude imitation of that of the Menai Bridge, is used through the whole chain, except in the last joint near to its termination in the masonry. At this last place, however, instead of the three small links, as at A, there was a single link, D E, of about equal strength to all the three, $3\frac{1}{2}$ inches broad; this link was fastened at the end E to a large plate of iron at the back of the masonry, and was connected at D by the bolt B C to the two main links of the chain represented above and below D, which it passed between, and the centers of which links were there 5¼ inches asunder. The bolts in the joint A could scarcely give way except they were shorn across by the three small links, as they are supported both at their ends and middle, but in the other joint the bolt, which is of the same diameter as the preceding ones, was unsustained at its ends B and C and supported only at its middle. The links or bars too, being round, only acted at their centers on the bolts, and consequently with a leverage of somewhat more than an inch on each side of D E; the direct tendency was therefore to break the bolt across; and in this manner fracture took place in it, near to its middle, or half way between B and C.*

To estimate the strength of the bolt so acted upon, supposing it not strained beyond its elastic force. In this case, the extensions and compressions being equal from equal forces, the neutral line would be in the middle, and the formula for the strength (Memoirs, vol. 4, page 282, Cor.) is $\frac{.7854\, s d^3}{8\, l} = .7854\, s\, d^2 \times \frac{d}{8 l}$, where s = the absolute strength of the particles in a unity of section, d = the diameter, and l = the length. In this case $d = 2$, and $l = 1$, $\therefore$ strength = $.7854\, s\, d^2 \times \frac{2}{8}$; but $.7854\, s\, d^2$ is the

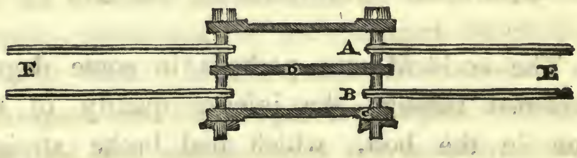
* The bolt B C is made too long, and D E too narrow, in the figure, but the mistake will create no wrong idea.

direct tensile strength of the bolt, its relative strength or the weight to break it across is therefore $\frac{1}{4}$ of that strength. Whence it appears that the bolt was but $\frac{1}{4}$ of the strength that it ought to have been to have resisted the full tension of the chain, with the eyes of its bars rounded and only acting at their centers; since the diameters of the bars and the bolt were the same.

We here see an absurd consequence of the theory of Galileo;—for if the strength of the bolt be estimated by that theory, the bolt would appear to be just as strong as the other parts of the chain; and therefore the failure of the structure may perhaps have arisen from the application of an erroneous theory.

The accident was perhaps in some degree hastened through the inferior quality of the iron in the bolt, which had broke straight across, presenting the granular aspect of cast iron; without any fibrous appearance, which is shown by good wrought iron. It appeared too, from the form of the fracture, that the surface of tension had been considerably larger than that of compression, as would have been the case, though not to be traced, in cast iron. It is probable, however, that the bolt was not much weakened through the inferiority of its metal, as the bolts, similarly situated to it, at the other three corners of the bridge, were all when taken out a good deal bent.

The joint A is of a description that may be very commonly used, and the proper proportions of its parts will differ according as the links tend to *bend* the bolt or to *shear* it *across*. In the former case the links and bars will not be in contact, or will act on the bolt in a portion only of their breadth, as at their centers; and if we reduce their thickness to that really acting upon the bolt, the action of the joint would be as in the figure below, where E and F represent portions of the bars.



If then the distance of leverage in the bolt, between the bars, be = twice that distance acting at each end of the bolt to bend it, or $AB = 2BC$, the small link D sustaining the middle of the bolt ought to be four times as strong as either of the small links acting at the ends of it. For, putting t for the tension exerted by one of the small side links, during the fracture of the bolt, $2t$ would be the tension in D required to break the bolt in the middle, if it were not sustained at its ends by the small side links; and therefore $4t$ would be required to break it in the middle, when it is sustained by them; since a beam firmly fixed at the ends will bear twice as much as one merely supported there. If the joint contain a greater number of bars than two, it is evident that all the internal small links must be equal in strength, and each four times as strong as one of the side links. As to the absolute strength of the small links, that ought to be regulated so that the sum of their sections shall be equal to the sections of the bars; and the thickness of the bolt must be such as to prevent the tension t, of either of the small side links, breaking the bolt across with its leverage.

If the tendency of the chain is to shear the bolt across, and not to act by leverage, the links and bars in the joint A must be in contact and act on the bolt through their whole breadth. The small link sustaining the middle of the bolt ought then to possess double the strength of either of the small side links; for it would have to shear the bolt through in two places, and a side link would have to cut it only in one.—This disposition of the joint is better than the other; for, though the sum of the strengths of the small links must be equal to that of the bars as stated before, a much smaller bolt will suffice; one of half the section of one of the bars might perhaps be sufficient, since to be broke it would have to be shorn four times through; and it is found by experiment to require a greater force to cut a body across, in this manner, than to tear it asunder longitudinally.

In the bridge in question the bolts are in some degree acted upon by leverage, and the small middle links are no stronger than the side ones; from these causes the joints A are somewhat weaker than the rest of the chain, the weakest part being in the middle of the joint, and where one of the small links was found long ago to have opened; a circumstance which was noticed in the additions to the paper on this Bridge, in the present volume.

The bridge is now opened again to the public, having been repaired by the engineer. It is to be regretted that the repairs have been too restricted and economical; but great care has been taken to remove any defective links, and the ends of the chain have been considerably strengthened. It seems not to have been convenient to change the form of the objectionable joints; but bolts of more than three times the strength (3 inch instead of 2 inch ones) have been substituted for the others, and connected so as scarcely to have any tendency to be bent by leverage.

The bridge, as it was, had stood some years, daily crossed by carts, waggons, &c.; and the Royal Artillery, when in Manchester, had, I understand, regularly crossed it with their horses, guns, &c. when passing to and from Kersal Moor. The joint that broke, and the others like it, were doubtless much the weakest part; and as these are now several times as strong as before, it is probable the bridge will bear any weight to which it is likely to be exposed, avoiding great vibration. But when heavy weights are on structures of this description, care should always be taken to prevent vibration; which, if considerable, offers, perhaps, one of the most serious trials to which they can be exposed. The effects of vibration on such structures do not seem to have been sufficiently considered; and the only experiments on it, which I have seen, are the interesting ones, by Mr. Telford, (Barlow's Essay, on the strength of Timber, Appendix.)

October, 1831. E. H.

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